Discovery as invention: a constructivist alternative to the classic science documentary.

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ABSTRACT

This thesis is a practice-led exploration of how science is represented in the documentary film. The practice part is a science documentary that deliberately eschews a number of key stylistic elements common to what may be called the 'classic' science documentary. The aim is not to arbitrarily restrict the filmmaker but rather to explore what sort of a documentary of science might be possible in the absence of certain features that, on the face of it at least, appear to be predicated on an out-dated positivism.

The film, *Hopeful Monsters: An Experiment,* is not in itself an argument for these post-positivist ideas but an experiment that implicitly critiques the philosophical underpinning of the classic science programme. This written dissertation is designed, therefore, to make that critique explicit. It demonstrates, first, how the classic science documentary is indeed informed by an outdated view of the nature of science —the so-called 'received view'—and second, it develops an alternative, 'constructivist' view of science in light of which the film, *Hopeful Monsters* is evaluated.

The dissertation concludes that in its combination of documentary modes and its inconclusive narrative structure, *Hopeful Monsters*, succeeds in representing science and the scientific-self as distinctly different from the representation of science in the classic science documentary. Furthermore, this alternative representation is indeed consonant with a post-positivist, 'constructivist' view of the nature of scientific practice and of the experience of the scientist in carrying out his or her work.

CONTENTS

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INTRODUCTION

This thesis is an exploration, both practical and theoretical, of the possibilities of the science documentary. It comprises two parts. The first is an experimental science documentary—my film, *Hopeful Monsters: an Experiment*—and the second is this written dissertation which explores a number of broadly philosophical issues that emerged during the making of the film. Taken together, the film and the dissertation argue two main points: (a) that the 'classic' science documentary represents an out-dated view of the nature of science and (b) that a more current view of that nature may only be represented by developing a different approach to the classic form. The dissertation concludes that *Hopeful Monsters* exhibits some important features of this alternative form.

Background to the issues

In 2009 a number of emails were stolen from the Climate Research Unit at the University of East Anglia. These messages supposedly revealed a scientific scandal. Climate scientists were stifling doubts and hiding inconvenient evidence that would otherwise undermine confidence in the reality of global warming. Whatever the basis of these allegations, what is illustrative from the point of view of this thesis is *why* this story had the potential to be scandalous in the first place. The reason, I suggest, is because good science is commonly understood as apolitical and governed by selflessness and trust and it is also considered a strictly logical process against which it is easy therefore to distinguish error and fraud. The scientists of the Climate Research Unit are villains because they have been cast, not least by their own rhetoric, in a drama governed by this particular and, I will argue, problematic view of the nature of science.

The picture of science implicit in the criticisms of the Climate Research Unit is a familiar one that, as Carl Gardner and Robert M. Young write, 'is positivist in that it privileges scientific knowledge above other forms of inquiry and in that it separates facts from their contexts of meaning and represents them as above the battle of competing interest groups and classes.'[1] Dubbed the 'received view' by philosophers of science, this notion of science as a method for extracting true theories from the observation of nature, unencumbered by social influences, is widely evident in the discourse of scientists and commonly repeated in the media but it has been found wanting by philosophers and sociologists for over half a century.[2] Indeed, many recent scholars of science who take issue with the received view would understand what went on at the University of East Anglia as anything but scandalous; instead they would recognize it as part of normal scientific practice. They would argue that discarding or massaging observation data to fit, for example, a pre-determined theoretical curve, is common in science; indeed, some of the great scientific achievements, such as Millikan's discovery of the charge on the electron or Eddington's confirmation of Einstein's theory of relativity, are founded on just this sort of practice.[3] The problem with the emails from East Anglia is not that they reveal unscientific behaviour so much as that they expose the failure of science to conform to a philosophically and sociologically naïve view of scientists and scientific practice and of the nature of scientific knowledge.

That naïve idea of science—the 'received view'— also, of course, governs our common understanding of how the documentary film itself works to provide a 'window on reality' and so any critique of the received view also threatens to undermine the confidence we place in the documentary, to undermine, that is, the commonsense distinction we maintain between documentary and fiction. The film theorist, Michael Renov, for example, queries the notion that there are fundamental distinctions between documentary and fiction. Instead, he defines the documentary film as a discourse adept at 'selling rhetorical arguments as truths, visions of the world as objective accounts of history.'[4] Some filmmakers, such as Trinh T. Minh-ha, go so far as to deny the very existence of the thing we casually call 'the documentary.'[5]

These challenges to the epistemological warrant of the non-fiction film are at the heart of nearly all documentary film theory. However, in *practice*

[1] Gardner, C. and R.M. Young (1981) p. 177
[2] See Suppe, F. (1977) p. 3ff
[3] For Millikan see Holton, G. (1978), for Eddington see Niaz, M. (2009)
[4] Renov, M. (2004) p. 133
[5] Minh-ha, Trinh T. (1993) p. 90

such postmodern concerns have impinged relatively little on the production techniques or popular understanding of the vast majority of documentaries made for television. It seems that the possibility of knowing and stating things about the world *with certainty* is so important to us that, whatever philosophical misgivings we might have, we nonetheless need to believe in what science, and its offspring, the documentary film, appear to promise.

In 'Science on TV: A Critique' (1981), Gardner and Young examine, as they put it, the 'existing ideologies and conceptions of science which TV 'feeds off,' and, in doing so, deconstruct 'the various televisual styles and techniques usually regarded inside television as 'common sense', 'natural, and 'transparent.'[6] In considering the idea of science that my film, *Hopeful Monsters*, represents, this dissertation will pick up and expand on some of the ideas that Gardner and Young began to explore in 1981. There are, of course, other issues that might be important in a thesis on the science documentary such as the social and cultural formation of TV's practitioners, the specific labour process of television and the economic determinations governing television science but these are not relevant in the narrow context of considering *Hopeful Monsters* which was made outside of these potentially determining factors. For the same reason, I will not examine the representation of science by various news and current affairs programmes that are not primarily designated as 'scientific'.[7]

This dissertation, then, focuses primarily on exploring what conceptions of science are communicated by *Hopeful Monsters* and by the 'classic' TV science programme, and asks how these compare to philosophical and sociological developments in our understanding of the nature of science. The question of how science is represented in documentaries is important not just because TV science is how the majority of the general population 'gets' their scientific knowledge, but because, as Gardner and Young, and the cultural theorist Roger Silverstone agree, the representation of science on television is a key to making sense of *all* non-fiction programmes on TV. Television documentary science, writes Roger Silverstone, 'sets the paradigm' because it so

[6] Gardiner, C. and R. M. Young (1981) p. 171
[7] As of May 2010, this question is being explored on behalf of the BBC Trust by The Science Communication Group at Imperial College London of which I am a member.

clearly 'mediates between two dominating discourses: the specialist and the general.'[8]

It is perhaps surprising, then, that the science documentary *per se* has received relatively little attention by theorists since the 1980s. Recently, however, there has been some new activity in the field. Mark Wolf and Anneke Metz, for example, have both explored how computer imaging technology has led to the development of increasingly fiction-driven science documentaries in a mode they term 'the subjunctive' while José van Dijck, in somewhat similar vein, has examined the use of computer simulations, particularly in medical documentaries.[9] In addition, Tim Boon, Chief Curator at the Science Museum, London, has published a history of the science documentary in the UK titled *Films of Fact* (2008) which traces the story up to the launch of the first BBC *Horizon* programmes in the mid 1960s. But although these works are interesting and valuable, the most useful studies, for the purposes of this dissertation, remain Gardner and Young's 1981 article and a number of scholarly works by Roger Silverstone, specifically his 1984 paper, 'Narrative strategies in television science—a case study'; an ethnographic study, *Framing Science: the making of a BBC documentary* (1985) and a follow-up paper, 'The Agonistic Narratives of Television Science' (1986).[10]

In *Framing Science* Silverstone reports on his 'participant observation' of the making of a science documentary called 'A New Green Revolution?'. He argues that although the film production he chose to study is 'not typical', nevertheless his case is of general value: 'Every single film,' he writes, 'is unique in its content and is the product of the work of unique individuals working in unique situations and working creatively in them. But if no film is typical then every film is.' Having said this, Silverstone also acknowledges that every case study necessarily has limitations. For example in making 'A New Green Revolution?' the director never had to 'come to grips with the difficulties of presenting a detailed explanation of a complex piece of natural science to the lay television audience…' and so in his book Silverstone does not analyse how

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[8] Silverstone, R. (1986) p. 81
[9] Wolf, M. J. P. (1999); Metz, A. (2008); van Dijck (2005)
[10] Silverstone, R. (1984, 1985, 1986)

this might be done.[11] Each case study can only deliver a partial perspective on the kinds of issues that face the makers and viewers of science documentaries. However, one issue that is necessarily explored, at least implicitly, in *all* cases is how the nature of scientific practice and scientific knowledge are represented in the documentary film. In the films Silverstone analyses science is represented according to the received view and the formal characteristics Silverstone identifies in these science documentaries match those of what the documentary theorist Bill Nichols calls the 'expository mode', a format that continues to be popular among producers of science programmes today.[12] It is central to this dissertation that the received view and an expository presentation seem to constitute defining characteristics of the 'classic' science documentary.

The Received View

Ian Hacking sets out the key features of the received view in his introduction to *Scientific Revolutions* (1981). Among them are the following: science is a set of statements with a logical structure that can be tested empirically by the observation of reality (any other sorts of statements are ruled inadmissible as science); scientific knowledge is objective, that is to say, transcultural and independent of the personality or social position of the scientist and is thereby different from other kinds of belief; scientific knowledge is cumulative, each new correct theory expands our scientific knowledge and merges seamlessly with earlier true theories; and thus, in principle, there is but one science that underlies all scientific disciplines, one way of understanding a singular reality.[13]

Such a view is well-matched by the 'expository mode' of the classic science documentary which commonly takes the form of an illustrated lecture. The classic science documentary, however, is distinct from a lecture in that the lecturer is generally unseen, elevated to a so-called 'voice-of-God' that commands authority. In such films, this voice tells a story or develops an argument in the soundtrack and supports this argument by evidence from interviewees and illustrations of one sort or another. This style of presentation is entirely appropriate to the received view of science as a method for

[11] Silverstone, R. (1985) p. 162
[12] See Nichols, B. (1991) pp. 34-38
[13] Hacking, I. (1981)

discovering laws of nature that are true in all circumstances, transcending the limits of individual experiments and observations. The expository mode, in reflecting this view, purveys reassuring certainties to the viewer.

As I will explore more fully in chapter three, the received view has come under scrutiny from a number of directions. Over half a century ago, philosophers of science found they were struggling to specify a satisfactory and robust principle by which scientific statements could be empirically verified; and since the 1970s, ethnomethodologists and other scholars of 'science studies', interested in the actual practices of science have become sceptical that there is, after all, a single method shared by all scientists in all disciplines. An alternative view about science has gradually come into focus that pictures scientific knowledge as resulting from a messier 'political' process of knowledge-manufacture in which persuasion rather than proof is the name of the game.

'What has been involved,' writes Roger Silverstone, 'is a deconstruction: a radical redefinition of both science and…the factual media, as social products, a radical reconstruction of them as discourses—relative, indeterminate, ideological, whose relationship to something called reality, truth, facts is at the very least bracketed, at the most totally denied.'[14] Silverstone perhaps overstates the case but certainly many philosophers, sociologists and others have come to the conclusion that science may not be governed by an epistemology that distinguishes between science and pseudo-science or between science and metaphysics on a basis that is as sound as the received view would have us believe.

Taken together and worked through, these recent doubts about the apparent objectivity and simplicity of the scientific 'method' and the consequent safety of scientific knowledge have given rise to an alternative view of how science gets done that is known as social epistemology or constructivism. In this view, a new scientific theory, especially one that challenges accepted wisdom, is freighted with political significance for members of the scientific community and its reception is understood by many philosophers and

[14] Silverstone, R. (1989) p.187

sociologists of science studies to be not so much logically as *socially* determined.

Constructivism

The difference between the constructivist and received views of science is encapsulated in the distinction that the philosopher of science, Hans Reichenbach made in the 1920s between the so-called *context of discovery* and the *context of justification*.[15] The context of discovery, he argued, is traditionally explored by historians, sociologists and psychologists who are interested in who made a scientific discovery and when and how, while, by contrast, the end point of a discovery is an intellectual product like a theory or a law and *that* emerges from the context of justification which is traditionally the province of epistemologists concerned with the stringency of testing, i.e., the soundness of methodologies and the logic of arguments. According to Reichenbach, how the scientist hits upon a good idea is not open to logical analysis but *testing* that theory is. Testing is a matter of timeless logic entirely sequestered from social influence, its historical and social context irrelevant.

By 1960, Reichenbach's distinction, so crucial to the received view, had been shown to be philosophically untenable and the received view had all but collapsed. Sustained criticism from philosophers of science such as Carl Gustav Hempel, Karl Popper and Thomas Kuhn had made it appear rooted in a mythical view of science. These philosophers argued against the received view by noting, for instance, that all observations of nature are necessarily impregnated with theoretical assumptions thus blurring Reichenbach's commonsense distinction between the contexts of 'discovery' and 'justification'.

Inspired, in particular, by Kuhn's *The Structure of Scientific Revolutions* (1962), a number of other scholars began to look at science afresh. These included the sociologists David Bloor and Michael Mulkay and the ethnomethodologists Karin Knorr-Cetina and Bruno Latour whose ideas I will explore later.[16] Their field of 'science studies' covers a range of 'postmodern' approaches. Emblematic of these, as the critics Gross and Levitt write, is the treatment of science as a discursive practice and scientific texts as rhetorical

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[15] Reichenbach, H. (1938) chapter 1
[16] Kuhn, T. (1996 [1962])

devices by which scientists claim authority, plausibility, coherency and truth.[17] This means that scientific discoveries are to be understood not as discrete events with clear causes but as *categorizations* achieved by scientists pursuing particular aims in particular professional and social contexts. The outcome of this multi-faceted assault on the received view has been the development of a different conception of science. Called constructivism, this view reconceives discovery as *invention*; it understands scientific knowledge as *manufactured* within the institutions of science.

Some areas of documentary production, in particular ethnographic or anthropological filmmaking have responded to these philosophical challenges; indeed Jean Rouch's phrase 'shared anthropology' might be applied equally to the activities of scholars in science studies. Rouch writes: 'the observer has left the ivory tower; his camera, tape recorder, and projector have driven him, by a strange road of initiation, to the heart of knowledge itself.'[18] But outside of ethnographic filmmaking, the documentary, including the 'science' documentary, remains relatively untouched by the philosophical scepticism of the constructivist view. The science documentary takes much the same form and expresses much the same attitude to science as it did thirty or more years ago when *Horizon* was first broadcast on the BBC.

The aim of this thesis, then, is to explore first in practice (the film *Hopeful Monsters*) and then in theory (this written dissertation), how a science documentary informed by a constructivist epistemology might differ from the familiar, 'classic' form that is predicated on what I will argue is a philosophically naïve view of science. My thesis, therefore, fits beneath the combined umbrellas of science studies and film studies as it sets out the rationale for this rethinking of the science documentary and lays out the terms by which we may ultimately evaluate the success of *Hopeful Monsters*.

The science

In January 2000, a radio programme of mine was broadcast in the 'Discovery' strand of the BBC World Service, a series of weekly half-hour science

[17] Gross, P. R. and Levitt, N. (1998)
[18] Rouch, J. (2003) p. 44

documentaries. My contribution explained the theory of evolution by symbiosis. In the course of researching the programme I had come across Donald Williamson of the Marine Biology Station of the University of Liverpool on the Isle of Man. He had contributed a chapter to a symposium report I was reading (*Organisms and the Origins of Self*, 1991) titled, 'Sequential Chimeras' and I was extremely struck by the highly unorthodox explanation he gave for how animals with larvae in their life histories evolved their multiphasic life cycles.[19]

A great many animal species have life-histories comprising multiple phenotypes at different stages of their life cycles. Each of these phenotypes is adapted to a different environment and is ecologically distinct from its other life-history stages. The larval form of a starfish, for example, is a relatively tiny organism that drifts or swims only feebly among the plankton, consuming diatoms or other micro-organisms but when it metamorphoses into a juvenile starfish it takes up a very different life on the sea floor where it grows to maturity.[20] This form of multiphasic life history is found in thousands of animal species and across most major lineages. We are all familiar with the tadpoles of frogs and toads, the caterpillars of butterflies and moths and there are numerous other examples from fish and marine invertebrates.[21]

The orthodox view of the evolution of multiphasic life histories is that they result from natural selection adapting the early and late stages of an animal's life cycle to different ecological conditions. Thus the larva of the starfish floats in the plankton, sifting microbes from the water and drifting to unexplored regions far from its parents while the adult phase, by contrast, has become adapted for eating crabs in a relatively small territory on the sea floor and reproducing. Classical theory argues that while evolution favours the divergence of the body-forms of each life cycle phase to optimise their fit to different ecologies, it also constrains that divergence so that the two forms do not become so dissimilar as to undermine the biological processes of metamorphosis.[22] The outcome of this combination of adaptation and constraint, as the biologists, Hart and Grosberg write, has 'produced an

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[19] Williamson, D. I. (1991)
[20] Emlet, Maslakova, Shanks and Young, (2009); Williamson, D. (1992 & 2003)
[21] See Ruppert E. (2004)
[22] Hart and Grosberg, (2009)

astonishing variety of similarities in larval form among species with different adults, differences among larvae that metamorphose into similar adults, and similarities between the larvae of some species and the adults of others.'[23] This can present a very confusing picture to the traditional taxonomist who must choose which phenotype—larval or adult—to use in deciding a species' correct place in the so-called tree of life.

The tree of life, at least as we know it, is largely Charles Darwin's idea. Faced with the problem of placing animals with larvae on his tree, Darwin argued that the larval form was less derived, 'closer' to the ancestral form of the animal than the adult phenotype, and should therefore be used for the purposes of classification:

> It is highly probable that with many animals the embryonic or larval stages show us, more or less completely, the condition of the progenitor of the whole group in its adult state. In the great class of the Crustacea, forms wonderfully distinct from each other, namely suctorial parasites, cirripedes, entomostracan, and even the malacostraca, appear at first as larvae under the naupliusform…It is probable that at some very remote period an independent adult animal, resembling the Nauplius, existed, and subsequently produced, along several divergent lines of descent, the above-named great Crustacean groups.[24]

Although an adult barnacle looks like a mollusc (and Cuvier thought it was), it is correctly classified as a crustacean because it has nauplius larvae. The so-called 'parasitic barnacle' or rhizocephalan (literally 'root head'), on the other hand, is a parasite of crabs (which are also crustaceans) and although it is termed a barnacle, it looks nothing like one. It neither resembles a mollusc nor any class of crustacean but it is nonetheless classified as a crustacean because it too has nauplius larvae.

This approach to taxonomy takes for granted the Darwinian view of speciation; it only makes sense if one assumes larvae are *ancestral* to adult forms, but Don Williamson, the scientist at the heart of my film, is unusual among biologists in being sceptical of this orthodox view. He does not believe larvae resemble the ancestral form of current adults with multiphasic life histories. He claims the nauplius larva of rhizocephalans was added to the life-

[23] *Ibid.* p. 1
[24] Darwin, C. *The Origin of Species*, 6th London edition, p. 437

history of the animal *after* the adult form had already evolved and that the source of the larva was probably the crab that the rhizocephalan parasitized. He argues that the genes coding for the nauplius larva were somehow added to the rhizocephalan's genome in one generation creating a multiphasic life-history that conferred an advantage over the ancestral rhizocephalan that has been retained by all descendants of this hybrid organism ever since. (The crab, in its turn, must also have already been a hybrid that had gained its nauplius larva from another source before somehow passing it to the rhizocephalan.)

The parasitic barnacles are one of literally hundreds of species whose life-histories, Williamson argues, are inexplicable in orthodox evolutionary theory and can only be adequately explained by what he calls his theory of larval transfer. Here are two other examples. First, the starfish *Luidia sarsii*. As an adult, this animal is *radially* symmetrical but as a larva its body-plan is *bilaterally* symmetrical, a profoundly different structure. The radial juvenile grows like a parasite within the very large bilateral larva (called a bipinnaria). When it is ready, it migrates through the outer wall of the bipinnaria and drops off to take up life crawling on the sea floor where it grows to adult maturity. Meanwhile, the larva goes on swimming in the water above and does not degenerate and die for many months. One can hardly say, argues Williamson, that in this case the larva *develops into* the adult and it is certainly difficult to understand how classical theory can account for the evolution of such a complex life history in which two individuals with entirely distinct basic body-plans (and, presumably, genetic programmes of development) hatch from the same egg.[25]

A second example also comes from the echinoderms but this time from a group of animals called brittle-stars. In this group the majority of species have a planktonic (bilateral) larval stage called a pluteus but a minority are without any larval stage and instead develop directly from egg into adult.[26] Brittle-star eggs (in common with many other animals) develop by first dividing until they have formed a hollow ball of cells, one layer thick, called a blastula. The blastula then invaginates so that this single layer of cells folds in on itself, like a finger pushed into the skin of a balloon, to form a two-layered structure called a gastrula which now has a hollow space inside that will later form the primitive

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[25] Williamson, D. I. (1987)
[26] Fell, H. Barraclough, (1941)

gut or archenteron. The point where the blastula invaginates—the mouth of the archenteron—is a hole called the blastopore. In those brittle-stars that develop directly into an adult, the blastopore eventually becomes the mouth of the adult animal but in those brittle-stars that develop first into a larva, the blastopore eventually becomes the anus of the larva. These two distinct forms of embryonic development are said to represent a profound division of the animal kingdom and taxonomists use the distinction to decide the basic relatedness of all animal species.[27] It is most bizarre therefore to find a class of animals that spans such a fundamental divide. Something is wrong, argues Williamson, with the classical idea that larvae are the key to adult taxonomy.

Williamson argues instead that the adult phenotype and the larval phenotype of all animals with larvae were once separate species, free-living individuals in their own right that came together only by chance at some point in the distant past.[28] His explanation of how the butterfly got its caterpillar can serve as an exemplary narrative. In the beginning, a butterfly (or its ancestor) grew from egg to adult without an intervening caterpillar stage while at the same time some caterpillar-like animal (e.g., an onycophoran like *Peripatus*) enjoyed a full life, breeding with other caterpillar-like animals to produce caterpillar-like offspring. Somehow these two unrelated organisms accidentally came together; the eggs of one were fertilised by the sperm of the other but rather than producing a blended (and infertile) mixture like a mule (or nothing viable at all), what resulted was a fertile hybrid. This hybrid, however, did not express its new mixture of genes simultaneously (like the mule) but *in sequence*. First one set of genes in the fertilised egg was activated giving rise to the caterpillar then the other set switched on and in a somewhat violent immune reaction orchestrated by this second set of chromosomes, the first animal (the caterpillar) was destroyed. At the same time, from just a few surviving stem cells or basic

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[27] Of animals with bilateral symmetry, those in which the blastopore becomes the mouth are called protostomes while those in which the blastopore becomes the anus are called deuterostomes. The echinoderms are radially symmetrical as adults but are all classed as deuterostomes because their larvae (when they have them) are bilateral deuterostomes. Humans (indeed all vertebrates) are also deuterostomes while molluscs and arthropods for example are protostomes.

[28] See Williamson, D. I. (1992, 2003, 2009); Williamson also speculates that the blastula stage may itself represent the addition of, in this case, an 'embryonic' stage, intercalated in the same fashion as the more strictly larval stages he discusses in his papers and books. The source of the blastula, a hollow ball of cells, could have been something like a colony of *Volvox*. (Personal communication)

structures, now under the influence of the second set of genes, the butterfly was constructed, completing the process we call metamorphosis—change of form.

Thus metamorphosis, in Williamson's theory, is not a switch from one phenotype to another within a species (the classical view) but from one lineage to another (i.e., a change of taxon) and the more distant the lineages the more catastrophic the metamorphosis.[29] Larvae cannot be a safe guide to the taxonomy of animals as Darwin suggested because they and their corresponding adults are not, Williamson argues, necessarily derived from the same lineages. Williamson names this mechanism of speciation 'Hybridogenesis' and it is difficult to overemphasise just how unorthodox the idea is.

In conventional neo-Darwinian evolution and in orthodox genetics such an evolutionary mechanism for making new species is outlawed.[30] Indeed, the word species is defined precisely to rule out the possibility of hybridisation or at least to rule out the possibility of hybridisation producing fertile offspring that might contribute to evolution.[31] Williamson's theory is therefore quite radical in its implications and may be compared to other saltationistevolutionary theories such as Richard Goldschmidt's thesis of the 1930s in which macro-mutations provide the genetic variation required for the evolution of new species in one single, large mutation event. [32] Goldschmidt called such saltations, 'hopeful monsters'; Williamson's creatures are similarly hopeful monsters created not gradually but in a single generation and I have borrowed Goldschmidt's resonant phrase as the title of my film.

Darwin had explicitly argued that nature does not work in leaps and saltationist theories of evolution like Richard Goldschmidt's were largely swept away by the so-called 'modern synthesis' of Darwinism and Mendelian genetics that the zoologist Julian Huxley developed in the 1940s and that today we call neo-Darwinism. Neo-Darwinism stresses the gradual, incremental nature of

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[29] Williamson D. I. (1991) and Williamson, D. I. (2009)

[30] Note that Williamson's thesis argues for sympatric speciation whereas the orthodox view of Ernst Mayr is that all speciation is allopatric (see, e.g. Mayr, E. (1942))

[31] Having said this, botanists recognise that many species of flowering plants evolved, some very rapidly, as a result of hybridisation. See for example Reiseberg, L. H. (1997); Grant, B. (1981)

[32] Darwin, by contrast, does not believe in saltations (from the Latin word, 'saltus', meaning leap): 'On the theory of natural selection we can clearly understand the full meaning of that old canon in natural history, "Natura non facit saltum." This canon, if we look only to the present inhabitants of the world, is not strictly correct, but if we include all those of past times, it must by my theory be strictly true.' Darwin, C. (1964) p. 206

evolutionary change.[33] This gradualist picture remains largely unchallenged in scientific circles and is today championed with great energy by writers and broadcasters such as Richard Dawkins. It is in this context of a dominant and dominating neo-Darwinism that we must position Williamson's somewhat anachronistic theory of hybridogenesis.[34]

Context

This Ph.D. thesis is not of course about biology, nor theories of evolution, but it is important to stress, despite the hegemony of neo-Darwinism, just how unresolved some basic problems in evolution remain (at least in the opinion of certain scientists). Darwin's great contribution to biology was to persuade us that change really happens. As Theodosius Dobzhansky famously wrote over a century after Darwin: 'Nothing in biology makes sense except in the light of evolution.'[35] Ernst Mayr points out, however, that Darwinian evolution actually comprises five sub-theories: evolution *per se* (i.e., change), common descent, gradualism, speciation and natural selection and one may be persuaded by one or more without being persuaded by all.[36] Darwinian evolution is the theory that all life forms descend (i.e., change, evolve) from one or a very few ancestral forms (common descent) by a process called 'natural selection' (by analogy with the 'unnatural', deliberate selection by plant and animal breeders) which acts on heritable variation within populations to select the best adapted individuals, a process that under some circumstances and over many generations (gradualism) may produce a population of a new species (speciation).

Darwin could not, however, explain the nature and causes of the heritable variation on which natural selection acts and a great deal of the history of evolutionary biology since Darwin has been concerned with solving this particular problem. In fact, Darwin's theory or theories of evolution are actually extremely general. As Jablonka and Lamb point out in their recent textbook:

[33] See Huxley, J. (1964); Goldschmidt, R. 1933 pp. 539-547 and Goldschmidt, R. 1940, pp. 390-393 and Huxley, J. (1942/1964). There have been more recent attempts to revive saltationism, most notably Eldredge, N. and Gould, S. (1972)
[34] Williamson D. I. (2009)
[35] Dobzhansky, T. (1973)
[36] Mayr, E. (1985)

> [Darwin's theory] says nothing about the processes of heredity and multiplication, nothing about the origin of the heritable variation, and nothing about the nature of the entity that is evolving through natural selection. … it is possible to be a perfectly good Darwinian without believing in Mendel's laws, mutating genes, DNA codes, or any of the other accoutrements of modern evolutionary biology.[37]

Although Hybridogenesis forces a reappraisal of two of the five sub-theories of Darwinism, namely gradualism and common descent, Williamson is not wrong in positioning his hypothesis within the Darwinian tradition. Indeed, we can even imagine that Darwin might have welcomed it as offering a solution to a problem that stumped him, namely the origin of heritable variation. It is important to remember that Darwin himself had less confidence in the totality of his ideas than many of his followers today. For example, despite what one reads in school textbooks, in Darwin's own day his theory did not displace from evolutionary thinking the so-called Lamarckian 'mistake' that use and disuse of an organ or structure might be a source of permanent, heritable variation. Indeed, as long as there was no agreed theory of heredity the idea of the inheritance of 'acquired characters' persisted in the mainstream of evolutionary thinking and in *The Variation of Animals and Plants under Domestication* (1868) Darwin himself essayed a possible mechanism to explain it:

> I assume that cells, before their conversion into completely passive or 'formed material,' throw off minute granules or atoms, which circulate freely throughout the system, and when supplied with proper nutriment multiply by self division, subsequently becoming developed into cells like those from which they were derived. These granules for the sake of distinctness may be called cell-gemmules, or, as the cellular theory is not fully established, simply gemmules. They are supposed to be transmitted from the parents to the offspring. … Gemmules are supposed to be thrown off by every cell or unit, not only during the adult state, but during all stages of development. … Hence, strictly speaking, it is not the reproductive elements, nor the buds, which generate new organisms, but the cells themselves throughout the body. These assumptions constitute the provisional hypothesis which I have called Pangenesis.[38]

It would require a lengthy book to trace the development of theories of heredity and evolution since Darwin. However, it is crucial to appreciate that despite the

[37] Jablonka and Lamb (2005) pp. 11-12
[38] Darwin, C. (1868) p. 374

successes of molecular biology and today's neo-Darwinian orthodoxy there is still scope for a range of theses about the nature and sources of variation and consequent mechanisms of evolution. The neo-Darwinian consensus is that variation arises by a combination of gene mutation and chromosomal recombination but some biologists deny that these sources of variation are sufficient or claim there are additional mechanisms like symbiosis, lateral gene transfer, epigenetic imprinting, macromutations and Williamson's hybridogenesis which are equally, if not more, significant as sources of the variation on which natural selection acts. These are still live issues in biology which is why, like Goldschmidt in his day, Williamson's most recent paper (2009), published in the peer-reviewed, *Proceedings of the National Academy of Sciences*, was met with such heated debate. A review article in *Scientific American*, for instance, quotes Fred Nijhout of Duke University saying 'it would be better suited for the '*National Enquirer* than the National Academy.'[39]

Pre-Production

When I first came across Williamson's ideas they were less well known than today and I simply thought they chimed nicely with the theory of endosymbiosis I was then exploring (a theory about the origin of eukaryotic cells by the physical and genetic merging of early prokaryotes (i.e., bacteria)). Williamson had worked mostly on marine invertebrates like starfish, sea urchins and sea squirts which, like flowering plants, release their gametes into the ocean currents where they mix randomly, a form of cross-fertilisation he could recreate artificially in the laboratory.[40] However, by the time I interviewed him for my Symbiosis programme his experimenting days were over, cut short in 1990 by a stroke after a fall on the beach collecting sea urchins. His halting, slurred speech proved too difficult to follow on the radio and failed to make the final cut. I was, however, fascinated by his unorthodox ideas and impressed with his

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[39] Borrell, B. 'National Academy as *National Enquirer*? *PNAS* Publishes Theory That Caterpillars Originated from Interspecies Sex', *Scientific American* Aug 24th 2009 (http://www.scientificamerican.com/article.cfm?id=national-academy-as-national-enquirer; accessed September 2009)

[40] Many flowering plants shed pollen into the air and botanists have known for years that a great many species of flowering plants are perfectly fertile hybrids. Indeed, this has been one of the chief arguments made against the safety of growing GMOs near to other crops. (See note 28 above.)

frustrated passion. Although he has many detractors, I found that a number of scientists, and one very eminent one, Professor Lynn Margulis of the University of Massachusetts Amherst, supported Williamson's work and believed he was on to something significant.

Margulis had been the star of my radio programme on symbiosis, a theory of evolution she spent forty years persuading the scientific establishment to accept. She has since included an entire chapter on Williamson in her book, *Acquiring Genomes* (2002) and promoted his ideas strongly at the International Symbiosis Society congress in Halifax, Nova Scotia which I attended (and filmed) in August 2003.[41] Williamson is also the joint subject with the entomologist Vincent Wigglesworth of a new book by Frank Ryan titled *Metamorphosis*.[42] Ryan, Margulis and Williamson all contributed to the 2008 conference, *The Driving Forces of Evolution: from Darwin to the modern age*, at the Linnean society, that was organised to celebrate the 150th anniversary of the first reading of the Darwin and Wallace papers and at which Margulis was honoured with a Darwin-Wallace medal in recognition of her work in evolutionary science.

Both Margulis's and Ryan's books lay in the future when I first met Williamson but I could see then that his unorthodox theory fitted in with both Margulis's theory of endosymbiosis and a number of what one might call neo-Lamarckian ideas that have been developed recently.[43] Furthermore, his struggle to persuade others to take him seriously and to publish his ideas seemed a good example of the dynamics of scientific discovery. (What goes unpublished, after all, can never play a role as scientific knowledge.) In Williamson's theory I recognized an example of what Thomas Kuhn calls 'extraordinary' research, work done under the sign of 'crisis': speculative, questioning of fundamentals, in a word, controversial. It was also, importantly,

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[41] Margulis was also featured in a more recent programme, 'A Life With Microbes' (BBC Radio 4, 15th July 2009) produced and presented by Paul Evans that includes material I recorded when Margulis debated evolution with Richard Dawkins at Balliol College, Oxford on May 8th, 2009 where she was Eastman Professor.

[42] Ryan, F. (in press) I have read a proof-copy.

[43] These ideas are summarised in a *New Scientist* article by Graham Lawton, 'Why Darwin was wrong about the tree of life' (Jan 2009) which argues for the importance of lateral gene transfers (whatever the mechanism) in the history of life. In this non-Darwinian conception of evolution the branches of Darwin's tree turn back on themselves and anastomose, producing a net that makes it much more complicated to infer an organism's ancestry from its genetic make-up.

incomplete. Indeed all science that is controversial is, by definition, incomplete—the closure of controversy goes hand in hand with its completion, its acceptance as fact by the majority of scientists concerned.

In exploring how controversies in science are settled, the constructivist scholar of science, Bruno Latour, distinguishes between what he calls 'ready made science' and 'science in the making'.[44] If Latour were to make a film about Williamson's science he would chart the processes by which Williamson's controversial ideas (science in the making) are transformed into the stuff of textbooks (ready made science), a process that constructivism pictures very differently to the received view. In the received view a scientific controversy is closed (i.e., becomes *ready made*) when the facts support the theory but in constructivism it is the process of closure that itself 'precipitates out' the facts. This constructivist picture (which I will explore in detail in a later chapter) is unfamiliar and represents a serious challenge to our received view of how scientific knowledge comes about. To explore how the science programme might represent such a counter-intuitive process, I needed to document an example of incomplete and controversial science and Williamson's work has, therefore, provided an ideal subject for me.

Making the film

Williamson was born in 1922 and he had already retired when he suffered his stroke in 1990. When I met him a decade later he had largely recovered mentally but remained physically disabled. We therefore made a mutually beneficial agreement. I would help him resume his experiments but in return for my assistance he would let me film our work together. I did the bulk of filming in the first year of the project, spending two months living with Williamson on the Isle of Man and acting as his laboratory technician and general factotum in a series of breeding experiments.

Williamson has not been a 'maverick' all his career and he is no stranger to what Thomas Kuhn calls 'normal' scientific research. Indeed, he contributed greatly to the mainstream work of the Marine Biology Laboratories of the University of Liverpool at Port Erin for over forty years and was head of the

[44] Latour, B. (1986)

labs for some of that time. When I met him, however, he held an emeritus position which meant we would be working in a different context to the rest of the scientists at Port Erin. Williamson's status meant we enjoyed the same technical and financial support as the others but without the usual pressures felt by employees who, more often than not, were on short contracts and whose work was therefore expected to bear fruit quickly. In contrast to this industrialised, post-doctoral scientific research, our work more closely resembled 'pre-professional' science of the sort the founders of the Royal Society would have recognized, in which, as the sociologist, Steve Fuller writes, 'leisure was the sole necessary condition for sustained reflections'.[45]

I recorded our efforts over that first summer and interviewed Don a number of times about his ideas. Three years later I took him (by then largely wheelchair-bound) to Italy to a conference in Bellagio on Lake Como organised by Professor Margulis. There he delivered a paper to a small group of scientists and I was able to record his experience. I have also returned periodically to visit Don to record further experimental work. In July 2008 I brought him to London to the Linnean Society for a reunion with Margulis. I also visited the USA to interview a number of key figures in marine biology whose comments provide a context and contrast to Don's.

Don was only interested in the scientific work and, while he co-operated fully in the filming, he never asked to see any of the rushes or even the finished film. For the most part, I filmed hand-held but whenever I needed my hands free I used a tripod. Our laboratory was a cramped, damp and noisy environment and much of the day was spent simply waiting for animals to spawn. While this was initially frustrating, I gradually became accustomed to the leisurely pace and repetitious nature of our work and indeed benefited from it because it meant I often had several chances to document particular procedures. Of course eggs and sperm are small and much of our work required microscopes which meant some specialised equipment for gathering images but by far the greatest problem, both during filming and later in editing, was to locate the lines of a narrative. I amassed around 100 hours of material and it was a considerable task to construct from this a story of our work together. But

[45] Fuller, S. (1997) p. 36

this is not quite the right way to put it. The story I finally constructed is the outcome of an exploration of the formal possibilities of the material. This written dissertation, then, sets out to explore the nature of those possibilities. It seeks to understand how this particular documentary constructs a world on screen and what picture of science, broadly speaking, it represents; furthermore it seeks to evaluate the extent to which this picture is consonant with a constructivist view of science.

Structure of the dissertation

The dissertation is divided into two parts. In Part one, (chapters one and two), I lay out the picture of science I have here called the 'received view' and analyse a 'classic' science documentary, *The Ghost in Your Genes* (2005), in light of that view. In part two of the dissertation, I outline (chapter three) those developments in the philosophy of science that have resulted in the 'constructivist' view of science. In chapter four I explore their implication for our understanding of scientific practice and in chapter five I examine the impact of such ideas on our understanding of the nature of scientific discourse. These chapters lay the groundwork for an analysis of my own attempt at a science documentary, *Hopeful Monsters: an Experiment*. This analysis is divided between chapters six and seven. Chapter six deals with issues of documentary style while chapter seven looks at the narrative structure of the film. I conclude that *Hopeful Monsters* does go some way to representing an alternative, 'constructivist' picture of science and consider the implications of the film's representation of science for our appreciation of the nature and meaning of the scientific life.

PART ONE

THE RECEIVED VIEW

CHAPTER ONE
THE RECEIVED VIEW OF SCIENCE

Introduction

In this opening chapter I lay out the main features of the received view, exploring the orthodox idea of scientific practice and of scientific discourse so that, in the following chapter, we may evaluate the effectiveness of the 'classic' science documentary in representing this view of science.

According to Isaiah Berlin, the central propositions of the received view arose in the Enlightenment and may be boiled down to three:

> First, that all genuine questions can be answered, that if a question cannot be answered it is not a question. … The second proposition is that all these answers are knowable. … The third proposition is that all the answers must be compatible with one another, because, if they are not compatible, then chaos will result. … If all answers to all questions are to be put in the form of propositions, and if all true propositions are in principle discoverable, then it must follow that there is a description of an ideal universe—a Utopia, if you like—which is simply that which is described by all true answers to all serious questions.[46]

The important idea is that every serious question has only one correct answer and that that answer is to be obtained not by revelation or tradition or dogma but by the correct use of observation and reason. Science, in this Enlightenment view, is a painstaking activity rather like the piecing together of a difficult jigsaw puzzle and, so conceived, it is intolerant of contradiction and cannot live even with dissent for very long.[47] This commonsense picture of science and scientific practice persists today, evidenced in both technical and popular scientific writing as well as films and television programmes.

Modern science has stimulated many technological developments and there is a close, synergistic relationship between science and technology. The distinction, however, is important. Whereas technology seeks to control the

[46] Berlin, I. (2000) pp. 21-22
[47] This intolerance of dissent deeply troubled Isaiah Berlin who as a child had fled Bolshevism and anti-Semitism in Russia and Latvia to finally settle in Britain in 1921.

environment, science, in the received view, aims at the opposite—to be controlled by it. As the historian of technology, Maurice Richter, writes:

> Scientists characteristically seek to surrender their freedom of choice with respect to their interpretations of the natural world, and to have those interpretations determined precisely and completely by the natural environment… Scientists in their research are, in effect, asking questions of nature, and they commit themselves in advance to accept whatever answers nature may give, no matter what these answers may be.[48]

Although science and technology have different aims, scientific experiments often rely on technology to 'pose' those questions and indeed one might argue that the very idea of intervening artificially in nature marks the transition from a pre-modern science interested in explaining *why* things happen to a truly modern science that asks *how?*

In *The Great Instauration* of 1620, Francis Bacon's project for compiling all human knowledge, he explains the foundational value of manmade experiments compared to mere observations of nature (or 'accidental' experiments):

> I seek out and get together a kind of experiments (sic) much subtler and simpler than those which occur accidentally. For I drag into light many things which no one who was not proceeding by a regular and certain way to the discovery of causes would have thought of inquiring after, being indeed in themselves of no great use; which shows that they were not sought for on their own account, but having just the same relation to things and works which the letters of the alphabet have to speech and word—which, though in themselves useless, are the elements of which all discourse is made up.[49]

Bacon is also at pains to emphasise the fundamental difference between this empirical approach and the scholasticism it replaces. He implores King James to recognise the distinction and, like King Solomon…

> …further follow his example in taking order for the collecting and perfecting of a natural and experimental history, true and severe (*unincumbered with literature and book-learning*), such as philosophy may be built upon… so at length, after the lapse of so many ages,

[48] Richter Jr., M. N. (1972), p. 5
[49] Bacon, F. (1937) p. 261

> philosophy and the sciences may no longer float in air, but rest on the solid foundation of experience of every kind, and the same well examined and weighed. I have provided the machine, but the stuff must be gathered from the facts of nature.[50]

The 'machine' of Baconian empiricism remains a guiding light of the received view: no statement can claim scientific validity if it cannot be traced back to sensory data—'that which is laid down' (*positum*). The received view holds that nature obeys general laws which may remain hidden under normal circumstances but which can be made to reveal themselves through systematic observation and experiment. As the Nobel laureate, Richard Feynman writes on page one of his *Lectures on Physics* (1963): 'The principle of science, the definition, almost, is the following: *The test of all knowledge is experiment*. Experiment is the *sole judge* of scientific 'truth.''[51]

Despite what Feynman says, in practice experiment is not always possible and not all natural sciences employ exactly the same methodological practices. Modern science has become internally divided into different disciplines or 'branches' and into sub-disciplinary 'fields' of specialisation. Some disciplines are as old as science itself—physics for example—with an impressive body of knowledge, while others are newer and less highly developed. In some disciplines, like astronomy or geology, planned intervention is very difficult if not impossible. In these cases experimentation is replaced by structured observation. As for paleontological and evolutionary events that have been influenced by non-replicable contingencies, these must, according to Stephen Jay Gould, rely on historical methods of investigation.[52] However, modern science, in the received view, whether experimental, observational or historical, appeals to *nature* as the final arbiter of the truth of its propositions. What makes the statements of science meaningful is that they can be verified by comparing their claims to how nature really is. This means that science makes a fundamental distinction between subject and object, between value and fact.

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[50] *Ibid.,* (my emphasis) p. 242

[51] In Feynman, R. (1963) (His emphasis).

[52] 'We shall never be able to appreciate the full range and meaning of science until we shatter the stereotype of ordering by status and understanding the different forms of historical explanation as activities equal in merit to anything done by physics or chemistry.' Gould, S. J. (2000) p. 281

The role of experiment in science (accidental or manmade) has, however, taken time to develop. In his study, 'Reporting The Experiment', Charles Bazerman analyses a hundred papers from the first century and a half of *The Philosophical Transactions of The Royal Society* (from 1665-1800). He finds that experimental reports are actually rare before the nineteenth century: only between 5% and 20% of reports of any volume of the Transactions until 1800, when the number rises to just under 40%. And the experiments that are described change in character over that period:

> The definition of experiment moves from any made or done thing, to an intentional investigation, to a test of a theory, to finally a proof of, or evidence for a claim. The early definitions seem to include any disturbance or manipulation of nature, not necessarily focused on demonstration of a stated preexisting belief, nor even with the intention of discovery. With time, experiments are represented as more clearly investigative, corroborative, and argumentative.[53]

By volume 80 (1790) of the *Transactions*, experiments have become 'subordinated to the conclusions the authors have come to' and just ten years later, in volume 90 (1800), authors talk about 'the role of experiment in testing our beliefs as well as filling out our knowledge.'[54] In the beginning, then, experimental reports were treated as news about nature but as nature started to be treated 'as a matter of contention and then a puzzle,' experiment became part of an argument and was subordinated to theory.[55]

'Theories', writes the twentieth century philosopher, Frederick Suppe, 'are the vehicles of scientific knowledge, and one way or another become involved in most aspects of the scientific enterprise:'

> A philosophy of science's analysis of the nature of theories, including their roles in the growth of scientific knowledge, thus is its keystone; and should that analysis prove inadequate, that inadequacy is likely to extend to its account of the remaining aspects of the scientific enterprise and the knowledge it provides.[56]

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[53] Bazerman, C. (1988) pp. 65-66
[54] *Ibid.,* p. 68
[55] *Ibid.,* p. 77
[56] Suppe, F. (1977), p. 3

Theory, according to the received view, is arrived at by generalising from observations, in other words, by induction and it expresses an expectation of observing facts of a certain kind under certain conditions. A scientific ‘law’, on the other hand, can be defined as the proposition that under conditions of a certain kind, facts of a certain kind are uniformly observable. In the received view, any theory or law that cannot be defined in these terms is to be written off as pseudo-theory or pseudo-law. A central tenet, therefore, of the received view is that one can make a clear distinction between theory and observation—the latter guarantees the former—and in this way science provides *good reasons* to believe what we believe about nature. The received view of science then has two aspects: it is understood as a body of *validated knowledge* and a *set of practices* that ensures the rigour of the experiments and observations that lead to validation.

Scientific practice

Scientists often work in teams and within institutions that have relationships to government and civic society and they work within conceptual and methodological traditions. Thus, scientific work is done neither in a cognitive nor a social vacuum and scientific knowledge is therefore necessarily a sociological phenomenon. For the received view, this represents an implicit danger; there is a fear that social forces might distort objective science. This fear is hardly new. In his *Novum Organon* of 1620, Francis Bacon warns of the various ways our minds might be influenced away from the truth. These baleful influences he calls the ‘four idols’, mental images that cloud or confuse our knowledge of external reality (‘For what a man had rather were true he more readily believes’):[57]

> The idols and false notions which are now in possession of the human understanding, and have taken deep root therein, not only so beset men’s minds that truth can hardly find entrance, but even after entrance is obtained, they will again in the very instauration of the sciences meet

[57] Bacon, F. *Novum Organon*. Book One, aphorism LXIX. http://www.constitution.org/bacon/nov_org.htm

> and trouble us, unless men being forewarned of the danger fortify themselves as far as may be against their assaults.[58]

Bacon believes that true knowledge of the objective world is possible only if we guard against such deception.

Subsequent to Bacon, sociological theories of scientific knowledge were also construed as therapeutic, rooting out necessarily false—because socially influenced—beliefs. The philosophy of knowledge and the sociology of knowledge were thus kept comfortably apart. Epistemologists dealt with how we come to have true knowledge and sociologists with how this truth might be obscured and the institution of science maintains this distinction by the careful training of new practitioners in this distinction. Bad science—error—is only avoided by careful and vigilant acculturation of scientists who may only practice after a long apprenticeship during which they absorb the traditions and rules and corporate authority structure of a fundamentally conservative institution. It is a paradox that although science is concerned with innovation, this is sought within such an authoritarian framework.[59]

In the 1940s, the sociologist Robert Merton took up the challenge of explicating the ethos of this authoritarian institution. Merton acknowledges that individual scientists may have their own aims and interests in doing science, such as acquiring wealth, fame or power but, he suggests, the institution of science reconciles these divergent aims through a system of safeguards, penalties and rewards and through adherence to certain codes of behaviour. He identifies four behavioural norms as central to the ethos of science: universalism, communism, disinterestedness and organized scepticism.[60]

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[58]*Ibid*., XXXVIII. The four idols are described in aphorisms XXXVIII to LXVIII in Book One. They are (1) 'idols of the tribe' which are those we might call instinctive or genetic features of our minds that we share in common with other humans, e.g., our senses are fallible; we suppose the existence of more order than exists; once we adopt an idea we distort others to fit with it (i.e. we avoid cognitive dissonance); (2) 'idols of the cave' which are those prejudices we each individually develop through our education or experience; (3) 'idols of the marketplace'. 'These are the most troublesome of all' and comprise essentially language or culture. For example, 'words stand in the way and resist change'. This is very much what Kuhn meant by incommensurable paradigms or *weltanschauungen*; (4) 'idols of the theatre' which are dogmas i.e. philosophical systems which are 'but so many stage-plays representing worlds of their own creation'. Dogmatic beliefs prevent a person opening their mind to other possibilities.

[59] See Gross, A. (1990)

[60] Merton, R. K. (1957) Chapter 16

The norm of *Universalism* ensures that scientific claims are evaluated irrespective of the identity of the scientist making them and the peer review system is in place to safeguard this ideal. *Communism* expresses the idea that science is a collective activity and so knowledge should be shared openly. (Individuals may nonetheless be rewarded with prizes and by way of citation in each other's publications.) *Disinterestedness* ensures integrity and rules out fraud while *Organised Scepticism* guards against the acceptance, too easily, of new ideas; by this norm scientists should withhold judgement and fiercely question new ideas, even ones they wish to promote. It is important to note that these moral norms say nothing about the *content* of science but are understood to act *externally* (i.e., institutionally) on the conduct of scientists. Merton's norms remain, to this day, widely accepted and influential.[61]

We shall have reason to reconsider these assertions about the nature of scientific knowledge and the practice of science but for now let us accept this received view and examine how it might be promoted by the sort of scientific discourse that, as the media theorist, Roger Silverstone points out, is the most common source of scientific ideas for the documentary film maker: the journals and magazines of science and popular science.[62]

Scientific discourse

In *Literature and Science* (1963), Aldous Huxley seeks to characterise scientific writing by contrasting it to literature. 'All our experiences are strictly private', he writes, 'but some experiences are less private than others'.[63] Literature, he argues, reports private experience while science documents the less private. For example, faced with a burning building we can expect the visual, auditory and olfactory experiences of a group of people to be pretty much the same. This, then, says Huxley, is the domain of scientific writing. However we cannot

[61] In 2007, Sir David King, then chief scientific adviser for the UK government, drafted a code of ethics for scientists called *Rigour, Respect, Responsibility.* The code reworks Merton's norms, especially disinterestedness and communism, and elaborates a number of more specific virtues: 1) Act with skill and care, keep skills up to date; 2) Prevent corrupt practice and declare conflicts of interest; 3) Respect and acknowledge the work of other scientists; 4) Ensure that research is justified and lawful; 5) Minimise impacts on people, animals and the environment; 6) Discuss issues science raises for society; 7) Do not mislead; present evidence honestly. (http://www.berr.gov.uk/files/file41318.pdf)

[62] Silverstone, R. (1984) p.379

[63] Huxley, A. (1963) p. 7

expect these same people to share similar *emotional* responses. One person may find the fire beautiful, another ghastly; one may look upon the fire with malicious glee while another feels sympathy. These experiences, says Huxley, are more private than our sense experiences and they fall within the domain of literature. Scientific discourse, he suggests, orders and communicates our more public experiences while literature presents man's private emotions and explores the interaction of this private realm with the public spheres of social convention and 'objective reality'.

On the face of it, this looks like a compelling distinction. Certainly, from the beginning of his or her training, the contemporary scientist learns to present ideas and practices in a very particular form. In his paper, 'Linguistic Aspects of Science' (1987), Leonard Bloomfield describes it as follows:

> The forms of the scientist's speech are so peculiar in vocabulary and syntax that most members of his speech-community do not understand them. If one wants to read an English treatise on mechanics, it is not sufficient that one be a native speaker of English: one needs also a severe supplementary training.
>
> The effect upon hearers of the scientist's speech is even more remarkable. In a brief utterance the scientist manages to say things which in ordinary language would require a vast amount of talk. He 'manages to say things'—that is, his hearers respond uniformly and in a predictable way. Indeed, their response is even more uniform and predictable than is the hearer's response to ordinary speeches.[64]

The linguistic form of science, then, represents what Bloomfield calls a 'technical dialect' and hearers of the dialect do not respond, he says (in agreement with Huxley), to anything going on 'inside' the scientist:

> Science, we say, is 'objective.' And when his turn comes to speak, the scientist knows that he may not expect any allowance to be made for his 'subjective' adventures,—for anything that may go on inside him,—and that his audience will respond only to the exact stimulus-value of his words, determined according to well-fixed conventions….what is spoken is accepted at face value.[65]

For a number of reasons this technical dialect, especially in its written form, is often difficult to follow. First, scientific papers may use abstruse jargon

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[64] Bloomfield, L. (1987) p. 499
[65] *Ibid*., p. 503

understood only by others familiar with the field (exalting and reassuring those *inside* while rejecting or even offending those *outside*[66]); second, such writing is distinguished by the persistent use of the passive voice; and third, in scientific writing (in contrast to journalistic of 'creative' writing) there is a proliferation of noun-phrases.

The use of the passive voice in modern scientific papers has evolved over time and only became the style we recognise today when scientific writing made the switch from book to journal.[67] While the passive voice serves to emphasise the role of the scientist as an objective observer, nominalisation further 'passifies' the scientist by converting his perception of 'objects' such as lines in a bubble chamber or blots on an electrophoresis gel into noun-phrases that displace the scientist from the subject position in the sentences of scientific discourse. By this rhetorical means objects and processes become the active players of scientific statements—the *causes* of events. Alan Gross takes the example of a series of papers on cancer by Racker *et al.* in which there is a notable coincidence of scientific and linguistic change that is paradigmatic of nominalisation:

> In a typical instance, Racker and his associates cease to observe the laboratory event, the blob in the fifth lane of the autoradiogram, and begin to see the physical object: the kinase PKL. Next, the physical object PKL begins to appear in the subject position in their sentences. Afterwards, PKL becomes part of a network of meaning, a causal chain: the kinase phosphorylates, the phosphorylated kinase phosphorylates another kinase. Finally, the nature and activities of all phosphorylated kinases are abbreviated in a new noun phrase, a new scientific term: the kinase cascade.[68]

Thus does the language of the scientific paper convert the observations of the scientist into natural objects 'out there'. The scientist as a *subject* is rendered absent, leaving the physical world as the ontological source of the knowledge that research reveals. This absent story-teller is a common feature of the rhetoric of the classic science documentary in which, although there is commonly a narrating 'voice of God', it is a disembodied and omniscient voice,

[66] To paraphrase Roland Barthes, quoted in Montgomery, S. (1996) p. 7
[67] Montgomery, S. (1995)
[68] Gross, A. (1990) p. 71

not so much of an anthropomorphic god as of Nature, with a capital 'N', that is indifferent to the thoughts and desires of mere mortals.

Scott Montgomery traces the historical origin of this rhetoric of objectivity to the diary and the public talk, both forms of discourse that, ironically, stress the author as *subject*, a *witness* to his work. Montgomery notes, for example how, unlike Bloomfield's scientists, in 'New Theory About Light and Colour,' Isaac Newton vouchsafes certain details of his 'inner life' in order to authenticate his work:

> To perform my late promise to you, I shall without further ceremony acquaint you that in the beginning of the year 1666 (at which time I applied myself to the grinding of optic glasses of other figures than spherical) I procured me a triangular glass prism to try therewith the celebrated phenomena of colours. And in order thereto having darkened my chamber and made a small hole in my window-shuts to let in a convenient quantity of the sun's light, I placed my prism at his entrance that it might be thereby refracted to the opposite wall. It was at first a very pleasing divertissement to view the vivid and intense colours produced thereby; but after a while, applying myself to consider them more circumspectly, I became surprised to see them in an oblong form, which according to the received laws of refraction I expected should have been circular.[69]

In Huxley's terms, Newton is here poised between 'literature' and 'science'. He confesses to emotions ('pleasing divertissement') but also, as Montgomery says, gets 'down to business…he is interested in documentation more than autobiography.'[70]

In the early years of the Royal Society, experiments destined for report in the *Philosophical Transactions* were usually done in public places before a large audience who could witness what was demonstrated. However, as experiments became more subtle and had to be conducted in laboratories, large audiences became impracticable. Instead, individuals, the more distinguished the better, were invited to observe proceedings and validate them. Eventually, as lab work became more time consuming, involving sometimes a whole series of experiments and often carried out with the help of assistants, the practice of inviting witnesses died out entirely leaving only the author's (or authors') own

[69] Last few lines quoted in Montgomery, S., (1999) p. 44 Complete quotation found at http://en.wikisource.org/wiki/New_Theory_About_Light_and_Colour (last accesssed Dec 2009)
[70] Montgomery, S. (1999) p. 44

account to give access to events. This account, then, had to be plausible, an act of 'virtual witnessing' for the reader, as Charles Bazerman writes:

> Since neither the reader nor any surrogates or representatives, except for the author himself, have witnessed the series of experiments, the account must stand in place of the witness. The reader in order to understand the experimental argument must vicariously witness the experiment through the account. In order to earn the trust of the reader, the story of the experiment must be told plausibly if not persuasively, and the events reported must provide sufficient good cause for the investigator to come to the conclusions he reports.[71]

Bazerman argues that Newton's audience were not persuaded by his 'New Theory About Light and Colour' because Newton focused too much on describing his *personal* experience. In subsequent writing, in order to better compel consent, Newton took himself out of the picture altogether, developing the prooflike form of argument that we read in the *Principia* which, in being logical rather than descriptive, could forestall serious controversy.[72] And so the rhetoric of scientific reports gradually changes from Newton's day onwards. The personal journey is transformed into a retrospective tour of evidence that removes, by means of the passive voice and nominalisation, all trace of the scientist as a subject. Scientific discourse becomes the rhetoric of objectivity described by Bloomfield.

The objective observer

'To be objective.' argue Lorraine Daston and Peter Galison, in their book, *Objectivity* (2007), 'is to aspire to knowledge that *bears no trace of the knower*…[73] This is very much the rhetoric of the documentary film and it is hardly surprising, as Daston and Galison argue, that photography itself has played a part in the apotheosis of objectivity as the touchstone of science. The camera, like the anemometer, the hygrometer and any number of other scientific instruments that the sociologist, Bruno Latour calls 'inscription devices', transformed the idea of what counts as scientific knowledge in the nineteenth

[71] Bazerman, C. (1988) p. 74
[72] Bazerman C. (1991) p. 37
[73] Daston, L. & Galison, P. (2007) p. 17 (my emphasis)

century. Such devices seemed to 'speak without prejudice,' to 'say … out loud' what has been observed, writes Brian Winston in *Claiming the Real* (1995). [74]

Daston and Galison argue that before the invention of photography in the nineteenth century, prejudice (i.e., pre-judgement) had been a necessary practice in scientific representations. Before photography, images in science were hand-drawn and necessarily distillations of experience into which scientists put those features they recognised as important and relevant. This pre-judgment is what Daston and Galison call the Truth-to-Nature philosophy. Such a philosophy is predicated on an eighteenth century view of the self as an essentially passive centre receiving sensations from its periphery—what Karl Popper calls the bucket theory of the mind. In the centre of the self reside reason and the imagination and this centre must organise all the experience that comes flooding into the mind-bucket. To the extent that the centre holds, the self is functional, *compos mentis*, but this passive self risks being overwhelmed at any moment. To be True-to-Nature, then, requires the self to sift and synthesise experience, removing any 'noise' that might muddle perception. In the eighteenth century, write Daston and Galison, 'to register experience indiscriminately was to be at best confused and at worst indoctrinated.' [75]

Partly in response to the psychological effects of industrialisation, this notion of the self began to change during the nineteenth century resulting in a conception that was less passive, a self that could hold out, as it were, against the depersonalising impact of industrialized life. William James characterised this modern self in 1890 as a certain portion of the 'stream of consciousness' that:

> … is felt by all men as a sort of innermost centre within the circle, of sanctuary within the citadel, constituted by the subjective life as a whole. … Now, *what is this self of all the other selves*? Probably all men … would call it the *active* element in all consciousness; saying that whatever qualities a man's feelings may possess, or whatever content his thought may include, there is a spiritual something in him which seems to *go out* to meet these qualities and contents, whilst they seem to *come in* to be received by it. It is what welcomes or rejects. It presides over the

[74] Winston, B. (1995) p. 136
[75] Daston, L. & Galison, P. (2007) p. 203

> perception of sensations, and by giving or withholding its assent it influences the movements they tend to arouse.[76]

'This 'self of all the other selves'', write Daston and Galison, 'is that part of the stream of consciousness that endures amid the flux, and it is robust, unified and, above all, 'active'… like an energetic executive it 'comes out' to meet experience with outstretched hand … It is the assertive subject of subjectivity.'[77] As the aesthetician Willam Lycan describes it, the Jamesian mind is no bucket:

> [It] is not a passive receptacle in which sense-data are 'deposited,' 'processed,' or 'habitually correlated'; it is rather an active organ whereby pre-established concepts and perceptions interact. The having of 'sense data' is not epistemologically prior to the perception of physical objects…if anything the priority is the other way around.[78]

Here, for example, is Goethe in *Experience and Science* (1798), on the cusp of the nineteenth century, describing the activity of the scientific imagination:

> Phenomena, also called facts in lay language, are certain and definite in nature, but often indefinite and variable as they meet the eye. The scientist attempts to grasp and hold fast what is definite in the phenomena; in individual cases he is concerned not only with their actual but also with their ideal appearance. As I have occasion to notice in my present field of work, empirical breaks must often be disregarded in order to preserve a pure, constant phenomenon. However, as soon as I permit myself to do this, I am establishing a kind of ideal.[79]

To the psychoanalyst, Adam Phillips, one can view this active, idealising mind either as a source of disciplined invention (as Goethe did) or by the same token, an obstacle to objective observation and therefore to proper science:

> In one version the self is the instrument, in the other it is the obstacle. In one version the so-called self is privileged, in the other version something beyond the self is revealed. At one extreme of this strange dualistic vision there is the cult of personality…and at the other extreme there is a cult of the object, of a world whose virtue and substance

[76] James, W. (1890) p. 298
[77] Daston, L. & Galison, P. (2007) p. 201
[78] Lycan, W. G.(1971) p. 230
[79] Naydler, J. (ed.) (1996) p. 104

> resides in the fact that it resists manipulation. Creative experience is either self-promotion or self-surrender.[80]

For the positivist of the received view, self-promotion is anathema. The good scientist abjures such subjectivity and so scientific culture promotes a notion of mental self-discipline, of willed will-lessness that is expressed in the passive voice and the noun-phrases of scientific discourse. The development of this discourse of objectivity gives form to the positivist conviction that, as Alan Gross puts it, '*we* are at the causal center of our world; *physical objects* are at the causal centre of the world of science.'[81]

In order to maintain this rhetoric of objectivity, the passive voice and nominalisation became conventions of good scientific writing and the *structure* of scientific papers also developed so as to marginalise human agency. The modern paper is subdivided into a number of headings whose order, regardless of scientific discipline, rarely varies: Introduction, Methods, Results, Discussion. This ordering represents science as an inductive process in which the good scientist waits, open-minded, for the data to be in before engaging in 'discussion' and drawing conclusions. This structure supports the idea of discovery as a 'eureka moment', a bolt from the blue, an idea sometimes expressed in popular accounts of science by the phrase 'I was *struck*'.[82]

Conclusion

In 'Narrative Form and Normative Force: Baconian Story-Telling in Popular Science' Ron Curtis examines the magazine, *Science 80-86,* published by the *American Association for the Advancement of Science* (AAAS). The magazine's writers, says Curtis, seem to accept the structure of the scientific paper as 'natural' and simply translate it into its literary counterpart: the detective story. The detective trail thereby becomes a model of good scientific practice, serving to reinforce an intuition about the nature of science as a 'straightforward' extension of commonsense. The discourse of this received view is aimed at emphasising how the scientist is not part of that nature, not an

[80] Phillips, A. (2006) p. 80-81
[81] Gross, A. (1990) p. 70 (his emphases)
[82] E.g., James Watson and Francis Crick famously write in the conclsion of their paper on the structure of DNA: 'It has not escaped our notice that the specific pairing we have postulated immediately suggests a possible copying mechanism for the genetic material.' (1953) p. 738

influence on it, but rather, in standing outside of it, is able to see it 'objectively'. This rhetoric is embodied in the narrative structure of scientific discourse and thus influences the classic science documentary; for, as Roger Silverstone writes 'it is in the semi-popular journals like *New Scientist* and *Scientific American*, and…the public rather than the private talk of scientists that television embraces its views of science, and from them begins its own work of mediation.'[83] Gardner and Young confirm this view:

> The question of how programme topics get chosen leads us back to a startlingly complacent source. We are told by *Horizon's* Editor that they select themselves: 'There they are, staring up at you in the literature' — *Nature* and *New Scientist* are the favourite sources of ideas. This puts them in close touch with a consensus and with the latest developments but can hardly be said to take them beneath established views.[84]

My own experience gives me no reason to believe that things have changed. Later we shall have reason to re-examine the 'detective-trail' rhetoric of popular science writing but our current discussion has served the purpose of highlighting those features of scientific discourse that 'stare up' from the literature to become the ground of further mediations by the television documentary to which we now turn.

[83] Silverstone, R. (1984) p. 379
[84] Gardner, C. and R. M. Young (1981) p. 178

CHAPTER TWO
THE CLASSIC SCIENCE DOCUMENTARY

Introduction

In the *New York Sun* on February 8, 1926, John Grierson coined the term 'documentary': 'Of course *Moana*, being a visual account of events in the daily life of a Polynesian youth and his family, has documentary value'. Documentary value lies in the indexical relationship between the film and the reality that caused it. As Edward Branigan writes, a document 'testifies to something because it has been produced by the thing itself'.[85] A documentary, then, may be taken as evidence or proof of what happened in front of a camera and microphone at some time in the past. The viewer of such a document assumes, furthermore, that the causal relations between the events depicted on the screen are the same causal relations responsible for the order in which those events come to be placed there in the first place.

Of course, indexicality is not all there is to the documentary film. The indexical material must be selected from an infinite number of possibilities and here, as Grierson puts it in *First Principles of Documentary* (1932), 'we pass from the plain (or fancy) descriptions of natural material, to arrangements, rearrangements, and creative shapings of it.'[86] Some argue that the arrangement and rearrangement of the indexical material of the documentary means we should understand the documentary more as a kind of argument about the world than a kind of story. As Bill Nichols writes:

> We expect to apply a distinct form of literalism (or realism) to documentary. We are less engaged by fictional characters and their destiny than by social actors and destiny itself (or social praxis). We prepare ourselves not to comprehend a story but to grasp an argument.[87]

The key to this statement is 'we prepare ourselves', for although Nichols is not here explicit, the implication is that although we may prepare ourselves to grasp the documentary as an argument, we may, if we wish, take it as a story—it

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[85] Branigan, E. (1992) p. 202
[86] Aitken, I. (ed.) (1998) p. 83
[87] Nichols, B. (1991) p. 5

depends how we prepare ourselves—and this is a matter of knowing how we *ought* to understand what we see and of meeting that expectation. Of course, we are helped to get our preparation right not only by the *Radio Times* but by certain textual markers. For example, the classical documentary, like Aldous Huxley's view of scientific writing, seeks to present only the public aspects of events and so, because a mental experience is not intersubjective, it eschews certain devices common to the fiction film such as dream sequences and subjective flashbacks that represent 'private' experience. But these rules of representation are, of course, cultural conventions and as such are time-bound and changeable. Indeed, preparing ourselves to view a film as a fiction rather than a documentary is equally determined by discursive conventions.

Consider, for example *Le Jardinier et le petit espiegle (or, L'Arroseur arrosé)*, a short movie included in the Lumière Brothers' first public screening in the basement room of the Grand Café, Paris on December 28th, 1895.[88] Like all the others in the programme, this film is one continuous shot lasting about 40 seconds. We open on a gardener watering a flower bed. A naughty boy creeps up behind him and steps on the hose, cutting off the flow. Puzzled, the gardener puts his eye to the nozzle. The boy lifts his foot and water shoots into the gardener's face, knocking off his hat. The man catches the boy, spanks him perfunctorily and then, without further hesitation, resumes his watering. In his book, *For Documentary* (1999) Dai Vaughan asks of this film: is it documentary or fiction? And the answer is: both.[89] It was obviously intended as a fiction, as a film version of the sorts of skits common in vaudeville shows of the period, and presumably this is how the nineteenth century viewer 'prepared' him or herself to see it, but to the modern filmgoer, the knowledge of when it was made and the gaucheness of the performances means that it comes across as a *record* of an *attempt* to make a fiction. ('No film,' writes Stephen Heath, 'is not a document of itself'.[90]) The narrativity of the film has no bearing on whether the viewer takes it for a documentary or a fiction film.

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[88] The film is available in *Early Cinema: Primitives and Pioneers*, Vol. 1 (BFI). This screening was repeated later at the Regent Street Polytechnic, London (now University of Westminster), on 20 February 1896.
[89] Vaughan, D. (1999) p. 6 ff
[90] Heath, S. (1981) p. 238

I belabour this, perhaps obvious point about conventions and the eye of the viewer because in this chapter I would like to clear a space for considering the science documentary not primarily as an argument but as a form of non-fiction narrative. While the science documentary may argue for a particular scientific theory it also tells a story about science and it is this, rather than the specific scientific argument, that is of relevance to this dissertation; for, as Roger Silverstone writes:

> The work which television science does in the mediation of science is…neither simple nor neutral, and the texts which emerge express and anticipate the conflicts that sustain both their production and reception. They are the products of, in the broadest of terms, social and cultural struggle. Each text, in its own way attempts a resolution and, in the forms and structures through which that resolution is attempted, provides a model or an example for the everyday.[91]

A model of where knowledge comes from is presented in the science documentary not so much by the film's argument for or against a particular scientific explanation as by the narrative of scientific activity the film, perhaps only tangentially or implicitly, represents. Because the received view would have us understand the science documentary as concerned primarily with re-presenting the evidence and argument of a scientific paper or papers, it is important to insist that one may also legitimately explore the science documentary as a narrative form. Indeed, I would suggest that although a documentary may obviously *convey* an argument it is not necessarily argumentative in its deeper structure. This appears to run counter to Nichols's view:

> Documentaries take shape around an informing logic. The economy of this logic requires a representation, case, or argument about the historical world….A paradigmatic structure for documentary would involve the establishment of an issue or problem, the presentation of the background to the problem, followed by an examination of its current extent or complexity, often including more than one perspective or point of view. This would lead to a concluding section where a solution or path toward a solution is introduced.[92]

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[91] Silverstone, R. (1986) p. 83
[92] Nichols, B., (1991) p. 18

There is surely an obvious problem with this notion of the documentary? If, as Nichols would have us accept, it is argument and not narrative that distinguishes the documentary from other kinds of film then he requires we treat film as a *language*, for one can only argue persuasively in language. I would not, however, be the first to point out that film cannot, at root, be such a *linguistic* system for without the human voice or written text, the 'concreteness' of the film image resists the necessary form of linear, propositional argument. The documentary film may yet have a 'grammar' of sorts but it is not the codified, *a priori* grammar of spoken or written language.

In *Semiotics and the Analysis of Film*, Jean Mitry addresses himself to this issue:

> In the cinema, where there is no such thing as image-verb, image-subject, image-adjective, where the briefest shot incorporates all these designations, it is not possible for a signification to be distributed by the structure. Which is another way of saying that the shot has nothing in common with the word. It is a unit of construction, but one which includes a whole series of relationships; a *signifying* unit, not a unit *of* signification.[93]

'I defy anyone,' challenges Jean Mitry, 'to translate into audio-visual terms: 'Every day at the same time the marquise went for a spin in the woods' for an image can never translate the indefinite article. All I will ever see is *that particular* marquise, never *the* marquise or *a* marquise. And always from a particular angle, in a particular context.' [94] Few arguments, I submit, can dispense with the indefinite article for they must surely wish to make a case about a class, rarely a specific. There are an infinite number of ways of trying to give audio-visual form to the sentence about the marquise and each different way will necessarily confer a specific and different meaning. Visual evidence and argument are not the same things. For this reason, Mitry concludes, 'the shortest documentary, the smallest news item, whether good or bad, are already *works of art*, or are on the margins of a work of art.'[95]

As lexical signs, words can be used over and over again in different arrangements to mean a multiplicity of things. If this were not the case, if each

[93] Mitry, J. (2000) p. 27
[94] Mitry, J. (2000) p.35
[95] *Ibid.,* p. 36

word, for example, were the sign only of an individual object then we should have to remember an infinite number of words in order to speak of an unlimited number of objects. The image, the shot, on the other hand has a *specific* value but its *semantic* content depends on a whole network of circumstances. As Barthes puts it, 'all images are polysemous; they imply, underlying their signifiers, a 'floating chain' of signifieds.' [96] The image is thus a *gestalt-sign*, a polysemous sign whose meaning emerges from a 'floating chain' of signifieds arrested, at different positions perhaps, by each individual spectator. By 'argument' then, Nichols refers to the *verbal*, signifiers that often accompany the documentary image and that are intended to fix this floating chain. Without the linguistic element of narration (or on-screen titles) to anchor meaning, the polysemicity of the film image undermines its value as a statement in an argument.

In Nichols' 2001 book, *Introduction to Documentary*, he does in fact set aside the argument/narrative distinction in favour of a distinction between 'kinds' of narrative. Instead of defining the documentary in contradistinction to the fiction film he turns this familiar strategy on its head and instead defines the fiction in terms of the documentary: 'Every film is a documentary', he writes, but there are two 'kinds', the documentary of 'wish-fulfilment' and the documentary of 'social representation'.[97] The former is what we call the fiction film, the latter the non-fiction:

> Documentaries of wish-fulfilment…give tangible expression to our wishes and dreams, our nightmares and dreads. They make the stuff of the imagination concrete—visible and audible. They give a sense of what we wish, or fear, reality itself might be or become. Such films convey truth if we decide they do…Documentaries of social representation…give tangible representation to aspects of the world we already inhabit and share. They make the stuff of social reality visible and audible in a distinctive way, according to the acts of selection and arrangement carried out by a filmmaker. They give a sense of what we understand reality itself to have been, or what it is now, or what it may become.[98]

[96] Barthes, R. (1977) p. 39
[97] Nichols, B. (2001) p. 1
[98] *Ibid*

I find little to distinguish between these two kinds of story. In what sense are wishes or fears about what reality 'might be' different to 'a sense' of what reality 'may become'? To suggest, for example, that the British social documentaries of the 1930s are not expressions of fears or wishes about working class society is to misrepresent their all too evident purpose. Nichols writes that fiction films 'convey truth if we decide they do,' but surely this is the case with non-fiction too. One may join Nichols in saying that every film is a documentary—i.e., a record—but equally one may point out that every film is necessarily a construct, i.e., a sequence of images and sounds whose relation to reality is only indexically guaranteed in the trivial sense that its images are photographic and its sounds 'in synch'. Every film, as Stephen Heath remarks, is a documentary in this narrow sense but at the same time every film is a work of fiction. As Terry Eagleton reminds us, Gibbon and the authors of Genesis no doubt thought they were writing historical truth, 'but now they are read as 'fact' by some and 'fiction' by others.'[99] What determines the matter is how we prepare ourselves.

Both documentary and fiction films may convey arguments in their soundtracks and they may tell stories with their pictures or they may do both at the same time but these options do not distinguish them from each other in any absolute sense. It is no more the case that all films, as Nichols says, are documentaries (although one may say this) as that they are all *narratives* which the viewer interprets (guided by signifiers whose meaning is conventional and unstable) as fiction or non-fiction. Indeed, it seems that whether we prepare ourselves for an argument or not, we almost certainly prepare ourselves for a story, for narrative may be found, writes Roland Barthes, 'in myth, legend, fable, tale, novella, epic, history, tragedy, drama, comedy, mime, painting, ... stained glass windows, cinema, comics, news item, conversation ... it begins with the very history of mankind and there nowhere is nor has been a group of people without narrative.'[100] Narrative is ubiquitous, suggests the historiographer Hayden White, because it 'might well be considered a solution to a problem of general human concern, namely, the problem of how to translate knowing into telling, the problem of fashioning human experience into a form

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[99] Eagleton, T. (1983) p. 2
[100] Barthes (1977) p. 79

assimilable to structures of meaning that are generally human rather than culture-specific.'[101] Narrative, then, is certainly to be found in both scientific papers and the popular science of television documentaries. The interesting question, from our point of view, is what *idea of science* does the narrative we call the science documentary convey and how does it do so *persuasively*?

Narrative and myth

To answer these questions we must understand the nature of this narrative *langue* of which we have so many distinct *paroles*. What are the units and combinatory rules of narrative that we all seem to recognize instinctively and that transcend cultural difference?

In *Film Art* (1993), Bordwell and Thompson define narrative as 'a chain of events in cause-effect relationship occurring in time and space.'[102] This causal chain links the beginning to the ending in a transformative movement characterised by Tsvetan Todorov as having a number of discrete stages. The first is the pre-story stage, a period of equilibrium whose disruption we expect imminently. It is the disruption of this order that has the potential to set a narrative in motion (but only once that disruption is *recognised* by someone). In narrative, recognition of disruption sets in train attempts to recover order and these efforts form the central action of the narrative which only comes to an end when equilibrium has been restored. Almost certainly the restored state is not identical to the beginning but is a new, stable order.[103]

Todorov's analysis does not mean there may not be complicated temporal structures within a narrative framework. Susan Sontag, for example, famously quotes the following conversation between Jean Luc Godard and George Franju: '"But surely, Monsieur Godard...you do at least acknowledge the necessity of having a beginning, middle, and end in your films?". "Certainly...but not necessarily in that order"'. [104] Godard is being playful about the possibilities of emplotment but however playful or perverse the story-teller wishes to be in this regard, for a narrative to *be* a narrative one must be able to discern a beginning and an ending for that is how narrative works. This idea

[101] White, H. (1987) p. 1
[102] Bordwell and Thompson (1993) p. 65
[103] Todorov, T., (1981)
[104] Sontag, S., (1969) p. 157

makes meaningful Vivian Mercier's famous quip by about Samuel Beckett's play, *Waiting for Godot* (1953): 'Nothing happens, twice'.[105] It is not that nothing at all happens in *Waiting for Godot* but that there is a passage from one nothingness to a new nothingness.

But, of course, the world does not come to us in narrative form, speaking for itself; it is, as Christian Metz says, 'uttered by no one'.[106] The unconsidered past is at most a series of events—a *chronicle*—without beginning and without an ending (except in as much as the events terminate in the present). It cannot be said to have any particular meaning. Speaking semiologically, the time-signifier of chronicle floats above its signified, disconnected until we designate a beginning: 'once upon a time…' This is a signal that the recounted events are being presented as leading to an ending and this passage from opening to closing is narrative. Narrative, then, is a specific sort of relationship between the beginning and the ending embodied in a real object that can be subjected to analysis.

In his 1984 paper, 'Narrative strategies in television science—a case study,' Roger Silverstone analyses one such object, the BBC *Horizon* programme, *The Death of the Dinosaurs*.[107] The film tells the story of how a number of palaeontologists came to favour what at the time was a new and controversial hypothesis, namely that the dinosaurs were wiped out by an asteroid impact and in dying off they vacated many ecological niches which were subsequently filled in a rapid radiation of new species based on a mammalian rather than reptilian body-plan and physiology. The film concludes that without this chance event we humans could not have evolved. Silverstone argues that *The Death of the Dinosaurs* tells its story simultaneously on two narrative levels: the 'mythic', which he analyses using ideas from the Russian formalist, Vladimir Propp and the structural anthropologist, Claude Lévi-Strauss, and the 'mimetic' which he subjects to a rhetorical analysis appropriate to examining the structure of argument.

The mythic narrative is a tale of heroic adventure such as found in the oral tradition of folk and fairy tales. In *The Death of the Dinosaurs*, it is a story

[105] See Mercier, V., *Irish Times*, 18 February 1956, p. 6.
[106] Metz, C. (1974)
[107] Transmitted in 1981

of scientists on a quest to uncover the mystery of the dinosaur's sudden extinction. The progress of our scientists is threatened by villains with mistaken scientific ideas against which our heroes and their helpers struggle and eventually prevail. By the mimetic narrative, Silverstone means a narrative dominated by an argument carried by the sound track that makes the logical case for the film's asteroid impact thesis, abstracted from the actions and causal relations of the mythic dimension.

These two dimensions, the mythic and the mimetic are in tension, writes Silverstone: 'every programme…appears as the result of a negotiation between these two, as producers/directors in the management of their programmes must continually make decisions of inclusion, exclusion, stress and emphasis, which bear materially on one or other of these competing narrative frames.'[108] The impact of this tension is perhaps most clearly seen in the film's ending where the mythic narrative presses, as it were, for the closure we expect of a good story while the mimetic urges a degree of caution or doubt. The mythic narrative invites the viewer to imagine themselves a hero; the mimetic invites them to be a judge. Any science documentary, Silverstone concludes, 'will define for itself a particular, though frail, compromise between myth and mimesis, between the heroic and the naturally historic forms of story telling.'[109]

Although Silverstone speaks of the mimetic narrative having a 'naturally historic form', this seems to beg the question of what form history 'naturally' has. I would argue, with Frederick Jameson, that there is nothing natural about history. History, he writes, is an 'absent cause' on which our desire for story, for meaning, must come to grief. 'Yet it follows that this Real—this absent cause, which is fundamentally unrepresentable and non-narrative, and detectable only in its effects—can be disclosed by Desire itself, whose wish-fulfilling

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[108] Silverstone, R. (1984) p. 388

[109] *Ibid.,* p. 388; This reflects a historic tension within the BBC itself which, in the 1950's, under the guidance of Paul Rotha, shifted the documentary department's emphasis away from the previously popular mode of documentary-drama (i.e. the mythic) towards current affairs and talk (the mimetic). Tim Boon quotes the producer Caryl Doncaster: 'The talk informs. The dramatized story documentary interprets. One appeals to the intellect, the other to the emotions.' (Boon, 2008:204) This tension persists and is continually revisited by television producers. In a conversation I had with Jana Bennett in 2001 she explained how prior to becoming the editor of *Horizon* in 1990 she had been sent to Hollywood to study script-writing with Robert McKee. This, she said, alerted her to the importance of the mythic in documentary programmes. During her tenure at *Horizon* she re-emphasized the dramatic or mythic narratives that had been a staple of science output in earlier days at the BBC.

mechanisms are the instruments through which this resistant surface must be scanned.'[110] The filmmaker/historian is the desiring energy that makes a historical account accessible to us. The filmmaker gives a particular form to a historical account and this form in itself communicates an ideology because 'all discourse constitutes the objects it pretends only to describe realistically and to analyze objectively'—in short, as Hayden White puts it, form has *content*.[111]

It is my aim in this dissertation, then, to explore what attitude to knowledge is expressed by the form of the science documentary. Silverstone's notion of the naturalness of the mimetic narrative may be helpful in distinguishing it from myth but we must nevertheless examine the ideology inherent in its structure rather than take its 'naturalness' for granted.

Formalism

In his analysis of the narrative strategies of a television science documentary, Roger Silverstone exploits both Proppian formalism and Lévi-Strauss's structuralism but he does not explore the difference between them. The two however are quite distinct analytical tools. The formalist approach (which we shall explore first) focuses on the general morphology of the tale in order to demonstrate the commonality of narratives and allow us to evaluate the extent to which an individual story, even a story of scientific discovery, may follow a conventional pattern involving the predictable actions of heroes and villains. The structuralist approach seeks to uncover the thematic tensions that the story acts to resolve and so reveal what it is 'really' about at a deeper level.

In *Morphology of the Folktale* (1968), the most significant distinction Propp makes in his search for the commonality of all folktales is between the *dramatis personae* who vary from tale to tale and their actions whose function in each tale, he argues, is constant. These constant functions, properly characterized and delineated give Propp his common morphology:

> Function is understood as an act of a character, defined from the point of view of its significance for the course of the action…Functions of characters serve as stable, constant elements in a tale, independent of

[110] Jameson F. (1981) p. 175
[111] White, H. (1985) p. 2 and White, H. (1987)

> how and by whom they are fulfilled. They constitute the fundamental components of a tale.'[112]

Propp finds there are thirty-one of these constant functional elements and he summarizes them into short headings like 'absentation' or 'interdiction'. He finds that not only is the number of functions finite but so is their order fixed, giving the general scheme of a fairy tale as follows:

> An 'initial situation' is established ('once upon a time in a little village there lived…'). Then one of the characters absents themselves. As a result, through the violation of a prohibition, a misfortune befalls those left behind. A villain enters the scene and does harm to his victim that results in a 'lack'. This lack is then recognized and a hero assigned the task of remedying or 'liquidating' it.

At this point the tale can go in one of two directions. Either the victim becomes the hero or the victim is rescued by another character. In the later case the hero is split but there is only one 'hero-function', supported by one or other character at any one time.

> The hero ('seeker' or 'victim') is dispatched and meets a 'donor' who tests him. The donor may be helpful or hostile and may even fight with the hero. The hero reacts and may acquire in this task supernatural help. ('It often happens that various magical creatures, without any warning, suddenly appear or are met on the way and offer their services and are accepted as helpers.'[113]) The experience with the donor prepares the hero for combat with the villain and it is during this struggle (which the hero ultimately wins) that the hero receives a mark of identification. In defeating the villain the initial lack is liquidated and the hero then sets off home only

[112] Propp, V. (1968) p. 21
[113] Propp, V. (1968) p. 46

> to be pursued by an enemy from whom he eventually escapes. Upon his return the hero is rewarded, often with the prize of marriage.

Sometimes this series of functions is arrested just as it is about to terminate and another misfortune befalls the hero; the villain then reappears and the entire sequence runs through to its conclusion a second time in what Propp calls a second 'move'.[114]

In Propp's scheme, the thirty-one functions are supported by the *dramatis personae*. The villain, for example, supports the functions of 'villainy', 'struggle' and 'pursuit'. Propp therefore speaks not of characters as such but of 'spheres of action' that, being thus tied to the thirty-one functions, are also limited in number. Propp identifies seven: the hero, the donor, the magical agent, the dispatcher, the sought-for person, the villain and the false hero. Each, no matter how it is constituted in a particular instance, plays the same role in the narrative. 'Reduced to its most abstract formula,' writes Lévi-Strauss, reflecting on Propp's morphology, ' the fairy tale can be defined as a development which starts with villainy and ends with a wedding, a reward, a liquidation of lack or harm, the transition being made by a series of intermediate functions.'[115] That the sequence of functions is constant in the tale constitutes the major insight of Propp's scheme. But despite its syntagmatic linearity, there is also a paradigmatic axis implicit in many of Propp's functions which can be grouped into pairs and organized into an unchanging system. It is in these pairings that Propp prefigures the structuralism of Lévi-Strauss.

In Propp's morphology there are two pairs that are rarely found in the same move: 'struggle with the villain'/ 'hero's victory' and 'assignation of a difficult task'/ 'solution'. 'It results from this,' writes Levi-Strauss, 'that four classes of tales can be defined: those using the first pair, those using the second pair; those using them both; and those rejecting them both.'[116] What Silverstone calls the mimetic or argumentative dimension of the classical science

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[114] *Ibid.*, p. 59
[115] Lévi-Strauss, C (1976) p. 124
[116] *Ibid.*, p. 125

documentary is commonly articulated by the second pair, 'assignation of a difficult task'/'solution' while the mythic dimension is given shape by the first, 'struggle with the villain'/'hero's victory'. The science documentary, as understood by Silverstone, is thereby simultaneously both didactic (problem—solution) and dramatic (struggle—victory).

Structuralism

Propp's scheme demonstrates the commonality of narratives but largely ignores content, making it difficult to draw conclusions about the specific ideological convictions expressed by a particular narrative work. As Lévi-Strauss puts it:

> Unless content is … reintegrated into the form, the latter is condemned to remain at such a level of abstraction that it neither signifies anything any longer nor has heuristic meaning. Formalism destroys its object….We know what the tale is, but as experience puts before us not an archetypal tale but a great number of concrete tales, we do not know how to classify them anymore… one can no longer come down from the abstract to the concrete.[117]

If Propp's formalist focus is on *langue*, Lévi-Strauss's structuralism is concerned with *parole*. Structuralism 'refuses to set the concrete against the abstract and to recognize a privileged value in the latter. … *Form* is defined by opposition to material other than itself. But *structure* has no distinct content; it is content itself, apprehended in a logical organization conceived as property of the real.'[118] By structure, then, Lévi-Strauss means what Hayden White calls 'the content of the form'. It is only by studying concrete instances of narratives that we are able to define the universe of a particular tale. This is achieved by identifying the pairs of opposing ideas that are embodied by the characters and actions of the tale.

A myth or narrative is conceived by Lévi-Strauss, then, not so much as a story than as a 'logical model capable of overcoming a [fundamental] contradiction' in life and thought'.[119] Myths are linguistic vehicles for resolving

[117] *Ibid.*, p.132-133
[118] Lévi-Strauss, C. (1976) p. 115
[119] Lévi-Strauss, C. (1963) p. 229

the contradictions that matter for a particular culture in particular circumstances.[120]

In 'Beyond Morphology: Lévi-Strauss and the Analysis of Folktales' (1988), David Pace illustrates the methodological distinction between Propp's formalism and Lévi-Strauss's structuralism. We all know the tale of *Cinderella*. Beautiful and sweet-natured, she lives with her father, wicked stepmother and ugly step-sisters but her father dies and Cinderella is forced by her step-mother to become a servant. A royal ball is held at the castle but Cinderella is forbidden to attend. However, after the others have left, a fairy godmother appears and magically transforms Cinderella's clothes and surroundings into a beautiful dress, coach and coachmen. She goes to the royal palace where she captures The Prince's heart. But at midnight the magic wears off and Cinderella flees, leaving behind one of her glass slippers. The besotted prince travels the length and breadth of the kingdom searching for the foot that fits the slipper. Finally he enters Cinderella's house. The ugly sisters fight each other for the slipper but it only fits Cinderella. And so The Prince and Cinderella are married and the wicked step-mother and her children are humiliated.

A Proppian approach seeks to identify the elements of the Cinderella plot with the 31 functions common to this sort of narrative. For example, the death of Cinderella's father might be identified with *Absentation*; Cinderella's desire to go to the ball could be identified with the function of *Lack* and the receipt of beautiful clothes with *Receipt of Agent: hero acquires use of magical agent.* The whole tale would be ordered in this fashion and something could be concluded about the formal patterns that underlie this type of story. However, to reconstitute the semiotic codes at work in *Cinderella* and so understand the underlying ideology of the story, a structural analysis is called for.

Unlike Propp's formal analysis, a structural analysis is not necessarily concerned with the chronology of the story or a simple list of individual characters who appear in it but instead might begin, as David Pace does in his essay, by characterizing the *relationships* between characters in the 'initial situation'. Initially the stepmother and her biological children have a positive relationship to each other and a negative one to Cinderella. The father has a

[120] *Ibid*., p. 130

positive relationship to Cinderella but an uncertain one with the stepmother. The step-sisters are characterized as evil, vain, ugly, clean and of high status while Cinderella is good, humble, industrious, beautiful, dirty and of low status.

Immediately we can see, as Pace puts it, that 'the external, social signs of virtue have not been assigned to the right persons. This contradiction provides the dramatic core of the story and is resolved at the end of the tale.'[121] The tale's meaning is articulated by the antinomies of evil/good, vain/humble, ugly/beautiful, clean/dirty, high status/low status. Through the transformation of one into the other the contradiction is corrected and justice prevails.

An examination of the relationships at the start reveals the ideology implicit in the story: Pace points out, for example, that the story can be understood as an economy of males: the father's death removes a male and causes the imbalance that the Prince's marriage to Cinderella then corrects. Psychologically, the tale also delivers a deep message embodied in the binary opposition between the qualities of the fairy godmother and those of the stepmother. Together these two characters represent the attributes of a whole mother. Splitting the mother into two opposing spheres of action enables the tale to articulate the emotional landscape of the child. The stepmother (i.e., the *wife* of Cinderella's dead father) is selfish and favours her biological children while the godmother is kind and generous and has no sexual connection to the father. Thus the story precipitates out the child's Oedipal emotions which become focused on the person of the evil stepmother (who has sex with the father and may be hated without guilt) while the child's loving feelings towards her mother may become attached to the fairy godmother without reservation.

The story is set in a society where only ties of blood are considered strong enough to hold a family together but at the same time it also expresses the social ambivalence of mothers: the stepmother is concerned only with Cinderella's *economic* value while the fairy godmother expends her own resources to prepare Cinderella for marriage. The fairy godmother, as a supernatural being, recognizes Cinderella's true value beneath her signs of low status and as a force for social justice she transforms the very symbols of Cinderella's degradation (rags, mice etc.) into the outward signs befitting her

[121] Pace, D. (1988) p. 253

inner virtues. In this transformation she thus reveals to society the girl's true value.[122]

Pace's analysis demonstrates how a formalist approach may be used to discover the abstract shape of a particular tale while a structuralist analysis opens the story out to the world: 'With the formalist we have a method which is automatically apolitical (i.e., conservative), while the structuralist offers an approach which can be used to reveal the origin and nature of ideology.'[123] As it is my concern, above all, to explore the ideology of the science documentary, this form of analysis is invaluable. I apply it here to an example of a classic science documentary, *The Ghost in Your Genes* (2005), a film that in a number of ways explores similarly 'heretical' views on inheritance to my own film. In a later chapter I will make a similar analysis of *Hopeful Monsters: An Experiment* and thereby compare it to *The Ghost in Your Genes*.

***Analysis of* The Ghost in Your Genes**

The Ghost in Your Genes was produced and directed by Nigel Paterson of Clear Cut Pictures and transmitted as a BBC *Horizon* programme in November 2005.[124] As with most *Horizon* programmes of recent years it runs for about 50 minutes. The film opens with a montage of children upon whose naked bodies are projected black and white 'archive' or newsreel images of past world events—a World War II anti-aircraft gun firing into the sky, the dockyards ablaze during the London blitz, a line of migrants on the deck of a ship; and images of everyday life at that time—a child being cradled, a man smoking.

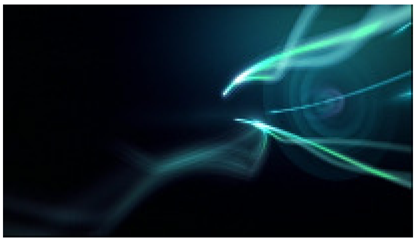
[122] *King Lear*, *Pygmalian* and the film, *Pretty Woman* (1990) are some of no doubt hundreds of texts that share many of the functions and binary oppositions of the Cinderella story.
[123] Pace, D. (1988) p. 257
[124]A full transcript of the film is available in Appendix 1

On the soundtrack we hear a low, menacing note. About four seconds from the start, the BBC logo appears and we hear a woman's voice with an RP English accent, the recognizable voice of BBC *Horizon*: 'We are on the brink of uncovering a hidden world, a world that connects past and future generations in ways we never imagined possible.' The montage continues as a man's voice breaks in and we see his 'talking head' against a dark, abstract background:

'…an environmental exposure that your grandmother had could cause a disease in you, even though you've never been exposed to the toxin…' The image reverts to the montage but the voice continues '…and you are going to pass it onto your great-grandkids.' We see an extreme close-up of a child's eyes and hear the narrator's voice again:

'These extraordinary discoveries have the potential to affect every aspect of our lives.' Again we see images projected on children's bodies: women picking potatoes or gleaning a field; a woman smoking, with her child in her arms.

A new male voice: ‘It’s not just the genes but also the environment in the early life of your ancestors.’ We see the speaker’s face (again, a ‘talking head’ against an abstract background): ‘It’s not so much you are what you eat…’ (Cut to the shots of children again on which are superimposed faces of adults smoking and eating) ‘…it’s that you are what your mother ate, and maybe you are what your grandmother ate…’ We see the speaker’s face again: ‘…and if you take our data, you are what stress your grandmother or grandfather had.’ Montage of archive images: children in a playground, a baby lowered into a crib. The narrator: ‘It will change the way we think about our relationship with every generation.’ Children playing old fashioned school-yard games. A new male voice: ‘It makes me feel closer to my children. What I experience in terms of environment…’ (we see the man’s face) ‘…will have some type of legacy in my children and my grandchildren.’ Back to images of naked children on which are projected women harvesting a field. The narrator: ‘The science of inheritance is being turned on its head.’ We see yet another man who says: ‘We are changing the view of what inheritance is.’ The screen goes black and we hear the swell of the familiar *Horizon* theme music as animated streaks of light curve and snake across a deep blue background until they hit a point on a ‘horizon’ and resolve into the word, ‘Horizon’.



This then cuts abruptly to a second title: ‘The Ghost in Your Genes’ against bright moving hexagonal ‘circles’ that seem to dance—an image of the run-out of a spool of home movie footage. Music mimics the sound of a projector. The screen fades to black and the sound dissolves into bird song and a distant church bell tolling. The screen lightens again revealing a village far off…

This two-minute pre-title sequence is characteristic of modern *Horizon* programmes. It features a voice-over narration linking brief snippets of ‘common’ interviews that we expect to hear again more fully and contextualised as the film unfolds. These talking heads are placeless, set against an abstract background while the other images we see (i.e., the montage) appear to illustrate what these voices say. The images of children and their mothers bring to mind the idea of ‘inheritance’ while the images of war and smoking clearly stand for the ‘experiences’ that have had an impact on subsequent generations—the mystery the film promises to explain.

The whole is ‘pre-title’ so that, separated from the film proper, it acts as a prologue, an extra-narrative presentation of theme and characters delivered, as in classical Greek drama, by a deity, the unseen narrator of great authority. The narrator speaks the final words of the prologue in the passive voice, summing up the claim the film will now support: ‘The science of inheritance is being turned on its head’. This is emphasised, ‘proved’ if you will, by the face and voice of the next contributor who we will soon learn is the seeker-hero of the piece, Marcus Pembrey: ‘We are changing the view of what inheritance is’. All contributors so far (note, Pembrey says, 'We') seem to support the narrator's claim. The prologue promises, in breathless tones, that we shall be let into 'a hidden world' and discover a science 'we never imagined possible'. We will redefine how we think about 'our relationship with every generation'. By implication this new science will solve an important problem or 'liquidate a lack', in Propp's terms. The problem is not yet clearly stated but it involves inheritance and the well-being of vulnerable (naked) children.

Immediately after the introduction, as we look down on a distant village menaced by dark clouds, the narrator, speaking with the authority not only of the BBC but of Science—a true Demiurge—inaugurates the story that promises to explain the mystery of the children's vulnerability:

'This small Swedish town may hold the evidence to launch a medical revolution. Overkalix lies huddled on the edge of the Arctic Circle, inaccessible and remote. It was cut off from the rest of the world for most of its history.' The town is 'inaccessible and remote' but, the narration implies, we will be impressed by how the hero of this story nonetheless does whatever it takes to bring to light the important evidence the town holds. At this point the mythic and argumentative dimensions of the film's narrative may be clearly discerned. On the one hand a dramatic narrative is being set in motion—we see two men walking through some woods and into a graveyard outside Overkalix (our heroes stepping onto the stage): 'Marcus Pembrey has travelled here to meet his colleague, Olov Bygren...' On the other hand, an argument with evidence and proof is also beginning…

‘This small Swedish town *may* hold the evidence…They believe that the story lying buried in these graveyards *may* hold the proof to their radical ideas.’ A voice (Pembrey’s) agrees: ‘This group of people *could* contribute to a sea change in the way we think about inheritance.’

The narrator continues: ‘They have come to this churchyard to find grandmothers and granddaughters, grandfathers and grandsons, connecting people who lived almost a hundred years apart in entirely new ways, uncovering links that confound scientific thinking.’ Dramatic music underscores the phrase ‘confound scientific thinking’ and then Marcus Pembrey, facing the camera, speaks to an unseen interlocutor while a caption identifies him as a credentialed scientist—a professor—signifier of trustworthiness and rationality:

‘Up till now inheritance is just the genes, the DNA sequence. I suspect that we’re going to demonstrate that inheritance was more than that.’ Pembrey walks among the headstones with Bygren, and the female demiurge continues to explain:

> “It is the culmination of more than twenty years’ work. And for the first time, Pembrey is confronting the magnitude of their discovery …

> Marcus Pembrey is one of a select band of scientists, a band of scientists who are daring to challenge an orthodoxy. They believe the lives of our parents, grandparents and even our great-grandparents can directly affect our well-being, despite never experiencing any of these things ourselves. To many, these ideas are regarded as scientific heresy." (00:03:21)

The co-existence, simultaneously, of what Silverstone identifies as two distinct narrative dimensions is evident from the temporal ambiguity of this graveyard scene. First we are told that the science about to be presented by the film is the 'culmination of more than twenty years' work;' this means that the scene in the Overkalix graveyard *postdates* all the events of the upcoming film. The scene appears to be the terminus, the end of a story, and yet it is placed here, at the start, because it makes sense in the structure of the film's *argument*. But at the same time the scene *is* the temporal origin of a *mythic* story because Pembrey's speech to camera cues us to understand this moment as the beginning of a diachronic tale, a journey of discovery that will unfold during the rest of the film. He is one of a select band of detective-scientists who *are* (present tense) daring to challenge an orthodoxy. 'I suspect that we're *going* [i.e. from now on in this film] to demonstrate that inheritance was more than that.'

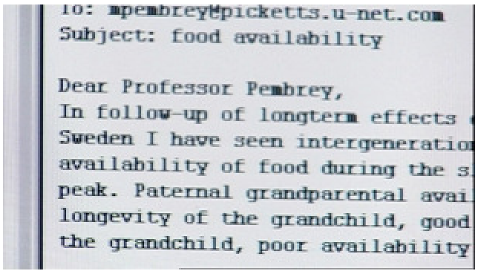
Narratologists of course distinguish between plot time and fabula time and it is surely correct that the viewer reconstructs the fabula in the course of viewing the plot. What is interesting about this scene is that there appear to be two different fabulae to construct from the same plot. In one (the mythic narrative) the scene is positioned chronologically (at the start), in the other (the mimetic narrative) the scene is understood as the conclusion of an argument, placed here as an introductory summary to whet our appetites. The ambiguous temporality of this scene, then, neatly illustrates Silverstone's contention that there are, as he puts it, two narratives running through the science documentary. On the one hand the scene can be understood as part of the exordium of an argument while on the other it represents the first meeting of the Proppian hero, Pembrey with the donor, Bygren.

A further distinction between the argumentative and mythic dimensions of the film is apparent in the way each treats or adapts its source materials. *The Ghost in Your Genes* adapts a series of scientific papers to build its argument while other textual sources are adapted in the construction of the mythic narrative. Dudley Andrews argues that there are two possible modes of relation between film and such source materials. The film adaptation can either *borrow* or *intersect* with the source texts and it can be more or less faithful to the 'letter' or 'spirit' of those texts.[125] In the fiction film at least, borrowing is the most frequent mode. One need only think of the numerous film versions of Shakespeare or of Dickens or of Agatha Christie. An audience views such films with a pre-conceived notion of what they will experience and looks forward to it. Whereas in Dickens or Christie the filmgoer perhaps hopes for fidelity (of one sort or another), in more 'mythic' tales such as Shakespeare's the viewer is excited also by the richness of archetypes that reach back into myth.

By contrast, the mode of 'intersecting' approaches the source text in the opposite way, as Dudley Andrews explains: 'Here the original text is preserved to such an extent that it is intentionally left unassimilated in adaptation. The cinema, as a separate mechanism, records its confrontation with an ultimately intransigent text.'[126] The film comes up against or illuminates a portion of the text in question but whatever is revealed, like the corner of a darkened room by flashlight, is *seen* by the film not *transformed*. Referring to Pasolini's confrontations with St. Matthew's Gospel (1964), and his *Medea* (1969), *Decameron* (1971) and *Canterbury Tales* (1972), Andrews writes, 'All such works refuse to adapt. Instead they present the otherness and distinctiveness of the original text, initiating a dialectical interplay between the aesthetic forms of one period with the cinematic forms of our own period.'[127] The analyst of adaptations in the intersecting mode is concerned with how the original lives on, has its own specific life within the cinema.

In *The Ghost in Your Genes*, the narrator tells the viewer that Pembrey "published his ideas in an obscure journal and largely forgot about it. After all, there was no evidence for any of this. It was pure speculation. Then four years later Marcus received an e-mail from a doctor in Sweden." (00:25:36) We never see the paper published in the obscure journal but its contents are paraphrased by the narrator. By contrast, it is Pembrey who describes the e-mail which we see on screen:

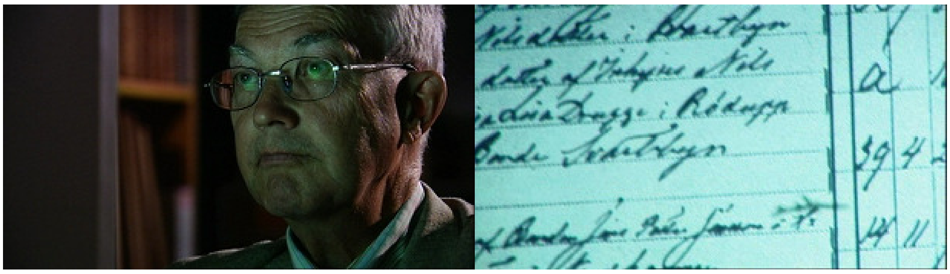
[125] Andrews, D. (1984) p. 98
[126] *Ibid.*, p. 100
[127] *Ibid.*


To: mpembrey@picketts.u-net.com
Subject: food availability

Dear Professor Pembrey,
In follow-up of longterm effects
Sweden I have seen intergeneratio
availability of food during the s
peak. Paternal grandparental avai
longevity of the grandchild, good
the grandchild, poor availability

> "It really came as a bolt out of the blue. I just got an e-mail in May 2000 saying my paper was the only thing he could find in the literature that in any way sort of tied in with his basic observations." (00:26:02)

Similarly, we are also afforded a representation of the parish records that Bygren has written about to Pembrey:



The *mythic* narrative 'intersects' with its sources but the film never shows us any of the scientific papers that inform the film's *argument*. These sources are 'borrowed'. The mode of borrowing supports a realist aesthetic whereas the intersecting mode of adaptation draws attention to the film text as a signifying system distinct from that of its sources. The mythic narrative distinguishes itself from argument in this regard, treating the sources of the myth as elements in a drama with their own form, while the sources of the argument are represented as pure content—knowledge.

Thus in two clear ways the argument and the mythic narrative are distinguished one from the other. Let us first trace the course of the film's argument before returning to examine its mythic dimension.

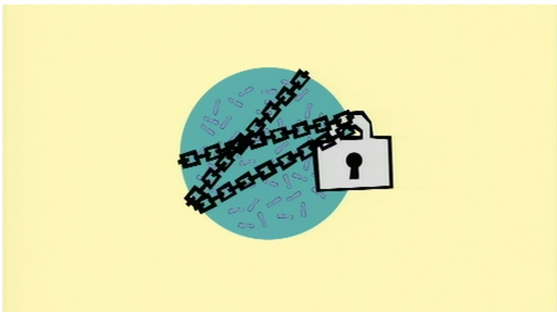
The Argument

The argument of *The Ghost in Your Genes* follows the structure known since classical times: *exordium* and *narratio* where the problem is introduced, *divisio* where it is divided into manageable parts, *confirmatio* and *refutatio* where the proffered solution is confirmed and alternatives refuted and lastly a *peroratio* that sums up the argument.

As in Silverstone's analysis of *The Death of the Dinosaurs,* we may locate in *The Ghost in Your Genes* arguments for and against the thesis the film presents, a thesis that, we are told, 'to many' is 'regarded as scientific heresy' (00:04:10). But, unlike *The Death of the Dinosaurs*, in *The Ghost in Your Genes* no one person represents the 'many' who adhere to the 'conventional biology' and whose short-comings this film will demonstrate and resolve. Instead, the demiurgic narrator takes full responsibility for presenting the orthodox, conventional perspective and of orchestrating the material that will form a convincing and, as we've been told, 'revolutionary' response to it. This strategy, then, takes for granted that there is a clear consensus that the orthodox view is inadequate and in addition it leaves no space for disagreement, implying that the 'many', if they are rational, must immediately accept the truth of the radical solution about to be presented by the film. It only remains for the narrator to lay out this argument for it to meet with universal agreement.

Although the mythic narrative, as we will see, presents a story of scientific discovery, the argument has an explicitly *didactic* purpose which is to demonstrate *post facto*, the reasons for believing what the narrator is already convinced of, i.e., the correctness of a radical new theory of inheritance. The aims of the mythic narrative and the argument are therefore at cross-purposes. The argument seeks to explicate a scientific idea that is implicitly already acceptable to scientists while the mythic narrative tries to tell a story of 'science in the making'. I say 'tries' because we approach a mythic narrative of this sort with the understanding that the hero will succeed in his or her task and so, this 'science in the making' is actually already made. The mythic narrative tells a story of science in the making by telling it in the present tense, making it an ongoing investigation, while the argument is a timeless structure, like a textbook. As a textbook, it is unconcerned with relating the history of a scientific idea (a history that, to be complete, must contain failed experiments, for example) but simply of laying out the reasons for believing the new theory, reasons that have already compelled assent by all fair-minded scientists.

In *The Ghost in Your Genes*, the argument begins with an animated sequence explaining the orthodox theory of inheritance. It is notable that this sequence is set outside of the temporal structure of the film's narratives. Narrative is a metaphor of time, inventing one sort of temporality in terms of another. The chronicle level refers to events that exist 'within time' but the narrative level refers to another sort of temporality—metaphorical or, as the philosopher Paul Ricoeur calls it, 'historical time'—in which endings are linked to beginnings. Narrative then creates time within time. This contrasts with *description* which creates *space* within time or the *image* which creates space within another space. In narrative, then, the signified is temporalized ('this happened then this happened') but in description it is spatialized ('there it is'). To move from a narrative mode to a descriptive moment is to 'pass through a change of intelligibility, in the sense in which one speaks of a change of gears in automobiles.'[128] This change of intelligibility cues the viewer to relate to this animated sequence as outside of the narrative structure of the film proper. Effectively it is a passage of description: "In classic genetics, your parents and grandparents simply pass on their genes. The experiences they accumulate in a lifetime are never inherited, lost forever as the genes pass untouched through generation after generation": (00: 05: 13)



"After conception, it was assumed that our genes are locked away inside every cell of the body, protected and untouched by the way we live." (00:04:52)

[128] Metz, C. (1974) p. 20

The style of the animated sequence is itself a rhetorical device: we understand that this cartoon reflects a view that such a story is overly simplistic.

Next the voice-over informs us that the human genome project was instituted on the (implicitly foolish) assumption that if we could learn the entire sequence of the genetic code we would be in a position to understand all of human biology. But knowing the code, we are told, has proved less useful than we thought. There remain problems that it cannot solve. One is the case of Angelman's syndrome and Prader-Willi syndrome, two very different diseases caused by the same deletion of a gene from chromosome 15. If the genetic code is all there is to biology then how can the same gene (or its absence) cause two very different diseases?

In fact, we are told, there is reason to believe that the complete code is far from the final key to human biology simply because the human genome turns out to be much smaller than expected. It contains only about 30,000 genes, fewer than some plants. As Mike Skinner puts it, 'If the genome has less genes in this species versus this species and we're more complex potentially, what's going on here?' (00:10:40) This is the problem the film seeks to answer: how can such a small genome give rise to the complexity of the human body and its variety of diseases?

Having laid out the problem and its background, the argument now subdivides it into smaller parts (*divisio*). First, exploring the mystery posed by Angelman's and Prader-Willi syndromes we learn from Marcus Pembrey that the *origin* of the damaged chromosome 15 determines which syndrome a child inherits (if the damaged chromosome is from the father this causes Prader-Willi, if from the mother, Angelman's). Conclusion: if the chromosome can remember where it comes from then some kind of marking or 'imprinting' must be taking place when it is still in the egg or sperm.

Now Wolf Reik and then Mike Skinner develop the idea. Reik: "You can think of it as a light switch. Switch on the gene, the light is shining, the gene is active ... makes the cell do a certain thing. Or the light switch is off, everything is dark. That gene is off. The switches remain on or remain off, and that gives the cells their identity." Skinner: "Whether those genes are turned on or off is called epigenetics." Pembrey reinforces the word: "Epigenetics, you know 'upon the, the genes'" and finally Jonathan Seckl: "So clearly we have additional levels of complexity that we now need to understand, that are well beyond the DNA." Reik sums up this section: "The next huge challenge for modern biology is to now decipher the epigenetic code, to understand all the combinations of switches that exist." (00:15:52)

The argument now interjects a confirming case study. Baby Ciaran was conceived by IVF and he suffers from the very rare Beckwith Wiedemann syndrome. Reik has discovered that simply placing a mouse embryo in a culture dish can trigger genes to switch off. Noting that during IVF the human egg spends a short time in a culture dish, the narrator asks, "Could IVF be switching genes on or off as in the mouse example? Could IVF itself cause the syndrome?" And Reik answers immediately, (*confirmatio*), "What we found was an increased occurrence of this epigenetic syndrome in the IVF population." Note the noun-phrase: the syndrome is now *defined* as 'epigenetic', reified or 'black-boxed' as Bruno Latour puts it—a process that *acts*.[129]

I think it should be clear that the argument is nearly entirely comprehensible just from the words we hear. The images that accompany those words, whether of the faces of the speakers or other elements (images of mice, culture dishes etc.) mostly serve as the familiar signifiers of 'scientificity'. That the talking heads of the scientists are set against abstract backgrounds indicates their words are of primary interest but while these words may appear to be driving the argument they are of course orchestrated by the narrator. Furthermore, the abstract nature of these talking head images indicates we are to consider the heads' statements

[129] Latour, B. (1987); Latour's ideas will be explored in greater detail in Chapter 4

about the nature of *nature* as beyond doubt for although notionally these statements are made by fallible individuals, these scientists are not presented as such, rather they are 'scientists' in inverted commas, signifiers of certainty who contribute their bodies and white coats as rhetorical resources. Their evidence is incontestable. As Nichols writes of this approach to the documentary:

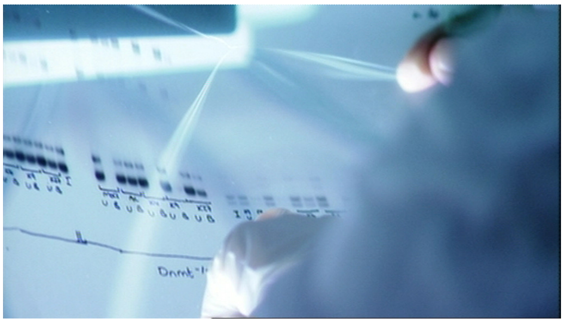
> The voices of others are woven into a textual logic that subsumes and orchestrates them. They retain little responsibility for making the argument, but are used to support it or provide evidence or substantiation for what the commentary addresses.[130]

The narrating voice can make sense of what we see (these talking heads etc.) precisely because it is not of the same order as these images. The theologian, Frederick Ruf terms this form of narration, 'magisterial' in that the relationship of the narrating voice to the persons, events, objects that are narrated is *external*.' [131] The magisterial voice is what Nichols terms the Griersonian voice, 'in which the corporeal I who speaks dissolves into a disembodied, depersonalized, institutional discourse of power and knowledge.'[132]

In Ruf's terms, in the classic (Griersonian) documentary, 'The narrator sees actions, events and objects from without … The narrator is *master* of the events, persons, objects and their meaning.'[133] The narration is magisterial precisely because it is disembodied. It comes from a place 'removed from the fallibility of the human sphere.'[134] Here we may note the debt that the classic science documentary has to the rhetoric of the received view. Like the passive voice of the technical paper, the magisterial voice of the classic science programme eliminates the human element and by thus objectifying, eliminates ambiguity and eliminates *society*. And as we noted above, along with the magisterial voice that renders the scientist a mere cipher, we also have, in this documentary, examples of the nominalistion we find in written scientific discourse. One occurs when Wolf Reik handles the electrophoresis gel that he

[130] Nichols, B. (1991) p. 37
[131] Ruf, F., (1993) p. 294 (my emphasis)
[132] Nichols, B.(1993) chapter 10
[133] Ruf, F. (1996) p. 803; We shall be more nuanced about Grierson's views in Chapter Six.
[134] Beattie, K. (2008) p. 12

believes bears signs that demonstrate the heritable epigenetic effect of placing a mouse embryo in a culture dish:



> "You had dots that you were looking at and every dot means a gene is on, and all of a sudden you know somebody said, wow look at that. The epigenetic switch thrown in one generation was clearly also present in this second generation" (00:22:21)

These dots become not just signs of switches but the switches themselves. And so, the voice-over concludes:

> "This meant that the genes were not locked away. A simple environmental event could affect the way genes worked and that could be inherited. As if a memory of an event was being passed down through generations. It was something many scientists regarded as impossible. If this effect could be observed in humans the implications would be profound. It would mean that what we experience could affect not just us but our children and our grandchildren." (00:23:15)

The narrator thus speaks of nature as the self-evident cause of the phenomenon witnessed. Those dots *are* switched-on genes.

Having demonstrated the reality of epigenetic switches, the argument moves on to consider the evolutionary significance of epigenetic effects in conferring some kind of transgenerational adaptation. Pembrey explains that he has been puzzled by the problem of how a mother born in one generation could pass on important information to her child growing in the next. For instance, if the mother had been undernourished as a child and had matured with a small pelvis, how would the baby know not to grow so big as to jam the mother's birth canal? There must be a mechanism, says Pembrey, which would allow the mother to pass on information about her life experience to the growing embryo. The answer must be that the mother's genome becomes epigenetically altered in a way that, passed to her unborn child, ensures the baby's genes limit its prenatal growth. Note that the film takes this idea not as speculation but as the only and obvious answer to a puzzle. It is taken for granted that (a) babies could potentially grow so big as to jam the birth canal and (b) but for an epigenetic effect that prevents it, they would do so.

Refutatio: In New York, a psychologist, Rachel Yehuda studies the transgenerational effects of the holocaust. She presumes the children of survivors experience symptoms of trauma because they have been constantly exposed to their parents' shocking stories. But in Edinburgh, Jonathan Seckl studies the effects of stress in rats. He finds that stress applied to pregnant rats alters the stress response of their offspring. This, we are told, is a refutation of Yehuda's purely psychological explanation for the emotional problems suffered by the children of holocaust survivors. The children are stressed, Seckl argues, because, like the rats he studies, their mothers were stressed when their babies were in the womb and this stress response is passed on by means of an epigenetic imprint on the genes coding for the 'stress hormone', cortisol. Yehuda then, in a 'crucial test' of this suggestion, expands her study to include women who were pregnant when caught up in the attack on the Twin Towers. For example, a woman called Ailsa Gilliam. Yehuda finds that both Gilliam and her baby have abnormal cortisol levels in their saliva.

> Narrator: "It appeared that epigenetics might be responsible, that an event had altered the stress response in the children."
>
> Yehuda: "What these findings did was suggest to us that we need to be looking where we hadn't even considered looking before."
>
> Narrator: "To know for certain that this was an epigenetic effect, they'll need to be sure that their observations weren't simply due to high levels of stress hormones in the womb."

Here it is noteworthy that the narrator reframes Yehuda's conclusion (in which she is careful not to conclusively invoke epigenetics) as clear evidence for the epigenetic hypothesis: "The work of Yehuda and Seckl offers tantalising *evidence of proof* of inherited epigenetic effects in humans." (00:35:19) (No mention is made of whether, as seems unlikely, the holocaust-surviving mothers were pregnant during their traumatic experiences.) What is significant from our perspective is the epistemic authority of the film—its certainty. The magisterial voice-over speaks, as the sociologist David Bloor writes, "as if experience can deliver a series of decisive judgments on the applicability of a concept to reality."[135]

The voice-over concedes that Yehuda's work, while almost certainly '*evidence of proof*' cannot act as a conclusive crucial experiment because it deals with only one generation. We are then neatly brought back to the Swedish work with which the argument began:

> "The only way forward was to look back to the past. In Sweden Pembrey and Bygren had data that provided the chance to study the effects of famine through many generations. Olov Bygren was looking to see if poor nutrition had an effect on health when he stumbled on something curious. It appeared that a famine could affect people almost a hundred years later even if they never suffered a famine themselves. He wanted to know how this might be possible, so he asked Marcus Pembrey." (00:35:19)

Pembrey finds the incidence of diabetes in the Overkalix grandchildren is correlated with their grandparents' experience of famine. He then finds that the effect only occurs if there is a famine during certain 'sensitive' periods which for the men is during adolescence and for the women when they are still in the womb. Pembrey argues therefore that the epigenetic switch is thrown when the germ cells of the grandparents are being laid down.

> Narrator: "Pembrey and Bygren have the first *conclusive proof* of an environmental effect being inherited in humans. The impact of a famine being captured by the genes in the eggs and sperm and the memory of this event was being carried forward to affect the grandchildren generations later." (00:42:14)

This concludes the argument.

Summary

The argumentative structure of *The Ghost in Your Genes* (Silverstone's mimetic narrative) is entirely consistent with those Ron Curtis finds in the popular

[135] Bloor, D. (2007) p. 272

science magazine of the AAAS.[136] Such structures present a picture of linear development via crucial experiments to 'conclusive proof'. As Silverstone writes: 'like so much on television this framing logic is both remarkably simple and remarkably effective. It mirrors the…view of how science itself is undertaken.' [137] This means, for example, that the mimetic narrative gives almost no space to disagreement, presenting its argument as if there were a shared algorithm that all scientists employ. The narration itself says that "to many, these ideas are regarded as scientific heresy" (00:04:11) but does not acknowledge the reasons why it is heretical, i.e., that there may be good reasons to think it wrong. Indeed, the narrator implies that these 'many' who think it heresy nonetheless cannot fail to recognize the truth once it is explained to them—as it now has been. (Not much of a heresy, then.) The process of theory choice is reduced, by the rhetoric of the classic science documentary, to a series of crucial experiments that guide the supposed scientist-detective to certain truth through a garden of forking paths. The argument of the classic science documentary, at least as exemplified by *The Ghost in Your Genes* focuses entirely on how scientists map the external relations between a theory and the 'nature' it purportedly describes. We shall see in the next chapter how this picture of theory choice in science is seriously undermined by the work of Thomas Kuhn, but before re-examining the assumptions displayed by the argument of *The Ghost in Your Genes* let us examine the story of scientists and scientific practice that it tells—its mythic narrative.

The mythic narrative

Applying Propp's rubric, the morphology of the film's mythic narrative may be summarized as follows:

Initial situation: There lives in London a doctor called Marcus Pembrey. Pembrey works in a children's hospital and is a good clinician and a kind, sensitive man.

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[136] American Association for the Advancement of Science. See above, chapter one.
[137] Silverstone, R. (1984) p. 398

Absentation: He is very successful in his narrow field of clinical genetics but is an outsider (i.e., professionally absent) in the field of evolutionary studies in which he also takes an interest.

Interdiction: Through obedience to the injunction against dabbling in areas outside of his clinical expertise, a misfortune is befalling many of the children in his care.

Villainy: These children suffer from congenital diseases that afflict them strangely and orthodox medicine and science (represented by The Human Genome Project) cannot help them.

Lack: A lack of explanation persists, exacerbated by the rigidity of the scientific establishment and the stranglehold of classical genetics.

Mediation: Marcus Pembrey is identified as the man to liquidate this lack:

(The seeker-hero strides towards us in slow motion, scattering pigeons and people before him)

Pembrey publishes some speculations in an obscure journal which is read by Olov Bygren in Sweden. Bygren has been looking at births, deaths, and records of famine in the parish archives of Overkalix. He notices what looks like transgenerational genetic effects similar to those that Pembrey has speculated on. Bygren writes to Pembrey to ask his opinion.

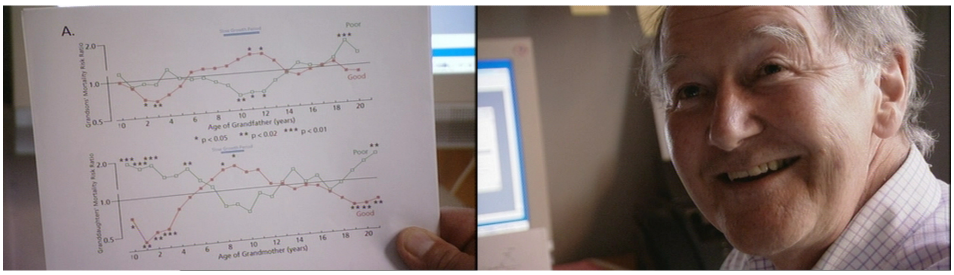
Departure: Pembrey is dispatched to Overkalix, a village that 'lies huddled on the edge of the Arctic Circle, inaccessible and remote, cut off from the rest of the world for most of its history.'

1st Donor function: Bygren alerts Pembrey to the data he has gathered from parish records, data that defies classical explanation.

The other scientists that are now introduced to the story function, in Proppian terms, as *helpers* to our hero, Pembrey. Although they are separated spatially and temporally (no one scientist even mentions any of the others), all these other scientists are presented as co-workers in a sort of virtual laboratory. In this way the film constructs a picture of a 'scientific community' in which data and ideas (the *magical agents* in Propp's terms) are openly shared:

Receipt of magical agent: Pembrey receives help and support from the work of these other scientists.

Victory, Liquidation, Return:
The final crucial piece of data on diabetes and famine is sent as a rough diagram by Bygren to Pembrey (in terms of Propp's spheres of action this diagram is the 'sought for person'). This causes a liquidation of the lack:



> Pembrey: "Hand drawn, this is what Olly sent me; you know he was too excited to wait for the thing to be drawn out properly. You know he sent me the data and in fact I was recovering from having something done on my heart so he sent it saying you know I hope this helps you get better quickly you know because it was so exciting."
>
> Narrator: "When Pembrey plotted out the diagram he was immediately struck by its significance." (00:40:29)

Note that this phrase, 'immediately struck', as described in the previous chapter, is an expression of inductivism. The blank slate of the scientist's open mind is struck and the truth impressed upon it like a coin struck from a metal blank:

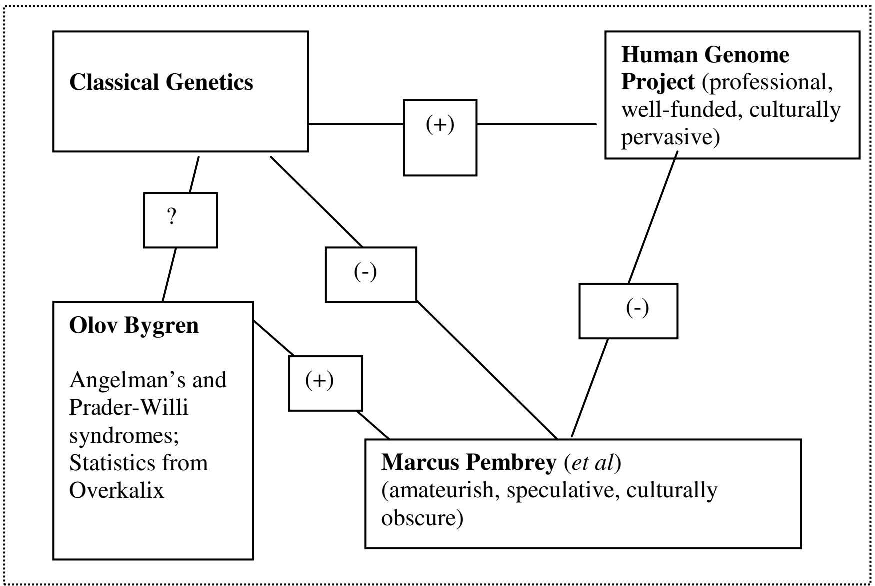
> Narrator: "The more they looked, the more patterns started to appear."
>
> Pembrey: "Once I had plotted out the full extent of those results, it was so beautiful and such a clear pattern I knew then quite definitely that we were dealing with a trans-generational response. It was so coherent and that's important in science, that the effect was coherent, in some way was tying in when eggs and sperm were being formed."
>
> Narrator: "The diagram showed a significant link between generations, between the diet in one and the life expectancy of another."

Recognition, exposure, transfiguration, punishment (of the villain), wedding:

> Pembrey: "This is going to become a famous diagram, I'm convinced about that. I get so excited every time I see it. It's just amazing. Every time I look at it, I find it really exciting. It's fantastic."
>
> Narrator: "Pembrey and Bygren have the first *conclusive proof* of an environmental effect being inherited in humans." (00:42:00)

Structuralist analysis

The diagram bellow summarizes the *initial situation* of the mythic narrative:



In this diagram we can discern how the contradiction between the qualities associated with the hero and with the villain provides the dramatic core of the story. Classical genetics, ‘personified’ by The Human Genome Project, is assigned the virtues of professionalism while the hero, Marcus Pembrey, is a self-confessed amateur and speculator. He is obscure and his ideas are without corroborative evidence while the orthodox view is solidly supported and commonly accepted. The tale of *The Ghost in your Genes*, then, effects a transformation such that the virtues attributed to the Human Genome Project are transferred to Pembrey. It is noteworthy that whereas in *Cinderella*, the scullery maid’s inappropriately assigned attributes are reassigned to the step-sisters, in *The Ghost in your Genes*, those inappropriately assigned to Pembrey are not transferred to the Human Genome Project. Such a mutual switch would run counter to the idea of science as progressive, the idea that epigenetics is a *refinement*, not a replacement for classical genetics. Despite the voice-over’s talk of ‘revolution’, epigenetics is here represented as an *evolution* of classical ideas.

The sense, then, of *moral closure* is weakened in this narrative of what Thomas Kuhn calls ‘normal’ science. Weakened but not entirely absent. Just as *Cinderella* divides the role of mother between the evil step-mother and the fairy godmother, *The Ghost in Your Genes* divides the role of ‘causal factors in inheritance’ between epigenetic markers and classical genes. The effect of splitting one from the other is to precipitate out Pembrey’s emotional attachment to orthodox genetics which may then be criticized (with good reason) while his guilty pleasure in speculation may become attached to the epigenetic thesis.

I say ‘guilty’ because although speculation is, as we shall see later, an essential first step in more recent views of the scientific method, it is not publicly encouraged *per se*. ‘As business speculation may seem to the hard-working artisan to be playing around with other people’s money,’ writes the rhetorician, Greg Myers, ‘scientific speculation may seem to the hard-working experimentalist to be playing around with other people’s evidence…scientific speculation may seem to run ahead of the facts, … an affront to those doing the hard work…’[138] The guilty pleasure arises from Pembrey’s position as an

[138] Myers, G. (1991) pp. 325-326

outsider to the world of orthodox genetics: Pembrey "published his ideas in an obscure journal and largely forgot about it. After all, there was no evidence for any of this. It was pure speculation." (00:35:37) but Bygren, like the fairy godmother, is able to see Pembrey's true value beneath his signs of low scientific status and as a force for scientific justice, helps transform the symbols of Pembrey's obscurity into the outward signs befitting his inner virtues. In this transformation Bygren thus reveals to scientific society Pembrey's true value. To some extent, the status of The Human Genome Project is undermined by Pembrey's accession to the throne but it is not banished entirely from the Kingdom of Genetics.

Although the narration speaks of heresy and medical revolution and Pembrey says he likes "to stir things up a bit" (00:24:04), the overriding sense given by the narrative is of cooperation between scientists. The stories of the helpers create an impression of a world-wide effort by particularly insightful and open-minded scientists (recall they are dubbed a "select band of scientists"). No hint is given, for example, that these different scientists might be motivated by individualist or aggressive instincts; there is none of the secrecy or competitiveness James Watson writes about in his account of the race to discover the structure of DNA.[139] This is a world governed by Robert Merton's norms. The narrative of *The Ghost in Your Genes* 'reaffirms,' as Ron Curtis writes, 'the Baconian myth that the scientist who labours with his colleagues in the proper spirit of cooperation is the one who will succeed in his endeavours to reveal Nature's secrets.'[140] This classic science documentary presents a picture of scientific method consonant with Francis Bacon's. As Curtis writes, the film's narrative creates a 'cognitive space' in which this view of science as an inductive, detective trail is tacitly expressed:

> The scientific detective story is a modern version of the story Bacon himself told…where he interpreted the classical myth of the Sphinx as an allegory about science. It was a warning against premature speculation, said Bacon, for the Sphinx (that is Nature) was in the end subdued by a lame man with club feet, a slow-moving interpreter, not anticipator. The hard-working scientific 'gumshoe' is his modern counterpart.[141]

[139] Watson, J. (1980)
[140] Curtis, R. (1994)
[141] *Ibid*., p. 422

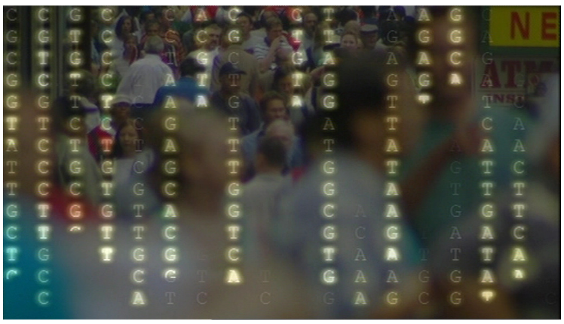
The viewer's experience is further coloured by the use of self-consciously constructed visual metaphors that serve to anchor meaning. Many of these images are expressions of the film's central thesis. For example, right from the start we see colour images of children onto whose bodies are projected black and white 'archive' images of other bodies. This visual trope is reprised elsewhere in the film by a series of images of faces hidden behind developing Polaroid images of the same:

The clear meaning of both these visual tropes is that our bodies are in some sense inhabited or impressed upon by the environment and by the histories of our immediate ancestors. Both the superimpositions of the pre-title images and these Polaroid moments thereby express the central binary opposition of the film between the genetic and the epigenetic. We are haunted either by the ghosts of ancestors or the ghosts of our younger selves who experienced physiological challenges like smoking or drinking or exposure to toxic chemicals. This same idea is expressed by another recurring visual motif: an image of people crossing a busy bridge. The image is made with a long lens so the perspective is foreshortened and people seem to bob up and down on the spot as if their lives are on hold for us to contemplate. Superimposed on this image of 'humanity' is another: the same image shot from the same spot but at a slightly different time. This second image is fainter than the first and the effect of the superimposition is to create 'ghosts' who walk amongst the solid, living citizens:

(00:11:00)

There are also over a dozen shots of families posing for the camera on the streets of London and New York throughout the film:

These images contrast with the crowd on the bridge, articulating an opposition between the generic and the specific, a meaning heavily underscored by the image below of the letters of the decoded human genome superimposed and streaming down the screen over an image of the crowd on the bridge.



(00:09:47)

The Human Genome Project, this image says, is insensitive to the facts of *individual* history.

Conclusion

The overriding effect of the formal and structural features of the film is to persuade the viewer to accept a representation of scientific practice as a detective trail that leads the gumshoe scientist unerringly to whodunit—to nature. Furthermore, the behaviour of the detective and his helpers is governed by open-mindedness and a generous sharing just as Robert Merton says it should. However, we witness almost no scientific *process* in the film at all. Although the film presents a few images of experimental 'work' they are largely generic images of white mice, petri dishes and microscopes, all very shiny and glossy—'science'. The actual processes of investigation are not depicted. Consequently *The Ghost in Your Genes* presents a history of science in which scientific behaviour is governed by Mertonian norms and rational decisions are made based on philosophically secure evidence.

It is interesting to note how the argument and the mythic narrative combine in the closing moments of the film. Recall that the mythic narrative begins by setting out an initial situation in which, because of adherence to the orthodox view of genetics, children were suffering. By the end of the narrative, children are *still* suffering only now Pembrey knows why—epigenetics. In a sense then, the mythic narrative is only partly closed. The villainous Human Genome Project has been vanquished but the suffering children have not yet been saved. Knowing there is more to genetics than the genetic code is not in itself a cure for genetic disease. Society—which has been largely absent from the diegesis of the film—has to act on that knowledge:

> Pembrey: "It may get to a point where they realise that you live your life as a sort of - I don't know - as a sort of guardian of your genome. It seems to me you've got to be careful of it because it's not just you. You can't be selfish because you can't say well I'll smoke or I'll do whatever it is because I'm prepared to die early. You're also looking after it for your children and grandchildren." (00:48:00)

The individual victims with which the mythic narrative began have been largely forgotten by the end; they have become "your children and grandchildren". The mythic narrative with its 'spheres of action' has simply acted as a vehicle for the film's argument, persuading the viewer to accept its scientific conclusions because they are supported by the activities and statements of persuasively authoritative individuals—scientific experts. At the same time, the film's argument has coloured our reading of the motivations of the heroic scientists, all of whom seem to be working together against the orthodox view for the benefit of us all. The mimetic and mythic narratives are in this way mutually reinforcing. They paint a familiar, 'received' view of scientific activity as the application of a shared algorithm of discovery (the 'scientific method') by cooperating scientists who thereby succeed in inserting another piece into the Great Jigsaw Puzzle of our ever-expanding picture of the natural world.

Carl Gardner and Robert M. Young come to much the same conclusion in their critique of TV science:

> As things now stand, the eyes of programme-makers are firmly fixed on the content of knowledge and the process of discovery. There is, in addition, another topic which tends to be considered separately from the substance of knowledge (in itself regarded as 'neutral'): its social impact. The result is that discovery and substance are presented as internal to science, while social impact is seen as an interacting variable. Science is one thing, context another.[142]

'Science is one thing, context another.' Thus does the classic science documentary reinforce the received view's distinction between the contexts of discovery and of justification. Gardner and Young go on to point out that this treatment of science is unlike the way academics, historians and filmmakers treat other areas of culture:

> Literature, drama, plastic and graphic arts, cinema, and television itself are currently studied according to models which attempt to relate the context, presentation, content and impact into a single coherent account of meanings. This is also a commonplace in the treatment of science from periods other than our own. Historians of ancient, medieval, Arab, Renaissance, seventeenth, eighteenth and nineteenth-century science go to considerable lengths to show how the science is constituted by the historical forces of the period, including frames of reference, major theoretical concepts and even specific research topics. All bear the stamp of their times and places.[143]

[142] Gardner, C. and R. M. Young (1981) pp. 174-175
[143] *Ibid.,* p. 175; See, for example, Foucault, M. (1970, 1971).

But not the classic science documentary. And so, as Ron Curtis asks of the popular science article, I would wish to ask (with *Hopeful Monsters* in mind): might other forms of the science documentary 'reflect more accurately and critically alternative interpretations of scientific practice?'[144] Might another form of the science documentary register the stamp of time and place on scientific knowledge?

[144] Curtis, R. (1994) p. 445

PART TWO

CONSTRUCTIVISM

CHAPTER THREE
DEVELOPMENTS IN THE PHILOSOPHY OF SCIENCE

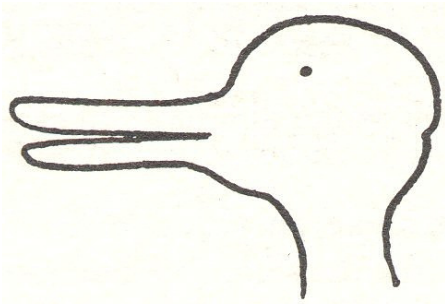
The following chapter outlines a history of developments in ideas about the nature of science that have resulted in the view of science I have called 'constructivism', a view that acknowledges the influences of time and place on what comes to be designated scientific discovery. In tracing this history we will identify those features of the constructivist epistemology that we may expect to find reflected in any 'alternative' to the classic science documentary. In subsequent chapters I will analyse my experimental film, *Hopeful Monsters* and consider its effectiveness in representing a constructivist view of science, but my aim in this chapter is first to sketch out the grounds for this alternative view.

Let me begin by inviting you into the laboratory of French physicist, Pierre Duhem, around the year 1900:

> …approach the table crowded with an assortment of apparatus, an electric cell, a silk-covered copper wire, small cups of mercury, spools of wire, a mirror mounted on an iron bar; the experimenter is inserting into small openings the metal ends of ebony-headed pins; the iron oscillates, and the mirror attached to it throws a luminous band upon a celluloid scale; the forward-backward motion of this luminous spot enables the physicist to observe the minute oscillations of the iron bar. But ask him what he is doing. Will he answer "I am studying the oscillations of an iron bar which carries a mirror"? No, he will answer that he is measuring the electric resistance of the spools. If you are astonished, if you ask him what his words mean, *what relation they have with the phenomena he has been observing and which you have noted at the same time as he*, he will answer that your question requires a long explanation and that you should take a course in electricity.[145]

Quoted by N. Russell Hanson in his book *Perception and Discovery* (1969), this passage illustrates the important idea that to show the visitor what he *sees*, the physicist must first explain what he *knows*. The same, argues Hanson, goes for *all* our perception. To make his point clearer, Hanson invites us to consider the following famously ambiguous image:

[145] Quoted in Hanson, N. R. (2002) p. 321



Hanson uses the duck-rabbit figure to explore the difference between what it is to *see* and what it is to *interpret*. His aim is to question a central tenet of the received view, namely that observing and theorising (seeing and interpreting) are distinct operations. In the received view, to 'see' is taken to mean the apprehension of sense-data while everything additional to that basic apprehension is considered interpretation. But this, argues Hanson is surely not what we mean when we speak of seeing the figure above as either a duck or a rabbit. One does not have to interpret—to *think*—in order to switch the figure from duck to rabbit one simply sees it *as* a rabbit or *as* a duck. We see either a duck or a rabbit due to how we organise the lines of the figure but that organisation is not an act of interpretation separable from the seeing, it is what it means to see the figure in the first place.

If the figure were surrounded by other less ambiguous images of ducks or of rabbits we would 'get' the figure immediately as one or the other and it would remain that way. What we see depends on what we expect to see or believe we ought to be seeing—interpretation is built-in, if you like, to the very experience we call seeing:

> The conceptual organization of one's visual field is the all-important factor here. It is not something visually apprehended in the way that lines and shapes and colours are visually apprehended. It is rather the way in which lines, shapes and colours are visually apprehended. … No case of seeing…is wholly independent of the knowledge of the percipient.[146]

To grasp the significance of this idea for an understanding of the nature of science we shall have to ignore for a time what many scientists seem to believe

[146] Hanson, N. R. (2002) pp. 333-334

they are doing when they do science. This is not to suggest that scientists don't know how to do science, only that what they describe in public and that is subsequently 'fed on' by TV programmes need not be taken entirely at face value. As Gardner and Young write:

> It is our impression, backed up by discussions and interviews with people taking part in the making of science programmes, that belief in the relative autonomy of knowledge is being uncritically propagated on television. … As far as we can tell, this is principally a matter of public relations, since it certainly isn't a view propagated by scientists at work.[147]

Alan Chalmers puts the philosophical position:

> Modern developments in the philosophy of science have pinpointed and stressed deep-seated difficulties associated with the idea that science rests on a sure foundation acquired through observation and experiment and with the idea that there is some kind of inference procedure that enables us to derive scientific theories from such a base in a reliable way. There is just no method that enables scientific theories to be proven true or even probably true…there is no method that enables scientific theories to be conclusively disproved either. [148]

There are many surveys and commentaries on this analysis, for example, Chalmers (1982), Losee (1972), Gillies (1992), Harré (1972), Gower (1997) and Suppe (1977). All these authors agree that at the heart of the matter, as exemplified by our experience of making sense of the duck-rabbit figure, lies debate over the nature of theories and their relation to observation. To explore more fully the issues at stake in this relationship it is necessary to revisit some well-known epistemological problems that date back to Ancient Greece.

Deduction and Induction

In Plato's dialogue, *The Meno*, Socrates invites the soldier, Meno, to distinguish between knowing and guessing the correct route from Athens to Larissa. By his arguments he leads Meno to agree that provided a man guesses the correct way then 'true opinion is as good a guide to rightness of action as knowledge.' Meno is troubled by this conclusion and responds, '…I wonder, Socrates, this

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[147] Gardner, C. and R. M. Young (1981) p. 176
[148] Chalmers, A. (1982) p. xvi

being the case, that knowledge should ever be more prized than right opinion, and why they should be two distinct and separate things.' The difference between guessing correctly and *knowing*, replies Socrates, is that:

> True opinions…do not care to stay for long, and run away out of the human soul, and thus are of no great value until one makes them fast with causal reasoning. … But when once they are fastened, in the first place they turn into knowledge, and in the second, are abiding. And this is why knowledge is more prized than right opinion: the one transcends the other by its trammels.[149]

By 'trammels', Socrates means an argument with reasons as premises and the statement in question as the conclusion. But he means more than this: if the premises are to constitute a *good* reason for the conclusion then, in the first place, the premises must be true and, in the second place, the argument from these premises to that conclusion must be a sound one. In a sound argument the premises provide a good, because *conclusive*, reason for the truth of the conclusion. Such a *deductively valid* argument is bound to lead to certain knowledge. Note, however, that deduction (as in mathematics or logic) generates no *new* knowledge; nothing emerges from the conclusion of a deductive argument that was not already implicit in its premises.

In making scientific arguments, by contrast, we are interested in generating new knowledge but this means that many if not most of the statements we accept as true in science are accepted on the basis of reasons which are not conclusive. For example, that 'the sun will rise tomorrow', that 'sugar is sweet', that 'global warming is caused by human activity', are all statements whose truth is supported by evidence that is less than conclusive. In other words, an argument which reports in its premises the evidence of relevant experience, and has as its conclusion one or more of the statements mentioned, will not be a deductively valid argument. When we need to argue for the truth of a prediction like the sun will rise tomorrow or that industrial activity causes global warming we make use of *inductive* arguments. The facts we appeal to in the premises of our argument constitute *inductive reasons* for the prediction.

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[149] Plato (1924) 97A-97B; see also, Plato (1956) 97B-98B for a different translation

The question is, then, whether any inductive arguments are sound; whether inductive reasons are ever *good* reasons.

A deductive argument counts as sound because if its premises are true then its conclusion *must* also be true. To deny the truth of the conclusion of a valid deductive argument is to assert a contradiction. The same does not hold for an inductive argument. Even if the premises of an inductive argument are true I need not accept its conclusions. You may shout at me that the sun has risen every day 'since records began' but it is no contradiction to deny the sun will rise tomorrow. Clearly the strength of an inductive argument, unlike deductive validity, is open to dispute. People are not compelled, *logically*, to accept the same evidence as constituting good reasons for the conclusion of an inductive argument. The philosopher W. Kneale puts the distinction this way:

> The situation which I have been trying to describe can be made more intelligible by a comparison between the way in which we talk of probability and the way in which we talk of necessity. If I know a fact A and also know that A is *conclusive* evidence for B, I may say 'Because A, therefore necessarily B'. But if there is no special reason to mention the evidence for my conclusion I may content myself with the remark 'Necessarily B'. This shows that I put B forward as the conclusion of an inference, but does not specify the evidence for it. If I do not know A, or am not concerned for the moment at least to claim knowledge of it, but wish to point out that A would be conclusive evidence for B, I use the hypothetical form and say 'If A then necessarily B'. For the case in which A is *inconclusive* evidence for B all these phrases can be adapted by the substitution of 'probably' for 'necessarily'; and as in the first case we can say that A necessitates (or would necessitate) B, so in the second case we can say that A probabilifies (or would probabilify) B. There is, however, an important difference. Whereas, if A necessitates B, any conjunction of propositions which includes A must also necessitate B, it is possible for A to probabilify B to some degree although some conjunctions containing A would not probabilify B to the same degree or even at all.[150]

It would appear that the soundness of an inductive argument depends on its being inductively strong, i.e., on it having premises which make its conclusion *probably* true. Should I continue to doubt that the evidence of the sun rising every day *does* constitute good, although admittedly inconclusive, evidence for the prediction that it will rise tomorrow, you can simply point out that since the

[150] Kneale, W. (1949)

evidence makes this prediction probably true, it follows *by definition*, that the argument from evidence to prediction is inductively strong and, consequently, sound. But this way of showing that inductive arguments can be sound is altogether *too* easy. The statement we are trying to establish is:

> Inductive arguments are sometimes sound
> (or, inductive evidence can be good evidence)

The statement being offered as a reason for believing this is:

> Some inductive arguments report in their premises evidence which makes their conclusions probable
> (i.e., some inductive arguments are inductively strong)

If the second statement were true then the first would follow but is the second statement true? The problem of induction arises once we recognise that the only way in which we could reply to this doubt is by appealing to the truth of what we are trying to establish, namely the first statement. For, in order to decide whether the premises of some particular inductive argument *do* make its conclusion probably true, we would have to ask and answer the question: is this inductive argument sound? We cannot use the truth of the second statement as a reason for believing the first because the truth of the second relies on believing the truth of the first *in the first place*. In other words, such a justification would beg the question, the question being 'are any inductive arguments sound?'

Causality

In philosophy, discussions of induction have often been interwoven with discussions of causality. This is because many of the inferences we call inductive could also be described as causal. We often conclude that an event will happen on the grounds that another event, taken to be its cause, has happened. For example we predict that our coffee will taste sweet because a spoon of sugar has been added to it. Or we argue from effects back to causes such as when people outside are holding umbrellas we conclude it must be raining. (This is the form of much reasoning both for and against climate change.) If we can show that the connection between a putative 'cause' and its 'effect' is strong enough then we might diffuse scepticism about the legitimacy of such inductive inferences. But although we speak as if adding sugar to coffee *must* make it sweet or that because people are holding umbrellas, therefore it *must* be raining, these inferences cannot be *logically* necessary. No contradiction arises if we deny that an event of a certain kind will have the same kind of effect, or the same kind of a cause, as it has always been found to have in the past.

In his *Enquiry Concerning Human Understanding*, David Hume argues that our connection of cause with effect is not based on a rational foundation. We are forced, he points out, when faced with the challenge of justifying our belief in causality, to appeal to past experience and so are confronted with the problem of induction once again, for conclusions based on past experiences are inductive conclusions:

> It is certain that the most ignorant and stupid peasants—nay infants, nay even brute beasts—improve by experience, and learn the qualities of natural objects, by observing the effects which result from them. When a child has felt the sensation of pain from touching the flame of a candle, he will be careful not to put his hand near any candle, but will expect a similar effect from a cause which is similar in its sensible qualities and appearance. If you assert, therefore, that the understanding of the child is led into this conclusion by any process of argument or ratiocination, I may justly require you to produce that argument, nor have you any pretence to refuse so equitable a demand. You cannot say that the argument is abstruse, and may possibly escape your enquiry; since you confess that it is obvious to the capacity of a mere infant. If you hesitate, therefore, a moment, or if, after reflection, you produce any intricate or profound argument, you, in a manner, give up the question, and confess that it is not reasoning which engages us to suppose the past resembling the future, and to expect similar effects from causes which are, to appearance, similar. This is the proposition which I intended to enforce in the present section. If I be right, I pretend not to have made any mighty discovery. And if I be wrong, I must acknowledge myself to be indeed a very backward scholar, since I cannot now discover an argument which, it seems, was perfectly familiar to me long before I was out of my cradle.[151]

[151] Hume, D. (1826) Chapter 4 'Sceptical Doubts Concerning the Operation of the Understanding' p. 48

Inductive conclusions 'are not founded on reasoning or any process of the understanding' but rather 'proceed upon the supposition that the future will be conformable to the past' and the 'proof of this…supposition by probable arguments [i.e., induction] … must be evidently going in a circle and taking that for granted which is the very point in question.'[152] But if causal inferences apparently lack a rational foundation it does not follow that they lack any foundation whatsoever. Hume suggests that causal inferences are founded on a psychological *habit*. We are psychologically conditioned, he argues, to make inferences from cause to effect and vice versa. Such inferences, therefore 'are effects of custom, not of reasoning.' But just because we are psychologically conditioned to place our confidence in inductive inference does not solve the problem of induction which is, after all, to provide a *logically defensible* reason to rely on it.

It is hard to overestimate the scope of the problem of induction. If Hume is right, our capacity to learn from experience has no adequate rational basis and much practical and useful knowledge lacks what Plato argued distinguishes knowledge from correct guesswork. In addition, all of what passes for scientific knowledge such as that plants photosynthesise, plutonium disintegrates, light travels and the earth moves is similarly suspect; it does not count as knowledge in Plato's sense.

'It is therefore important to discover whether there is any answer to Hume within the framework of a philosophy that is wholly or mainly empirical,' writes the philosopher, Bertrand Russell:

> If not, there is no intellectual difference between sanity and insanity. The lunatic who believes he is a poached egg is to be condemned solely on the grounds that he is a minority, or rather—since we must not assume democracy—on the ground that the government does not agree with him. This is a desperate point of view, and it must be hoped that there is some way of escaping from it.[153]

It was Karl Popper who broke with the received view by proposing a radical escape from 'Hume's problem'.

[152] *Ibid.,* p. 44
[153] Russell, B. (1945) p. 673

Popper's epistemology

Karl Popper (1902-1994) was born in Austria. During his PhD in philosophy at the University of Vienna, Popper also trained as a cabinet-maker but on graduation became a school teacher. He was intensely interested in how children learn and his model of science derives as much from his own observations of children and from studies of animal behaviour as from strictly 'logical' considerations.

In 1934 he published his first book, *Logik der Forchung* proposing a radical solution to 'Hume's problem'. The book was translated into English only in 1959 as *The Logic of Scientific Discovery*. By this time, Popper was living in London and teaching at The London School of Economics. He had fled Vienna in 1937 when the *Anschluss* seemed inevitable and spent the war years in Christchurch, New Zealand at Canterbury University College. There he developed his critique of historicism and a profound defence of liberal democracy.[154]

In his autobiography, *Unended Quest* (1993) Popper tells of the master cabinet-maker to whom he was apprenticed in the early 1920s. The master loved to ask his apprentice historical questions and then to answer them himself when Popper could not. 'It was my master who taught me not only how very little I knew but also that any wisdom to which I might ever aspire could consist only in realizing more fully the infinity of my ignorance.'[155] *The Logic of Scientific Discovery* (1959) is inspired by this insight. It can be understood as a response to the positivistic views of his contemporaries in the so-called Vienna Circle.

According to positivism, there are only two sorts of meaningful statements: *a priori* analytical statements like mathematical ones and *a posteriori* synthetic statements like scientific ones based upon 'sense data'. To the positivists, as Popper puts it, science 'is not a system of concepts but rather a system of statements…that are reducible to elementary (or 'atomic') statements of experience.'[156] But this is not a view of science that Popper finds tenable. Scientific laws, he argues, cannot be reduced to elementary statements of

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[154] See Popper's *The Open Society and Its Enemies* (1945) and *The Poverty of Historicism* (1957).

[155] Popper, K. (1993) p. 7

[156] Popper, K. (1975) pp. 34-35

experience because all scientific statements necessarily go beyond the *particular* experiences that inspired them:

> Every statement has the character of a theory, of a hypothesis. The statement, 'Here is a glass of water' cannot be verified by any observational experience. The reason is that the universals which appear in it cannot be correlated with any specific sense-experience. (An immediate experience is *only once* 'immediately given'; it is unique.) By the word 'glass', for example, we denote physical bodies which exhibit a certain *law-like behaviour*, and the same holds for the word 'water'. Universals cannot be reduced to classes of experiences; they cannot be 'constituted'.[157]

Popper agrees with the positivists that only observation can give us (non-tautological) knowledge concerning facts but, he argues, this knowledge cannot justify the truth of any universal statement because of the problem of induction. Positivists, says Popper, ask the wrong question about the nature of science when they ask '…on what does our knowledge rest? … or more exactly, how can I, having had the experience S. justify my description of it, and defend it against doubt?'[158] Such a question implies that the statements of science are arrived at and then defended inductively but this is simply not possible he says: 'The belief in inductive logic is largely due to a confusion of psychological problems with epistemological ones.'[159]

Popper argues that a scientific theory such as Newton's laws of motion cannot be verified by any observations or experimental result because, logically, such results do not rule out different results next time the test is carried out. Verification by observation (i.e., positivism) must founder on the problem of induction. Popper's solution to this problem is the recognition that science is not, in the end, concerned with questions of fact but with questions of justification and validity. 'Accordingly', writes Popper, 'I shall distinguish between the process of conceiving an idea, and the methods and results of examining it logically.'[160] Deriving a hypothesis is one aspect of science but the all-important part of science concerns the testing of that hypothesis. This testing

[157] Popper, K. (1975) pp. 94-95 (his emphases)
[158] *Ibid.,* p. 98
[159] *Ibid.*, pp. 29-30
[160] *Ibid.,* p. 31

requires not the accumulation of confirming data (induction) but a logical argument of *deduction*.

Popper's 'hypothetico-deductive' model may be summarised as follows: (1) a new idea, theory, hypothesis is arrived at (it matters not how) and is in the beginning not yet justified in any way. (2) From this theory a number of conclusions is deductively derived and then each is compared to the others to check if they are logically compatible with each other. (3) Then the form of the whole theory may be examined to determine if it truly has the form of a scientific theory (one that may in principle be false) or if it is, for example, tautological. (4) From the theory are extracted a number of singular predictive statements which are then tested by observation of the world. If these tests are positive the theory stands—for the time being—if the conclusions are falsified then the theory falls.

A theory that is not yet falsified is said to be corroborated not verified. This distinction evades the problem of induction for, while in verification, a singular statement derived from an empirical observation cannot attest to the truth of a hypothesis (one more white swan does not verify the hypothesis that 'all swans are white'), a singular observation statement attesting to the existence of a black swan completely and absolutely falsifies the hypothesis. There is an asymmetry between verification and falsification. Scientific verification runs up against Hume's problem while falsification is entirely a matter of observation and deductive logic. The price paid, however, for letting go of the ambition of verification is that all theories persist, logically, on sufferance, never proven, simply not yet falsified and the scientist remains, in the same logical sense, infinitely ignorant.

The Popperian view of science, then, is of a body of *conjectures* and a set of practices that may be divided between 'hypothesising' and 'refuting'. Framing hypotheses is a matter of imagination, insight etc., refutation a matter of deductive logic. Science, according to Popper is not, as I write in chapter one, a body of validated knowledge but a body of conjectures; it is a '*set of practices*' but these are not positivistic, inductive practices.

The distinction between verification and falsification is perhaps Popper's chief contribution to the philosophy of science. He himself wrote that 'within the framework of this analysis, all the problems can be dealt with that are usually called ‘epistemological’. Those problems, more especially, to which inductive logic gives rise, can be eliminated without creating new ones in their place.’[161]

The Problem of Demarcation

Positivism had argued that any non-tautological statement that cannot be reduced to statements of experience is meaningless but according to Popper such statements of experience, gathered together for example as scientific laws, are at best corroborated, not verified. The distinction, then, between meaningful statements of fact and meaningless metaphysical statements cannot be upheld in Popper’s philosophy. His epistemology cannot apparently keep scientific statements apart from other sorts of statements. Popper calls this the problem of demarcation and it was a particularly pressing problem for him in a world he saw endangered by totalitarian (and unfalsifiable) notions of historical destiny.

To solve the problem, Popper invites us to ‘renounce this [positivist] requirement and admit as empirical … statements which are decidable in one sense only … which may be tested by systematic attempts to falsify them.’ This is the only safe way to do science because ‘the method of falsification presupposes no inductive inference, but only tautological transformations of deductive logic whose validity is not in dispute’.[162] *No theory that cannot in principle be falsified by evidence may be admitted as scientific*. Freud’s theory that actions may be the effects of unconscious thoughts for example is, in Popper’s view, not science because it is not clear how it could ever be falsified, but this does not make it meaningless. Ideas that lie outside of science may be meaningful and may form the basis of scientific theories in the future. Indeed it is assumed that science grows, in the first place, from non-science:

> …historically speaking all—or very nearly all—scientific theories originate from myths, and…a myth may contain important anticipations of scientific theories. Examples are Empedocles’ theory of evolution by trial and error, or Parmenides’ myth of the unchanging block universe in which nothing ever happens and which, if we add another dimension, becomes Einstein’s block universe (in which, too, nothing ever happens, since everything is, four dimensionally speaking, determined and laid

[161] Popper, K. (1975) pp. 33-34
[162] *Ibid.*, p. 42

> down from the beginning). I thus felt that if a theory is found to be non-scientific, or 'metaphysical' (as we might say), it is not thereby found to be unimportant, or insignificant or 'meaningless', or 'nonsensical'. But it cannot claim to be backed by empirical evidence in the scientific sense—although it may easily be, in some genetic sense, the result of 'observation'.[163]

Although induction is evaded, the hypothetico-deductive model does not entirely dispense with the issues facing the philosopher of science. Is it really the case that Freudianism is non-scientific because non-falsifiable while Newtonian physics is falsifiable and therefore to be welcomed into the fold? There remain serious problems with falsification, the most troublesome being what Popper calls 'the problem of the empirical basis' or the theory-dependence of observation.

Observation is theory-dependent

Frederick Suppe considers the theory-dependence or value-ladenness of observation one of the key issues undermining the received view.[164] The issue here is not so much whether one can observe the world (which one clearly can) as the relationship between such 'perceptual experiences' and 'basic statements' about those experiences. 'Basic statements' or 'observation statements' (i.e., statements of singular facts) are required to serve as the premises in deductive arguments of falsification. Such observation statements cannot, then, be themselves dependent on the theory they are testing, or indeed any theory—they should be 'pure' observation statements—but it is hard to understand how any statement can be pure in this sense. Andrew Norman puts it this way:

> All seeing—or at least all seeing that is in any way epistemically significant—is interpretive. The point of adding 'epistemically significant' is that observations must be articulated into statements of some kind for them to carry any justificatory burden. In such cases something must be predicated of whatever it is that one sees. And this means that one must see the thing in some aspect, or take the thing in some way or other. But to do this is just to interpret. Thus interpretation must happen for an observation to have epistemic significance.[165]

[163] *Ibid.,* p. 38
[164] Suppe, F. (1977)
[165] Norman, A. (1998) p. 505

As Popper himself argues in his analysis of the phrase ‘a glass of water’ all observation statements involve interpretation in light of theoretical knowledge: ‘by the word “glass”…we denote physical bodies which exhibit certain *law-like* behaviour, and the same holds for the word “water”’.[166] Popper’s colleague, Imre Lakatos, argues that it is not only observation *statements* that are necessarily interpretive but, as illustrated by our perception of the duck-rabbit figure, observation itself: ‘There can be no sensations unimpregnated by expectations and therefore *there is no natural (i.e. psychological) demarcation between observational and theoretical propositions*’.[167] No sharp distinction can be made between an ‘empirical language’ and a ‘theoretical language’—we are theorizing all the time.

The anthropologist, Edmund Leach asks how we can be confident that our perception of the world is independent of our social environment and the concepts we have absorbed from it. He notes that the conventions for artistic representation of common objects vary widely between different cultures. This seems significant, he writes:

> It is perfectly possible that every individual perceives his world to be what his or her cultural background suggests. Today most of the world is dominated by the ‘realistic’ images provided by our use of cameras. But it is self-deception if you imagine—as you probably do—that your eye “naturally” perceives the world as it might appear in a photograph.[168]

In *Doubt and Certainty in Science—a Biologist’s Reflections on the Brain*, the zoologist, J. Z. Young speaks of the ‘creative’ activity of the brain that necessarily precedes seeing in the first place:

> The visual receiving system in its untrained state has only very limited powers. We are perhaps deceived by the fact that the eye is a sort of camera. Contrary to what we might suppose, the eyes and brain do not simply record in a sort of photographic manner the pictures that pass in front of us. The brain is not by any means a simple recording system like a film….Many of our affairs are conducted on the assumption that our sense organs provide us with an accurate record, independent of ourselves. What we are now beginning to realize is that much of this is an illusion, that we have to learn to see the world as we do.

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[166] Popper, K. (1975) p. 95
[167] Lakatos, I. (1970) p. 99
[168] Leach, E. (1976) p. 21

And he goes on:

> ….The point to grasp is that we cannot speak simply as if there is a world around us of which our senses give true information. In trying to speak about what the world is like we must remember all the time that what we see and what we say depends on what we have learned; we ourselves come into the process.[169]

According to J. Z. Young, to perceive is to *conceive*; facts are necessarily dependent on the conceptual frame within which they are identified and the empirical basis of objective science has thus nothing 'absolute' about it. After all, as Hanson points out, although the lens and cornea are developments of our skin, the retina and optic nerve are outgrowths of the brain. 'It could not alarm anyone, except a person with a theory to the contrary, to hear that alterations in the general state of the brain, alterations like learning…could affect the whole character of seeing, particularly in its conceptual organization…'[170]

It is worth recalling how Williamson speaks of the putatively hybrid larva he has made, reporting his visual sensations in as flat and 'phenomenal' a way as possible:

"Ah. That's different. It's not completely spherical. But we can't make out any internal structure…" (01:12:45)

Such observational situations contrast with the more usual way of seeing, as Hanson writes:

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[169] In Williams, R. (1984) p. 33
[170] Hanson, N. R. (2002) p. 335; The film camera too is attached to an outgrowth of the brain, as are the lenses and corneas of filmgoers.

> The language of shapes, colour patches, oscillations, and pointer-readings is the language appropriate to the unsettled experimental situation, where confusion and perhaps even conceptual muddlement dominate. And the *seeing* that figures in such situations is of the sort where the observer *does not know what he is seeing*. He will not be satisfied until he does know, until his observations cohere and are intelligible as against the general background of his already accepted and established knowledge. And it is this latter kind of seeing that is the goal of observation. For it is largely in terms of it, and seldom in terms of merely phenomenal seeing, that new inquiry will proceed.[171]

While conventionally we distinguish two levels in accounts of reality: the phenomenal 'facts' (data) and the interpretation (explanation), this distinction obscures the difficulty of distinguishing these two levels within any given discourse, as the historiographer Hayden White argues:

> It is not the case that a fact is one thing and its interpretation another. The fact is presented where and how it is in the discourse in order to sanction the interpretation to which it is meant to contribute. And the interpretation derives its force of plausibility from the arrangement of the facts in the order and manner in which they are presented in the discourse.[172]

To see, then, in a way appropriate to testing a theory is to see ***as***, to already have assumed that very theory as an organising context. 'Perception is conceptually articulated, and must be to be epistemically significant,' writes Andrew Norman.[173] Popper therefore proposes that all statements, even the 'basic statements' reporting an observation have therefore to be seen as incorrigibly conjectural:

> Systems of theories are tested by deducing from them statements of a lesser level of universality. These statements in their turn, since they are to be inter-subjectively testable, must be testable in like manner—and so *ad infinitum.* [174]

No statement, no matter how lowly, is 'basic' or 'pure'. And so, as Norman writes:

[171] *Ibid.,* pp. 336-337 (emphasis in original)
[172] White, H. (1975) p. 55
[173] Norman, A. (1998) p. 501
[174] Popper, K. (1976) p. 47

> Past experience, background knowledge, linguistic training and the like serve to structure visual experience—not before our eyes, as it were, but behind our backs. The perceptual and conceptual elements of seeing, it seems, are not separable. ... Observation is thoroughly 'laden' with theory.[175]

Underdetermination

A further problem with the received view arises when we consider another dimension of the testing of theories. Even were there a clear distinction between observation statements and theoretical statements and if observation statements could be established by appeal to sensory experience alone, still it is always possible to evade the force of a falsifying observation. There are two parts to this idea. First, empirical statements do not exist in isolation, they are all interconnected and so may not be singly disconfirmed; second, we may hold a statement true, despite evidence to the contrary, by adjusting others on which it depends. Popper acknowledges this:

> Even if the asymmetry [between verification and falsification] is admitted, it is still impossible … that any theoretical system should ever be conclusively falsified. For it is always possible to find some way of evading falsification, for example by introducing *ad hoc* an auxiliary hypothesis, or by changing *ad hoc* a definition. It is even possible without logical inconsistency to adopt the position of simply refusing to acknowledge any falsifying experience whatsoever. Admittedly, scientists do not usually proceed this way, but logically the procedure is possible.[176]

This is sometimes known as the Quine-Duhem thesis of underdetermination. The thesis is that in order to rule that a particular observation should entail the rejection of a theory, the scientist must make an inferential decision but nature does not *force* that decision upon the scientist. Indeed, if the scientist is willing to change enough of his or her assumptions and background beliefs then any observation may be consistent with the theory under test. Imre Lakatos provides a story to illustrate the point:

[175] *Op Cit.* p. 504
[176] Popper, K. (1976) p. 42

> The story is about an imaginary case of planetary misbehaviour. A physicist of the pre-Einsteinian era takes Newton's mechanics and his law of gravitation (*N*), the accepted conditions, *I*, and calculates, with their help, the path of a newly discovered small planet, *p*. But the planet deviates from the calculated path. Does our Newtonian physicist consider that the deviation was forbidden by Newton's theory and therefore that, once established, it refutes the theory, *N*? No. He suggests that there must be a hitherto unknown planet *p′* which perturbs the path of *p*. He calculates the mass, orbit etc., of this hypothetical planet and then asks an experimental astronomer to test his hypothesis. The planet *p′* is so small that even the biggest available telescopes cannot possibly observe it: the experimental astronomer applies for a research grant to build yet a bigger one.[177]

Three years later the new telescope is ready. If *p′* is discovered, 'it will be hailed as a new victory of Newtonian science'. But it is not. Rather than abandon Newton, Lakatos' scientist proposes that a cloud of cosmic dust hides the planet. He calculates the properties of the cloud, obtains a grant to design a satellite, and launches it into orbit. If the cloud is discovered (possibly using new instruments based on a little-tested theory) then we can celebrate a new victory for Newtonian science. But the cloud is not discovered, nor the magnetic field that must therefore be disturbing the instruments in the satellite. Nor is this taken as a refutation.

> Either yet another ingenious auxiliary hypothesis is proposed or … the whole story is buried in the dusty volumes of periodicals and the story never mentioned again.[178]

'Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system,' writes the philosopher, W. V. O. Quine.[179]

In summary, Popper's attempt to place science on a sound logical footing by developing a hypothetico-deductive model of scientific inference exposes the necessarily speculative, conjectural nature of scientific knowledge:

> Science does not rest upon solid bedrock. The bold structure of its theories rises, as it were, above a swamp. It is like a building erected on piles. The piles are driven down from above into the swamp, but not down to any natural or 'given' base; and if we stop driving the piles

[177] Lakatos, I. (1970) p. 100
[178] *Ibid.,*
[179] Quine, W. V. O. (1953) p. 43

> deeper, it is not because we have reached firm ground. We simply stop when we are satisfied that the piles are firm enough to carry the structure, at least for the time being.[180]

How scientists decide that the pile-driving, test period of a theory (which could be carried on *ad infinitum*) is over ('at least for the time being') and that the theory is sufficiently corroborated is not therefore a matter of logical necessity but rather a question for historians and sociologists to answer. As Imre Lakatos's anecdote of the mystery planet suggests, when a theory is deemed *falsified* is not in the end a logical matter, but a *social* one.

This is no doubt a conclusion that would have dismayed Popper (and many of his scientist fans) but it does seem that Popper's destructive analysis of positivism ends up undermining his own idea of a logic of scientific discovery and with it any easy notion of progress in science. Put simply, Popper's analysis renders mistaken the notion that scientists learn from their mistakes, as Thomas Kuhn explains:

> …it appeals to the residual inductivist in us all. Believing that valid theories are the product of correct inductions from facts, the inductivist must hold that a false theory is the result of a mistake in induction. In principle, at least, he is prepared to answer the questions: what mistake was made, what rule broken, when and by whom, in arriving at, say, the Ptolemaic system? ... But neither Sir Karl nor I is an inductivist. We do not believe there are rules for inducing correct theories from facts … Instead we view them as imaginative posits, invented in one piece for application to nature…In our view, then, no mistake was made in arriving at the Ptolemaic system, and it is therefore difficult for me to understand what [it means to] call that system, or any other out-of-date theory, a mistake. [181]

In the hands of Thomas Kuhn, Popper's analysis of the logical difficulties of the scientific project focuses our minds onto the role that scientific *society* must play in establishing certain ideas as 'true' and in promoting, discouraging or inspiring further lines of research.

David Hume and Thomas Kuhn (whom we shall consider in due course) both argue that a proper account of science demands a cognitive or psychological approach rather than a strictly rational or philosophical one. The

[180] Popper, K. (1975) p. 111
[181] Kuhn, T. (1970) pp. 11-12

key idea common to both thinkers is that knowledge has its origin in the human mind and that human minds do not exist in a vacuum. What constitutes scientific knowledge is agreed upon collectively by the human minds that comprise scientific society. This is very far from Robert Merton's sociology of science in which his norms were imagined as prophylactic, a means to ensure social forces are kept *out* of scientific inference. Popper's work, perhaps unwittingly, opens the door to the opposite view, namely that scientific knowledge is not so much discovered as invented, justified not logically but necessarily socially by scientists who may choose either to endorse or ignore such inventions.

Radical Constructivism

In the following quotation, Albert Einstein articulates in graphic terms the conclusion that Popper seems to lead us to and that is spur to Thomas Kuhn's notion of scientific knowledge, namely that fundamental principles in science have a 'fictional character':

> Newton, the first creator of a comprehensive, workable system of theoretical physics, still believed that the basic concepts and laws of his system could be derived from experience. This is no doubt the meaning of his saying, *hypotheses non fingo*….the tremendous practical success of his doctrines may well have prevented him and the physicists of the eighteenth and nineteenth centuries from recognising the fictitious character of the foundations of his system. The natural philosophers of those days were, on the contrary, most of them possessed with the idea that the fundamental concepts and postulates of physics were not in the logical sense free inventions of the human mind but could be deduced from experience by 'abstraction'—that is to say by logical means. A clear recognition of the erroneousness of this notion really only came with the general theory of relativity, which showed that one could take account of a wider range of empirical facts, and that too in a more complete manner, on a foundation quite different from the Newtonian. But quite apart from the question of the superiority of one or the other, the fictitious character of fundamental principles is perfectly evident from the fact that we can point to two essentially different principles, both of which correspond with experience to a large extent; this proves at the same time that every attempt at a logical deduction of the basic concepts and postulates of mechanics from elementary experiences is doomed to failure.[182]

[182] In Schilpp, P. A. (1949) p. 393

And yet, Einstein also writes a few pages earlier that 'pure logical thinking cannot yield us any knowledge of the empirical world; all knowledge of reality starts from experience and ends in it.'[183] This is the conundrum that Popper wrestles with—how to make true (i.e., objective) statements about the empirical world when access to that world is unavailable except at one remove, through necessarily subjective *experience*:

> The theory of knowledge, whose task is the analysis of the method or procedure peculiar to empirical science, may accordingly be described as a theory of the empirical method—a theory of what is usually called 'experience'.[184]

'Constructivism' is one such theory of experience that seems to be implicit in Popper's work. It holds, as Ernst von Glasersfeld puts it, that 'knowledge, no matter how it be defined, is in the heads of persons, and that the thinking subject has no alternative but to construct what he or she knows on the basis of his or her own experience.'[185] The purview of reason is limited by constructivism to the realm of experience; thus it is agnostic about representing reality, knowledge of which is necessarily unavailable to experience and reason. von Glasersfeld again:

Unless you claim some form of direct mystical revelation, whatever you call knowledge—your ideas or concepts, the relations that connect them, your images of yourself and the world—will be human, because the way you have produced them was yours, and you, whether you like it or not, are bound by the human ways.[186]

The constructivist approach is underpinned by the view expressed by Socrates's rival, the sceptic, Protagoras in Plato's *Theaetetus*: 'Man is the measure of all things.' We cannot stand outside of our own ways of knowing and therefore can have no access to a reality independent of those ways of knowing.[187] But what are those human ways of knowing?

[183] *Ibid.,* p. 391
[184] Popper, K. (1976) p. 39
[185] von Glasersfeld, E. (1995) p. 1
[186] von Glasersfeld, E. (1995) p. 27
[187] Plato (1987)

Locke, Berkeley and Hume, the so-called British empiricists, were among those who, prior to Popper, tried to answer this question. None of them was empiricist in the positivist way the word is often used today. The so-called 'hard-nosed' empiricist of today is hard-nosed because he believes that experimental evidence reflects the character of an observer-independent real world but neither Locke, Berkeley nor Hume is *realist* in that sense. Here, at some length, is Locke's *Essay Concerning Human Understanding* where he tackles the question:

> *All ideas come from sensation or reflection*. Let us suppose the mind to be, as we say, white paper, void of all characters, without any *ideas*. How does it come to be furnished? ... To this I answer, in one word, from *experience*; our knowledge is founded in all that, and from that it ultimately derives itself. Our observation employed either about *external sensible objects or about the internal operations of our minds, perceived and reflected on by ourselves, is that which supplies our understandings with all the materials of thinking*. These two are the fountains of knowledge from which all the ideas we have, or can naturally have, do spring.

There are, then, two sources of ideas, argues Locke. One is 'the objects of sensation': 'our senses *do convey into the mind* several distinct *perceptions* of things, according to those various ways in which those objects do affect them. And thus we come by those ideas we have of *yellow, white, heat…*' The second is '*the perception of the operations of our own mind* within us, as it is employed about the ideas it has gotten—which operations, when the soul comes to reflect on and consider, do furnish the understanding with another set of *ideas*, which could not be had from things without.'

He continues to elaborate on what he means by these operations of the mind:

> This source of ideas every man has wholly in himself; and though it is not sense, as having nothing to do with external objects, yet it is very like it, and might properly enough be called internal sense. But as I call the other [i.e., the first source of ideas] *sensation*, so I call this REFLECTION, the ideas it affords being such only as the mind gets by reflecting on its own operations within itself. By reflection then, in the following part of this discourse, I would be understood to mean that notice which the mind takes of its own operations, and the manner of

> them, by reason whereof there come to be *ideas* of those operations in the understanding. [188]

The important idea here is that 'reflection' is an essential component of understanding. In reasoning about sensations the mind generates ideas that could not be found elsewhere. For example, relational notions like causality. Locke writes of wood and of ashes, for example, as 'complex ideas' each consisting of a collection of 'simple ideas' (i.e., sensations). 'We consider fire,' he says, 'in relation to ashes as cause, and the ashes as effect. So that whatever is considered by us to conduce or operate to the producing any particular simple idea, or collection of simple ideas,…which did not before exist, hath thereby in our minds the relation of a cause, and so is denominated by us.'[189]

Locke, then, saw that certain characters of things exist in the mind as ideas—colours for example—and that relational ideas are also constructed in the mind. George Berkeley read Locke and wondered why Locke had stopped there. Where, he asked, does one find qualities like extension in space and motion and time if not also in the mind? He argued that these so-called 'primary' qualities were 'mathematical' and all mathematical thinking is the result of abstraction and reflection. von Glasersfeld draws the lesson:

> The important point in this is the realization that the features that were considered primary (in the sense that they reflect properties of real objects) depend on concepts that are formed from a succession of at least two experiential frames and an act of relating them. The succession then merely provides the experiencing subject with an opportunity to establish a relation; it does not require it. Nor does the succession itself determine what kind of relation should be established.[190]

If our basic relational ideas like extension, motion, time and causation are constructed by the reflective subject then, writes von Glasersfeld, 'one cannot describe in human terms what "reality" would be like before it is experienced'.[191]

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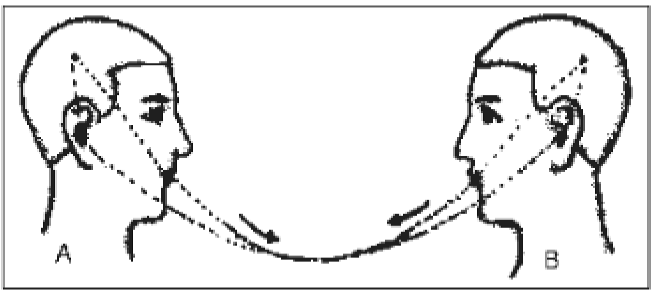
[188] Locke, *Essay Concerning Human Understanding* (1690), Book II, Chapter 1, par 1-4 in Ariew, R. and Watkins, E (1998) pp. 276-277
[189] Locke, *Essay Concerning Human Understanding* (1690), Book II, Chapter 26, par 1.
[190] von Glasersfeld E. (1995) p. 34
[191] *Ibid.*

This distinction between certain knowledge (*certum*) and the truth constructed by human minds (*verum*) is at the heart of Giambattista Vico's epistemology. This division between the knower and knowledge is there also in the Cartesian split at the beginning of the Enlightenment but while Descartes' solution lay in mysticism, in the characterisation of mathematics, for example, as God-given reality, Vico in the early eighteenth-century argued that mathematics was a human invention which can be logically guaranteed precisely for that reason. 'It is not,' writes Isaiah Berlin, 'as Descartes supposed, discovery of an objective structure, the eternal and most general characteristics of the real world, but rather invention: invention of a symbolic system which men can logically guarantee only because men have made it themselves, irrefutable only because it is a figment of man's creative intellect…the criterion and rule of truth is to have made it...if it is not factum by us it is not verum for us.'[192] Vico's verum-factum principle is an early expression of constructivism: knowing and making are two sides of the same coin.[193]

Saussure developed this insight when he rejected the common-sense idea that language, as he put it, is 'a naming process only—a list of words, each corresponding to the thing that it names. … [that] assumes that ready-made ideas exist before words.'[194] Consider his schematic illustrating the 'social crystallisation' of language:[195]



> Suppose that the opening of the circuit is in A's brain, where mental facts (concepts) are associated with representations of the linguistic sounds (sound-images) that are used for expression. A given concept unlocks a corresponding sound-image in the brain; this purely

[192] Berlin, I. (1997) pp. 111-112
[193] Miner, R. C. (1998) and Luft, S. R. (2003) p. 20
[194] Saussure, F. (2000) pp. 25-26
[195] *Ibid.,* p. 23

> *psychological* phenomenon is followed in turn by a *physiological* process: the brain transmits an impulse corresponding to the image to the organs used in producing sounds. Then the sound waves travel from the mouth of A to the ear of B: a purely *physical* process. Next, the circuit continues in B, but the order is reversed: from the ear to the brain, the physiological transmission of the sound-image; in the brain, the psychological association of the image with the concept. If B then speaks, the new act will follow—from his brain to A's—exactly the same course as the first act and pass through the same successive phases.[196]

The diagram distinguishes speaking (*parole*) from language (*langue*). 'In separating *langue* from *parole*', writes Saussure, 'we are separating what is social from what is individual and what is essential from what is ancillary or accidental'.[197] Each person makes meaning of the sound-images they hear by associating them with concepts. These associations are based on subjective experience. But, argues Saussure, 'language is not complete in any speaker; it exists perfectly only within a collectivity'[198] No one person has had all the experience of the group of individuals sharing a language, experiences that contribute to the associations between sound-image and concept, and so the language (*langue*) is unavailable to the individual. Thus learning a language 'will be seen as a never ending process of adaptation of one's own concepts, governed by the need and the wish to establish mutually compatible associations to the speech sounds one is hearing and producing.'[199] The speaker does not learn to fit her words to the world as such (the realist position) but learns to fit her words to the world as *experienced* and that experience includes listening to other people's speech; as Roland Barthes writes: 'to listen is not only to perceive a language, it is also to construct it.'[200] This accords with Wittgenstein's argument against the possibility of private language: language, to be language, is necessarily social.

[196] *Ibid.*
[197] In Culler, J. (1985) p. 30
[198] Saussure, (2000) p. 23
[199] Von Glasersfeld, E. (1995) p. 48
[200] Barthes, R. (1982) p. 102

Tradition

Like Vico, Thomas Kuhn (1922-1996) recognises the importance of tradition in our understanding. Kuhn was an undergraduate physicist at Harvard in 1940 when Popper was already living in New Zealand. When the Japanese attacked Pearl Harbour he joined other students on the editorial board of the Harvard *Crimson* in speaking out in support of Harvard president, James Conant's plan to militarize the nation's colleges. Harvard's physics department quickly adapted, as an exemplar for the country, and Kuhn was trained in electronics and radar research. After the war he began a PhD in Physics at Harvard and, with Conant's encouragement, assisted in the development of a general education course in science for undergraduates. The approach was to be historical and the assignment was decisive for Kuhn's intellectual development.

To develop his course, Kuhn set out to give an account of Aristotelian mechanics but he found he could make no sense of it. Aristotle's views on many other topics were clear and intelligent so how, Kuhn wondered, was it possible for such a brilliant thinker to be so stupid about motion? It was only when Kuhn realised that 'the permanent ingredients of Aristotle's universe, its ontologically primary and indestructible elements, were not material bodies but rather...qualities...' (an idea quite alien to his modern mind-set) that he was able to grasp Aristotle's mechanics. Kuhn came to understand that for Aristotle position itself was a *quality* and so a body which moves from one place to another transforms in the process and is only the same body in the sense that a child is the same as the adult it becomes: 'motion was necessarily a change-of-state rather than a state.'[201] After this realisation, this shift in his mental categories, Kuhn was able to read Aristotle with relative ease and found that 'much apparent absurdity vanished'.

In Kuhn's constructivism, truths are inseparable from the language that expresses them and, as Saussure argued, language is socially determined and bound-up with tradition. This means that when scientists attach symbolic expressions to nature (i.e., say things about nature in formal, scientific language) they are modelling one problem solution onto another, older one. Thomas Kuhn gives an example of this 'use theory' of meaning (echoing Wittgenstein's

[201] Kuhn , T. (1977) p. xii

thought experiment of defining the word 'game'[202]) by imagining a boy walking with his father by a lake. Johnny has previously learned to recognize birds and to discriminate robin redbreasts. The primary teaching method in this situation is 'ostension'—Father points and names. 'Phrases like 'all swans are white' may play a role, but they need not.'[203] Father points: 'Look, Johnny, there's a swan.'

> A short time later Johnny himself points to a bird, saying, 'Daddy, another swan.' He has not yet, however, learned what swans are and must be corrected: 'No, Johnny, that's a goose.' Johnny's next identification of a swan proves correct, but his next 'goose' is, in fact, a duck, and he is again set straight. After a few more such encounters…Johnny's ability to identify these waterfowl is as great as his father's.

Johnny has been programmed to recognise what his community already knows. It is not that he has learned the meaning of words by learning the things that each word stand for, the point is rather that ostensive definitions presuppose an understanding of the way each word is *used*. But, then, does Johnny know what 'geese', 'ducks' and 'swans' mean? 'In any useful sense, yes, for he can apply these labels unequivocally and without effort, drawing behavioural conclusions from their application.'[204] Johnny has learned to apply symbolic labels to nature but without needing criteria, definitions or correspondence rules. He has learned a primitive conception of similarity and difference that is now embedded in the similarity relationship itself, not in a generalisation or rule. Grasping this cognitive process, suggests Kuhn, is essential to an adequate reconstruction of what he calls 'normal' scientific understanding. Johnny has been inducted into a world view but…

…the world that the student then enters is not … fixed once and for all by the nature of the environment, on the one hand, and of science, on the other. Rather, it is determined *jointly* by the environment and the particular normal-scientific tradition that the student has been trained to pursue.[205]

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202 Wittgenstein, L. (2001) Section 66
203 Suppe (1977) p. 473
204 *Ibid.*, p. 475
205 Kuhn (1962/1996) pp. 112-113

This picture of the historically situated nature of knowledge leads Kuhn to propose, in *The Structure of Scientific Revolutions* (1962), what, from a 'received' point-of-view, looks like a very peculiar idea of scientific progress:

> The more carefully [historians] study, say, Aristotelian dynamics, phlogistic chemistry, or caloric thermodynamics, the more certain they feel that these once current views of nature were, as a whole, neither less scientific nor more the product of human idiosyncrasy than those current today. If these out-of-date beliefs are to be called myths, then myths can be produced by the same sort of methods and held for the same sort or reasons that now lead to scientific knowledge. If, on the other hand, they are to be called science, then science has included bodies of belief quite incompatible with the ones we hold today. Given these alternatives, the historian must choose the latter.[206]

The idea that in the course of history perfectly respectable scientific theories have been discarding makes it impossible to understand the history of science as simply the progressive accretion of knowledge. While Kuhn, like Popper, is concerned to explain how ideas are accepted or rejected, he does not, like Popper, expect to find the answer in an ahistorical logic of discovery but rather in a historically-situated study of the beliefs and behaviour of scientific communities. Such a study will produce a profoundly different understanding of science to one based on the received view:

> History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed. That image has previously been drawn, even by scientists themselves, mainly from the study of finished scientific achievements as these are recorded in the classics and, more recently, in textbooks from which each new scientific generation learns to practice its trade. Inevitably the aim of such books is persuasive and pedagogic; a concept of science drawn from them is no more likely to fit the enterprise that produces them than an image of a national culture drawn from a tourist brochure or a language text.[207]

Conventional histories of science present scientific practice as if it consists of a series of 'crucial experiments' that lead the scientist from ignorance to enlightenment. New ideas are thus arrived at by a series of logical,

[206] Kuhn, (1996) p. 2
[207] *Ibid.,* p. 1

self-evidently true steps. This picture, Kuhn argues, is mistaken and it cannot account for the history of science. In fact, Kuhn argues, really new ideas in science come along only rarely. Most of science comprises long stretches of time during which scientists are engaged in 'normal science'. Normal scientific ideas were once unprecedented but have now become the orthodoxy. Normal science is science done against the background of past achievements, science founded on ideas one finds in the textbooks that form the basis of a scientist's education. These books contain answers to questions such as what are the fundamental entities of which the universe is composed and what may the scientist legitimately enquire into and by what techniques? In forming the ground of a scientist's education and training, these ideas, argues Kuhn, 'come to exert a deep hold on the scientific mind.'[208] This deep hold steers scientists in certain directions which in turn results in the efficiency of scientific work—everyone pulls together.

Scientists work within these traditions because they are inculcated into them and because such traditions offer many opportunities for continuing investigation. Kuhn uses the word 'paradigm' to describe such scientific traditions. Examples of paradigms include 'Ptolemaic astronomy', 'Newtonian mechanics' and 'neo-Darwinism'. The aim, then, of normal science is to solve puzzles, thus demonstrating that nothing is novel, that nothing lies outside of the exemplars of the paradigm or what Kuhn sometimes calls the 'disciplinary matrix'. Indeed, claims Kuhn, 'Normal science…often suppresses fundamental novelties because they are necessarily subversive of its basic commitments.'[209]

The Austrian philosopher of science, Otto Neurath, therefore compares scientists to sailors 'who must rebuild their ship on the open sea, never able to dismantle it in dry-dock and to reconstruct it there out of the best materials.'[210] The boat stays afloat, continues Quine, 'because at each alteration we keep the bulk of it intact as a going concern. Our words continue to make sense because of continuity of change of theory: we warp usage gradually enough to avoid rupture.' [211]

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[208] *Ibid.,* p. 5
[209] *Ibid*
[210] Neurath, O. (1959) p. 201
[211] Quine, W. V. O. (1960) p. 4; Of course the same goes for the presentation of any novel ideas. For instance, the fact of telling a story with moving pictures did not exist before the cinema. For

A paradigm in science, argues Kuhn, is, therefore, always to an extent arbitrary, a product of historical circumstance. Scientists will try to accommodate all phenomena to the current paradigm, warping usage gradually, as Quine puts it, but some problems may yet resist such accommodation. As in Lakatos's story of the mystery planet, despite attempts to expand the scope of the disciplinary matrix by postulating new planets or clouds of gas some phenomena cannot be accommodated to any exemplar. If all efforts fail then a crisis may ensue. This sets the stage for a Kuhnian 'revolution' but does not immediately bring it about.

Such anomalies can only be spotted against the background of a precisely characterised discipline and so a scientist must be very well versed in the exemplars of the paradigm to notice that a phenomenon cannot be accommodated.[212] Furthermore, as Popper acknowledges, and Lakatos demonstrates, anomalies need not be allowed to kill a theory if scientists are determined to protect it. The accumulation of anomalies does not force an abandonment of the paradigm; they are not taken as falsifying counter-instances:

> Anomalous experiences may not be identified with falsifying ones. Indeed, I doubt that the latter exist. As has repeatedly been emphasised before, no theory ever solves all the puzzles with which it is confronted at any given time; nor are the solutions already achieved often perfect. On the contrary, it is just the incompleteness and imperfection of the existing data-theory fit that, at any time, define many of the puzzles that characterize normal science. If any and every failure to fit were ground for theory rejection, all theories ought to be rejected at all times.[213]

Instead, increasingly *ad hoc* attempts are made by different scientists to alter the matrix of ideas that comprise the scientific discipline in question and so improve the 'data-theory fit'. These efforts are met with decreasing unanimity as to their legitimacy within the community. But here is the important point Kuhn wants

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the fledgling medium to be intelligible it had to refer to known values and therefore theatrical forms were pressed into service. Jean Mitry (2000) remarks that while it was acceptable to people that 'views' of nature should be presented with a camera that moved about because it is natural to move about in the world, for an audience used to the theatre, it took some time before the same approach could be applied to 'drama'. Audiences had to understand *why* they were being shown images of people's heads but not their full bodies, for example, before the close-up was intelligible.

[212] Note that even the word 'phenomenon' implies a theorising of what is mysterious because it puts boundaries around it, marks it out as an 'event'. The phenomenon is invisible until it is seen as such and seeing is theorising.

[213] Kuhn (1996), p. 146

to make about scientific revolutions: this process can go on and on and a discipline can simply degenerate *unless and until* a new, alternative theory is proposed. 'The decision to reject one theory is always *simultaneously* the decision to accept another.'[214]

This new theory cannot be a result of communal, 'normal' science but is the fruit of what Kuhn calls 'extraordinary' research. Such work is generally carried out by an individual who, at those moments at least, is unconstrained by the disciplinary matrix. Extraordinary research is more random research:

> Confronted with crisis, scientists take a different attitude towards existing disciplinary matrices and their exemplars, and the nature of their research changes accordingly. The proliferation of competing articulations, the willingness to try anything, the expression of explicit discontent, the recourse to philosophy and the debate over fundamentals, all these are symptoms of a transition from normal to extraordinary research.[215]

If the problem is resolved by the adoption of a new theory and its ensuing paradigm (i.e., if it can render the crisis-provoking anomalies lawful) then it must be a theory that leads one to predictions that could not have been derived from its predecessor. If this is the case, Kuhn argues, then the new theory and the old one are incompatible. In taking on a new theory one also takes on new exemplars and a new set of oughts and interpretations—a new vocabulary. If the new theory uses some of the terms of the old theory it will use them meaning something different and so the two disciplinary matrices are 'incommensurable', two quite different paradigms or worldviews.

If the two world views are incommensurable then there can be little meaningful communication between them.

> This is not to say that such arguments need be irrational, for there are rational means of persuasion. In particular, one reason why the arguments are at cross-purposes is that the same terminology is being used with different empirical meanings by the two camps…there is not even a neutral observation language since the exemplars, *inter alia*, involve interpreting and classifying the phenomena to which the symbolic generalisations are applied differently.[216]

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214 *Ibid.*, p. 77 (my emphasis)
215 *Ibid.*, p. 90
216 Suppe (1977) p. 147

There is therefore nothing irrational in refusing to jump ship. For one thing, accepting the new theory brings in its train an acceptance of it as a new archetype for future science and one may doubt that, despite its solution to the present anomalies, it will be a fruitful direction to go in (that it will provide enough new puzzles for future normal scientific work for instance). So, only if and when most of the community has switched allegiance can the revolution be said to be complete and a new paradigm established. The Copernican revolution, for example, took a hundred years.

Summary

Scientific discourse is commonly understood as language describing a universe independent of any observer. Sceptics of this received view have argued that what we take for nature speaking to us is in fact ourselves speaking to each other about our experiences and our reflections on the workings of our own minds. Locke argued that it is through reflecting on our own mental operations that we create certain ideas; Berkeley extended this idea to embrace the ‘primary qualities’ of succession, extension and time. All of these ideas are mental constructs; Hume explained that we create relational ideas like causation by associating ideas in our minds. These various related ideas also form the core of Vico’s *New Science*, in which he articulates a constructivist doctrine, separating knowledge (verum) from certainty (certum). Human knowledge, he argued, is only possible of that which is made by man.

This is not an argument ‘against reality’ (whatever that might mean). The constructivist does not wish to argue that reality is ‘all in the mind’ but rather to point out that it is not the immediate cause of our understanding—it cannot speak to us directly. We are, of course, part of reality and a constructivist includes this fact in how they conceive the nature of science. The world we understand and live in appears to have one particular form rather than another (a form in which, for example, objects fall under the influence of gravity as Newton saw, rather than moving to their ‘proper’ place as Aristotle suggested) because, argues Ernst von Glasersfeld, ‘we complete the picture by means of rational heuristic fictions’.

Saussure describes the social process by which we develop these fictions, this shared language (the Kuhnuian *paradigm*) through a process of matching concepts to sound-images in our minds, a process that is carried out by each individual within a network of social relations so that each member of the community negotiates meanings that are compatible with others.

Taken together, these considerations lead to the principles of constructivism:

> 1 Knowledge is not passively received either through the senses or by way of communication; knowledge is actively built up by the cognizing subject.
> 2 The function of cognition is adaptive, in the biological sense of the term, tending towards fit or viability; cognition serves the subject's organization of the experiential world, not the discovery of an objective ontological reality.[217]

One might say that constructivism conceives its mode of operation as declarative rather than descriptive; it denies the positivist illusion of simple referentiality. Based implicitly on such a constructivist epistemology, Kuhn's historical account of scientific revolutions seems to commit him to saying that, in the absence of a logic of scientific discovery, theory choice and scientific knowledge have no factual base. However, Kuhn himself would be dismayed at such a bald conclusion. The constructivist view does not deny that there is such a thing as material reality and understands that of course this is what scientists run up against in doing their work. It is not the case that *anything* goes. However, the constructivist, in distinction to the positivist, holds that what the scientist has to make sense of is his or her *experience* of the world which is not the same as the world itself, as von Glasersfeld puts it, 'cognition serves the subject's organization of the experiential world, not the discovery of an objective ontological reality.'[218] Furthermore, this organisation is achieved in a social context and so it is not that man is the measure of all things so much as his community is (there is no private language). It is a scientific *group* that determines when a theory has been tested enough or when it is dead. Meaning

[217] von Glasersfeld (1995) p. 51
[218] *Ibid*

is grounded in consensus. ‘Knowledge, therefore,’ as Hans Reichenbach acknowledges, ‘is a very concrete thing; and the examination into its properties means studying the features of a *sociological phenomenon*.’[219]

Positivists will argue that consensus is only possible because there is a readily available common external reality about which people agree but constructivists see it the other way around—scientists (indeed all of us) come to agree (or not) about what is real. What is real is decided in the same way that we learn our mother tongue: by practical consensus. Knowledge is fundamentally a sociological phenomenon and the objective reality of the received view is simply not available for arbitration. This, then, is the constructivist view of science that the film, *Hopeful Monsters*, brings to mind.

[219] Reichenbach, H. (1938) Chapter One (my emphasis)

CHAPTER FOUR
THE SOCIOLOGY OF SCIENTIFIC KNOWLEDGE

In this short chapter, I want to explore how a constructivist view of scientific knowledge alters our analysis of what principles govern scientific *practice*. This is important if we are to evaluate the extent to which *Hopeful Monsters* does indeed represent an alternative to the received view.

When I was making *Hopeful Monsters*, I wrote to Professor Eric Davidson, at the California Institute of Technology explaining that I was making a film about Don Williamson and I asked him for an interview. Davidson is an expert on the developmental biology of echinoderms and also a significant pioneer of molecular biology and I acknowledged in my letter that I had heard he was critical of Williamson's ideas. I explained that I wished in my film to explore the social dynamics of how controversial ideas make their way into mainstream scientific thought (or, indeed, fail to). He replied as follows:

> I cannot IMAGINE a more trivial and non-exemplary example of scientific theory making than Williamson's productions, and why someone serious would waste their time with it is beyond me. I would most certainly not! He represents no aspect of 'scientific practice', and in a world well stocked with superb scientists he is a quasi-scientific bit player at best. I would advise you to have some concern for your own reputation in this!

When I suggested that Williamson was a serious scientist with many years' experience, Davidson responded:

> To me it would be like discussing creationism, something I will never have the time for. History of REAL science is another thing: that I will always have the time for.

I asked him to reconsider but, in a third and final e-mail, Davidson wrote:

> If you were historically inclined and actually interested in the history of real science you would be investigating the conditions and precursors and the pathway toward real discoveries, instead of investigating the alleged affronts suffered by a well meaning non-contributor to any

> serious science, applying the illegitimate idea that this illuminates the history of science. No scientist would make that mistake...[220]

In chapter one, we noted Robert Merton's norms of good scientific practice, among them the norm of 'organised scepticism' by which scientists ought to be sceptical of all new ideas in the sense of being critically open-minded. There is no doubt that Davidson would characterise himself as a defender of such a norm and Williamson of flouting it but this illuminates a difficulty with interpreting scientific behaviour for what looks to Davidson like Williamson's betrayal of Merton's norms looks to Williamson like conforming to them. The problem lies with Merton's analysis of scientific behaviour. 'Scientists themselves are naturally quite impressed by this set of ideals,' writes Nick Russell in *Communicating Science* (2010) 'but the extent to which scientific behaviour is actually constrained by them is open to debate.'[221]

In exploring the extent to which Merton's norms might be said to operate, the sociologist Michael Mulkay notes that Merton presumed a rather flat playing field on which all scientists and scientific ideas are treated with equal respect. However, when sociologists began to look in greater detail at the conditions in which science is done they found that the playing field was far from level. Mulkay, for example, suggests that today's 'Big Science' (i.e., Davidson's area of recombinant DNA technologies) has created unreasonable expectations on other domains of biological enquiry (such as Williamson's) that may not yet be ripe for similar exploitation. Such emerging or less prestigious fields must compete with biotechnology not only for grants and support but for space in prestigious publications. This means that in these more marginal fields, pressure builds up to cheat.[222] Indeed, Williamson has been accused of cheating, albeit indirectly, not by Davidson but by an erstwhile collaborator, the biologist Michael Hart:

> I've probably never said to you what I've suggested to a few others who have asked about this 'controversy':

[220] Personal communication.
[221] Russell, N. (2010) p. 44
[222] Mulkay, M. (1992); see also Rothman, R. A. (1972)

> I strongly suspect that in 1989 and 1990 someone at Port Erin Marine Laboratory played a very cruel hoax on you by carefully substituting known numbers of sea urchin embryos into your cultures of known numbers of 'tunicate' eggs. I suspect that the reason you were never again able to get 'larval transfer'' in a laboratory hybridization—that is, the paternal larval phenotype developing from eggs that should have had a very different maternal larval form—is because the hoaxster never struck again after 1990. I'm fully aware of possible objections to this suggestion, including the personal integrity of the PEML staff as well as your ability to distinguish tunicate eggs and embryos from sea urchin eggs and embryos. But…a skeptical mind is led inevitably to such thoughts.[223]

Williamson replied two days later:

> Utterly ridiculous and completely impossible, but it does show the farcical lengths you are prepared to go to rather than admit that I crossed a sea squirt with a sea urchin. Your hypothetical prankster, who, at an exact time and date in March 1989 and again in February 1990, had a culture of exactly the right number of *Echinus* eggs at exactly the right degree of development, must have been invisible. I worked alone in a small lab, and I certainly did not leave the room while I was waiting for the first cleavage.[224]

Of course, Hart was being disingenuous. By hoaxster he means to suggest, indirectly, that Williamson cheated.

But pressure to cheat is also felt, perhaps more so, not only by those engaged in controversial science but also by those toiling at the rich seams of 'normal' science, i.e., science that is well-funded, prestigious and developing rapidly. Organised scepticism even among molecular biologists may go out the window when there is pressure to be first to discover something in a highly competitive field. Indeed, replication of experiments (the supposed 'supreme court' and 'safety mechanism' of the scientific system) is far from standard practice because replicating another's experiment wins you no kudos and costs money. Merton's functionalist picture of science is, in its initial formulation at least, too idealistic. As Nick Russell puts it,

> There is little reward in science for coming second. … and many scientists exist in a competitive snake-pit, constantly applying for short-

[223] From an Email between Michael Hart and Don Williamson dated Fri 26/10/2007
[224] From an Email between Don Williamson and Michael Hart dated Sunday 28/10/2007

> term grants to survive. … As the late veteran science watcher John Ziman remarked in a commentary in *Nature* in 1999, 'What price now those noble (Mertonian) norms? Tied without budgets into a system of projects and proposals, budgets and assessments, how open, how disinterested, how self-critical, how riskily original can one afford to be?'[225]

It seems obvious that the institutions of science benefit greatly in power and prestige by at least paying lip-service to the moral norms that Merton identifies, (norms whose origins may be traced to the Puritan culture of seventeenth century England[226]) but if or how this ethos guides scientists' daily activities is less clear. Indeed, some sociologists report evidence of widespread deviation from it.[227]

Ian Mitroff, for example, studied the working practices of scientists in the Apollo programme. He concluded that, among these scientists at least, the Mertonian norm of 'emotional neutrality' (organised scepticism) is countered by a norm of 'emotional commitment'. This commitment, argues Mitroff, is essential if scientists are to bring lengthy and laborious projects to fruition and tolerate setbacks along the way. Furthermore, being dogmatic and stubbornly holding to one's beliefs allows others to build on one's work without worrying about foundational matters. Mitroff suggests that in scientific culture there are actually two sets of norms functioning together in a dynamic way. *Universalism*, for instance, may be countered by a norm of 'particularism' in which claims are, contrary to Merton's ethos, judged by *who* makes them. Similarly the norm of *Communism* may be countered by one of 'secrecy' because secrecy allows scientists to work without the fear of others doing the same work and perhaps getting the credit for it first.[228]

Not only may scientists operate counter norms as Mittroff describes but, provided it proves useful to the scientific community, they may even lie about their findings without facing censure. The physicist and historian, Gerald Holton, for example, finds that the physicist Robert Millikan claimed to have used all his data set in establishing the 'charge on the electron' when he actually

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[225] Russell, N. (2010) pp. 44-45
[226] Merton, R. (1970)
[227] See Rothman, R. A. (1972)
[228] I.I. Mitroff (1974) p. 581.

selected only those data points that fitted his theory.[229] Holton explains that selecting data allows 'the experimenter to assert that he believes the discordant observations do not go to the heart of the matter, that is to say, are not grounded in a serious way in the phenomenon being studied. ... [This allows] him to avoid the interruptions, delays, and detailed research that might be necessary to pin down the exact disturbing causes behind the discrepant observations.' [230]

Here we are back with the theory-ladenness of observation for of course this is *always* a problem in scientific research. Everyday in laboratories around the world, experimental scientists face this so-called 'experimenter's regress', the circle of dependence between experiment and theory. As Holton argues:

> It is generally true that prior to the absorption of research results into canonical knowledge, the selection of the relevant portion of a range of experience ... is guided by a hypothesis. That hypothesis in turn is stabilized chiefly by its success in handling that 'relevant' portion [of experience].[231]

Put simply, theory takes priority over observation or, as the philosopher of science, Alexander Koyré puts it, 'good physics is made *a priori*'.[232]

For Merton, rationality and validity are *natural* goals for people and no further explanation of them is needed. In other words, nothing makes people do or believe things that are correct and only error and bad science are open to sociological explanation. But the theory-dependence of observation mandates serious objection to any claim that tidying up data represents a crime against science. A scientific mind should not be, indeed cannot be, a blank slate, a virgin receptacle. It is the expectations and understanding of the laws of nature that tell the observer what is a good experiment and what is a failed experiment, what are good data and what are bad or insignificant data that can be ignored and kept unpublished. And so it must be recognised that 'error', and especially cheating or 'fraud' are the *result* of controversy, not its cause. There is no unique, timeless and efficacious scientific method. Evidence is amassed, *post hoc*, to support a hypothesis, a guess, a hunch and Merton's organised scepticism is scarcely to be found.

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[229] Mendel: Fisher (1936) and Wright (1966); Millikan: Holton (1978)
[230] Holton G. (1978) p. 194
[231] *Ibid*., p. 211
[232] Koyré, A. (1968) p. 88

Kuhn recognises that this picture of scientific practice is quite unlike the Mertonian one predicated on a positivist epistemology. To discuss the nature of scientific practice, he writes, is 'to talk about techniques of persuasion, or about argument and counterargument in a situation in which there can be no proof,' and so, 'in the absence of criteria able to dictate the choice of each individual…we do well to trust the collective judgment of scientists…the decision of the scientific group'.[233] The social historian of science, Derek Phillips concurs: 'truth-claims are settled in the scientific or intellectual community…by meeting various public criteria which satisfy other scientists or thinkers that truth is established.'[234]

If science is at heart a social process then understanding how it functions requires that we investigate the influence of social dynamics not only when science goes wrong or 'bad' but when it 'works' too. 'Good' science should be explained by the same mechanisms we use to explain 'bad' science. Such an agnostic and 'open' approach is vanishingly rare in television science, as Gardner and Young explain:

> Significantly, the *Horizon* team are very preoccupied with retaining the good will of the scientific community and don't often go in for hard hitting analyses unless the topic is already an established scandal. Even there, in the case of the IQ controversy, they are preoccupied with whether or not it's 'good science', where the real point at issue in this case is the ideological power of a particularly influential form of scientism which legitimates social and racial hierarchies by 'scientific' means. We asked a *Horizon* researcher about their relations with the growing community of people who think, do research and make critical stands on the history, philosophy and social relations of science as well as the new disciplines such as science policy, 'science, technology and society', bioethics, technology assessment. He replied, 'We have no regard for that community.' When taxed about this, he made it very clear that it was the scientific community, not the people who think *about* science, to which *Horizon* directs its attention.[235]

The sociologist, David Bloor is one of those who thinks *about* science. He calls his approach the strong programme in the sociology of science to contrast it with

[233] Kuhn, T. (1977) pp. 320-321
[234] Phillips, D., (1974) p. 82
[235] Gardner, C. and R. M. Young (1981) p. 180

the 'weak programme' exemplified by the work of 'externalist' sociologists like Robert Merton.

The Strong Programme in the Sociology of Scientific Knowledge

Instead of Plato's 'justified true belief' the strong programme defines knowledge, as Vico does, as that which people take to be knowledge. The strong programme acknowledges that people, some of them scientists, take a variety of sometimes contradictory ideas to be knowledge. What then, asks Bloor, accounts for this variance? He requires of any answer that it explain *both* true and false belief and that this explanation be reflexive, applicable that is to the strong programme itself.[236] Methodologically this means the strong programme sociologist is agnostic about scientific truths and, taking less for granted, looks further in attempts to explain the development of scientific knowledge. The strong programme, then, is predicated on a constructivist view of knowledge, as explained by the anthropologist of science, Karin Knorr-Cetina:

> Rather than view empirical observation as questions put to nature in a language she understands, we will take all references to the 'constitutive' role of science seriously, and regard scientific enquiry as a process of production. Rather than considering scientific products as somehow capturing what is, we will consider them as selectively carved out, transformed and constructed from whatever is. And rather than examine the external relations between science and the 'nature' we are told it describes, we will look at those internal affairs of scientific enterprise which we take to be *constructive*.[237]

The strong programme, then, teases out three dimensions for study that comprise the constructivist view: (1) science's social dimension; (2) science as a practical *activity* (a thought reinforced by the etymology of the word 'fact' with its Latin root *facere*, to make); and (3) the non-naturalness of the products of science—that is, there is no direct route from nature to ideas about nature. The constructivist approach has evolved from the 1970s to the present day, becoming less and less concerned with the foundational matters of traditional epistemology such as the nature of 'truth' and 'reality' and more with enquiring

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[236] Bloor, D. (1991) p. 7
[237] Knorr-Cetina, K. (1981) p. 3

where in the scientist's laboratory or in their discourse we find these things that the traditional positivist values: 'nature', 'truth' and 'reality'? The question for this dissertation is whether the film, *Hopeful Monsters*, represents science as Knorr-Cetina suggests, i.e., as a social and practical activity, as *factum*.

In a documentary that attempts to represent science in a post-positivist manner as *Hopeful Monsters* appears to do, we should expect to find a shift in emphasis from the concerns of positivism (is the theory true?) to those of constructivism (how is the theory being *made*?). We have something of a model for this in the work of Bruno Latour who seeks, within a constructivist paradigm, to locate 'nature', 'truth' and 'reality' in scientific *practice*. In doing this, Latour attempts to reveal the interconnectedness of the substance of knowledge and the process of its origination so as to uncover the values involved in that process. As Gardner and Young write, this is decidedly *not* what the narrative of the classic science documentary (like *The Ghost in Your Genes*) is concerned to do; for in those films representation of the values involved in scientific culture and practice are 'precluded by the breathless form of presentation which operates at an expository pace and conveys a sense of inevitability rather than one of social *choice*.'[238]

In *Science in Action* (1987), Bruno Latour tries to understand what determines the choices scientists make by beginning with the endpoint of scientific research—a sentence in a college textbook. He then jumps back in time to the laboratory bench where this sentence is uttered among only a few people and then moves forwards in time, noting the transformative 'journey' the sentence makes from being shared between colleagues to being a fact in a textbook where it is used to educate the next generation of scientists:

> We start with a textbook sentence which is devoid of any trace of fabrication, construction or ownership; we then put it in quotation marks, surround it with a bubble, place it in the mouth of someone who speaks; then we add to this speaking character another character *to whom* it is speaking; then we place all of them in a specific situation, somewhere in time and space, surrounded by equipment, machines, colleagues; then, when the controversy heats up a bit we look at *where* the disputing people go and *what* sort of new elements they fetch, recruit or seduce in order to convince their colleagues; then we see how the people being

[238] Gardner, C. and R.M. Young (1981) p. 174 (my emphasis)

> convinced stop discussing with one another; situations, localisations, even people start being slowly erased; on the last picture we see a new sentence, without any quotation marks, written in a text book similar to the one we started with in the first picture.[239]

Textbook science is what Latour calls *Ready Made Science* and he distinguishes it from *Science in the Making*. Ready made science is like the black box cyberneticians use in their circuit diagrammes: 'whenever a piece of machinery or a set of commands is too complex…in its place they draw a little box about which they need to know nothing but its input and output.'[240] The black box 'contains' knowledge that is considered established and that may be used, as it were, without doubt, that is without looking inside the box to check how the fact came to be a fact in the first place. The word 'gene' is an example of a black box used again and again in today's molecular biology labs. It was once a controversial idea that a gene is a section of a piece of DNA and that it takes the form of a double helix comprising a certain arrangement of bases (C, T, G, A) but this notion is now so well established that scientists refer to it as a fact and no longer feel it is necessary even to cite the source of the original idea (e.g., the papers published by Watson and Crick in *Nature* in 1953).[241]

Controversy and Fact

The question for Latour is how these black boxes are created, in other words how controversies are settled and facts established. He seeks to persuade us that, contrary to the received view, it is not facts that settle a controversy; rather, facts are the *outcome* of that settlement. Only once a controversy is closed, argues Latour, do the facts take on the appearance of having caused this closure.

Latour develops a metaphor to dramatise this dynamic process: 'Science has two faces,' he writes, 'one that knows, the other that does not know yet.'[242] The Janus-headed science speaks like a positivist from one mouth, saying, 'when things are true they hold' and as a constructivist from the other, saying

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[239] Latour, B. (1987) p. 15
[240] *Ibid.,* p. 3
[241] Here is Francis Crick reflecting on their discovery twenty years later: 'It can now be taken as firmly established that DNA usually consists of two chains, wound together and running in opposite directions. The evidence for this statement is so extensive that it would take too long to quote it all here.' Crick, F. (1974) 'The Double Helix: A Personal View' *Nature*, Vol. 248
[242] Latour, B. (1987) p. 7

‘when things hold they start becoming true’.[243] *Science in the Making* is articulated by the constructivist side of the face while *Ready Made Science* is heard from the positivist side. Science then is a process of transformation. Controversial ideas described by the constructivist side are transformed through a social process of *persuasion* into facts in the mouth of the positivist side. This dynamic may be seen in action by examining, for example, the history of citations in scientific papers.

When a controversy flares up scientists will recruit allies by citing in their papers what other scientists have written; the more footnotes and quotations the more serious the paper:

> It is a question of numbers. A paper that does not have references is like a child without an escort walking at night in a big city it does not know: isolated, lost, anything may happen to it. On the contrary, attacking a paper heavy with footnotes means the dissenter has to weaken each of the other papers, or will at least be threatened with having to do so…The difference at this point between technical and non-technical literature is not that one is about fact and the other about fiction, but that the latter gathers only a few resources at hand, and the former a lot of resources, even from far away in time and space.[244]

This, of course, runs counter to Robert Merton’s notion that the purpose of citation is to acknowledge intellectual property rights and provide an ‘income’ to the owner of the property. In ‘Referencing as Persuasion’ Nigel Gilbert points out that such ideas cannot account for those references that are included by an author in order to challenge or contradict them, or those ‘perfunctory’ references ‘which cite, almost as an aside, work which is not apparently strictly relevant to an author’s immediate concerns.’[245] Gilbert also argues that the metaphor of ‘property’ is not useful because the ‘findings’ reported in a paper and later cited by another are not property that can be exploited as we commonly understand that word. They cannot yield a rent for example nor be sold. Indeed, the cited author may not even be aware that he or she has been

[243] *Ibid.*, p. 12
[244] *Ibid.*, p. 33
[245] Gilbert, G. N. (1977) p. 115

cited.[246] Gilbert concludes that the purpose of citation, as Latour suggests, is not to acknowledge rights but to enhance persuasion:

> [The] referencing of earlier research achieves more than the mere incorporation of the referenced work into the new paper; inasmuch as this work has already been accepted as 'valid science', it also provides a measure of persuasive support for the newly announced findings.
>
> It follows that it may be more effective to cite an authoritative paper, thus trading on its acknowledged adequacy, than to redescribe the research without reference to the original paper. One can therefore argue that the scientific 'norm' that one should cite the research on which one's work depends, may not be a product of a pervasive concern to acknowledge 'property rights', but rather may arise from scientists' interest in persuading their colleagues by using all the resources available to them, including those respected papers which can be cited to bolster their own arguments.[247]

For Latour, then, a fact is 'a rare event' that can be said to have taken place when, within the scientific community and over a period of time, one generation of authors after the next makes reference in their papers to the same new idea, all of which are positively reinforcing:

> This rare event is what people usually have in mind when they talk of a 'fact'. I hope it is clear by now that this event does not make it qualitatively different from fiction; a fact is what is collectively stabilised from the midst of controversies when the activity of later papers does not consist only of criticism or deformation but also of confirmation. The strength of the original statement does not lie in itself, but is derived from any of the papers that incorporate it.[248]

Whether reality fits a concept is always a collective matter for those operating within the same paradigm. The meaning or content of a scientific theory then has the character of an institution in that it relies for its existence on scientists' moment-by-moment continuing belief in it. Kuhn's normal science is defined by this on-going activity. 'Nature' only appears to be the adjudicator *in retrospect*, once an idea is no longer controversial, that is, once the idea has been accepted through a process of argument, publication and counter publication. Reality is not decisive and neither are an individual's claims. 'It is

[246] *Ibid*
[247] *Ibid.,* p. 116
[248] Latour, B. (1987) p. 42

not, after all, the individual who decides whether his discoveries or theoretical inventions shall become part of the body of established science,' writes Thomas Kuhn, 'rather it is his professional community, a community which has and sometimes exercises the privilege of declaring him a deviant.'[249]

If Latour's Janus-headed science speaks from one mouth as a positivist and from the other as a constructivist then the establishment of a scientific fact effects an acoustic shift. The constructivist voice is drowned out by the positivist one and two-faced science goes from speaking in stereo to mono or what Frederick Ruf terms the magisterial voice. And so the key, as Latour sees it, to a more adequate representation of science than the received view is to show that, despite the monophonic voice of science found in textbooks and classic science documentaries, the voice of science is properly understood as *stereophonic.*

Conclusion

It seems, then, that scholars of science studies are suggesting that scientists fail to properly appreciate the nature of what they do. Not that there is anything sinister about the persistence of the received view among scientists, just that its persistence is in itself a phenomenon worthy of investigation. This dissertation is not a work of sociology or psychology but it does seem that, for whatever reason, one can say the received view is a deeply ingrained and perhaps necessary habit among scientists. Indeed, the psychoanalyst Adam Phillips argues it is how all of us choose to deal with our experience. Our beliefs, as he puts it, are *vehicles* for experience and people choose beliefs that afford them the experiences they desire.[250] Peter Medawar won the *Nobel Prize in Physiology / Medicine* in 1960 and has written a great deal about the nature of science. He writes that scientists are wedded to the idea that they work inductively, that they bring nothing of their subjective selves to their apprehension of the world: 'if scientists sometimes like to think—as Darwin certainly did—that they proceed by induction it can only be because the myth of induction is that which accords best with the self-image a scientist may have formed of himself: as a regular, straightforward, plain-thinking man of facts and

[249] Kuhn, T. S. (1963) pp. 395
[250] Phillips, A. (2006)

calculations—someone very different from a philosopher, a poet fellow or an imaginative writer.'[251]

The rhetorician, Alan Gross sums this up, arguing that scientists, as David Hume suggested, are 'motivational realists': 'It is not that scientists intend to create realist theories, rather, the possibility of such theories is the psychological anchor that makes a life in science meaningful.'[252] However, it is important to note that motivational realism is not an epistemology; it does not tell us what makes a scientific theory true. Popper, Kuhn, Latour, Gross and others show us, on the contrary, that the picture of scientists as plain-thinking men of facts and calculations is a rhetorical invention. As Terry Eagleton reminds us:

> All of our descriptive statements move within an often invisible network of value-categories, and indeed without such categories we would have nothing to say to each other at all. It is not just as though we have something called factual knowledge which may then be distorted by particular interests and judgements, although this is certainly possible; it is also that without particular interests we would have no knowledge at all, because we would not see the point of bothering to get to know anything. Interests are constitutive of our knowledge, not merely prejudices which imperil it. The claim that knowledge should be 'value-free' is itself a value-judgement.[253]

Scientists employ logic and dialectic as rhetorical strategies to achieve a value-free, i.e., realistic-sounding, monophonic voice that is, as Alan Gross says, 'so persuasive as to seem unrhetorical—to seem, simply, the way the world is'[254] But scientific descriptions of the world are not logically entailed and true for all time, they are conjectural and their truth is determined and maintained by the scientific institution that values them. It is this self-denying value-system of science that is reproduced by the rhetoric of the classic science documentary.

In the next chapter I will briefly return to our analysis of scientific discourse in order to see more clearly the ways that that discourse denies its implicit interests and draw from this analysis the features we may reasonably

[251] Medawar, P. (1984) p. 17
[252] Gross, A. (1990) p. 200
[253] Eagleton, T. (1983) p. 14
[254] Gross, A. (1990) p. 207

expect to find in any science documentary representing, by contrast, a constructivist epistemology.

CHAPTER FIVE
SCIENTIFIC DISCOURSE REVISITED

In our earlier analysis, scientific discourse was understood as 'rhetorical' only up to a point, designed to communicate a received view about the nature of science and scientific knowledge but without undermining its own ability to accurately reflect how science indeed discovers facts about nature. I have, I hope, undermined the rationality of this view, arguing that contrary to received notions of science, the facts do not speak for themselves and science is about invention not discovery: in short, knowledge is socially constructed. The philosophical and sociological considerations that lead to this conclusion suggest we ought also to reconsider our analysis of scientific discourse. Might there be, as Ron Curtis asks in 'Narrative Form and Normative Force,' other forms of representation than the classic scientific paper (or, in our case, the classic science documentary) that 'reflect more accurately and critically alternative interpretations of scientific practice' such as we have developed over the last two chapters?[255] Curtis argues, for example, that the Socratic dialogue is an appropriate alternative to classical scientific writing, and in this thesis I have essayed a filmic alternative: the documentary, *Hopeful Monsters*.[256] In this section, then, I attempt to look a little more closely at the rhetoric of scientific discourse so as to be in a position, in later chapters, to evaluate the success of *Hopeful Monsters* as an alternative representation of science.

Ideology: The Old Tune

Although Huxley's distinction between literary discourse and scientific discourse (the one a report of private experience, the other of public) seemed borne out by Bloomfield's description of the 'technical dialect' of science, one need not reflect long on this view to recognise its short-comings.[257] First, such an idea entirely ignores the problem of the theory-dependence of observation, (an unavoidable 'intrusion' of the private into the public) and, second, it ignores

[255] Curtis, R. (1994) p. 445
[256] *Ibid.*, p. 446
[257] Huxley, A. (1963); Bloomfield, L. (1935)

the contents of most modern scientific statements that are often highly abstruse and very far from everyday (i.e., 'public') experience. The notion, then, that scientists work toward a series of plain statements, each reflecting demonstrable experience is far from a fair representation of what occurs in practice. This is not of course to invalidate modern science but merely to reiterate, as the chemist turned professor of English, David Locke puts it, that:

> The subject matter of modern science is meaningful only in terms of its own conceptual schemes, not by direct appeal to ordinary experience. ... There is no real world that scientists know independently of the linguistic, graphic and mathematical formulations by which they conceive it. ... One cannot conceive that the complex world of modern science could be directly accessible to the mind unmediated by language and mathematics. Ultimately, to claim that the scientist somehow explores the real world directly, without the mediation of language, and then represents, reflects or transcribes this world picture is unthinkable.[258]

Roger Silverstone writes that 'science comes to television fully clothed', but from what we have argued thus far we must conclude that his metaphor is not quite right—science cannot, even in principle, remove those clothes for it has no existence outside of them, outside, that is, of discourse.[259] What looks to the positivist like more or less shape-distorting garments are, to the constructivist, science's skin, indeed its muscle, sinew, organs and bones. To the constructivist, science is always and unavoidably a discourse *to begin with*. We notice this less when we read contemporary scientific accounts because we are so familiar with the language but Kuhn's experience of reading Aristotle indicates the extent to which learning science and learning the language of science necessarily go hand in hand. The universe that scientific texts bring to us is in an important sense constituted by the language used to describe it.

Scientific discourse, then, whether technical or popular both embodies an idea of nature and an idea of *science* and they are necessarily bound up with each other. Getting the words of scientific discourse right, as Charles Bazerman writes, 'is more than a fine tuning of grace and clarity; it is defining the whole

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[258] Locke, D. (1992) pp. 27-28
[259] Silverstone, R. (1989) p. 189

enterprise,' for how can we know, as the poet asks, 'the dancer from the dance?'[260]

From a constructivist perspective, the scientific paper is not a record but a presentation, writes David Locke:

> When scientists come to compose their papers (and the verb must be an active one), they proceed not from the scientific activity itself but from a model, a model of what a scientific paper should be…One leafs through the pages of a scientific journal, and the pattern sanctioned by the traditions of the discipline at once becomes apparent: often the internal heads from paper to paper are precisely the same: Introduction, Methods, Results, Discussion. Each scientist merely rings a set of changes on the old tune…the scientific paper fulfils in its own way the pattern that shapes it. It is in this sense that the scientific paper must be considered a construct.[261]

So too, then, must the science documentary be considered a construct. The question becomes what idea of science does it construct? In a 1963 radio talk titled, 'Is the Scientific Paper a Fraud?', Peter Medawar explored the nature of Locke's 'old tune' and concluded it was far from what he understood the scientific process to be. On the contrary, he said, 'the scientific paper in its orthodox form does embody a totally mistaken conception, even a travesty, of the nature of scientific thought.'[262] Its structure—Introduction, Methods, Results, Discussion—is, he complained, entirely inductive, seeming to imply that the scientific mind is 'a virgin receptacle, an empty vessel. …you [apparently] reserve all appraisal of the scientific evidence until the 'discussion' section at the end, and in the discussion you adopt the ludicrous pretence of asking yourself if the information you have collected actually means anything.'[263] The actual activity of science, claims Medawar (consciously following Popper) is of hypothesising (i.e., 'discussion') *before* experiment, and this non-inductive process is obscured by the rhetoric of the scientific paper.

In *Laboratory Life* (1979), Bruno Latour and Steve Woolgar characterize the experimental paper as an *a posteriori* rationalisation of the real process. 'Not only do scientists' statements create problems for historical elucidation,'

[260] Bazerman, C. (1988) p. 47; 'Among School Children' by W. B. Yeats
[261] Locke, D. (1992) p. 117
[262] Medawar, P. (1963) p. 228
[263] Medawar, P. (1963) p. 229

they write, 'they also systematically conceal the nature of the activity which typically gives rise to their research reports.'[264] There is, then, a serious problem for the would-be filmmaker interested in representing science by adapting the idea of science he or she finds expressed by the structure of the scientific paper. In an essay published a few years after his radio broadcast, Medawar reinforced this view: 'It is no use looking to scientific 'papers' for they not merely conceal, but actively distort the reasoning that goes into the work they describe.'[265] 'The truth,' he writes in a later essay, 'is that there is no such thing as "scientific inference"':

> A scientist commands a dozen different stratagems of inquiry in his approximation to the truth, and of course he has his way of going about things and more or less of the quality often described as 'professionalism'—an address that includes an ability to get on with things, abetted by a sanguine expectation of success and that ability to imagine what the truth might be which Shelley believed to be cognate with a poet's imagination.[266]

That the scientist's approach might be 'cognate with a poet's imagination' is precisely what the scientific paper (and the classic science documentary) is designed to deny. This denial serves a social and political function, writes Ron Curtis: 'by giving the impression that the results presented were not mere opinion or hypothesis but were somehow instilled in the author by nature, the inductive style was intended to silence potential critics and prevent controversy.'[267]

The arrangement of the scientific paper is thus an enactment of an ideological norm by which the results of a laboratory experiment may progress implicitly from the artificial to the natural. 'It is of no consequence that such progress is far from problematic,' writes Alan Gross, 'or that the philosophical bases of this version of the scientific method have been undermined. In experimental reports, arrangement is regarded as a sacred given.'[268]

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[264] Latour, B. and Woolgar, S. (1979) p. 252
[265] Medawar, P. B. (1969) p. 169
[266] Medawar, P. B. (1984) pp. 17-18
[267] Curtis, R. (1994) p. 420
[268] Gross, A. (1990) p. 16

The view that scientific writing is ideological is supported by studies of historical texts whose form is alien to us and therefore easier to see for the rhetoric it is. For example, in their book *Leviathan and the Air Pump* (1985), Stephen Shapin and Simon Schaffer compare Robert Boyle's usage of the dialogue form in *Sceptical Chymist* (1661) to Hobbes's literary practices in the natural philosophical dialogues of *Dialogus physicus* (1661), *Problemata physica* (1662) and the *Decameron physiologicum* (1678).[269] In Boyle's work the reader is presented with four knowledgeable participants who freely exchange facts until they reach a consensus. Hobbes's dialogues, however, are distinctly different. In Hobbes there are only two voices, one represents the author and the other his interlocutor. In these Socratic dialogues the truth does not emerge from an exchange of views but is already fully contained in Hobbes's philosophy; the interlocutor does not offer ideas or information as in Boyle's dialogues but simply receives ideas from the master. The dialogues consist of Hobbes correcting the interlocutor's mistakes or answering those questions that perplex the interlocutor. Sometimes Hobbes probes the interlocutor's terms, demanding clear definitions which the interlocutor admits he lacks. Hobbes then supplies these. The interlocutor may argue a point but only for Hobbes to expose the logical flaw in the argument or the interlocutor may demand that Hobbes support a point with more detail and this Hobbes then supplies. As the dialogues proceed, the interlocutor is gradually persuaded to Hobbes's views; he ceases to represent the adversary and becomes a possible convert. Eventually the conversion is complete but there remains a final step: at the very end of the dialogue, the interlocutor himself has the confidence to correct Hobbes and Hobbes acknowledges his mistake. Thus Hobbes's dialogues demonstrate by their very form that truth arrived at by the correct method always commands assent.[270]

[269] Shapin, S. and Schaffer, S. (1985) pp 143-145
[270] Paul Rotha's multi-voiced documentaries apply this Hobbesian approach. For example, in *New Worlds for Old* (1938) Alistair Cooke speaks the main commentary that structures the diegesis while an alternative voice feeds questions and issues to him. In *World of Plenty* (1943) and in *Land of Promise* (1944-45) the interlocutors are increased in number. In *Land of Promise*, for example, there are 'The voice' (John Mills) which presents the main thrust of the argument, as Hobbes does in his dialogues, but there are other voices that interrupt and challenge: 'Know-all', 'Observer', 'History', 'The Woman', 'Hansard', 'The Housewife'.

The Hobbesian dialogue posits a particular relationship between the method for arriving at knowledge and the method for demonstrating it. In short, one teaches or demonstrates by guiding the student along the same track that one followed oneself in inventing (Hobbes's word) the knowledge in the first place. Hobbes's dialogues are designed to demonstrate that method alone commands assent and thereby mobilizes consensus. Boyle's view by contrast represents the scientific method as a process of cooperative fact sharing. 'Thus,' conclude Shapin and Schaffer, 'in both Boyle's and Hobbes's writings, literary structure and process dramatize the social relations and practices deemed appropriate to the production of knowledge. Differences in theories of knowledge-production and evaluation are displayed in different literary technologies.'[271]

Ron Curtis's analysis of the writing in the popular journal put out by the *American Association for the Advancement of Science* reveals that such discourse promotes a received view about knowledge production and evaluation. Taking a lead from the implicitly inductive structure of technical scientific papers, many journalists and authors of popular science writing, writes Curtis, 'affect a naïve realism, and pretend that their subject already exists in the form of a story … which they simply stumble upon and report to the reader'. The narratives of popular scientific discourse, thereby 'moralize surreptitiously about events in science while purporting, in accordance with a positivist ethic and a naïve realism, merely to describe them.'[272] The reader (or viewer) of such discourse is interpellated into a normative view of science as a detective-trail that is immune to criticism because it is never explicit.

As Roland Barthes writes, rhetoric is the signifying aspect of ideology and this detective-trail rhetoric signifies the familiar, received view of science.[273] It satisfies a deep-seated psychological need to believe in the realism of theories and a logic of theory choice that, in their turn, underpin confidence in the unity of science and its institutional structures. The historian of science, Frederick Holmes summarizes this view: (1) '[Scientific papers] are retrospective formulations of work previously completed'; (2) 'They do not accurately represent the work they make public'; (3) 'They are stereotyped

[271] *Ibid*., p. 145
[272] Ron Curtis. (1994), p. 425
[273] Barthes, R. (1977) p. 32 ff

according to canons of form dictated by the authority structure of scientific disciplines'; (4) 'They purvey an image of scientific activity that fits an ideology rather than actual practice'.[274]

This does not mean that 'non-received' narratives of science like the one I am trying to develop in this thesis are any less ideological. Holmes's point is that all narratives of science are necessarily imaginative constructions guided by certain principles or themata. Holmes's own work on scientific records, for example, is guided by the spatial metaphors of 'investigative *pathways*' or 'research *trails*' and the '*fine structure* of scientific activity' and he writes: 'the historical narratives that I, or others with other guiding ideas, can produce from such records are imaginative constructions, just as are the scientific writings that the scientists who kept the records produce from them.'[275] All scientific discourse is figural or 'rhetorical' but this does not invalidate it as a record of scientific activity, it merely confirms that scientific discourse is, whatever Huxley says, necessarily a species of literature. As Hayden White writes in *Figural Realism* (1999), 'the very distinction between literal and figurative speech is a purely conventional(ist) distinction and is to be understood by its relevance to the sociopolitical context in which it arises.'[276]

Figuration in written discourse

It would seem, then, that scientific discourse is always something more than 'plain' speaking and we may expect this characteristic to extend to the representations of science found in the science documentary. It is not such a surprising idea, for the only way to describe something unfamiliar is by reference to the familiar. The use of metaphor in science communication, then, is unavoidable. 'Science cannot do without these 'semantically bizarre sentences'', writes Gross; 'it is universally recognized that metaphor is indispensible.'[277] (This is the 'warping' of scientific language that Quine speaks of.) The scientist should therefore expect new research to be replete with metaphors; indeed, many metaphors remain embedded in scientific knowledge

[274] Holmes, F. L. (1987) p. 220
[275] *Ibid.*, p. 235
[276] White, H. (1999) p. vii
[277] Gross, A. (1990) p. 80

even as it develops away from the research front. David Locke gives the following example:

> When scientists appropriate terms like *force*, *energy* and *power*, like *inertia* and *momentum*, and invest them with their own specific meaning, they are simply inserting the old signifiers into new systems of signification. … When subatomic physicists wish to distinguish a family of entities by a variable property that has no counterpart in the world of ordinary dimension, they designate the various state of the property as 'flavors,' just as Baskin-Robbins does the varying states of its product. One can never signify the new, the unfamiliar, without reference to the old, the familiar, the comfortable. … There is always in scientific change a reassemblage, a bricolage, a repackaging of always old ingredients.[278]

Saussure demonstrates that language is not a simple naming of pre-existent things and scientific language is no different. Science does not have, as Scott Montgomery writes, 'the power to change language into a form of technology, i.e., a device able to transfer knowledge without ever touching it in any way. … Language is indelibly a cultural phenomenon, and science, in its major portion, is no less so.'[279] Metaphor is not simply a means for scientists to distinguish new objects; such language also *suggests* new ideas. Pasteur and Koch's recommendation of the germ theory of disease—'*contagium vivum*'—fostered militaristic metaphors so directing research towards, among other things, mechanisms of 'invasion' and 'resistance'.[280] This lead to the notion of self-non-self recognition by an adaptive immune system (in a battle, opposing forces must wear distinct uniforms) and, a century later, Peter Medawar would receive a Nobel Prize for his work on this idea.[281] At the same time, these metaphors of warfare stifled research into symbiosis, an area of investigation that identified bacteria not as *contagium vivum* but as organisms with a natural

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[278] Locke, D. (1992) pp. 170-171
[279] Montgomery, S.(1999) p. 33
[280] 'The History of the Germ Theory,' *The British Medical Journal* vol. 1 no. 1415 (1888), p.312.
[281] In 1959 Burnet and Medawar proposed a 'Self-Non-self Model' for regulation of the immune response in which each lymphocyte was viewed as having a single receptor that would react with a specific antigen or specific antigens. In this model, self-reactive lymphocytes are deleted at an early stage so preventing autoimmunity. Burnet and Medawar shared the Nobel Prize in 1960 for their work on these mechanisms. In 1994 Polly Matzinger proposed the 'Danger Model' of immune response in which the system reacts to stress not to the discovery of invaders. Matzinger keeps sheep and has dogs which guard them and this she says gave her the idea that certain cells spot danger signals (which might, but might not, be caused by non-self invaders).

history of their own and relations that might be other than pathogenic.[282] In this way the metaphor directs *future* research as well as characterising the immediate phenomenon.

Figures of speech exert a profound influence on science, indeed the figure known as metonymy is at the heart of statements of causality. While metaphor means ‘transfer’ and makes the contents of experience intelligible by characterizing phenomena on the basis of their similarity, metonymy literally means ‘name change’. In this trope a thing may be renamed by reference to an essential part of it; for example the entire person and apparatus of the ruler may be renamed ‘the throne’. In metonymy ‘the throne’ is not transferred, it does not stand in a relation of similarity to the ruler but rather it is a part, a contiguous element of the ruler that it stands in for. One calls psychiatrists the ‘white coats’ for example. Metaphor is *representational*—two objects are related by being figuratively identified—but if we think about phenomena metonymically, we distinguish those parts that are representative of the whole from those that are merely aspects of it.

Thus, for example, ‘the cold of ice’ is a metonymical expression. In this phrase the feeling of, say, an ice cube in our mouth is divided into two phenomena, the cause (the ice) and the effect (the cold). Having made this division one then relates the two phenomena in a cause-effect relationship: the ice causes the cold. Now this relationship may in fact be expressed in one of two ways: the effect’s cause (‘the cold of ice’) and in an agent-act relationship (‘the ice cools’). As Hayden White writes:

> By such reductions…the phenomenal world can be populated with a host of agents and agencies that are presumed to exist *behind* it. Once the world of phenomena is separated into two orders of being (agents and causes on the one hand, acts and effects on the other) the primitive consciousness is endowed, by *purely linguistic means alone*, with the conceptual categories (agents, causes, spirits, essences) necessary for the theology, science, and philosophy of civilized reflection.[283]

In creating agents and causes, metonymy is the trope of mechanistic discourse.

[282] Sapp, J. (1994)
[283] White, H. (1973) p. 35 (emphasis in original)

Figuration in the Science Documentary

Felicity Mellor analyses the distinction between metaphor and metonym in her paper, ‘The Politics of Accuracy in Judging Global Warming Films’ (2009). In this paper, she argues that the figural status of the image in science documentaries dealing with future events such as global warming (or, I would add, past events such as evolutionary history) is more often metaphorical than metonymic. This is because, in making images of future events—through computer animations for example—the filmmaker is working in the ‘subjunctive’ mode.[284] In this mode, writes Mellor, the documentary image necessarily ‘establishes a relationship of similarity rather than of contiguity’.[285] In a science documentary that pictures the future, this use of metaphor becomes problematic because it makes it difficult for the viewer to read the image as they are used to, namely as having a metonymic relationship to reality. The consequent figural ambiguity means that viewers may experience difficulties in adjudicating between rival representations of the future on the basis solely of scientific ‘accuracy’. This contributes to confusion over claims about global warming.

It is interesting in this regard to recall the images in the BBC’s evolution series, *Walking With Dinosaurs* (1999). The scenes in this series were almost entirely computer generated animations of dinosaurs; however, they were designed to be perceived like an Attenborough-style nature programme—as if a human camera operator had been present on Earth at the time. For example, at one moment the ‘camera’ appears to duck to avoid the swinging tail of a dinosaur. In *Walking With Dinosaurs* the style of the animation aims at metonymy, at relating one shot to the next in spatiotemporal contiguity. This conceit (borrowed from the syntax of the observational nature documentary) avoids, locally, the potential for ambiguity that Mellor speaks of even though globally the entire series is, tropologically speaking, metaphorical, or, we might argue, ironic, for in irony, explains Hayden White, ‘entities can be characterized

[284] See Woolf, M. J. P. (1999)
[285] Mellor, F. (2009) p. 146

by way of negating on the figurative level what is positively affirmed on the literal level.' [286]

Certainly, in the light of the theory of evolution, the style of *Walking With Dinosaurs* is absurd, not so very different from *King Kong* (1933) in which a giant gorilla wrestles with a *Tyrannosaurus Rex* while observed by a woman. However, irony presupposes, as White says, that the viewer 'already knows, or is capable of recognizing, the absurdity of the characterisation of the thing designated in the metaphor, metonymy or synecdoche used to give form to it.'[287] The viewer of *Walking With Dinosaurs* picks up on the documentary's irony (the irony of it being a documentary at all) and reads the film as a heuristic fiction, a game of speculation with a non-fictive address.

In *Walking with Dinosaurs*, or in Chris Marker's *Sans Soleil* (1983) or Peter Watkins's The *War Game* (1965), a conditional mode is adopted in which, as Michael Renov writes, 'the depiction of potential rather than experientially available worlds is faithfully (or whimsically) rendered'.[288] By contrast, in Kevin Macdonald's documentary, *Touching the Void* (2003), we have a good example of the problems caused for the viewer when the mode is *not* conditional. The film tells the story of mountaineer Joe Simpson's remarkable survival after an accident climbing Siula Grande in Peru in 1985. The text mixes climbing scenes performed by actors in Peru (filmed on the actual spot of the original events) with other shots recreated in the Swiss Alps and narrates the story through interviews with Joe Simpson and his climbing partner, shot in an abstract space rather like that of *The Ghost in Your Genes*. As long as the re-enacted images of Simpson's climbing accident are 'generic' they remain metaphors for his experience but when the actors speak, when 'Joe' gasps with pain for example, what we had engaged with as metaphor is suddenly figured as metonymy and that makes no sense because the implicit contiguity with the post-climb Joe Simpson cannot be. As Nichols put it, 'When an actor reincarnates a historical personage, the actor's very presence testifies to a gap between the text and the life to which it refers.'[289] This gap is traversed by metaphor but not by metonymy. In the documentary, such figuration is troubled

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[286] White, H. (1973) p. 34; See also Metz, A. (2008) on *Walking With Dinosaurs*
[287] White, H. (1973) p. 37
[288] Renov, M. (1993) p. 196 (note 18)
[289] Nichols, B. (1991) p. 249

by the clash between conflicting criteria of authenticity. When the actor playing Joe cries out in feigned agony his voice acts not as a metaphor for Joe's pain but as a metonym for his own performance and by that token fails to persuade, undermining the effectiveness of the re-enactments. The more the re-enactments comply with the conventions of the realist cinema the less effective they are as metaphor.

The mixed mode of *Touching the Void* also has impact in the other direction, weakening the reality claim of the interviews. The 'Joe Simpson' of the interviews must conform to the *Joe Simpson* of the re-enactments (i.e., a mythic fiction) which means denying his own historical agency as the 'Joe Simpson' we see interviewed. In the 'making-of' documentary, *Touching the Void*: *Return to Siula Grande* (2004), Simpson reveals the confusion and pain he suffered in returning to the scene of his accident but none of this is articulated in *Touching the Void*. Unlike the interviews of holocaust survivors in Claude Lanzmann's *Shoah* (1985), for example, Simpson's role in *Touching the Void* is entirely constrained by the film's mythic narrative which leaves no space for exploring his current state of mind. He is not on screen to testify to the pain of *remembering* but only to relate the pain he remembers. And what he remembers, especially after so many tellings, has become as much of a myth to him as it is for us hearing it for the first time.

On Darwin and progress—a kind of conclusion

The figurative or tropological aspects of scientific discourse and its manifest content are tied up with each other. Scientific language, no matter how 'dry' is more than a mirroring (i.e., an icon) of events, it is a mode of *explanation* in which the figurative and the factual are indissoluble. Style—rhetoric—not only affects meaning it creates it. Even in science, our thoughts about the world are shaped by the bricolage of concepts and patterns of discourse we are steeped in and so in an important sense the only 'real' world is the one constructed out of this material. As Hilary Lawson puts it:

> Signs are not transparent; they are not simply marks for something that is wholly other. Put in the language of semiology, there is no signified which is independent of the signifier. There is no realm of meaning which can be isolated from the marks which are used to point to it.

> There is therefore no logos, no unified and coherent account of the world, that lies outside the sign or system of signs, that is independent of the marks by which 'it' is described.[290]

If our world is held in play by a system of signifiers which are not independent of their meanings then there are no fixed meanings, there are in a sense no signifieds but just a system of signifiers from which there is no escape. If a particular signifier fails to have a unique meaning then all meaning is ultimately undecidable. Statements of fact do not refer to some independent entity because we cannot have experience of that entity, nor speak of it outside of thought, of language, of signifying. The raw data are beyond our reach. It is not so much that one should doubt the existence of the external world as grasp that the scientific language that would describe the world is unavoidably caught up in its own linguistic universe. The meaning of a sentence is determined by the play that takes place within that web of language. Experience cannot stand outside that play.[291] It is not, in other words, that science does not *work*, only that one need not, after all, accept that the scientist's 'real world' is the real world.[292] The scientist is perfectly within his rights to assume whatever he wants in order to do his job (to be a motivational realist for example) but one is not compelled in any logical sense to go along with those assumptions.

The Cartesian paradigm of the received view holds nature to be bifurcated, the mind of the scientist separate from that which is observed, the form of our representations separate from their content. But in a post-Cartesian, constructivist paradigm, form and content are inextricably bound up. Analysis of a text means its deconstruction or 'desedimentation' and rather than revealing its 'true' meaning can only serve to reveal the necessarily linguistic nature of

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[290] Lawson, H. (1985) p. 98

[291] The idea that language is constitutive of thought, if not of experience itself, has a long history but I am particularly indebted to Bruner and Lacariello (2006) who build on Lev Vygotsky's *Thought and Language* (1962) to argue that, for the child, language begins as a tool of social interaction which the child then uses to direct itself in making sense of its world (monologuing as 'thinking aloud'). Bruner and Lucariello emphasise that the child's external monologuing takes a narrative form. Vygotzky theorises that such 'thinking aloud' gradually becomes internalised 'thought' that, in its high degree of compression, hardly resembles the language it used to be. Nonetheless, it is at root language that is socially constructed.

[292] This position collapses the distinction between science and technology that I describe in chapter one as central to the Enlightenment project: 'Whereas technology seeks to control the environment, science aims at the opposite—to be controlled by it.'

knowledge, the never-ending series of discourses from which knowledge is constructed, as David Locke writes:

> By its 'desedimentation,' the deconstructionist disentangles the assemblage and reveals the problematic nature of its formulation. When the task is complete, the deconstructed language lives on, its terms, however, now held 'under erasure,' altered in their signification by the deconstructive process itself.[293]

The Kuhnian model of scientific history is one of deconstruction—periods of 'normal' scientific activity separated by 'revolutions' during which fundamentals are interrogated in such a way that terms no longer mean what they did: the current paradigm is thus recast or even destroyed. Darwin's great book, *On the Origin of Species*, for example, is a profound work of deconstruction. As David Locke points out, the title itself is ironic because by the end of the book the notion of species has been entirely undermined and what remains of it is not the sort of a thing that can be said to have clear origins.[294] Darwin achieves this new sense by inverting the old hierarchy. The fixed species 'type' of Linnaeus becomes, under Darwin's view, an arbitrary stopping place in a more or less infinitely graduated series of individuals. The decision of which individuals to designate the 'type specimen' of the 'species' and which mere 'variety' is, as Darwin writes, 'arbitrarily given, for the sake of convenience, to a set of individuals closely resembling each other, and … does not essentially differ from the term variety,' which in its turn is arbitrarily assigned to emphasise relatively larger variations than between individuals.[295] This inversion of the commonly held idea of species as something fixed, clearly definable (and God-given), is achieved at the expense of two chapters (sixty pages) of examples of disagreements between botanists about what is or is not a variety or a species. Once the notion of species has been deconstructed in this way we can see how the origin of a species cannot easily be assigned either. Origin cannot make much sense when a species is but an arbitrarily chosen moment in a process of continual change: there is no natural place to draw the line between ancestor and descendant. Darwin's book then is not about the

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[293] Locke, D. (1992) p. 172
[294] *Ibid.,* pp 177 ff
[295] Darwin, C. (1964) p. 52

origin of species but about a process of continual change. When Darwin uses the word species he is using the term under erasure: ~~species~~.

Looking at the language of evolution today we find that Darwin's discourse has itself been reconstructed and deconstructed many times since 1859. For instance the so-called 'modern synthesis' of Darwinism and Mendelism which was developed throughout the 1930s and 40s deconstructs Darwin's genetics, re-reading or mis-reading the original text in such a way as to provide an explanation for the persistence of phenotypic changes that Darwin failed to make convincing in *Origin*. Then there are more recent re-readings: population genetics (it is populations not individuals that are selected by nature); molecular biology (genes are lengths of DNA, chemicals that can be altered by random events); epigenetics (heritable changes in gene *expression* can be caused by mechanisms other than changes in the underlying chemical composition of the DNA sequence); horizontal or lateral gene transfer (significantly large pieces of DNA representing many genes can enter a genome from an unrelated (i.e., only very distantly related) organism; endosymbiosis (whole genomes are transferred horizontally between microbes); hybridogenesis (whole genomes of multicellular animals are horizontally transferred). Each of these re- or mis-readings adds further layers on top of Darwin's discourse.

It is this many-layered, sedimentary formation of the language of evolution (for example) that constitutes our current picture of the real world. We read the top layer as reality but this process of sedimentation will surely continue so that the final content of science is continually deferred, temporally extended and never fully reified. Scientific discourse is an ever-changing structure that forever deconstructs and reconstitutes the world. As David Locke puts it:

> The world is always real…but it is also always an invention because it is seen through the medium of thought…The world is ever a story, and science is but one of the stories of the world. This is the great dialectic of the word and the world. The word is in the world, but the world is in the word; the word is of the world, and the world is of the word; indeed the word *is* the world, and the world *is* the word.[296]

[296] Locke, D. (1992) p. 199

This notion must affect our idea of progress in science. The experimental report satisfies, as Gross puts it, 'a recurrent need to justify the enterprise of experimental science in the face of the problematic nature of the inductions on which that science relies for the creation and certainty of its knowledge.'[297] In Lévi-Straussian terms such narratives of science are myths; they are linguistic vehicles for resolving a deep contradiction within scientific culture, namely that between the certain knowledge that science seeks and the sense experience that it must use as a basis or a test of that certainty and that cannot, I have argued, be so used.[298] The myth of induction instantiated in the arrangement of the scientific paper is designed to cope with a contradiction that cannot be overcome. Although each scientific paper exhibits what Gross calls 'terminological stability, the *sine qua non* of certain knowledge,' and thus assures us of an indissoluble link between sense experience and the transsensual world, this stability is only local. The history of science as represented by the totality of scientific papers—the entirety of the discourse of science—'exhibits terminological instability, the *sine qua non* of opinion.'[299]

This contradiction is inherent in Kuhn's distinction between normal and revolutionary science. Just as the myth of induction acts to resolve the contradiction between objective certainty and subjective experience so too a myth that science progresses ever closer to the truth resolves the contradiction between the local coherence *within* a paradigm and the conceptual gaps *between* paradigms. Extraordinary research that on occasion precipitates a scientific revolution—a paradigm shift—cannot be thought of as progressive in the same sense that the work of normal science is. Of course the winners in a revolution must claim their victory is progress but along with victory goes the destruction of the old paradigm: Newton's light travels through the aether; but there is no aether. Cuvier's species are fixed; but there are no species. There are losses and gains in scientific revolutions. The traditional view is that scientific progress is evolution-toward-what-we-wish-to-know but Kuhn would have us understand it as evolution-*from*-what-we-do-know.[300]

[297] Gross, A. (1990) p. 86
[298] Levi-Strauss (1963); See Chapter Three of this thesis
[299] Gross, A. (1990) p. 96
[300] Kuhn, T. (1996) p. 171

The vanquishing of previous paradigms also means their expulsion from the textbooks and so, argues Kuhn, the scientist, trained from an early age by such books, has a very distorted view of his own discipline's past. 'More than the practitioners of other creative fields, he comes to see it as leading in a straight line to the discipline's present vantage. In short, he comes to see it as progress. No alternative is available to him while he remains in the field.'[301] This myth of progress then is reflected in the traditional form of the science documentary that I would wish to subvert in my attempt to fashion a constructivist account. It remains to explore the extent to which *Hopeful Monsters* does indeed reflect the critique I have tried to mount over the last three chapters and thereby subvert the received view.

[301] *Ibid.,* p. 167

CHAPTER SIX

THE STYLE OF *HOPEFUL MONSTERS: AN EXPERIMENT* [302]

Having laid out the case for a constructivist view of science and demonstrated the rhetorical nature of scientific discourse, I now wish to revisit my own documentary, *Hopeful Monsters: an Experiment*, in light of these ideas. On the face of it at least, one could describe *Hopeful Monsters* as 'odd' for a science documentary, both in style and in narrative structure. As we shall see, *Hopeful Monsters* draws on a number of documentary modes that, in combination with an unusual narrative structure, foreground the nature of the film's construction. In this chapter I will analyse the style of *Hopeful Monsters* and then turn, in the following chapter, to its narrative structure.

Introduction

To the positivist of the received view, talk of 'style' in documentary ought to be as troubling as speaking of rhetoric in scientific discourse. The documentary is surely 'documentary' precisely because it lacks style, it is a mirror held up to reality, plain prose not poetry. But the fact is, there are different styles of documentary and this serves to remind us that the documentary film is a discourse like any other. Style or figuration is, as we have seen, an unavoidable characteristic of discourse, mediating, as Hayden White describes, between the poles of 'poetry' and 'prose':

> …stylistics must seek to analyze the poetic dimension in every merely putatively *prose* discourse, just as it must seek to uncover the prosaic kernel of "message" contained in every manifestly *poetic* utterance. This conflation of the prosaic and the poetic within a general theory of discourse has important implications for our understanding of what is involved in those fields of study which…seek to be "objective" and "realistic" in their representations of the world but which, by virtue of the unacknowledged *poetic* element in their discourse, hide their own "subjectivity" and "culture-boundedness" from themselves.[303]

Perhaps the most significant difference between the 'objective' style of a classic science documentary like *The Ghost in Your Genes* and the style of *Hopeful*

[302] A full transcript of the film is available in Appendix 2
[303] White, H. (1975) p. 52

Monsters is that the former is marked, as we have noted, by a disembodied, narrating voice that organises all the images while the latter eschews such a voice. The documentary theorist, Bill Nichols terms this style of the classic science documentary the 'expository mode' while *Hopeful Monsters* may at times be called 'observational', 'interactive', or 'reflexive' but each of these styles (whose characteristics we will explore presently) is nonetheless a style of *documentary* and we must acknowledge the significance of that possibility.[304]

As the duck-rabbit figure demonstrates (see above, p. 82), representation rests on 'guided perception'. Even the most seemingly prosaic representation of reality has, according to Hayden White, a poetical 'understructure' that guides our perception.[305] The sense we make of a particular documentary is therefore determined as much by its style as by the logic of whatever argument the film offers as an explanation of reality. Style and the 'reality effect' of the documentary film are bound up together and in this way the documentary attitude toward knowledge is embodied in its aesthetics.

In his book *For Documentary* (1999), Dai Vaughan relates an anecdote that illustrates how this plays out in practice. He describes a discussion he once had about editing an ethnographic film. The film was to include a scene of female circumcision but when the ethnographers in the field had wanted to record the event they had been barred from entering the hut where the operation was taking place. All they could do was film the people waiting outside. The problem, then, was how to represent the circumcision. One editor argued they should lay the sound of a scream over the image of the outside of the hut so that the viewer might at least gain the idea that the surgery going on inside was painful. Another remarked that they had in fact recorded a scream during the operation so they could use that but a third editor argued that screaming was highly unusual so, even though there had been a scream, it would be misleading to include it. 'What is significant about these three views,' writes Vaughan, 'is that they reflect three distinct assumptions about the claim documentary stakes upon the world: in the first case, symbolic (a scream stands for pain); in the second, referential (this is what our equipment actually recorded); in the third,

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[304] Nichols, B. (1991) Chapter II and Nichols, B. (2001) Chapter 6
[305] *Ibid.*, p. 53

generalisatory (to include the atypical is misleading).'[306] There is no such thing as 'the scene itself' to dictate the right answer. In the documentary, an aesthetic decision becomes an ideological decision because it guides the perception of the viewer who takes the documentary image for 'reality'. As Dai Vaughan puts it:

> Stated at its simplest: the documentary response is one in which the image is perceived as signifying what it appears to record; a documentary film is one which seeks, by whatever means, to elicit this response; and the documentary movement is the history of the strategies which have been adopted to this end.[307]

In its short history, the documentary has developed a number of strategies for creating this reality effect; indeed, as Michael Renov writes, 'the documentary has availed itself of nearly every constructive device known to fiction and has employed virtually every register of cinematic syntax in the process.'[308] Brian Winston gives several examples from the early days of documentary filmmaking in which reconstructions of events were used in ways that would surely be unacceptable as realism today: the Vitagraph newsreels of the 'Battle of Santiago Bay' filmed on a tabletop using cut-out models of ships in 1898 and the Boxer Rebellion restaged on a Philadelphia roof in 1900.[309] That the conventions of realism have changed over time goes to underscore the constructivist idea that the documentary persists, as the philosopher Richard Wollheim argues, in an incorrigibly aesthetic condition that is permanently at cognitive risk through changes of culture, convention and perception.[310]

Just as the meaning or content of a scientific theory has the character of an institution in that it relies for its existence on scientists' moment-by-moment continuing belief in it, so too the documentary is an institution in that it performs only by virtue of being recognized as 'documentary'. The difference between the documentary and the fiction film lies, then, simply in the moment-by-moment continuing belief of the audience in the reality of the events depicted and this belief is sustained by those predictable stylistic strategies or 'modes' that have evolved under the selective pressure of changes in perception and

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[306] Vaughan (1999) p. xiv
[307] *Op Cit.,* p. 58
[308] Renov, M. (1993) p. 6
[309] Winston, B. 1995) pp. 120-121
[310] Wollheim, R. (1980) p. 183

culture. The documentary can be understood therefore as functioning only intermittently, raising an inherently constructivist question: not *what* is documentary, but *when* is documentary? For example, in the case of 'Le Jardinier et le petit espiegle', the short Lumière Brothers film of the gardener and the boy, we saw that what was once meant as a vaudeville sketch—a 'fiction'—may today appear as 'documentary' while the reverse is the case with a documentary newsreel of the same period, such as 'The Battle of Santiago Bay'.

In light of this idea, Nichols understands the documentary modes as species of the same genus that may be arranged into a kind of a phylogenetic tree according to their order of evolution. One of the earlier modes was the 'expository' which sought to address issues in the historical world directly but whose didactic, authoritarian style eventually became less acceptable to audiences, giving rise to a new, more 'open' mode: the 'observational'. The perceived short-comings of the observational mode (its lack of historical context, for example) gave way to the 'interactive' mode which in turn fell short in being too intrusive and relying too heavily on witnesses and was superseded by the most recent mode, the 'reflexive'.[311] The first three modes have definable characteristics that will be discussed later but the last, the 'reflexive', rather like irony, which Hayden White characterises as 'metatropical', is metamodal in that it does not offer a worldview of its own but instead acts to place quotation marks around the other modes, drawing our attention to how they work and inviting us to see how they construct their implicit claims. However, as Bill Nichols concedes, all modes were, in principle, available from the start and one must beware, as Carl Plantinga urges, the implicit teleology of such a 'phylogeny' that holds the expository mode to be 'the most naïve or politically retrograde' and the reflexive the most sophisticated and politically advanced.[312]

The driver of this evolution of documentary styles is trust. In viewing a documentary in the appropriate way, i.e., as a representation of *reality*, the viewer is entering a trusting relationship with the filmmaker and changes in

[311] Nichols, B. (1991); Nichols, B. (2001) (I have excluded the poetic and the performative modes for the sake of simplicity.)
[312] Plantinga, C. (1997) p. 101

style have been driven by the imperative of maintaining that trust. This raises the question of how we-as-viewers are able to enter that relationship, how, in other words, we come to understand the *intentions* of the filmmaker.

Authorship and Tradition

In many critical circles, talking about intentions, and the success of an author or artist in meeting those intentions, is not only out-dated but out of bounds. William Wimsatt and Monroe Beardsley famously coined the phrase 'the intentional fallacy' in the 1940s, arguing that, 'the design or intention of the author is neither available nor desirable as a standard for judging the success of a work.'[313] But the philosopher, Noël Carroll disagrees. The artwork, he argues, is an artefact that is intended to *do* something and should be evaluated as such.[314] The Wimsatt and Beardsley model of narration—'a communication with no communicator—indeed, a creation with no creator,' as Seymour Chatman puts it, cannot fully explain how we engage with works of art and evaluate them because there are aspects of an artwork, for example its innovativeness or its historical influence that we value highly but which are not directly part of our immediate experience.[315] Even works that rely on aleatoric effects like Jackson Pollack's drips or Francis Bacon's flung smears or John Cage's 4' 33" of silence are nonetheless clearly *intended* to make use of chance effects and can be evaluated in that light.[316] Most importantly, science documentaries (like scientific theories) are created within or against traditions (e.g., Nichols's 'modes') that provide exemplars. If we assume that filmmakers intend to communicate with their audience then it follows that any similarity to an exemplar or mode in their work is intended to cue the receiver to associate that work with others in its tradition. Documentaries we watch resemble others we have already viewed and we evaluate each (and come to trust what we see)

[313] Wimsatt, Jr. W.K., and Beardsley, M. C. (1954) p. 3. The phrase 'intentional fallacy' was first coined by Wimsatt and Beardsley in their entry on the subject for the 1943 *Dictionary of World Literature,* Joseph T. Shipley, (ed.), New York: Philosophical Library pp. 326-29.
[314] Carroll, N. (2009) p. 64
[315] Chatman, S. (1990) p. 127
[316] *Ibid.,* p. 66; Cage's 1952 musical composition is a piece in three movements that lasts 4' 33" during which not a note is played. The composition consists, therefore, of the sounds the audience hears from the environment around them.

according to how it matches the conventions of its mode or the rest of a filmmaker's oeuvre.

For Chatman then, the viewer of a film is not constructing the narrative from unauthored stimuli (as David Bordwell argues[317]) but necessarily *re*constructing it according to cues and constraints that are understood to originate from an organising authority. By ascribing the inventional tasks to this trustworthy organising authority, we are able to both evaluate the skill of the filmmaker (which will affect our understanding of what we are viewing) and to interpret inflections in the stylistic conventions we have learned to expect.[318]

Arguing that intentions may, in this way, be available to the viewer does not necessarily reduce all criticism to 'biography', as Wimsatt and Beardsley suggest.[319] We grasp the intended meaning of the observational documentary quite differently to the meaning intended by an expository documentary because in each case we recognise a particular discursive style that embodies a distinct attitude to knowledge. For example, when the filmmaker, Jill Godmilow was asked what enabled her to produce the innovations of her film *Far from Poland* (1984) she replied: 'When I started that film, I had no intention of 'expanding vocabulary' or any such thing. I had to learn to make that film by making it and trying to solve the paradoxical documentary issues it presented.'[320] The paradoxical issues arose because she wished to make a documentary about Poland but was barred from entering the country. As she describes, the process of making the film clarified her attitude to the documentary and this epistemological position is embodied in her finished film. This is no different to, say, Cezanne's experience of grappling with certain problems of representing space that are embodied in his paintings of the Montagne Sainte-Victoire. Even if Godmilow and Cezanne are unable to say why they finally settled on the finished forms of their works there is no reason to think they were not acting intentionally in a way that a critic might go on to explain solely through an examination of their works since these artists have obviously ratified them.

[317] Bordwell, D. (1985) p. 113
[318] R.G. Colingwood, in *The Principles of Art* (1938) understands art as expression and the spectator of art as grasping the work of art by reconstructing the experience the artist went through in making it in the first place.
[319] Wimsatt, Jr. W.K., and Beardsley, M. C. (1954) p. 21
[320] Godmilow, J. (1997) p. 88 (my emphasis)

Like Saussure's *parole*, each documentary film is an instance of the *langue* of documentary. Each film repeats or inflects a tradition and that is *how* it communicates. It is this that creates the struggles of the 'avant-garde' whose works are at the leading edge of formal innovation and furthest from those traditional norms with which we are more familiar. As Samuel Beckett writes in *Worstward Ho*: 'Ever tried. Ever failed. No matter. Try again. Fail again. Fail better.'[321] But to speak of failure at all and especially of failing 'better' means to take a measure of the distance between intention and achievement. Even if the artist of the avant-garde says, 'I don't know why it works but it does' this indicates that there is a goal he or she evaluates has been reached and that goal may be discerned and attributed to the artist by an examination of the artwork itself.

Tradition, then, embodied in the documentary modes, gives the viewer and critic an important fix on the filmmaker's attitude to knowledge. The question for us is, what attitude does the maker of *Hopeful Monsters* intend to articulate and how well does he do so?

The Expository mode

As we found in our analysis of *The Ghost in Your Genes*, the classic science documentary features a voice-over as well as interviews and a variety of other images and sounds—features noted by Gardner and Young as keys to the expository mode:

> The course of the programme alternates between voice-over and 'talking head'. A talking head is television's way of saying 'this is brought directly to you without distortion or mediation'. In the case of science programmes this form of presentation is usually reinforced by racks of test tubes or an impressive piece of apparatus directly behind the talking head, a white lab coat or other apparel, and the knowledge that we are being addressed by 'the top man (sic) in the field' or the 'rising star'. The talking head is either directly addressing the camera or speaking across camera to an unseen interviewer whose questions have been edited out. This is in striking contrast with interviews on programmes where it is accepted that the issue is controversial and open, to some minimal degree at least, to public scrutiny, doubt, debate, etc.[322]

[321] Beckett, S. (1983)
[322] Gardner, C. and R. M. Young (1981) p. 177

As we have seen, the expository documentary, with its anonymous narration, reflects the passive, objective voice of scientific discourse and is strongly aligned with the received view of science. As Edward Branigan writes, the voice-over narration of the classic documentary 'asserts (usually implicitly) a power to know through access to a privileged method or technology'.[323] It is a voice, as Kevin Beattie puts it, 'removed from the fallibility of the human sphere.'[324]

In 'The Consequences of Genre' (1996), the philosopher, Frederick Ruf terms this expository voice 'magisterial' in that its relationship to the persons, events, objects narrated is *external*: 'The narrator sees actions, events and objects from without … The narrator is *master* of the events, persons, objects and their meaning.'[325] Carl Plantinga calls the mode 'formal' and argues that it has two operations: it poses clear questions and it answers them, 'reserving for itself a high degree of epistemic authority.'[326] The expository mode is something like the voice of the Enlightenment, demonstrating to the viewer, as Isaiah Berlin puts it, 'that all genuine questions can be answered, that if a question cannot be answered it is not a question,' and the effect, as Gardner and Young describe, is 'hegemonic'[327] :

> …in the precise sense that it induces deference and organises consent by eliciting willingness to be the passive recipient of versions of history organised and presented for our edification. Patient, restrained, conveying in some cases real enthusiasm, but never shrill.[328]

The film historian, David Pearson concurs:

> Whereas in narrative cinema the diegesis is furthered primarily through the image track as an autonomous, spatio-temporal universe, in [the expository] documentary the diegesis exists as a conceptual universe, dependent not so much upon the illusions projected by the film's images, but rather upon the rhetoric of the commentary and the illusion that creates. Thus the commentary becomes crucial in ideologically fixing the spectator-subject, in locating the individual's relationship to the conditions of existence haphazardly represented in the images.

[323] Branigan, E. (1992) p. 206
[324] Beattie, K. (2008) p. 12
[325] Ruf, F. (1996) p. 803
[326] Plantinga, C. (1997) p. 107
[327] See Chapter One.
[328] *Op. Cit.* p. 177

> Contradictions are resolved for the spectator, who is thus encouraged to settle into a passive position of acceptance.[329]

For the magisterial voice of the expository mode to appear to have all the answers means, as Pearson puts it, that 'the *way* the message is communicated can become more important in documentary film than the message itself.'[330] The medium is the message and the viewer of the expository form is interpellated (no doubt, willingly) into the rational, progressive and certain world that science apparently offers. Many science documentaries therefore stress their expository mode of address above all else. All subjects are presented under the same rubric, just as the Methods-Results-Discussion format of the scientific paper treats all scientific work the same way. The epistemology of the science documentary is thus self-reinforcing and all science documentaries have come to look and sound more and more alike. Any challenge to this form threatens to undermine the ideology of the received view that the mode embodies.

In being external to and 'above' the diegesis, the voice-over of the expository documentary organizes all the images of the film, and because the narrator's voice must be heard and the meaning of the narration takes priority, the film's images are often stripped of their own synchronous sound and spatiotemporal continuity. *Hopeful Monsters*, by contrast, emphasizes the continuity of its images, preserves their synchronized sound and eschews the use of voice-over. Its so-called 'observational' mode productively exploits, as Kevin Beattie puts it, 'the knowledge and pleasure (knowledge as pleasure) located in showing.' It thus offers the viewer an entirely different experience to the expository mode: the open-ended process of interpretation and epistemic *diffidence* that arises from a showing rather than a telling and this represents a challenge to the hegemonic voice of the expository mode and to the received view that that mode embodies.[331]

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[329] Pearson, D. (1982) p. 71
[330] *Ibid*., (my emphasis)
[331] Beattie, K. (2008) p. 13

The Observational Mode

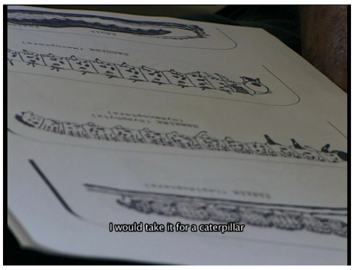
In her analysis of narration, Mieke Bal distinguishes between the narrator and the focalisor. The narrator 'tells' the story but the focalisor *sees* it, 'colouring', as Bal puts it, the fabula through his or her perception.[332] In Bal's terms, then, the 'expository' mode of *The Ghost in Your Genes* is characterised by an absence of 'internal focalisation' in that the gaze of the camera is not aligned with any of the characters in the diegesis but instead with the omniscient narrator who is external to the screen-world. In the observational mode of *Hopeful Monsters*, speech is overheard and the action is focalised internally by human agents and divided into 'scenes' that are spatiotemporally and therefore 'dramatically' coherent. Thus, where the expository mode is 'presentational', addressing the viewer directly, the observational mode is 'representational', addressing the viewer only indirectly.

In the absence of presentation (i.e., of commentary) the viewer overhears, as in the theatre. The voice of the observational mode is therefore multiple, like the voices of a play in which, as Frederick Ruf puts it, 'the innermost self, in fact, is hidden from view, compared with the penetrations of [magisterial] narrative.'[333] Plantinga describes this 'dramatic' voice as 'open': 'The open voice *observes* and *explores* rather than explains,' 'it is more hesitant in its epistemological position, and at times opposes the dissemination of knowledge within a clear-cut conventional framework.'[334]

The scene in which Williamson is interviewed by the journalist (00:16:22) illustrates the distinction between the expository and observational modes. It begins with a sequence of four shots: (1) A girl looks off screen at (2) the page of a book spread on Williamson's knee; (3) the journalist leans over to study it while (4) Williamson, looking down, explains its significance:

[332] Bal, M. (1997) p. 22
[333] Ruf, F., (1996) p. 807
[334] Plantinga, C. (1997) pp. 108 and 115

1. 

2. 

3. 

4. 

WILLIAMSON:
That looks like a caterpillar. I would take it for a caterpillar, but it is the larva of a wood wasp, which is nothing like a butterfly or a moth.
In fact, the wood wasp is related to the stinging wasp and bees and wasps, and bees and wasps have grubs, not caterpillars.

REPORTER:
Yeah.

WILLIAMSON:
So we get these forms turning up in different groups. Under conventional theory, this is quite inexplicable, if the larva and the adult evolved together.

I puzzled over this for years and years and years and I used to lecture to students and point out there's several other anomalies, like this, of species apparently having the wrong larvae which couldn't adequately be explained.

In the expository mode, this scene might well have been replaced with a straight forwardly didactic (and externally narrated) explanation, (perhaps like the animated genetics lesson in *The Ghost in Your Genes*), but in *Hopeful Monsters* something quite different takes place. For a start, although the material is apparently 'introductory', the scene begins a full sixteen minutes into the film and then, when we are given this introduction to Williamson's hypothesis, we 'overhear' it in the form of an 'internally focalised' conversation. As Plantinga writes, the open voice is signalled by an avoidance of 'the overt narrational marks and knowledge claims' of the formal voice. Both the focalisation and the narrative timing of the scene indicate that it is not intended to be read as strictly didactic.

First, the scientific ideas in the scene are not presented with the authority of the magisterial voice; instead, any claim to epistemic authority is deliberately weakened by the use of a dramatic presentation. Second, although we do get to know them, the scene is not, anyway, a didactic presentation of Williamson's scientific ideas but a *drama* in which he explains them and expresses his feelings about them:

First, we witness Williamson's confidence:

> "I'm not blaming Darwin; it was just the knowledge of his day. He was explaining the evolution of adults. I am trying to explain the evolution of larvae."

Followed swiftly by his sense of dejection:

> "Well, the majority of biologists ignore my views, but there is a minority that support them, and some very enthusiastically, and I'm very grateful to them…"

Then, his determination to carry on:

> "I am satisfied in my own mind, but other people aren't, so what we want is more genuine hybrids. And Robert and I are trying to produce some right now."

And finally an expression of humility when the reporter thanks him, saying "Well, I hope one day to be able to say I've met someone whose theories were as important as Darwin's," and Williamson replies, "Well, I don't expect you will…mine is a PS to Darwin."[335]

What then is the purpose of this formal strategy? What does it indicate about the filmmaker's attitude to knowledge? The scene may be understood as a synecdoche of scientific communication in the constructivist view: if Williamson can convince the reporter, and if the reporter does a good job in convincing his readers, and if those readers are interested in the subject and care to look into it further (if perhaps one or two readers are interested scientists),

[335] Dialogue from ca. 00:24:30-00:26:30

then there may be the beginnings of a process of conversion to an unorthodox view about evolution.[336]

But is this scene an adequate representation of constructivism and is the observational mode really up to the task? The observational mode purports to be an 'open' record that leaves the viewer free to make up their own mind. But, of course, this is a rhetorical effect achieved by using conventions familiar from the fiction film, for example, the convention of the point-of-view shot by which the image is aligned with the focalisation of one or other character in the diegesis. This is a common trope in the history of the observational mode. Recall, for instance, the sequence in Frederick Wiseman's *High School* (1968) when the school monitor apparently spies on girls in gym class through a window in the gymnasium door; or the sequence of a monk feeding his cats in Philip Gröning's observational documentary, *Into Great Silence* (2005) about life in a French Carthusian Monastery:

Or this, from *Hopeful Monsters* (00:44:24):

The point-of-view is a central trope of psychological realism (i.e., of the fiction film) and, like the fiction film, in the observational documentary while *focalisation* may be internal and character-bound, *narration* remains external. The camera is 'on the scene' in the observational mode but, crucially, never *in*

[336] Plantinga, C. (1997) p. 108

the scene and so the narrator remains an omniscient 'fly on the wall'. It is important to recognise how indebted this makes the observational mode not just to conventions of the fiction film but to the positivism of the received view of science. In both the expository and observational modes the narrator is rhetorically absent from the diegesis. The scene with the journalist is represented as 'objective', a record of an overheard conversation externally narrated by the same omniscient story-teller whose magisterial voice we hear in the expository mode. In both these modes, the filmmaker, to use the historiographer Herbert Butterfield's phrase, 'whittles himself down to a mere transparency', apparently simply transcribing information 'with colourless, passionless impartiality.'[337] And so, despite the differences between the formal and open voices, both the expository and the observational modes, in the final analysis, align with a traditional, received view of knowledge in which, as Emile Durkheim puts it, truth is understood:

> as a simple thing, a thing quasi-divine, that draws its whole value from itself. Since it is seen as sufficient unto itself, it is necessarily placed above human life. It cannot conform to the demands of circumstances and differing temperaments. It is valid by itself and is good with an absolute goodness. It does not exist for our sake, but for its own. Its role is to let itself be contemplated. It is so to speak deified; it becomes the object of a real cult. This is still Plato's conception. It extends to the faculty by means of which we attain truth, that is, reason. Reason serves to explain things to us, but, in this conception, itself remains unexplained.[338]

Creativity

The styles of both the observational and the expository modes represent not only the same epistemology but also represent similar ideas about creativity. The art theorist, Adrian Stokes makes a useful distinction in this regard between two modes of artistry that he calls carving and modelling. The distinction is not unlike the one Dudley Andrews makes between intersecting and borrowing, designating two very different ways of thinking about what the artist does.[339] Richard Wollheim summarizes the idea in his introduction to Stokes's book on the art of Michelangelo:

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[337] Butterfield, H. (1931) p. 91
[338] Durkheim, E. (1983) p. 66
[339] Andrews, D. (1984)

> A work in the carving mode exhibits a distinctive 'out-therenesss', or independence from the spectator, while the forms of which it is composed blend into an unassertive, an uncompetitive, harmony. By contrast, a work in the modelling mode tends to envelop, or merge with, the spectator, while the forms that make it up are set over against one another, and can be reconciled only in an arbitrary, or what Stokes calls a 'masterful', way. [340]

Psychologically speaking, in carving the artist assumes that the block of stone contains within itself the form invented for it by nature and the artist simply liberates that form (like 'intersecting'). In modelling, on the other hand, the artist gives the stone his own truth and the truth of the stone as a different truth is not acknowledged (as in 'borrowing'). Stokes's distinction raises the question of how our beliefs about creativity might affect our experience of the documentary film.

If we believe the documentary film is carved, that in some sense it is 'found' and separated from its dross by the objective reporter, then we will have one experience; if, on the other hand, we believe it is modelled such that its coherence is due to a masterful technique—in short, that it is an *assertion*—we will have a different experience. In carving, truth and beauty are eternal objects that we locate and reveal while in modelling they are artefacts whose fundamental design we create and continually update.

The observational mode encourages us to believe in carving because it appeals to our experience of separateness: on screen is a record of what is out there in the world and the filmmaker simply reveals it to us. In its carved completeness this world also appears to be one we could enter ourselves. In the opening scene of *Hopeful Monsters*, for instance, we have the impression we are in the room with Williamson as he shuffles across to put on a video and that we might almost reach out and help him back into his chair. But a moment later, as he watches television, he smiles towards 'us' and we, of course, cannot acknowledge this look (00:02:00)

[340] Stokes, A. (2002) p. xv

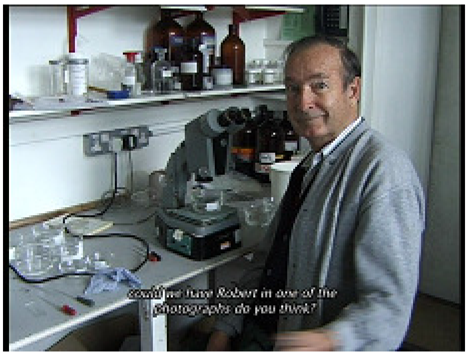
We come up against a serious ethical problem at the heart of the observational style: to allow the viewer to feel that a direct encounter is possible, the filmmaker who was actually there and able to make such a connection must withhold that possibility, withhold, that is, his humanity from his subject. The observational mode stakes its authority on the reality of the act of filming (of carving) but it pictures the world as if this has no tangible effect. The making of an observational documentary therefore constitutes an epistemological and ethical problem that its appearance of candour does not act to resolve.

One solution to this problem is for the observational documentary to 'come clean' about this 'carved' illusion it creates. This was the aim, for example, of Jean Rouch and Edgar Morin in making *Chronicle of a Summer* (1960). In that documentary they chose to discuss, on camera, the problem that being filmed posed for their social actors. In doing so they created a film text that acknowledged the extent to which the film was the result of collaboration between filmmakers and social actors. *Chronicle of a Summer* does not pretend to be a found object but instead acknowledges, within the text itself, its constructed nature. It is a *modelled* film that arises and is given coherence by the collaboration between those behind and those in front of the lens. Nichols calls this the 'participatory' or the 'interactive' mode.

The Interactive mode—pseudo-modelling

The contrast between the observational and the interactive modes becomes very evident in the scene in which Don is photographed for the article that the reporter will later write (00:14:13):

The otherwise observational mode of the scene is disrupted when both the photographer and Williamson acknowledge their participation with the camera. Photographer: “Could Robert be in one of the photographs do you think?”



This relationship is reinforced when the operator behind the camera steps into the frame to join the other two, becoming, as Nichols terms it, a ‘social actor’ just like them. Robert: “I’m totally incidental to this process…I’m merely a pair of hands.”

The photographer photographing is a trope from the early days of the observational mode. In Robert Drew’s *Primary* (1960) for instance, we view would-be presidential candidate John F. Kennedy being seated and posed for a publicity still. He looks stiff and uncomfortable. By contrast, the film suggests, he is unselfconscious in front of the documentary camera that records the scene. He is unselfconscious because, as we have noted, in the observational mode the narrator is external, in fact, rhetorically speaking, absent from the space; the events we see are apparently self-organising (carved) and speak for themselves. The same thing happens near the start of Donald Pennebaker's, *Don't Look Back* (1966) when a press photographer invites Joan Baez to pose for him. She mugs at his camera and then says, "I can't pose". The photographer scene from *Hopeful Monsters* makes reference to these well-known moments but with the opposite effect. Instead of reassuring the viewer of the transparency of the act of filming, the switch of mode from observational to interactive exposes the conceit of the invisibility and impartiality of the fly-on-the-wall.

By recording the interaction between the filmmaker and his subjects, the interactive mode of *Hopeful Monsters* goes some way towards articulating a constructivist epistemology. The mode lays stress on the dynamics of testimony and the social context within which witnesses speak and the filmmaker films. The rhetoric of the interactive mode represents the film as arising out of a process of exchange between filmmaker and social actor with the film posited as a record of its own process of construction. When the filmmaker questions the witnesses on screen, the viewer may judge the nature of those questions (their tone and fairness) and therefore also form an opinion about the validity or doubtfulness of the answer. In addition, this open sharing of the filmmaking process means that the authority of the text shifts from the filmmaker towards the social actors who may take the scene in an unexpected direction.

A clear example of this comes from the 'limerick' scene in *Hopeful Monsters* when Don is effectively ambushed by Robert and made to read the mocking limerick from his American critic, Richard Strathmann. When Williamson finishes reading it and has defended his ideas against Strathmann's criticism, Robert walks away to resume his work. As he passes Williamson, the man smiles gently before getting slowly to his feet and, excusing himself politely, leaves the lab, terminating the scene and preventing Robert from filming any further. Is he hurt, does he feel betrayed? The filmmaker has lost control (01:17:00):

In the interactive mode, then, authority is explicitly ceded, *on screen*, to the social actors. In his essay, 'Beyond Observational Cinema' (1975), the ethnographer, David MacDougal praises this approach because:

> By revealing his role, the filmmaker enhances the value of his material as evidence. By entering actively into the world of his subjects, he can provoke a greater flow of information about them. By giving them access to the film, he makes possible the corrections, additions, and illuminations that only their response to the material can elicit. [341]

But, in describing the mode as providing 'evidence', MacDougal suggests that the process of scene-construction we witness takes place entirely *within* a higher-level frame which remains invisible. In other words, the interactive mode is still a mode of carving, the scene is constructed by the interaction of filmmaker and his subjects but that process is seemingly recorded 'objectively' (thus providing 'evidence'). The mode represents its scenes as having a 'natural' form independent of the filmmaker's determinations. In this sense, then, the idea that the interactive mode is fundamentally distinct from the observational is undermined. As Stella Bruzzi writes in *New Documentary* (2000), 'purity in this context is unobtainable, there are always too many other issues spoiling the communion between subject and viewer across a transparent screen.'[342]

The rhetoric of the interactive mode does acknowledge the extent to which the meaning we make of the documentary results from the intervention that filmmaking necessarily requires but absent from that rhetoric is any hint that the screen, as Bruzzi rightly implies, is far from transparent. The interactive mode does not acknowledge an important aspect of the documentary experience,

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[341] *Op Cit.*(my emphasis)
[342] Bruzzi, S. (2000) p. 70

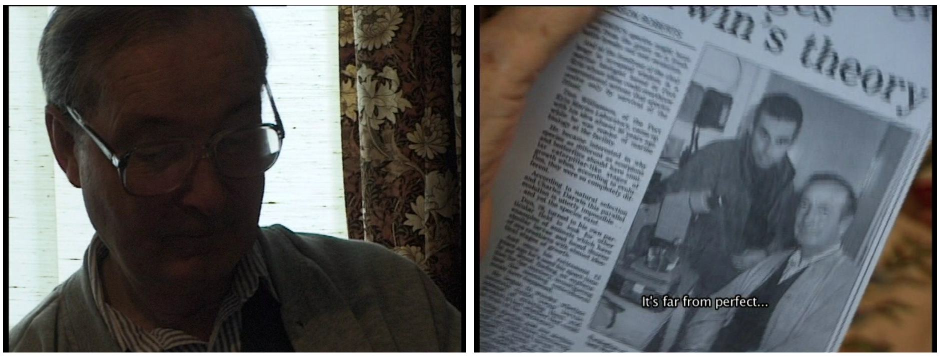
namely that the *viewer* is seated before an opaque screen, not a transparent window, and that the shadows that play on that screen are the achievement of the filmmaker who has ordered the film's rushes at the editing bench according to what Herbert Butterfield calls his or her 'imaginative sympathy':

> The historian is not merely the observer ... the historian is something more than the mere passive external spectator. Something more is necessary if only to enable him to seize the significant detail and discern the sympathies between events and find the facts that hang together. By imaginative sympathy he makes the past intelligible to the present.[343]

In the end, the interactive mode does not offer a comprehensive alternative to the positivism of the observational mode and it cannot be a model for representing science from a fully constructivist point of view. But is *Hopeful Monsters* really an example of the interactive mode?

The Reflexive mode—modelling proper

From the start of the film, and increasingly as it goes on, the viewer's attention is drawn to various combinations of narration and focalisation that cannot be fully understood as carving in either the observational or interactive modes. Consider the following moments:



(1) Character-bound narrator ('Robert'); character-bound focalisor (Don) (01:10:00)

[343] Butterfield, H. (1931) pp. 91-92

(2) External narrator (*Robert*); character-bound focalisor (Don) (01:01:40)

(3) External narrator (*Robert*); character-bound focalisor (‘Robert’) (00:44:24)

(4) External narrator (*Robert*); external focalisor (*ROBERT*) (00:50:24)

The implicitly character-bound focalisor of the observational mode becomes explicit in the interactive mode. In *Hopeful Monsters* it is ‘Robert’, the filmmaker/lab assistant whom we see and hear on screen. The interactive mode identifies ‘Robert’, the character-bound focalisor on screen with *Robert*, the character-bound narrator. His is both the literal and the figurative points-of view of the film. But this identification breaks down in the moments above and particularly when Robert’s focalisation is narrated externally as in sequences 2, 3 and 4. A ‘fictive’ syntax asserts itself at such moments (although it has been implicit almost from the beginning). At such moments, the film posits an impossible identity between *Robert*, the character-bound narrator and *ROBERT,* the external narrator or 'Robert' the character-bound focalisor and 'ROBERT' the external focalisor. This confusion of narrators and focalisors is not just complex, it is incoherent unless we switch our understanding of the nature of documentary filmmaking, unless, that is, we acknowledge, as John Corner puts it, the 'art of record' that is documentary production. These 'impossible' combinations (when conceived as 'record') make it impossible for the viewer to forget that they are viewing not something carved from the world 'out there' but something modelled—a *text*. In this way the film's form progressively acknowledges the higher level framing that the interactive obfuscates. In short, the film displays stylistic features of what Bill Nichols terms the reflexive mode.[344]

Space and Time

Mieke Bal's distinction between 'place' and 'space' is helpful in grasping how the reflexive mode places its 'quotation marks' around the observational and interactive styles.[345] 'Place' is *where* the story happens—in a lab on the Isle of Man, or a flat in London—while 'space' is an achievement of the modelling in rendering that place three dimensional according to certain filmic conventions.

In the expository mode, 'place' may be clearly communicated but a perception of 'space' or spatiotemporal continuity is minimal or lacking altogether: 'images are wrenched and torn from all manner of locations as example, model, and evidence,' writes Bill Nichols.[346] By contrast, in the observational and interactive modes, the representation of space and time as continuous is definitive. In the observational mode in particular, logicality arises from the apparent spatiotemporal continuity of shots because spatiotemporal continuity is the trope of metonymy—it demonstrates *causality*. As in the fiction film, the rhetoric of the observational mode persuades the viewer that, *within* a scene, screen time equals story-time. However, whereas in the fiction film the 'cuts' between shots do not cut out time, they merely

[344] See Nichols, B. (1991) p. 69ff
[345] Bal, M. (1997) p. 133
[346] Nichols, B. (1993) p. 183

reposition the view-point, in the observational documentary, which is posited as the *continuous record* of a fly's point of view, any cut *whatsoever* implies a gap, a discontinuity in space and time.

And yet, such cuts are commonplace. Without them, of course, time could not be compressed and controlled and all observational documentaries would be as long, uneventful and undramatic as CCTV images. Most scenes in the observational mode necessarily comprise a congeries of discrete elements but these elements are joined in sequence to give the impression that they are spatially and temporally contiguous. The purpose of this, as Dai Vaughan puts it, 'is to enable the character of film as record to survive, so far as is possible, its metamorphosis into language.'[347] But this aesthetic, as Calvin Pryluck says, has ethical consequences which the reflexive mode brings to our attention.[348]

Consider, for example, the following moment from an early scene in *Hopeful Monsters*: Don reaches for a syringe, explaining as he does so that finding a place to insert the needle is "very much trial and error…find a soft bit…" This moment is represented by two different shots, call them *A* and *B*; A is the action of Don's hand and *B* of his face. In the sequence, *A* is divided by *B* into two parts (00:04:30):

A *B* *A*

This type of parallel construction is common throughout the film but how are we to understand what it signifies about the pro-filmic, what indeed does it *record*? There is a range of possibilities:

1. Two cameras were used, one to capture shot *A* and the other for *B* and the continuity of action in the sequence is genuinely synchronous.

[347] Vaughan, D. (1990) p. 55
[348] Pryluck, C. in Rosenthal, A. (1988) p. 256

2. Both shots *A* and *B* are synchronous, created with one camera that tilted up to the face and down again to the hand. The tilting movement was removed in the edited scene and cuts in the sound smoothed over.

3. Shot *A* records what happened at the time but shot *B* is a 'cutaway' taken from the same context at an earlier or later time. Or, vice versa, shot *B* is continuous with the rest of scene two and *A* is a cutaway.

4. The cutaway (whether *A* or *B*) comes from a dissimilar context, perhaps another day, and/or another space.

5. The cutaway (whether *A* or *B*) was re-enacted after the event by Don.

6. The cutaway (*A*) was performed by an actor with a hand that looked similar to Don's.

This discontinuous construction masquerading as continuity is typical of the aesthetics of the observational mode (As Stella Bruzzi put it: 'what matters above all else is that a sequence of shots *appears* to be logical, not necessarily that it *is*.'[349]) But it raises a question: is this a *record* of a moment or a more or less generalised construct? The answer changes as we descend the list of possibilities from 1 to 6. Possibility 1 represents a record of 'Williamson injecting a starfish'; possibility 4 on the other hand is not what we might wish to call a record; it can be paraphrased as 'this is the *type* of activity Williamson does'. In possibility 6, where 'Williamson' is partially performed by an actor, the sequence means 'this is the type of activity this type of man (i.e., a scientist) does'. As Dai Vaughan points out, if we select interpretations from the top of the list we are in danger of being cheated, if from the lower part we lose the particularity of the scene. As we descend the list of optional readings the scene shifts from representing the record of a particular, contingent event to representing a repeatable, bowdlerised event-of-a-certain-kind. As Vaughan

[349] Bruzzi, S. (2000) p. 71 (her emphases)

concludes: '…the aspect of film as record, with its implication of uniqueness and contingency, dwindles into insignificance and the particular becomes only an exemplar of the abstraction it articulates.'[350]

The list is analogous to what I have described as nominalisation or what Jonathan Potter, in his book, *Representing Reality* (1996) calls 'a hierarchy of modalization':

> The process of fact construction is one of attempting to reify descriptions as solid and literal. The opposite process of deconstruction is one of attempting to ironize description as partial, interested, or defective in some other way.[351]

'Statements' in the documentary, like statements in science form a similar hierarchy that undercuts their putative positivism. In Vaughan's terms, the pro-filmic X is unavoidably 'linguified' (i.e., becoming 'X') in the process of being represented:

X
X is a fact
I know that X
I claim that X
I believe that X
I hypothesise that X
I think that X
I guess that X
X is possible
'X' [352]

While the expository, observational and interactive modes (like the discourse of science itself) obfuscate the moral dilemma this hierarchy poses, the reflexive mode brings it into the open, ironizing description.

Throughout *Hopeful Monsters*, then, the deliberate incoherency of focalisors and narrators ironizes description, creating a growing apprehension of the film's constructedness; a growing apprehension of the problem (of trust) that documentary filmmaking poses not just for those 'in' the film but for those 'outside' of it—the viewers. This becomes fully evident in the scene in the

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[350] Vaughan, D. (1999) p. 69.
[351] Potter, J (1996) pp. 112-113
[352] *Ibid* p. 112

London flat when ‘Robert’, seated at a table at the end of a corridor, talks on the phone to ‘Lynn’ (00:56:26):

Lynn: …and he said, ‘I’m looking out the window and I’m looking at…’ what is it, the Irish Sea, is that what you look at?

Robert (looks out of window): You do, yeah.

Until now, Robert has been represented both on the screen and behind the lens but if this phone call sequence is to be understood as a record of a continuous, profilmic event, then the multi-shot coverage of the scene serves to ironize and draw attention to that conceit. If the scene is to be read as ‘documentary’ then it forces the viewer to reconsider what may be meant by that term. Reflexivity is signalled by the scene’s impossible continuity that includes the sudden appearance of the Irish Sea outside the window of a city apartment. The constructedness of the scene is made still more explicit by the convention, familiar from fiction films, of hearing what ‘Robert’ apparently hears—Lynn’s voice on the other end of the line—and by the abrupt termination of the phone call once its ‘information’ has been served up.

The reflexive style of the scene breaks with the shibboleths of the observational documentary, demonstrating the conventionality of documentary realism. By thus drawing attention to the discursive nature of the documentary, the illusion of unmediated access to an independently existing world is retarded.

Thus reflexivity lifts the documentary experience, as Dudley Andrews puts it, 'from the obsessions of the imaginary to the realm of symbolic exchange'.[353] The fact of reconstruction dashes our illusions, but with a purpose: to 'confound any simple sense of truth, any reassurance that things are, indeed, just as they seem…to make us see our relation to the world anew through the experience of form…'[354] Such discourse, argues Terry Eagleton, 'estranges or alienates…but in doing so, paradoxically, brings us into a fuller, more intimate possession of experience':

> If a story breaks off and begins again, switches constantly from one narrative level to another and delays its climax to keep us in suspense, we become freshly conscious of how it is constructed at the same time as our engagement with it may be intensified.[355]

By deviating from the familiar norms of documentary representation, the reflexive mode reveals the techniques of the documentary as ideological. The mode awakens the viewer to an apprehension of the limitations of the documentary in representing the full range of determinations that make up the history that the documentary purports to simply 'record'. In psychoanalytic terms, the reflexive mode exposes the realist effect as predicated on the viewer's disavowal of their own experience. Like all modes of documentary, the reflexive invites the viewer to look and to believe their eyes but unlike the other modes it seeks, at the same time, to persuade them that seeing must not be believing.

By highlighting the conventionality of its representation of space and time, the film's reflexive style also undermines the illusion of access to the interiority of characters, revealing how interiority itself is a function of the formal strategies of the film, not their cause. Because of the reflexivity of the text, the viewer is decentred, prevented from easy identification with the subjectivities apparently offered in the film and in this way the authorial forces *exterior* to the characters may be felt and acknowledged. As 'Robert' watches the interviews he has done in the past and takes notes for a film he will make in

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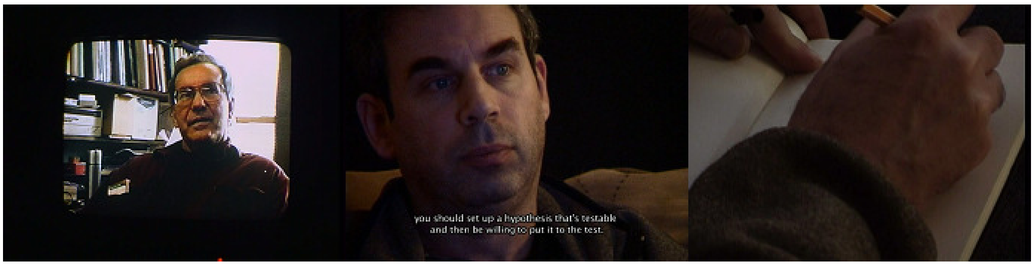
353 Andrews, D. (1984) p. 152
354 Nichols, B. (1991) p. 240
355 Eagleton, T. (1983) p. 4

the future (the film, *Hopeful Monsters*), we apparently have access to what he is thinking (01:01:00):



At the same time, we recognise that this effect is a consequence of the subjectivizing strategies of the film that are borrowed from the conventions of fiction. In short, the psychological unity of the character called 'Robert' is a result, not a cause, of the film's rhetoric. 'The body,' writes Jean-Louis Comolli, is 'an *empty mask*…the character will only appear later and bit by bit *as effects* of this mask, effects in the plural, changing, unstable, never quite achieved, thwarted, incomplete.'[356] The text is not a record of *Robert* making a film but a story about 'Robert' who is an unstable effect of the text.

The reflexive documentary makes this mythologizing process explicit, drawing attention to how the film constructs its own authority rather than being an agent of a pre-existing one. As we saw in our brief analysis of *Touching the Void* in the last chapter, it is the unavoidable fate of the historical person who consents to being filmed that they become a signifier in another's discourse and so, just as Robert and Williamson are mythic figures constructed by the text of *Hopeful Monsters*, so too are all the other 'characters' in the film, including the ill-fated hybrid larva. By making us aware of the film as a model, a construct, so too does the reflexive mode invite the viewer to recognise that scientific knowledge is also made not found. The film's reflexive style implicitly argues that truth is not an eternal object that we locate and reveal by *method*, carving it free of its dross but an artefact whose fundamental design we model and continually update within what Foucault calls *regimes* of truth:

> Each society has its regime of truth, "its general politics" of truth: that is, the types of discourses which it accepts and makes function as true; the

[356] Comolli, J-L (1977) p. 43 (original emphasis)

> mechanisms and instances which enable one to distinguish true and false statements, the means by which each is sanctioned; the techniques and procedures accorded value in the acquisition of truth; the status of those charged with saying what counts as true.[357]

This political conception of truth represented in *Hopeful Monsters* is described as 'pragmatic' or 'soft' by Emil Durkheim, a conception of truth consonant with a constructivist epistemology:

> It is placed in the series of facts, at the very heart of things having antecedents and consequences. It poses problems: we are authorised to ask ourselves where it comes from, what good it is and so on. It becomes itself an object of knowledge. Herein lies the interest of the pragmatist enterprise: we can see it as an effort to understand truth and reason themselves, to restore to them their human interest, to make of them human things that derive from temporal causes and give rise to temporal consequences. To 'soften' truth is to make it into something that can be analysed and explained.[358]

Conclusion

The expository and reflexive modes promote distinctly different ideas of truth. In the case of the expository documentary (e.g., *The Ghost in Your Genes*) the implicit epistemology is a somewhat rigid positivism whereas in the reflexive documentary (e.g., *Hopeful Monsters*) it is a pragmatic, social constructivism. The reflexivity of *Hopeful Monsters* encourages the viewer to lift their eyes from the immediate view and recognise the film's overall pattern. As in Brechtian theatre, we are made aware of forces at work in excess to those of the individual characters in our narrative. A certain distance between the viewer and these characters is thus established, replaced by a connection between the viewer and the film as a composition, a construct and an invention of the filmmaker operating within a regime of truth that includes the viewer—what Brecht calls an 'apparatus' of ideology.[359] In short, the reflexive mode undermines the realist pretensions of the documentary project by pointing out the ideological nature of all discourse, including its own reflexive discourse.

From the point of view of positivism's rhetoric, the reflexive mode seems pointless self-sabotage, problematizing a harmless illusion and making it

[357] Foucault, M. (1970) p. 73
[358] Durkheim, E. (1983) p. 67
[359] Brecht, B. (1964) pp. 34-35

harder for viewers to imaginatively enter the film's diegesis to 'get' the science. But of course, as with Brecht's theatre, the effect is to alert the viewer to the nature of discourse, to emphasise that when we engage with the science documentary or with the more technical texts of science we necessarily face what Hodge and Kress, in their book *Social Semiotics* (1988), call an 'ideological complex':

> a functionally related set of contradictory versions of the world, coercively imposed by one social group on another on behalf of its own distinctive interests or subversively offered by another social group in attempts at resistance in its own interests.'[360]

The expository documentary, with its anonymous, magisterial voice asserting its privileged access to knowledge would fain adjudicate, pass judgement on Williamson's integrity and competence and judge his hypothesis right or wrong. The reflexive style of *Hopeful Monsters* implicitly critiques the expository mode that is intended to justify the rights and privileges of science that exist in our present culture, demonstrating that the options of 'right' or 'wrong' are not 'real' so much as discursively determined. The reflexive mode of *Hopeful Monsters* is implicitly satirical, undercutting the certainties of the expository mode, expressing agnosticism with regard to the truth of the film's realist assertions ('this really happened') and by analogy with regard to the truth of Williamson's hypothesis.

Whereas perfection in the classic documentary is defined by invisibility and obfuscation of the documentary process, the reflexivity of *Hopeful Monsters* inverts this disavowal. If the expository science documentary represents what Charles Bazerman calls 'a Baconian history of the phenomena themselves' then the reflexive science documentary represents 'a history of the natural philosophy embodied' in the form itself.[361]

[360] Hodge and Kress (1988) p.3 cited in Rose, G. (2001) p. 71
[361] Bazerman, C. (1991) p. 29

CHAPTER SEVEN
UNENDED QUEST: THE NARRATIVE OF *HOPEFUL MONSTERS*

Introduction

In this final chapter I aim to compare the sorts of stories of science that *The Ghost in Your Genes* and *Hopeful Monsters* offer the viewer, hoping to demonstrate that these stories are decidedly distinct and that this distinction may be attributable to their differing attitudes to the nature of scientific practice and scientific knowledge. In making this comparison, my structuralist analysis will be informed to a large extent by the work of the historiographer Hayden White, who has devised a rubric for analyzing the basic story patterns or myths that histories commonly articulate. I am, then, interested in how both *Hopeful Monsters* and the classic science documentary transmogrify a given present of scientific activity into a past, and what lessons such histories offer the viewer about the nature of that scientific practice and the security of scientific knowledge.

Perhaps the most obvious difference between the histories of science represented by *Hopeful Monsters* and *The Ghost in Your Genes* is that the latter is a history of success while the former, seen from the perspective of the classic science documentary, is a history of failure. Of course, the received view of the history of science is replete with stories of failure, for example Johann Joachim Becher's development of the phlogiston theory in the seventeenth century or Michelson and Morley's search for the luminiferous aether in the late nineteenth or, as we learnt at school, Lamarck's eighteenth century theory of evolution. However, these failures are usually recounted, at least in textbooks and popular accounts of science, in the context of success: the phlogiston theory is replaced by the concept of oxidation, the absence of an aether wind gives support to Einstein's relativity, Lamarckism gives way to Darwinism.[362] The state of knowledge in the past is assumed by such accounts to have been inadequate or wrong. Failures are remembered in the received view only to show how such mistakes are left behind in the inevitable progress towards the present state of

[362] See, for instance, Bill Bryson's *A Short History of Nearly Everything* (2003)

correct knowledge. Such accounts of science, then, are what Herbert Butterfield terms ‘whiggish’:[363]

> It is part and parcel of the whig interpretation of history that it studies the past with reference to the present; and though there may be a sense in which this is unobjectionable if its implications are carefully considered, and there may be a sense in which it is inescapable, it has often been an obstruction to historical understanding because it has been taken to mean the study of the past with direct and perpetual reference to the present. Through this system of immediate reference to the present day, historical personages can easily and irresistibly be classed into the men who furthered progress and the men who tried to hinder it; so that a handy rule of thumb exists by which the historian can select and reject, and can make his points of emphasis.[364]

According to Butterfield, whiggish histories ‘emphasise certain principles of progress in the past and…produce a story which is the ratification if not the glorification of the present.’[365] As we have seen, the classic science documentary, exemplified by *The Ghost in Your Genes*, does precisely this: it ratifies the science of the past in the sense of representing it as a necessary, if flawed conception that paved the way to the glories of the present. Science is thus represented as a self-correcting *method*, an algorithm by which the continuous march of progress is made possible.

As we found, *The Ghost in Your Genes* communicates this received view in many ways, not least in the sequence that first introduces its chief protagonist, Marcus Pembrey, striding straight ahead across Trafalgar Square (in slow motion) while pigeons scatter and people swerve to avoid him:

(00:05:48)

[363] Named after the political party that perceived itself the inheritor of the Parliamentarian victory in the English Civil War, Whig historians write as if, as defenders of liberty, the Parliamentarians were always bound to defeat the Royalists.
[364] Butterfield, H. (1931) p. 11
[365] *Ibid*., p. v

Pembrey is a man heading in one direction only. By contrast, *Hopeful Monster* first introduces Williamson as a man literally travelling in a circle (00:00:10):

These two sequences encapsulate the narrative distinctions between the two films. Compared to *The Ghost in Your Genes*, the narrative of *Hopeful Monsters* is both more meandering and less resolved, representing science as a process of trying things out, backtracking and digression—far from the focused clarity and directionality that the word 'method' connotes. *Hopeful Monsters* presents a history that is cyclic, i.e., not necessarily progressive, while *The Ghost in Your Genes* represents the history of science as linear, progressing towards ever greater knowledge and understanding.

The cyclic narrative structure of *Hopeful Monsters* alerts us to the whiggishness of our usual assumptions about the history of science (assumptions that are taken for granted in *The Ghost in Your Genes*) for such an incomplete narrative raises the question of how one may distinguish success from failure in the first place. While *The Ghost in Your Genes* gives the 'received' answer to that question ('a successful theory is one that fits the facts'), the incomplete narrative of *Hopeful Monsters* reframes the question to ask not *what* is success but *when* is success. The film is concerned not to demonstrate the truth of Williamson's theory but to represent the process by which it is *made* true—a process that takes time. This distinction only makes sense in a constructivist view of science.

According to the whiggish narrative of *The Ghost in Your Genes*, history is synonymous with progress and so any ambiguity in representing that progress is simply a failure of exposition. The narrative of the classic science documentary therefore records a chronology of 'crucial' experiments or 'breakthroughs' that comprise successful research in the received view. Such a classic narrative leaves largely unrepresented the more routine business of scientific work that is pictured in *Hopeful Monsters*, especially if this work comes to nothing at the end of the day (consider, for example, the non-spawning starfish and dead urchins of the film's first experiment). The classic, diachronic narrative of science may, then, be described as 'thin' compared to the narrative of *Hopeful Monsters*; for it is concerned only to demonstrate the logic of a causal chain rather than the texture of the daily efforts that make up much of scientific research. (Such a narrative is thin too in what it demands of the viewer who is expected to be interested but unsophisticated.)

By contrast, the narrative of knowledge-manufacture that, for example, Karin Knorr-Cetina explicates in *The Manufacture of Knowledge* (1981) and Bruno Latour develops in *Science in Action* (1987) and that *Hopeful Monsters* clearly aims to emulate, is multilayered or what the anthropologist Clifford Geertz calls 'thick'.[366] A 'thick' narrative is one that emphasizes the *synchronic* over the merely chronological, as the cultural theorist Stefan Szczelkun describes:

> A synchronic approach allows us to create a picture of life at a time or place in much finer detail, showing the complexities and essential redundancies. It allows us to include that which is unchanging or mundane but which is essential to character and atmosphere, and so to a fuller understanding. Because it allows more to be included it can be a

[366] See Geertz, C. (1973)

> more democratic approach which can include the texture of lives and processes which are unremarkable by the traditional historical criteria. The unremarkable is also often the typical, that which is held in common.[367]

Of course all narratives, to *be* narratives, are organized as a chronological chain of events but those events may be more or less 'thickly' rendered. To move from the diachronic to the synchronic is to move from a narrative mode to a more descriptive mode. Narrative creates time within time but description creates space within time and so, as one moves from the diachronic to the synchronic one experiences a change in intelligibility. As Hayden White explains, there is consequently a dialectical relationship between information (description) and comprehension:

> The more information we seek to register about any field of occurrence, the less comprehension we can provide for that field; and the more comprehension we claim to offer of it, the less the information covered by the generalizations intended to explain it.[368]

Hopeful Monsters is a relatively thick description of the world, representing the micro-level of the historical field; in the film there is great particularity of description provided by a largely *metonymic* discourse. *The Ghost in Your Genes* is relatively thin as description, representing a macro-level of the historical field in which there is less particularity, scientists are ciphers and images are generic, in short, the film is a highly *metaphoric* discourse.

All discourse necessarily mediates between the poles of metaphor and metonym and the relative weight given to each in a history of science communicates what the author holds to be most significant for explaining that history. For example, in being highly metonymic and showing experimental procedures in more or less 'real time' and in great detail (i.e., in making space for description), the narrative of *Hopeful Monsters* gives greater weight to detailed process and to 'character' than does the metaphoric narrative of *The Ghost in Your Genes* that is determined by 'plot' and rules of inference.[369] Like

[367] Szczelkun, S. (2002) Chapter 10
[368] White, H. (1975) p. 50
[369] A good example of a highly metonymic programme is the BBC series, *Eye on Research*, produced by Aubrey Singer with the Outside Broadcast Unit in the late 50s and early 60s. It was

the structure of the scientific paper or a syllogism, the narrative of *The Ghost in Your Genes* is more generic (as we discussed in the previous chapter) than the narrative of *Hopeful Monsters*.

Metonym or metaphor are figurative choices that face the documentary filmmaker right from the start; for the filmmaker must determine which events to include in the film. The problem with this is that 'the number of details identifiable in any singular event is potentially infinite; and … the 'context' of any singular event is infinitely extensive or at least is not objectively determinable,' writes Hayden White.[370] Events are not 'givens' and so while on one hand documentary narratives point towards the details of the events they describe (metonym), on the other they point towards an explanatory, generic story form (metaphor). This means there can be no truly 'objective' history. There is an unavoidable perspectivalism—a 'Rashomon effect'—to all narratives and so all histories necessarily embody an ideology.[371]

The metaphor of the detective trail that explains the events of *The Ghost in Your Genes* embodies the ideology of the received view. The question for us here is what ideology does the more metonymic narrative of *Hopeful Monsters* embody?

Metahistory

To answer this question I turn to Hayden White's analysis of the historical narrative as first outlined in his book, *Metahistory* (1973) and developed in other books and papers in the years since.[372] White deconstructs the historical narrative into the following levels of conceptualization: (1) chronicle; (2) story; (3) mode of emplotment; (4) mode of argument; and (5) mode of ideological implication. The first elements of the historical field are organized as a chronicle or list of events in temporal sequence but this is not yet a history

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a half-hour *live* programme shot in laboratories around Britain. In 1960 Singer made an episode titled 'Absolute Zero' in which the presenter Raymond Baxter interviewed the scientist Nicholas Kurti who demonstrated and explained his efforts to cool crystals to within a millionth of a degree of absolute zero. Being live, all elements of the episode are contiguous, giving a powerful sense of the physicality and social dynamics of laboratory research.

[370] Quoted in Renov, M. (2004) p. 131

[371] *Rashomon* (1950), directed by Akira Kurosawa, is a fiction film about the apparent rape of a woman and the murder of her husband. The story is told from the perspective of four witnesses (including, through a medium, the dead husband). The stories are mutually incompatible and the viewer is left to decide for himself which if any to believe.

[372] White, H. (1973) and White, H. (1974, 1975, 1985, 1987, 1999)

because chroniclar statements contain no *explanations* of events. In order to provide such explanations the events of chronicle are transformed by characterizing some as ‘inaugural’, some as ‘terminating’ and others as ‘transitional’. When organized in this way the chronicle becomes a ‘story’ with a recognizable morphology that, by enclosing a passage of time explains the ending as a consequence of the beginning.

‘Story’ then is this first (Proppian) level of explanation, answering questions like ‘what happened next?’ or ‘how did it all end up?’ But to grasp the deeper meaning of a particular story, argues White, requires an appreciation of how the story might have been different; requires, that is, a comparison with other stories that might have been crafted from the ‘same’ chroniclar events. I write ‘same’ in inverted commas because, of course, as noted above, we cannot consider chroniclar events as either ‘raw’ or ‘hard’ data. Indeed, simply by designating some events as inaugural and others as terminating, the documentary filmmaker imposes a form on reality that cannot be found in the events themselves. All historians and documentary filmmakers must necessarily fall back on certain principles to guide their choices and we need not even elaborate these principles to recognise that they involve value judgements that cannot come from the level of the chronicle itself. A history then can only be told from a *metahistorical* standpoint.

Hayden White identifies three levels of metahistorical explanation: ‘explanation by emplotment’, ‘explanation by argument’ and ‘explanation by ideological implication’. According to White, historians choose particular options on each of these different levels and in this way, two people may create distinctly different explanations of the ‘same’ events without any ‘objective’ version to adjudicate between them.

Explanation by Emplotment: Tragedy, Comedy, Romance and Satire

Hayden White’s ‘explanation by emplotment’ is the same as Silverstone’s ‘mythic narrative’, the dramatic dimension that plots the hero’s struggle and victory. As White sees it, there are four possible types of plot by which to shape the historical events of the hero’s struggle and success: Romance, Tragedy, Comedy and Satire. To grasp the differences between them it is best to consider each in relation to Romance:

> Romance is fundamentally a drama of self-identification symbolized by the hero's transcendence of the world of experience, his victory over it and his final liberation from it—the sort of drama associated with the Grail legend or the story of the resurrection of Christ in Christian mythology.[373]

In a Romantic mode of emplotment we would expect to find binary oppositions between good and evil, light and darkness, truth and falsehood, failure and success etc. and these are precisely what we do find in the classic science documentary: Romance is ultimately a plot of redemption. Satire is its opposite, a plot that instead demonstrates the slavery of man to his fate which, in the end, is always death.

Comedy and Tragedy are less pessimistic than Satire but less fulsomely triumphal than Romance. 'In Comedy hope is held out for the temporary triumph of man over his world by the prospect of occasional reconciliations of the forces at play in the social and natural worlds.'[374] These reconciliations are symbolized by the festivities traditionally used to terminate comic narratives. In Tragedy, by contrast, there are no festivities at the end; instead there is an intimation of incorrigibly terrible states still existing between people. In Comedy, society is represented as being cleansed by the harmonization of apparently irreconcilable forces. Tragedy, by contrast, acknowledges limits to the degree of harmony that is possible. Its reconciliations are not so much harmonizations as resignations. Both Tragedy and Comedy find the Romantic notion of human redemption naïve and can be understood as *qualifications* of Romance in which the persistence of conflict is not denied. The difference between them is that Comedy sees conflict at least temporarily resolved whereas Tragedy reveals opposing forces to be irreconcilable.

Satire, in contrast to these three plots, is reflexive, including its own model of reality in its critique; for it 'presupposes the ultimate inadequacy of the visions of the world dramatically represented in the genres of Romance, Comedy and Tragedy alike. … Satire paints its 'gray on gray' in the awareness of its own inadequacy as an image of reality.'[375]

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[373] White, H. (1973) p. 8
[374] *Ibid*, p. 9
[375] *Ibid*, p. 10

By this analysis, *The Ghost in Your Genes* is emplotted as Romance while *Hopeful Monsters* appears to be a tragedy. But what does it mean to call a history of science a tragedy? What does that explain?

The History of Science as Tragedy

If Tragedy simply meant a plot of accidental misfortune then, outside of the perhaps pleasurable experience of feeling sympathy, we would probably not be interested in such stories, but a tragic plot offers a *lesson*, as Richard Eldridge puts it in *The Persistence of Romanticism*:

> Tragedies instruct us not only about the occurrence of particular incidents, as a chronicle or list of events might, but further about human life and its liabilities in general. … we can say that tragedy clarifies or illuminates what is pitiable and fearful in human life. It makes clear to us how the human life we share with tragedy's protagonists, with whom we identify, is typically liable to include significant, undeserved, and unanticipatable suffering.[376]

Tragedy is more than a lesson in the insecurity and illusory happiness of the human condition. For a story to be tragic, for it to illuminate what is pitiable and fearful in human suffering and thus bring about catharsis in its audience, the protagonist must suffer from a flaw, the notorious tragic flaw or error that is woven into the hero. Significantly, this flaw is not presented as blameworthy. The hero of a tragedy is always a good character and the disaster that befalls him or her is not the result of blind, natural misfortune, which would clarify nothing for us about the nature of existence, but a result of those very qualities that make the hero good: courage, wisdom, integrity and selflessness to name but a few. The tragedy, then, is that under certain circumstances these qualities may lead to disaster and the plots of tragedies demonstrate how this can happen. The force of Tragedy is that these highest virtues, 'the qualities that are most necessary for any well-led life—themselves can defeat the achievement of their appropriate end, *eudaimonia*, a well-led, happy human life.'[377] Our virtues cannot protect us. Indeed, in so far as certain social structures are themselves predicated on

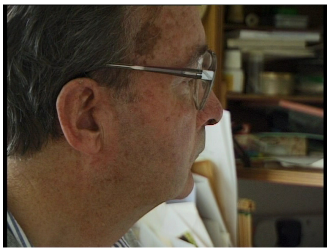
[376] Eldridge, R. (2001) p. 147-148
[377] *Ibid.*, p. 150

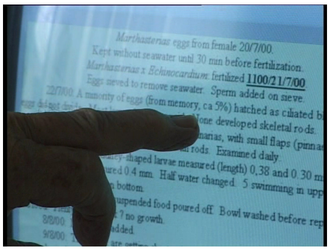
these same virtues, those structures are unstable; for the virtues may, as Eldridge puts it, come to war with themselves.

This internecine warfare, I submit, is what Bruno Latour identifies as characteristic of the social structures of science. Latour's 'science in the making' is a battleground; the weapons with which its warrior-scientists defend one theory or attack another are a series of generally accepted criteria or rules but (and here's where the trouble starts) these rules turn out to be vaguer than we thought and can become a source of potentially *irresolvable* dispute.

Five of these criteria are discussed by Kuhn in *The Essential Tension* (1977): accuracy, consistency, scope, simplicity, fruitfulness. The difficulty, as Kuhn explains, is that (a) the meanings of these words are fluid and open to challenge and (b) even if their meanings were stable, a theory may score highly on one criterion and do poorly on another. Even though these criteria are 'good' they may yet come to war with themselves.

Accuracy, Kuhn avers, is the most decisive of the criteria, 'partly because it is less equivocal than the others but especially because predictive and explanatory powers, which depend on it, are characteristics that scientists are particularly unwilling to give up.'[378] However, in practice accuracy is not always decisive. In *Hopeful Monsters*, for example, Williamson describes what has been happening during Robert's absence (01:08:00):

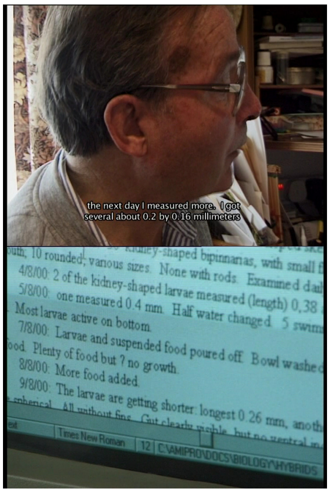




WILLIAMSON:
Well, this is the story so far. By the 2nd of August we had forty larvae, thirty of which were kidney-shaped bipinnarias. That is a normal starfish larva.

I thought they were continuing to grow, but by the 9th of August it became apparent that they were not growing, they were actually getting shorter. And I measured some on the 9th, the longest was .26 millimetres, whereas we'd had one .4 millimetres five days before. And the next day I measured more.

[378] Kuhn, T. (1977) p. 323

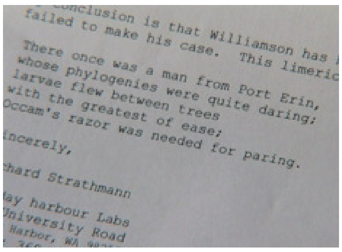


I got several about .2 by .16 millimetres. And others were completely spherical. And though there was food in the gut, the mouth and anus were probably closed. There was no... effectively no mouth or anus.

They are now dying off rapidly. We only had about thirty to start with. We're down to, err. Well, yesterday, the 13th, we were down to two, one of which was swimming, the other was apparently intact, looking like a good larva but no longer swimming."

Williamson implicitly interprets the shortening of the bipinnarias as a result of their hybridicity and therefore the accuracy of his hypothesis (and a justification for making such accurate measurements in the first place). When the one remaining 'hybrid' dies in the following scene the question arises: is this a failure of Williamson's theory? In the 'limerick' scene that follows, the viewer is made aware of another interpretation: the larval 'rounding-off' was a sign of unhealthiness not hybridness. But there is no way, at least as demonstrated in the film, of adjudicating between these two explanations on the basis of 'accuracy'.[379]

Simplicity or parsimony is another criterion of theory choice: the simpler theory is the better one. Strathmann invokes this rule in his limerick (01:17:00):



There once was a man from Port Erin,
whose phylogenies were quite daring;
larvae flew between trees
with the greatest of ease;
Occam's razor was needed for paring.

Williamson defends his position: "Nature with a capital 'N' is not sitting around thinking about the shortest ways to get from A to B. It does it by trial and error,

[379] Recall (p. 98) Lakatos's story of the accurate measurements that led the planetary scientist to posit a mystery planet that yet eluded discovery.

and trial and error seldom produces the shortest way."[380] Parsimony, in Williamson's view, is incompatible with another of Kuhn's criteria: consistency.

Epistemologists of the received view commonly concede that these criteria are vague but argue that they concern only the context of discovery, not the more important context of justification—of testing. As exemplars of objective testing, they point to famous crucial experiments, for example Foucault's pendulum.[381] But, says Kuhn, although Foucault's pendulum may demonstrate that the Earth moves, this so-called test of the hypothesis was only carried out *after* the hypothesis had already been accepted. To consider crucial experiments as the means by which choices are determined in the first place is to mistake the pedagogic context for the context of justification:

> The exemplary crucial experiments to which philosophers again and again refer would have been historically relevant to theory choice only if they had yielded unexpected results. Their use as illustrations provides needed economy to science pedagogy, but they scarcely illuminate the character of the choices that scientists are called upon to make. [382]

The Ghost in Your Genes is just such an economical, didactic narrative of crucial, 'breakthrough' experiments but does that make it an adequate history of science? While, as Henry Kissinger said, history never reveals its alternatives, those alternatives exist in the contemporary moment and choice is neither so unproblematic nor so obvious as the whiggish history of *The Ghost in Your Genes* implies. There are always some good reasons for conflicting choices and this means, as Kuhn points out, that:

> Considerations relevant to the context of discovery are then relevant to the context of justification as well; scientists who share the concerns and sensibilities of the individual who discovers a new theory are ipso facto likely to appear disproportionately frequently among that theory's first supporters.[383]

[380] 01:18:00
[381] Like a gyroscope, the pendulum keeps a fixed direction in space while the Earth rotates under it.
[382] Kuhn, T. (1977) p. 327-328
[383] *Ibid.*, p. 328

Kuhn's evidence comes from his study of the 'Copernican Revolution'. While Copernicus's universe might appear (whiggishly) to be a simpler model than Ptolemy's, it was, in its first description, no more accurate as a means of predicting celestial events. Nonetheless, Kepler adopted the heliocentric theory from early on and backed it strongly during the decades it took for him to adapt the model to be more accurate than Ptolemy's earth-centred system. Had Kepler not had other reasons for choosing Copernicus over Ptolemy the idea of a sun-centred planetary system might have been ignored and perhaps forgotten altogether. Accuracy and simplicity cannot be used as a sole or sufficient criterion of theory choice; indeed, there is no single objective algorithm for theory choice and even if, or when, the scientific community eventually comes to agree on a theory, we cannot be sure that each individual comes to agree for the same reasons.

In *Hopeful Monsters* we find a plot (and thereby an explanation) in which *unresolved* disagreement is central to the scientific method the film depicts. This is what it means to emplot the history of science as Tragedy. The narrative of *Hopeful Monsters* demonstrates that in the history of science the same 'good' values of accuracy, simplicity etc. may come to war with themselves. The criteria that Kuhn invites the reader to consider seem to have the quality of rules but in practice they are more like maxims. Values are shared but this does not imply that they can be objectively applied. We may all agree that a good theory ought to be parsimonious but in an individual case we may disagree about whether or not this *particular* theory is. Scientists know this all too well for they often disagree with each other but their journal articles (and the classic science documentaries that reflect that discourse) deny disagreement any place in scientific *method*. *Hopeful Monsters*, by contrast, makes it *central*.

A History of Science as Tragicomedy

But the plot of *Hopeful Monsters* is not wholly tragic. Williamson does not actually fail: he does not learn 'the error of his ways' or consider his hypothesis falsified by apparently negative experimental results. On the contrary, the film is a story of his determination to keep going *because of* disagreement. In Kuhn's version of the history of science, this is a common pattern:

> …much work, both theoretical and experimental, is ordinarily required before the new theory can display sufficient accuracy and scope to generate widespread conviction. In short, before the group accepts it, a new theory has been tested over time by a number of men, some working within it, others within its traditional rival. Such a mode of development, however, *requires* a decision process which permits rational men to disagree, and such disagreement would be barred by the shared algorithm which philosophers have generally sought. If it were at hand, all conforming scientists would make the same decision at the same time. ... what from one viewpoint may seem the looseness and imperfection of choice criteria conceived as rules may, when the same criteria is seen as values, appear an indispensible means of spreading the risk which the introduction or support of novelty always entails.[384]

In *Hopeful Monsters*, Williamson tries to make the hybrids he theorises. This is because in the Kuhnian/Latourian model of scientific activity, concrete results of a new theory may be *persuasive* to proponents of the opposing, traditional view even if these scientists are not yet willing to concede on the grounds of other criteria. As Williamson explains:

> "I am satisfied in my own mind, but other people aren't, so what we want is more genuine hybrids. And Robert and I are trying to produce some right now." (00:26:10)

Or:

> "The best we can do is show that hybrids between distantly related species are possible, and that some of these turn out to produce recognisable (a) larvae and (b) juveniles. We have been using concentrated sperm and in some cases at least, the barriers between distantly related species seem to be broken down." (01:14:00)

If persuaded that a hybrid can be made, a traditional neo-Darwinian scientist, perhaps even Williamson's enemy, Eric Davidson, may make the effort to imagine the world from Williamson's perspective (just as Kuhn did in trying to grasp Aristotle's mechanics) and thereby learn its language.[385] At some point in

[384] *Ibid.*, p. 332
[385] This is indeed what happened when Williamson first proposed his ideas in public at the Boston symposium on 'Organisms and the Origin of Self' in 1990. Faced with almost universal scepticism at the meeting, Williamson challenged the director of the marine biology laboratories at Woods Hole to assist Williamson in trying to make some hybrid larvae crossing *Ciona* (a sea-squirt) with *Echinus* (a sea urchin). The resulting offspring died before metamorphosis. However, *in vitro* fertilisations often fail to metamorphose in laboratory conditions even when

this process such people may discover they can speak the new theoretical language like a native. They will, in other words, find they are *practising* the new theory without having consciously made a choice. They have undergone a quiet conversion, risking doing so based on the values Kuhn has outlined. In Hayden White's terms, such a plot of reconciliation (at least a 'local' reconciliation) could be described as Comedy.

There is, however, a problem with this analysis because a narrative can only be a Comedy or Tragedy (i.e., deliver a *lesson*) if it is complete. Every child knows this. As Hayden White puts it: 'the demand for closure in the historical story is a demand for moral meaning, a demand that sequences of real events be assessed as to their significance as elements of a *moral* drama.'[386] But, as a Proppian analysis indicates, *Hopeful Monsters* has no clear ending:

> *Initial situation:* Once upon a time there lived a marine biologist who had spent his entire career at the same laboratory doing traditional scientific work.
>
> *Absentation:* He became successful in his narrow field of marine biology but is an outsider (i.e., professionally absent) in the field of evolutionary studies in which he has come to take an interest.
>
> *Interdiction:* Through obedience to the injunction against dabbling in areas outside of his expertise a misfortune befalls the science of evolution.
>
> *Villainy:* Anomalies abound in the understanding of the evolution of larvae.
>
> *Lack:* These anomalies cannot be squared with the traditional view.

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made within the same species and the director of Woods Hole was sufficiently convinced to admit the possibility that Williamson was on to something. (Personal communication from Williamson.)

[386] White, H., (1987) p. 21

Mediation: Don Williamson is (self-)identified as the man to liquidate this lack:

Departure: He formulates a hypothesis and begins a series of successful hybridization experiments.

1st Donor function: Lynn Margulis supports him to come to Boston to explain his ideas.

Receipt of magical agent: Williamson receives help from Robert for further work.

(Non-)*Victory,* (non-)*Liquidation,* The new experiments fail, papers are no longer published and opponents remain unconvinced. There is no victory or liquidation of the lack and consequently no *Return, Recognition, exposure, transfiguration, punishment of the villain* or *wedding*.

However, enthusiasm only temporarily dimmed, Williamson *departs* again…

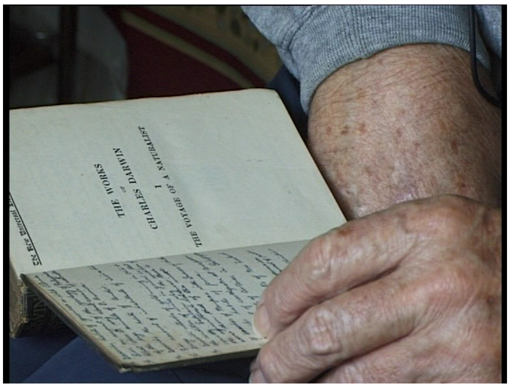
In *Hopeful Monsters* we see that Williamson will not be resigned to failure but neither can he celebrate even a temporary success and so the moral of this unresolved plot is ambiguous. *Hopeful Monsters* is neither Tragedy nor Comedy but a hybrid: Tragicomedy. The voice of Tragicomedy is characteristic of so-called 'art-house' cinema in which, as Carl Plantinga writes:

> The salient detail and the urgent moment are exchanged for meanderings and digressions, explorations that may or may not contribute to an answer to overarching…questions.[387]

During the drive from the lab to the beach, for example, Williamson describes his background and his education but the conversation is interrupted by Robert's hunt for heart urchins. Williamson picks up the threads of this conversation

[387] Plantinga, C. (1997) p. 108

later, in his study/bedroom, as he shows Robert his father's copy of Darwin's *Voyage of a Naturalist*. The film does not indicate which scene—the beach or the study—is more important; both are treated equally even though their connection is not clear or 'causal'. Robert's return to London is another sequence that, in a classic science documentary, would be a clear digression.



(00:34:00)

This sort of meandering plot creates an 'open', non-expository narrative that is then offered as a representation of scientific investigation itself—a voyage with a hoped-for destination but without a clear map or a sure way of reading it. This is quite a departure from the linear detective trail of the classic science documentary which often claims ignorance of the destination but expresses complete confidence in the method of map-reading that eventually leads there. The moral of the classic science programme, with its fully resolved narrative, is that, by the application of the scientific 'method' (with all that suggests of the received view), 'truth will out'. However, the 'science in action' narrative of *Hopeful Monsters*, with its meandering cycle of experiment, failure, despair and the resurgence of optimism, undermines such reassuring certainties.

Explanation by Formal Argument

Alongside the explanation by emplotment is another level of the organization of the events by which the historical narrative suggests meaning. It matches Silverstone's 'mimetic narrative' or what I have simply called 'argument', which is the term Hayden White prefers. There are four paradigms of explanation by 'formal argument': Formist, Organicist, Mechanist, and Contextualist.

The Formist explanation demonstrates the individuality of all the objects being scrutinised in order to dispel any impression that their history is the result of a historical process greater than each in its turn. One might say that Broomfield and Churchill's *Soldier Girls* (1981) is a Formist documentary because it seeks to demonstrate how a history of women recruits in the US army is a story of numerous biographies (in this, it is uncritical of the army as an engineer of human souls); similarly, a film like Les Blank's *Garlic is as Good as Ten Mothers* (1980) incorporates, as Carl Plantinga puts it, 'micro-narratives, or "small" stories, within the overarching structure of the film,' and is therefore Formist, dispersing the elements rather than integrating them.[388] (Plantinga calls such a structure 'topical'.) The weakness of a Formist approach is the weakness of all case studies: that its generalisations, ranging as they do across an array of individuated elements tend to lack focus, however they can be very vivid because they reconstruct the detailed lives of particular agents and actions.

The Organicist seeks, by contrast, to demonstrate that the disparate elements of the world are integrated. White suggests that the Organicist strategy is a metaphysical commitment to the paradigm of the microscopic-macroscopic relationship. Documentaries expressing an Organicist paradigm of reality will consequently tend to be somewhat abstract, emphasising the process of integration rather than characterising the individual elements. One can see how such a paradigm would tend to represent history as goal-oriented, as having a final end. A valid explanation in the Organicist mode however does not propose *causal* laws as guiding the integration of elements but rather *principles* or *ideas* that individuals hold. One might suggest that Riefenstahl's *Triumph of the Will* (1934) or Watt and Wright's *Night Mail* (1936) offer Organicist explanations of the social world.

The Mechanist historian or documentary filmmaker is similarly interested in demonstrating how individual people or social groupings are integrated but unlike Organicism, Mechanism does not explain the integration by the holding of shared ideas but by the action of causal laws that are

[388] Plantinga, C. (1997) p. 104

independent of a particular time and place. The individuality of people in a Mechanist narrative is less important than the over-arching law that guides their behaviour and so Mechanist histories, like Organicist ones, tend to abstraction and reduction. The mimetic narrative of a classic science documentary like *The Ghost in Your Genes* is a Mechanist history of science in which scientists are essentially ciphers, their individuality reduced to more or less interchangeable 'talking heads', seeming voices of authority that are 'operated' by the higher authority (represented by the narrator) of a syllogistic logic. In this view science is a 'meritocracy', blind to special interests; no social group holds power so an 'outsider' like Pembrey, provided he is logical, will always win through.

Contextualism, as the name implies, presupposes that events can be explained by the relation they have to other events in their immediate historical surroundings. Like Formism, Contextualism produces a representation of history that is dispersed, even chaotic and lacking in any overarching structural principle. Unlike a Formist argument that considers entities in their isolation and uniqueness, the Contextualist argument explains 'what happened' by examining the functional interrelationships between elements in the historical field:

> Contextualism seeks to avoid both the radically dispersive tendency of Formism and the abstractive tendencies of Organicism and Mechanism. It strives instead for a relative integration of the phenomena discerned in finite provinces of historical occurrence in terms of 'trends' or general physiognomies of periods and epochs.[389]

The Contextualist seeks to identify the threads that link the event to be explained to the features of the circumambient historical space. Contextualism looks backward in time to locate origins and forwards to suggest influence or impact on future events. The end point of such a history, the terminating event, is when the threads are swallowed up into the context of another discrete event—history as a chain of 'significant' events. Kuhn's history of science as comprising long periods of relative conceptual stability (the paradigm) divided by periods of intellectual uncertainty and dispute over fundamentals (revolutions) is a Contextualist explanation of history.

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[389] White, H. (1973) p. 18

And so too is *Hopeful Monsters*. On a number of occasions, the narrative demonstrates a clash of irresolvable intellectual trends and special interests. For example, Margulis's recounting of Eric Davidson's criticism:

> "And then one guy, it was Eric Davidson of Cal. Tech. He's a very important scientist. He's a Cal. Tech. *professor*. Right?! He gets up and he starts screaming at him saying... I mean, he was apoplectic. And I can't remember the details, but I don't think anybody can but it was, 'this is such wrong stuff, it is so based on nothing and there's no evidence…'" (00:59:30)

This recounting is then closely followed by Richard Strathmann's criticism that, among other things, appears to conflate Williamson's ideas with creationism:

> "You know if I were in Williamson's position, first of all, in good science, you should set up a hypothesis that's testable and then be willing to put it to the test. This is something the creationist's in the US don't want to do, which is why it's not really science. And it's something Williamson doesn't seem to *really* want to do." (01:01:10)

In fact, Strathmann compares Williamson's science to religion not once but twice:

> "I mean the book is sprinkled with the phrase 'I believe'. This is like, this isn't the Nicene or Apostolic creed, this is science! It's not a matter of deep belief, it's a matter of testable propositions and beliefs are provisional. And it's that constant scepticism and self-doubt, not quite paranoia, but always wondering 'could this be right, is there another possible explanation?' That's what makes science productive and possible and insightful. And that's what's disturbing to me about the direction Williamson's going." (01:04:00)

Strathmann and Davidson both position Williamson and his ideas in another world, another *weltanschauung*, which they ridicule (as does Jimmy Carr in the opening scene: "Oh, what he's done there is he's not understood and I mean fair enough because it is complicated"). As far as Strathmann and Davidson are concerned, Williamson and his idea are beyond the pale, outside of the accepted paradigm, and therefore can contribute nothing to science. From a Kuhnian perspective this attitude is quite to be expected but it gives the lie to the notion, articulated by the received view, of science as an incremental, mechanical accumulation of knowledge. Instead, *Hopeful Monsters* represents science as a contest between paradigms and such a contest may only be resolved, as Kuhn suggests, through struggle and revolution.

Every explanation of history therefore has implications for action in the contemporary moment, offering, as White puts it, ‘a set of prescriptions for taking a position in the present world of social praxis and acting upon it (either to change the world or to maintain it in its current state).’[390] Such a prescription is what he calls an ‘explanation by ideological implication.’

Explanation by ideological implication

White identifies four ideological prescriptions that may guide such action in the contemporary moment: Anarchism, Conservatism, Radicalism and Liberalism.

A history plotted as Tragedy, for example, might explain or justify its emplotment by reference to laws of causal determination or laws of human freedom. In the first, people are represented as essentially trapped in their fate while in the second they have some control of their destinies. The ideological thrust of the first is Conservative while that of the second is Radical. These ideological implications need not be drawn explicitly, says White, but emerge from the tone or mood of the resolution of the drama. One might say that Wiseman’s *Titicut Follies* is a Conservative Tragedy while the Maysles’ *Gray Gardens* is a Radical one because it suggests that the only way that Edie senior and her daughter can escape their awful mutual dependence is by taking the radical step of separating. That they do not, is a consequence of the ‘tragic flaw’ each possesses. (‘Tragic’ because in other circumstances such qualities of mutual care and support would be wholly positive.)

Conservatives and Liberals see change as best managed or brought about in a gradual, piecemeal fashion, not through the programmatic, structural transformations envisaged by Radicals and Anarchists. Conservatives and Liberals favour a ‘natural’ or ‘social’ pace of change, while Radicals and Anararchists embrace the possibility of cataclysmic transformations and are suspicious of the inertial power of institutions to prevent this happening. Conservatives see the current state of society and institutional structures as the best that can be hoped for. By contrast, Liberals project forward to a *remote*

[390] *Ibid.,* p. 22

future time when the current structures will have been improved (remote so as to discourage revolutionary, cataclysmic actions). Such precipitate action is envisioned by Radicals who picture Utopia as almost within our grasp and so encourage revolution while Anarchists hold to a picture of an ideal *remote past* in contrast to which today's society is far from ideal. Each of these positions argues for a distinct idea of what constitutes reality and of how we ought to behave.

There are, then, argues White, 'elective affinities' between these three metahistorical standpoints:[391]

Mode of Emplotment	*Mode of Argument*	*Mode of Ideological Implication*
Romantic	Formist	Anarchist
Tragic	Mechanist	Radical
Comic	Organicist	Conservative
Satirical	Contextualist	Liberal

Considered as a history of science, *The Ghost in Your Genes* is, by this analysis, emplotted as Romance. Its mode of argument however is Mechanist and its mode of ideological implication is Conservative, that is, representing the current state of the institution of science as the best that can be hoped for.

Unsurprisingly, this combination strains the elective affinities White posits; for recall that in our earlier analysis of *The Ghost in Your Genes*, we noted that the aims of the mythic narrative and of the film's argument are at cross-purposes. The argument seeks to explicate a scientific idea that is implicitly already acceptable to scientists because it is logically (i.e., mechanistically) compelling while the mythic narrative tries to tell a (romantic) story of 'science in the making' as Pembrey and his virtual band of helpers defeat the villainous orthodox model of genetics 'personified' by the Human Genome Project.

As for *Hopeful Monsters*: first, its inconclusive narrative denies the closure required for the story to be emplotted outright as either Tragedy or Comedy. Given the film's reflexive style we may, I would argue, understand

[391] *Ibid*., p.29

this plot as ‘aware of its own inadequacy’, i.e., what White terms Satire. Second, the highly metonymic figuration of the narrative emphasizes *context*. The fate of Williamson’s hypothesis is thereby shown to be determined not by mechanistic laws (as the fate of the epigenetic hypothesis of *The Ghost in Your Genes* is depicted), but by Williamson’s position, or that of his ideas, in relation to the values and beliefs of the *scientific community*. The formal argument of *Hopeful Monsters* thus explains the history of science as Contextual, in keeping with a Kuhnian model. Third, the explanation of *Hopeful Monsters* by ideological implication is apparently Radical, critical of the inertial power of the institution of science. But although the outcome of a general acceptance of Williamson’s ideas would be ‘revolutionary’, the narrative does not prescribe radicalism, rather it suggests that acceptance will come, if at all, only in a piecemeal fashion.

In White’s terms, then, *Hopeful Monsters* is a satire of ‘science in action’ that offers a view of theory choice in the contemporary moment that is thoroughly constructivist in explaining that choice as determined by the social and intellectual context in which the scientific work is conducted and disseminated. Furthermore, by reflexively drawing attention to its own inadequate narrativising, the film is implicitly critical of the certitudes (what Renov calls ‘epistemological violence’) that underpin narratives of the received view like *The Ghost in Your Genes*.[392] The history of science represented by *The Ghost in Your Genes*, with its romantic plot and mechanist argumentation seems too good to be true because it lacks the context and contingency that the metonymic, meandering narrative of *Hopeful Monsters* offers us.

Summary and concluding remarks

This thesis comprises two parts, the film, *Hopeful Monsters* and this written exegesis. *Hopeful Monsters* is ‘experimental’ in eschewing the key features of the classic, expository style and narrative structure of the science documentary. In this dissertation I have shown that the expository mode may be aligned with the so-called ‘received view’ of science, a view that philosophers and sociologists have found increasingly problematic in recent years. In trying,

[392] Renov, M. (2004) p. 136

therefore, to make a science programme that is unlike the classic science documentary, I have been exploring not just formal alternatives in filmmaking but philosophical alternatives to those aspects of the received view that are inherent in the classic form. In the second part of this dissertation I have attempted to lay out the grounds for doubting the received view of science and have developed an alternative called ‘constructivism’ that emphasises the social and rhetorical dimensions of science over the ‘logic of scientific discovery’. In these last two chapters, I have analysed the style and narrative structure of *Hopeful Monsters* to ascertain first, whether it does indeed offer a non-received view of science and second, whether that alternative represents a ‘constructivist’ view.

We may finally answer yes to both these questions. Before we even see the first images of *Hopeful Monsters*, we hear these words from Williamson: “I have an edited version of the film”. This sentence alerts us to the reflexive style of *Hopeful Monsters* by which, as Renov puts it, the film takes responsibility for its representations, demonstrating that they are not ‘natural’, not found ‘out there’ in the world but constructed by the filmmaker ‘up here’ on the screen.[393] Similarly, Williamson’s controversial hypothesis is represented as constructed by the discourse of science, a rhetorical invention that is stabilised (or not) in the economy of scientific discourse not because it matches an objective ‘reality’ (a philosophically problematic concept) but because of its on-going promotion within the scientific community whose members debate its merits in terms of a number of vague criteria like accuracy and simplicity.

Of course, no one film makes the perfect case study and, as I noted in the introduction to this dissertation, the work Williamson and I did together was carried out in a less socially complex environment than is commonly the case in science. In a typical molecular biology lab, for example, we may expect to find a hierarchy comprising a senior researcher, post-doctoral workers on short contracts and PhD and Masters students working for little or no pay. It is rare to find just two people working without financial constraints or institutional control. *Hopeful Monsters* cannot, therefore, represent the typical complex of interests that is to be found in larger labs. However, the film does offer some

[393] *Ibid*

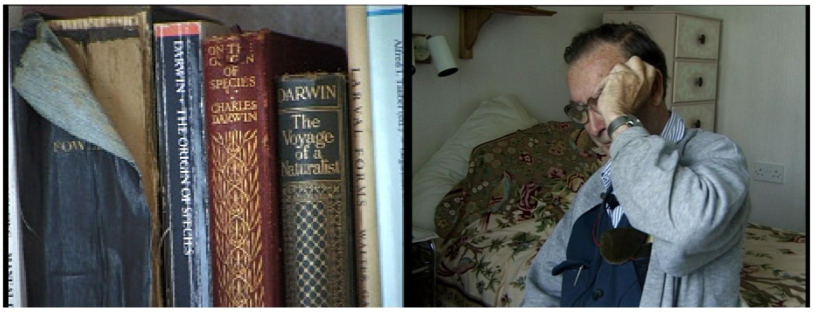
strong hints of the social forces in play in science, for instance through Lynn Margulis's telephone account of Williamson's Boston seminar, through the interview with Richard Strathmann and his limerick and through the comments of the referees that Don reads out in the penultimate scene of the film. Through all these, one is made aware of what Latour calls the 'hostile environment', the adversarial habitat of scientific culture.[394]

The adversarial structure of science acts as a brake on the acceptance of controversial (i.e., non-paradigmatic) ideas and so, as Kuhn has argued, science can only 'progress' by a series of revolutions which not only replace old ideas but destroy them in the process. The classic science documentary, then, with its traditional linear view of a more or less steady accumulation of knowledge, cannot successfully represent a Kuhnian view of scientific history while the cyclical narrative of *Hopeful Monsters*, in emphasising the resistance and inertia of established paradigms does so quite well.

This cyclic structure resembles the narratives of Akira Kurasawa which Donald Richie notes often take the form of 'the full circle, or the spiral, the return to the beginning with a difference, the cyclic.'[395] He argues that this form arises because the conflict of the narrative is 'one of character rather than situation'.[396] Kurosawa's films are concerned, writes Ritchie, with 'the totality of a character, the totality of a situation,' and certainly a constructivist narrative of science must also place its characters in the totality of a situation because it is this situation, this contextual web of special interests, that determines the fate of scientific ideas.[397] How complete, then, is this sense of the character's context in *Hopeful Monsters* and is it different to *The Ghost in Your Genes*?

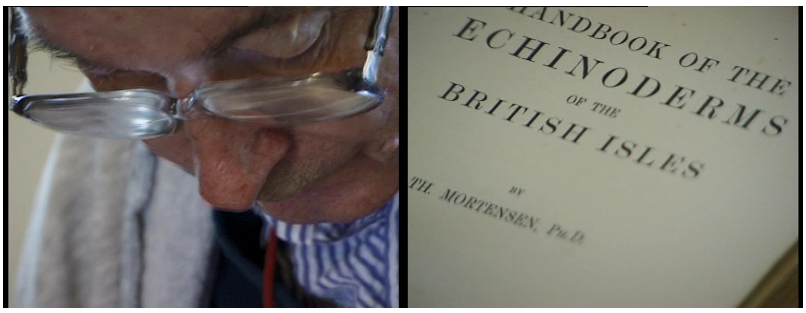
In *Hopeful Monsters*, we visit Williamson's home on two occasions and what we see of it (and of his little lab) creates an impression that the scientific life is rather monkish. Don lives in a small bungalow where he appears to work and sleep in a tiny cell furnished with a single bed and a narrow shelf of (holy) books (00:33:00):

[394] Latour, B. (1985) p. 45
[395] Richie, D. (1996) p. 232
[396] *Ibid.,* p. 131
[397] *Ibid.,* p. 233



On the face of it, this same picture of the scientific self is also painted by *The Ghost in Your Genes*; for in that film the scientific community is only a virtual one and highly dispersed. We either see scientists as isolated talking heads set against abstract backgrounds or (the graveyard notwithstanding) on their own in their offices. Here is Pembrey for example, alone, not in an office but in a garden speaking (in voice-over) of isolation as a positive feature that allows him freedom to speculate outside his main career. (00: 23:58).

The image may recall for some of us that stereotype of scientific isolation, the horticulturalist and 'father of genetics', Gregor Mendel. However, this idea of the scientist as a solitary, ascetic genius is actually *not* what *Hopeful Monsters* represents because in that film there is only one moment when Williamson is represented as truly alone. This is when he skims through a book after his first experiment fails to get off the ground:



This short scene, in the observational mode, comes quite early in the narrative (00:12:30). It is a lyrical moment that serves to contrast with the next scene (with the photographer) in which Robert comes out from behind the camera to take his place, permanently (and literally) as Don's right-hand man. In this moment, the isolated scientist becomes 'a team'. But Don and Robert are not simply two men working together; Robert is there to do what Don cannot, namely to work with his hands. Robert and Don, then, represent two aspects of the scientist. Just as *Cinderella* divides the mother into her 'good' and 'bad' attributes, personified by the fairy godmother and the step-mother, so *Hopeful Monsters* posits the scientific self as a dyad: Robert is body while Williamson is mind; Robert is the apprentice, Don the master; Robert represents connection (the phone call with Margulis, the interview with Strathmann) where Don is an island (signified, not least, by the Isle of Man itself).

One comes across many such dyads in narratives of science, for example, Darwin and Huxley, Einstein and Eddington, Watson and Crick, and although Williamson and Sternberg cannot, of course, be compared to these, the dyad affords the biographer/documentary filmmaker the same opportunity for separating and resolving any number of structural antinomies that comprise the idea of the scientific self. For example, unlike the interlocutor in Hobbes's dialogues, in the course of *Hopeful Monsters*, Robert does not become converted to his Master's beliefs, rather he becomes increasingly sceptical. The scientist is thus shown in *Hopeful Monsters* as torn between conviction and diffidence, his mental stability and the stability of his ideas always under threat. Don and Robert, in representing opposite poles, give the filmmaker a means to represent the scientist's oscillation between doubt and certainty. This is a very different dynamic to that represented by the actions of the hero of *The Ghost in Your Genes* who seems to travel, as befits an algorithmic view of the scientific method, with steady, machine-like competence in one direction only until he reaches his goal. It is true that Williamson appears similarly confident almost all the time (although he is clearly knocked-back by the death of his putative hybrid) but Robert acts as a counter-weight, expressing growing doubts and so together they paint a picture of the complete, scientific self.

The oscillating psycho-dynamics of the dyadic scientist of *Hopeful Monsters* matches the cyclic structure of the film's narrative and together they articulate an idea of the scientific life that accords with Popper and Kuhn's view of the history of science, namely that the quest to prove a scientific theory is, logically at least, unending and it may therefore be unending in practice too. Williamson himself remarks that he believes he is 'on a straight-line course for posthumous recognition' and later, as his voice tails off in the last moments of the film (he is making yet another pitch for his theory at the conference on Lake Como) we have a strong sense that this is not the end of the matter.[398] The inconclusive, cyclic narrative thus has the effect of creating a distant perspective on all the activity we have witnessed. We seem to step back and view the narrative from afar and the impact is something like what Edith Kern describes in her essay, 'Drama Stripped for Inaction' (1954):

> Seen from a distance, all human activity shrinks, in fact, into nothingness, its ultimate irrelevance, its qualities of repetition and habit come to the fore and deprive it of all claim to individual importance so that, almost, it becomes equivalent to inactivity. … In the perspective of distance, even the most active life becomes one long wait that is barren of fulfilment.[399]

However, this distant view that we get only at the end of the film contrasts sharply with the idea of science we construct as we witness the purposeful daily life of laboratory work in 'close-up' with its ups and downs, its frustrations (unripe starfish, urchins dead in their bucket, the hybrid larva 'cooked' under the microscope) and its brief triumphs (a beaker of eggs, hatching blastulas, a less negative referee).

In creating this bifocal perspective, *Hopeful Monsters* provides a more complex picture of the scientific life than is possible in the classic science documentary. The point I wish to emphasise is that *Hopeful Monsters* is able, because of its mode and narrative structure to represent an aspect of the scientific life that the classic documentary simply cannot, even in principle, because it represents science as a Baconian 'machine'. In *Hopeful Monsters*, the scientist is not the Baconian 'plain-thinking man of facts and calculations' that Peter Medawar lampoons but something closer to a 'poet fellow', a person who, through the 'versatile and ardent' scientific imagination that Goethe speaks of,

[398] *Hopeful Monsters* (00:57:10)
[399] Kern, E. (1954) p. 44

constructs a model of the world and tries to persuade the rest of his community to share his vision.[400]

In the end, style is all-important; for by not abandoning the sensory realm of spatiotemporal continuity for the abstract argumentation of the expository mode, the style of *Hopeful Monsters* engages the viewer more deeply than the classic science documentary can. The film invites the viewer not simply to acknowledge the *place* in which science is being done but to imagine themselves entering the diegetic *space* itself. At the same time, the film's reflexivity makes the viewer aware of *having* this imaginary experience and thus *Hopeful Monsters* empowers the viewer by demonstrating, as Goethe argues, that the well-spring of the scientific imagination—*experience* of nature—is available to us all:

> Experiencing, looking, observing, contemplating, connecting, discovering, inventing are mental activities which, singly and severally, are exercised a thousandfold by more or less gifted people … From these various powers named here, and many other related ones, Mother Nature has excluded no one.[401]

And so, in articulating a constructivist view of science in which, as Ernst von Glasersfeld says, 'the thinking subject has no alternative but to construct what he or she knows on the basis of his or her own experience,' *Hopeful Monsters* enriches our idea of science and invites the viewer to claim it as their own.[402]

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[400] Medawar, P. (p. 126 above); Goethe in Naydler, J. (ed.) (1996) p. 116
[401] Naydler, J. (ed.) (1996) p. 33
[402] von Glasersfeld, E. (1995) p. 1

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APPENDIX 1

TRANSCRIPT OF *THE GHOST IN YOUR GENES*

00:00:00	MUSIC IN	
	MONTAGE CHILDREN	
	OVERLAY IMAGES OF	
	WORLD EVENTS &	
	ARCHIVE FOOTAGE	
00:00:03	BBC LOGO IN	
00:00:04		V/O:
		We are on the brink of uncovering a
00:00:07	LOGO OUT	hidden world, a world that connects
		past and future generations in ways we
		never imagined possible.
00:00:15	MONTAGE CONTINUES	
		SKINNER:
00:00:20	TO CAMERA	What this means is an environmental
		exposure that your grandmother had
00:00:25	MONTAGE CONTINUES	could cause a disease in you, even
00:00:29	EX C/U CHILD'S EYE	though you've never been exposed to
		the toxin, and you are going to pass it
		onto your great-grandkids.
00:00:30		V/O:
		These extraordinary discoveries have
00:00:33	MONTAGE CONTINUES	the potential to affect every aspect of
		our lives.
00:00:35		
		SECKL:
00:00:38	TO CAMERA	It's not just the genes but also the
00:00:41	MONTAGE CONTINUES	environment in the early life of your
	OVERLAY B/W ARCHIVE	ancestors. It's not so much you are
	PEOPLE EATING	what you eat, it's that you are what
00:00:49	TO CAMERA	your mother ate and maybe you are
		what your grandmother ate, and if you
00:00:53	MONTAGE CONTINUES	take our data, you are what stress your
	WITH B/W ARCHIVE	grandmother or grandfather had.

	LIFESTYLE & EVENTS	
    00:01:03 00:01:07 00:01:12	 TO CAMERA MONTAGE B/W ARCHIVE CHILDREN AT PLAY TO CAMERA	V/O: It will change the way we think about our relationship with every generation. WOLF REIK: It makes me feel closer to my children. What I experience in terms of environment will have some type of a legacy in my children and my grandchildren.
00:01:15 00:01:20	MONTAGE B/W ARCHIVE HARVESTING TO CAMERA	V/O: The science of inheritance is being turned on its head. MARCUS PEMBREY: We are changing the view of what inheritance is.
00:01:26 00:01:45 00:01:46 00:01:51 00:01:52	TITLES IN *HORIZON* TITLES OUT CAPTION IN/MUSIC CHANGE *The Ghost in Your Genes* CAPTION OUT DISSOLVE	
00:01:54 00:01:58 00:02:05 00:02:09 00:02:10 00:02:12	WS OVER VILLAGE MONTAGE VILLAGE SIGHTS CROPS GROWING BLEED TO B/W B/W ARCHIVE STILL FARMERS & FAMILY	 V/O: This small Swedish town may hold the evidence to launch a medical revolution. Overkalix lies huddled on the edge of the Arctic Circle, inaccessible and remote.

	HARVESTING POTATOES	It was cut off from the rest of the world for most of its history.
00:02:16 00:02:19 00:02:25 00:02:29 00:02:35 00:02:37	MARCUS & OLOV WALKING IN WOODS MUSIC IN LS TO CHURCH MONTAGE GRAVESTONES BOTH IN GRAVEYARD OUT OF FOCUS	 Marcus Pembrey has travelled here to meet his colleague, Olov Bygren. They believe that the story lying buried in these graveyards may hold the proof to their radical ideas. BYGREN: Here we have at least two generations. PEMBREY: In the same grave? BYGREN: In the same grave.
00:02:39 00:02:40 00:02:45 00:02:46 00:02:48 00:02:57 00:03:04	 TO CAMERA BOTH AMONG GRAVES C/Us HEADSTONES BOTH EXAMINING HEADSTONES C/U HEADSTONE	PEMBREY: This group of people could contribute to really a sea change in the way we think about inheritance. V/O: They have come to this churchyard to find grandmothers and granddaughters, grandfathers and grandsons, connecting people who lived almost a hundred years apart in entirely new ways, uncovering links that confound scientific thinking.
00:03:06	TO CAMERA	PEMBREY:

	CAPTION IN	Up till now inheritance is just the
	Prof Marcus Pembrey,	genes, the DNA sequence. I suspect
	Institute of Child Health, UCL	there we're going to demonstrate that
00:03:11	CAPTION OUT	the inheritance was more than that.
00:03:15	BOTH AMONG	This is a grandson as it were in our
	HEADSTONES	study.
00:03:19	C/U HEADSTONE	V/O:
00:03:21	C/U MICROFICHES	It is the culmination of more than
00:03:23	BOTH IN GRAVEYARD	twenty years work. And for the first
		time, Pembrey is confronting the
		magnitude of their discovery.
00:03:30		PEMBREY:
00:03:32	TO CAMERA	Really it's come alive for me, coming
	MUSIC OUT	here, more than I had expected. It's,
		really quite sort of emotional about it.
		Wonderful!
00:03:40	MONTAGE BOTH	
	WANDERING IN	
	GRAVEYARD	
00:03:45	C/U PEMBREY HOLDING	
	POLAROID OF HIM &	
	WIFE IN FRONT OF HIS	
	FACE	V/O:
00:03:46		Marcus Pembrey is one of a select
	MS PEMBREY & WIFE AS	band of scientists, a band of scientists
00:03:49	HE LOWERS POLAROID	who are daring to challenge an
	C/U SCIENTIST HOLDING	orthodoxy.
00:03:51	POLAROID	
00:03:53	MS SCIENTIST & FAMILY	
	MONTAGE SCIENTIST	
00:03:57	FISHING	
	C/U OLD PAINTINGS OF	They believe the lives of our parents,
00:03:59	ANCESTORS	grandparents and even our great-
	MONTAGE SECKL IN ART	grandparents can directly affect our

00:04:05	GALLERY	well-being, despite never experiencing
	C/U SECKL HOLDING	any of these things ourselves.
00:04:07	POLAROID	
	POLAROID SHOWING	
00:04:08	FAMILY	
00:04:10	SECKL WITH FAMILY	
00:04:11	C/U WILD MEADOW	To many, these ideas are regarded as
	REIK WANDERING IN	scientific heresy.
00:04:12	MEADOW	
00:04:15	REIK HOLDING	
00:04:16	POLAROID	
	C/U REIK	
	REIK WITH FAMILY MS	
00:04:18	MUSIC OUT	REIK:
		You cannot predict where important
00:04:19	TO CAMERA/CAPTION IN	discoveries will be. The only thing
	Prof Wolf Reik, The	that you can do is to follow your
	Babraham Institute,	instinct.
00:04:24	*Cambridge*	
00:04:25	CAPTION OUT	
	MUSIC IN	
	MALE & FEMALE	
00:04:28	ANIMATION	
		V/O:
		Conventional biology has always
00:04:36		believed that our genetic inheritance is
	ANIMATION	set in stone at the moment of our
	FERTILISATION OF EGG	conception. At that instant, we each
00:04:43	PROCESS	receive a set of chromosomes from
	C/U CHROMOSOME &	both our mother and father. Within
	GENETIC CODES	these chromosomes are the genes,
		strips of coded DNA, the basic unit of
		inheritance.
00:04:52	C/U FERTILISED EGG IN	After conception, it was assumed that
	CHAIN	our genes are locked away inside
00:04:57	ANIMATION – CELLS	every cell of the body, protected and

   00:05:02 00:05:08	ZOOM OUT TO BABY DEVELOPING INTO ADULT DRINKING AND SMOKING ZOOM INTO LOCKED UP GENE IN BODY	untouched by the way we live. So what you do in your life may affect you but your genes remain untainted, unchanged for future generations.
00:05:12 00:05:13 00:05:16 00:05:19 00:05:21 00:05:25 00:05:27 00:05:30 00:05:31	 CUT TO SEPIA STILL VICTORIAN FAMILY OLDER MEMBERS WHITED OUT NEXT GENERATION PHOTO OLDER MEMBERS WHITED OUT COLOUR STILL NEXT GENERATION OLDER MEMBERS WHITED OUT MUSIC OUT FADE TO BLACK FAMILY GROUP IN COVENT GARDEN C/U FATHER PAN TO C/U DAUGHTER FAMILY GROUP	 In classic genetics, your parents and grandparents simply pass on their genes. The experiences they accumulate in a lifetime are never inherited, lost for ever as the genes pass untouched through generation after generation. The biology of inheritance was a reassuringly pure process – or so it seemed.
00:05:41 00:05:43 00:05:47	MUSIC IN PEMBREY HOLDING POLAROID IN FRONT OF FACE PEMBREY REMOVES POLAROID	 In the early 80s Marcus Pembrey headed the Clinical Genetics Department at Great Ormond Street

00:05:48 00:05:50 00:05:52	MONTAGE PEMBREY PEMBREY WANDERING AMONG PIGEONS PEMBREY IN OXFORD STREET	Hospital for Children. He was frequently treating families with unusual genetic conditions.
00:05:58 00:06:03 00:06:13	 TO CAMERA MUSIC OUT	PEMBREY: We were constantly coming across families which didn't fit the rules, didn't fit any of the patterns that genetics were supposed to fit so you think of chromosome abnormalities and you check the chromosomes and they're normal. So you then have to start imagining as it were you know what might be underlying this and you're really driven to try and work it out because the families needed some help.
00:06:24 00:06:26 00:06:33 00:06:38	C/U PEMBREY AT LAPTOP C/U GENETIC DIAGRAM B/W HOME MOVIE CHILDREN WITH ANGELMAN'S SYNDROME	 V/O: The more families he saw, the more the rules of inheritance appeared to break down, diseases and conditions that simply didn't fit with the textbook convention. One condition in particular caught his eye: Angelman's syndrome.
00:06:44	MONTAGE OF CHILDREN WITH CONDITION	PEMBREY: Named after Harry Angelman, the paediatrician who first described Angelman syndrome, he referred to them as happy puppet children because this described in some sense the features. They have a rather jerky

		sort of movement when they're walking. These children had no speech, they are severely incapacitated in terms of learning but are uncharacteristically happy. They are smiling all the time.
00:07:17 00:07:21 00:07:28	C/U BOYS WITH SYNDROME ANIMATION OF DNA PROBLEM TO CAMERA	V/O: The condition was caused by a genetic fault. A key sequence of DNA was missing, deleted from chromosome 15. PEMBREY: Then we came across a paradox. At the same time the same change, the same little deletion of chromosome 15 had been clearly associated with a quite different syndrome, much milder in terms of intellectual impairment, the Prader-Willi syndrome.
00:07:47 00:07:49 00:07:54 00:08:03 00:08:07 00:08:10 00:08:17	MUSIC IN COLOUR FOOTAGE CHILDREN WITH PRADER WILLI SYNDROME SUFFER SWIMMING SPLIT SCREEN – PRADER WILLI/ANGELMANS SUFFERERS ANIMATION GENETICS BACK TO SPLIT SCREEN	These children are characterised by being very floppy at birth but once they started eating properly and so on, they then had an insatiable appetite and would get very very large. V/O: What Pembrey saw simply made no sense. Here were two completely different diseases – Angelman's Syndrome and Prader-Willi Syndrome – being caused by exactly the same genetic fault.

00:08:18	MUSIC OUT	PEMBREY:
00:08:20	TO CAMERA	So here we're in a bizarre situation
		really. How could one propose that
		the same deletion could cause a
00:08:27	PEMBREY AT LAPTOP	different syndrome?
00:08:28		
		V/O:
00:08:34	C/U PEMBREY	It appeared to Pembrey as if the simple
		view of inheritance was beginning to
00:08:39	MUSIC IN	unravel. But his doubts were contrary
		to the tide of optimism sweeping the
		scientific community.
00:08:41	SLOW MOTION CROWDS	
	OF PEDESTRIANS	
00:08:45	OVERLAY ROWS OF	
	GENETIC CODES	In the early 1990s the biggest project
00:08:49	FADE TO OVERLAY	ever undertaken in biology was
	FRONT PAGE OF *NATURE*	captivating the world.
	MAGAZINE	
00:08:51	MUSIC OUT	NEWS:
		The human genome project will be
00:08:55	MUSIC IN	seen as the outstanding achievement in
		the history of mankind.
00:08:58	B/W ARCHIVE	
	LABORATORY	
	EXPERIMENTS	
00:08:59		V/O:
		The human genome project was to be
00:09:02	C/U ARTICLE	the pinnacle of a century of work on
00:09:04	FADE TO CROWD SCENE	genes and genetics. It seemed as if the
	& GENETIC CODES	secrets of life were at our fingertips.
00:09:11	ZOOM OUT TO FRONT	
	PAGE NEWS STORY	
00:09:12		NEWS:
		The genetic blueprint of mankind –
00:09:15	MONTAGE OF GENETIC	Mapping out nearly the whole human

	IMAGERY & HEADLINES	genetic code – A set of instructions to make a human being --
00:09:21 00:09:25 00:09:39	CROWD SCENE & GENETIC CODES TO CAMERA CROWD SCENE	REIK: Human genome is like a bible where everything was written down. The hope and the expectation was that once we had that book in front of us, and all the letters, we could just read down the pages and we would understand how the body was put together.
00:09:40 00:09:43 00:09:50 00:09:53 00:09:56 00:09:58 00:00:00	FADE TO C/U CELLS UNDER MICROSCOPE LAB SCENES CROWD SCENE NEW YORK TRAFFIC C/U TO NEWSCAST LIGHTS OUT OF FOCUS	V/O: It would offer a complete understanding of human biology at a molecular level. The hope was that once the code was written down, scientists could find the genetic cause and cure for every disease. NEWS: It could lead to the end of diseases like cancer and – CLINTON: Alzheimers, Parkinsons, diabetes – NEWS: The list is endless --
00:00:01 00:00:06 00:00:09	TO CAMERA CAPTION IN/MUSIC OUT *Prof Jonathan Seckl* *Edinburgh University* CAPTION OUT MUSIC IN/GENETIC CODE	SECKL: We were thinking of genes in a very mechanical way. We were thinking of them just in terms of the sequence of the letters.

	SEQUENCES	We were working out how we could work out what all the letters were in the book.
00:00:14 00:00:17 00:00:20 00:00:23	HI SPEED PHOTOGRAPH NEW YORKERS CROSSING BRIDGE HI SPEED PEDESTRIANS & TRAFFIC OVERLAY GENETIC CODES MUSIC OUT	V/O: Scientists estimated that the human genome, the book of life, would contain around a hundred thousand genes.
   00:00:28	TO CAMERA/CAPTION IN *Prof Michael Skinner* *Washington State University* CAPTION OUT	SKINNER: And then when they started sequencing, they realised there may be a hundred thousand genes and then it popped down to sixty and then it popped down to fifty. I mean – and slowly it went down to a much smaller number. In fact we found out that the human genome is probably not as complex and doesn't have as many genes as plants do.
00:00:39 00:00:41 00:00:45 00:00:51 00:00:52 00:00:53	MUSIC IN HI SPEED PEDESTRIANS TO CAMERA/MUSIC OUT MUSIC IN HI SPEED PEDESTRIANS & TRAFFIC WITH GENETIC CODE OVERLAY FADE OUT PEOPLE LEAVING CODES	So that then made us really question – well, if the genome has less genes in this species versus this species and we're more complex potentially, what's going on here? V/O: Now scientists estimate there are probably less than thirty thousand genes.

00:00:58	TO CAMERA/MUSIC OUT	SECKL:
00:11:01	C/U MOVING PETRI DISHES	We believed, I believed naively, that we would be able to find the genetic
00:11:07	ARTICLES & GENETIC CODE OVERLAY	components of common diseases and that's proven to be very difficult.
00:11:10	ZOOM INTO FIGURE OF *30,000*	
00:11:11	TO CAMERA	The idea of one gene, one disease does
00:11:14	SLO MO PEDESTRIANS ON BRIDGE	not explain it all.
00:11:16		V/O: Thirty thousand genes didn't appear enough to explain human complexity. There had to be something they'd missed.
00:11:23	PRADER WILLI SUFFERER SWIMMING UNDERWATER MUSIC OUT	
00:11:27		The first hints of what was missing lay
00:11:31	FADE FROM SWIMMER TO SPLIT SCREEN B/W ARCHIVE WITH ANGELMAN'S SUFFERERS	in the curious paradox of the Prader-Willi and Angelman syndromes: two quite different diseases caused by exactly the same genetic fault.
00:11:44	C/U PEMBREY AT LAPTOP	When Pembrey looked at the inheritance pattern for the conditions, he noticed something even stranger.
00:11:49		PEMBREY:
00:11:52	TO CAMERA	What really mattered was the origin of the chromosome 15 that had the deletion. If the deletion was on the chromosome 15 that the child had inherited from father, then you would have Prader-Willi syndrome whereas if the deletion was inherited from the

		mother, you had the Angelman syndrome.
00:12:11 00:12:24	MUSIC IN MICROSCOPIC PHOTOGRAPHY SPERMS APPROACHING EGG CELL DIVIDING	V/O: It was a complete surprise that the same missing strip of DNA could cause one disease when it came from the mother, and a completely different disease when it came from the father. It was as if the genes knew where they came from.
00:12:27 00:12:31	 MUSIC OUT TO CAMERA	PEMBREY: You've got a developing foetus manifesting this condition. How does the chromosome 15 know where it came from? It, there must be a tag or an imprint placed on that chromosome – join either egg or sperm formation in the previous generation – to say hi, I came from mother, I came from father and we are functioning differently so that's the key thing, that although the DNA sequence is the same, the different sets of genes were being silenced depending on whether it came from the mother or from the father.
00:13:04 00:13:06 00:13:13 00:13:19	CGI DNA SEQUENCING TO CAMERA CGI DNA SEQUENCING	 V/O: It showed that there was clearly more to inheritance than simply the coded sequence of DNA. PEMBREY: We then realised that we were dealing with what is now known as genomic imprinting. What genomic imprinting

00:13:23	TO CAMERA	means is in a nutshell that genes have
		a memory of where they came from.
00:13:30	MS FAMILY GROUP	
00:13:33	C/U YOUNGEST GIRL	V/O:
00:13:35	C/U SISTER	Something other than just the DNA
00:13:38	C/U FATHER	was capable of moving between
00:13:40	MUSIC IN	generations.
	MONTAGE CHILDREN'S	
	BODIES WITH B/W	
	ARCHIVE FOOTAGE OF	
	BABIES AT PLAY	
00:13:43		
00:13:45	CHANGE ARCHIVE TO	It was a tantalising glimpse into this
	OLDER CHILDREN AT	unknown and unexpected world, a
	PLAY	hidden layer acting on and able to
00:13:51	ARCHIVE CHANGE TO	directly control how or genes function.
	ADULTS	
00:13:57	FAR EASTERN FAMILY	
	GROUP	It meant that inheritance was not
00:13:59	C/U MOTHER	simply about which genes you
00:14:00	PAN TO SON	inherited,
00:14:02	CGIs GENES	
00:14:03	PAN TO FATHER	
00:14:06	CGIs GENES	
		but whether those genes were silenced,
		switched on or off.
00:14:07	TO CAMERA/MUSIC OUT	REIK:
00:14:09	LIGHTS ON & OFF	You can think of it as a light switch.
	MONTAGE & MUSIC IN	
00:14:14		Switch on the gene, the light is
00:14:18	C/U CELLS	shining, the gene is active. Makes
00:14:22	MONTAGE OF LIGHT	this, makes the cell do a certain thing.
	IMAGES OFF & ON	Or the light switch is off, everything is
00:14:29	TO CAMERA/MUSIC OUT	dark. That gene is off. The switches
		remain on or remain off, and that gives
		the cells their identity.

00:14:33 00:14:48 00:14:52	MUSIC IN ANIMATION – GENE & DNA CODES & CHEMICALS ATTACHING TO GENES TO CAMERA/MUSIC OUT TO CAMERA	V/O: The activity of genes was being controlled by a switch, the attachment of a simple chemical which dictated whether the gene was switched on or off. SKINNER: Whether those genes are turned on or off is called epigenetics. PEMBREY: Epigenetics, you know upon the, the genes.
00:14:58 00:15:01 00:15:05	MUSIC IN/MONTAGE LIGHT IMAGES TO CAMERA	 SKINNER: Not only is the sequence important of the DNA which we've studied for a long time, the past few decades, but we now understand that in addition to that there's this overlying epigenetic phenomenon that allows the genes to get turned on or off.
00:15:14 00:15:15 00:15:20 00:15:25 00:15:26	LIGHTS PULL TO FOCUS ON US TRAFFIC FAMILY GENE US MUSIC OUT TO CAMERA	 V/O: Epigenetics could explain how a human could be created with less than thirty thousand genes, and why the genome project didn't provide all the answers. SKINNER: Now if we actually put epigenetics on top of it, where it makes it much more

		complicated and where the genes get activated into a certain level and so forth, then you have a complexity that can start to explain biology much more effectively in the simple sequence of the DNA.
00:15:39 00:15:45 00:15:52	TO CAMERA MUSIC IN HI SPEED PEDESTRIANS ON BRIDGE TO CAMERA	SECKL: So clearly we have additional levels of complexity that we now need to understand, that are well beyond the DNA. REIK: The next huge challenge for modern biology is to now decipher the epigenetic code, to understand all the combinations of switches that exist.
00:15:57 00:15:58 00:16:15	HI SPEED PEDESTRIANS OVERLAY GENETIC CODES MUSIC OUT MONTAGE OF LIGHTS	 V/O: An accurate chemical map of the human genome tells us surprisingly little about how it actually works. Transcribing the code of the genes, the genome project, is not an end but simply a beginning.
00:16:20 00:16:23 00:16:26 00:16:30 00:16:32 00:16:35	FAMILY GROUP EXCLUDING FATHER C/U MOTHER & BABY PAN TO DAUGHTER PAN TO SON PAN TO ELDER SON FADE TO COLOURS THEN	If inheritance was not just about DNA, if these gene switches were so important, just what could turn them on, or off?

 00:16:36 00:16:41 00:16:43 00:16:44 00:16:45	DISSOLVE C/U STEPHANIE HOLDING DEVELOPING POLAROID C/U AS STEPHANIE LOWERS POLAROID MS FAMILY GROUP C/U CIARAN & DAD FAMILY GROUP	 Stephanie and Eamonn Mullins have two children, Ciaran -- and Charlotte.
00:16:46 00:16:48 00:16:51	 MUSIC OUT TO CAMERA/CAPTION IN *Stephanie Mullins* CAPTION OUT FAMILY PLAYING IN GARDEN	STEPHANIE MULLINS: When you're trying to conceive, and you see all your friends around you getting pregnant, having children, as each month went on you become more and more desperate.
00:16:56	CIARAN WITH DAD IN GARDEN	V/O: Doctors recommended IVF treatment. In the UK alone, around 8,000 babies are conceived every year using assisted reproduction techniques like IVF.
00:17:07 00:17:10 00:17:14	MS STEPHANIE CIARAN WITH DAD CIARAN STEPHANIE & DAD	 After the third attempt Stephanie became pregnant with Ciaran.
00:17:16 00:17:19 00:17:21 00:17:24	TO CAMERA IMAGES OF SCAN TO CAMERA	STEPHANIE: At the time they, they didn't really highlight any risk to us. And then we went for our routine scan and I did feel that the scan was taking an awful long time.
00:17:28 00:17:31	IMAGES OF SCAN TO CAMERA	 Basically what they'd found was something called an exophthalmos on

00:17:42	MUSIC IN	Ciaran's abdomen which basically means that part of the bowel is still on the outside of the abdomen.
00:17:43 00:17:45	COLOUR STEPHANIE PREGNANT PAN DOWN TO STEPHANIE'S TUMMY EXPOSED	 V/O: Doctors suspected that Ciaran might be suffering from Beckwith Wiedemann syndrome, a rare condition where babies are born very large, often have over-size tongues and have a high risk of developing childhood cancers.
00:18:01 00:18:04 00:18:06 00:18:10 00:18:13 00:18:16 00:18:19	 FADE SCAN ON STEPHANIE'S TUMMY OTHER SCANS HIGHLIGHT TONGUE ON SCAN TO CAMERA/MUSIC OUT COLOUR STILL STEPHANIE'S TUMMY	STEPHANIE: But they couldn't say one hundred percent that the baby did have Beckwith Wiedemann syndrome, but it was showing signs. They could see his tongue protruding on the scan and he said that he had very big thighs But until Ciaran was actually born we didn't know how severely affected he was going to be.
00:18:24 00:18:25	COLOUR STILL DAD AT COT PAN DOWN TO CIARAN	 V/O: When Ciaran was born, it was clear he did indeed have Beckwith Wiedemann syndrome.
00:18:31 00:18:35	MUSIC IN	 STEPHANIE:

00:18:39	TO CAMERA	Within a few hours of the birth, Ciaran had to have surgery to have the bowel that was on the outside of the abdomen basically put back inside, repaired.
00:18:44 00:18:46 00:18:49	MUSIC OUT C/U SCAR ON CIARAN'S TUMMY PAN UP TO CIARAN'S FACE STEPHANIE DRESSING CIARAN	 V/O: Ciaran also had surgery to reduce the size of his tongue and every few months he has scans to check for tumours.
00:18:54	CIARAN HAVING SCAN AT CLINIC	 DOCTOR: Good boy Ciaran, you are a very, very good boy indeed.
00:18:59 00:19:01 00:19:04	VIEW OF SCAN IMAGES EX C/U CIARAN'S FACE CIARAN LOOKING AT SCAN	 V/O: Cases of Beckwith Wiedemann syndrome caught the attention of Wolf Reik.
00:19:07 00:19:11 00:19:14 00:19:23 00:19:24 00:19:26	MUSIC IN C/U REIK HOLDING UP DEVELOPING POLAROID OF SELF WOLF LOWERS POLAROID WOLF AT MICROSCOPE C/U VIEW THROUGH MICROSCOPE ANIMATION MOUSE EXPERIMENT	 Wolf Reik worked in developmental genetics. He was fascinated by this emerging epigenetic ghost world. He wanted to know what could throw the switches on or off. To his surprise he found that simply placing a mouse embryo in a culture dish could trigger genes to switch off.

00:19:34 00:19:36 00:19:44 00:19:50 00:19:53 00:19:58 00:20:02 00:20:08	MUSIC OUT CGIs SWITCHES TURNING ON & OFF C/Us REIK IN LAB TO CAMERA C/U EMBRYOS TO CAMERA C/U CULTURE DISH C/U REID DOING EXPERIMENT TO CAMERA	 REIK: After we had seen relatively easy it was to change the switches in mouse embryos, we thought that perhaps the same could be true of human embryos. In IVF you also have the embryo for a brief period of time in a culture dish and so we were asking the question whether as in a mouse embryo, the mere fact of human embryos having been in a culture dish or been manipulated could alter their epigenetic switches.
00:20:13 00:20:15	MUSIC IN C/U CIARAN ON ROUNDABOUT	 V/O: Wolf knew that Beckwith Wiedemann syndrome was caused by a faulty switch.
00:20:21 00:20:24	 TO CAMERA	REIK: So what we were looking at was a group of babies, children that have Beckwith Wiedemann syndrome, what proportion of those were conceived by IVF.
00:20:32 00:20:33 00:20:36	C/U CIARAN ON ROUNDABOUT C/U IVF PROCEDURE C/U CIARAN	 V/O: Could IVF be switching genes on or off? Could IVF itself cause the syndrome?

00:20:37	MUSIC OUT	
00:20:40 00:20:41	LIGHTS SWITCHING ON & OFF TO CAMERA	 REIK: What we found was an increased occurrence of this epigenetic syndrome in the IVF population.
00:20:51 00:20:54 00:20:58 00:21:02	MUSIC IN C/U IVF PROCEDURES LS TO STEPHANIE & FAMILY AT PLAYGROUND C/U CIARAN & SISTER	 V/O: Although the disease is extremely rare, the risk appeared to increase three to four times with IVF. It seemed that the simple act of removing the embryo from its natural environment could trigger the disease.
00:21:08 00:21:10 00:21:15	TO CAMERA MUSIC OUT CIARAN & SISTER ON ROUNDABOUT	STEPHANIE: And I do feel frustrated that Ciaran might possibly have Beckwith Wiedemann syndrome because we had IVF, but at the time it was the right decision to make.
00:21:19 00:21:25 00:21:30 00:21:32	MONTAGE REIK WORKING IN LAB & C/Us IVF TO CAMERA CIARAN AT PLAYGROUND C/U IVF PROCEDURE MUSIC IN	 REIK: And I think that we should look again at the IVF procedures, the conditions that are being used, and carry out better and more precise experiments to see how we can avoid throwing these epigenetic switches.
00:21:34	LIGHTS GOING OFF & ON IN CORRIDOR AS REIK APPEARS	

00:21:37		V/O: Wolf had shown a simple change in environment was enough to turn a gene on or off. But there was more.
00:21:44 00:21:49 00:21:55 00:21:57	DISSOLVE MONTAGE C/Us WOLF AT MICROSCOPE ANIMATION MOUSE EXPERIMENT	 Everyone thought that any altered switches could not be inherited. He took some mice with altered gene switches and bred them.
00:22:02 00:22:04 00:22:08	 TO CAMERA C/U PROCEDURE	REIK: Our expectation was that as the altered genome was passed to the children, that any epigenetic changes would be wiped clean.
00:22:14 00:22:16 00:22:19	IMAGE ERASED FROM SCREEN REIK IN LAB ANIMATION MICE	 V/O: When he looked at the gene profile of the offspring he was amazed.
00:22:21 00:22:22 00:22:29	MUSIC OUT WOLF EXAMINING DOTS TO CAMERA	REIK: You had dots that you were looking at and every dot means a gene is on, and all of a sudden you know somebody said, wow look at that.
00:22:31 00:22:32 00:22:37	REIK IN LAB EXAMINING RESULTS MUSIC IN	 V/O: The epigenetic switch thrown in one generation was clearly also present in this second generation.
00:22:41		REIK: Nobody had, had seen this kind of

   00:22:50 00:22:57 00:23:00	 TO CAMERA REIK EXAMINING RESULTS TO CAMERA	thing before so it was the first time and all the people looking at the genome saying no this can't be right, you know it's the wrong gel, you know how you get excited about it and then you think oh maybe this is wrong and you're not on the right track and we were very excited. As excited as scientists ever get.
00:23:04 00:23:06 00:23:08 00:23:13 00:23:15	C/U RESULTS C/Us EXPERIMENT LIGHTS OFF & ON FAMILY GROUP SHOTS	 V/O: This meant that the genes were not locked away. A simple environmental event could affect the way genes worked and that could be inherited.
00:23:19 00:23:21 00:23:22 00:23:24 00:23:26 00:23:28 00:23:30 00:23:33 00:23:35 00:23:39	MUSIC OUT C/U DAUGHTER C/U MOTHER C/U PUNK DAUGHTER OTHER FAMILY GROUP C/U DAUGHTER C/U MOTHER C/U SMALLEST DAUGHTER C/U GRANDMOTHER C/U YOUNG WOMAN PAN TO GRANDMOTHER & DAUGHTER	 As if a memory of an event was being passed down through generations. It was something many scientists regarded as impossible. If this effect could be observed in humans the implications would be profound. It would mean that what we experience could affect not just us but our children and our grandchildren.
00:23:45 00:23:46 00:23:50 00:23:58	WHITE OUT MUSIC OUT WILD FLOWERS IN MEADOW PAN THROUGH FLOWERS TO PEMBREY	 While these observations were just emerging from laboratories, Pembrey was still working at Great Ormond Street. He began to wonder why these

		links between generations would exist.
00:24:04 00:24:08	 TO CAMERA	PEMBREY: Now my reputation was made as a clinical geneticist and so I was much freer to speculate outside my main career.
00:24:12 00:24:15 00:24:19	C/U SUNFLOWER PEMBREY AMID FLOWERS TO CAMERA	 I also like to stir things up a bit and it amuses me to speculate because I have got nothing to lose, and if I'm right well then that's very amusing.
00:24:26 00:24:27	MUSIC IN PEMBREY IN GARDEN PEELING SWEETCORN & OTHER VEG	 V/O: He speculated why genes would carry a memory from one generation to the next, what evolutionary purpose could it serve?
00:24:39	MUSIC OUT TO CAMERA	 PEMBREY: Maybe imprinting was used as a means of some sort of trans-generational adaption.
00:24:45 00:24:47	MUSIC IN ANIMATION MOTHER REVEALING BABY IN WOMB	 V/O: He thought it could be used for a mother to send messages to her baby in the next generation.
00:24:55 00:24:56	 ZOOM INTO BABY IN WOMB GOING INTO	PEMBREY: Something that always puzzled me ever since I was a medical student

00:25:03	BIRTH CANAL ANIMATION OF THEORY SHOWING SIZE OF PELVIS & EGGS & DEVELOPMENT OF BABY	was, what stops the baby's head jamming up in the birth canal? The baby of course is growing in one generation but the mother's pelvis was grown in the previous generation so if the mother was starving when she was growing so she had a small pelvis, maybe her eggs had captured that information and so they were instructing the growth genes of the future babies to not work so much and for the baby not to grow too much so as to jam up the birth canal. So there was some sort of co-ordination between the growth in two generations. That stuck me as entirely reasonable.
00:25:36 00:25:37 00:25:47	PEMBREY IN LONDON – MONTAGE ZOOM INTO BYGREN'S NAME ON COMPUTER SCREEN	 V/O: He published his ideas in an obscure journal and largely forgot about it. After all, there was no evidence for any of this. It was pure speculation.
00:25:49	MUSIC OUT C/U E-MAIL	 Then four years later Marcus received an e-mail from a doctor is Sweden.
00:25:54	C/Us PEMBREY AT LAPTOP	 PEMBREY: It really came as a bolt out of the blue. I just got an e-mail in May 2000 saying my paper was the only thing he

00:26:02	TO CAMERA	could find in the literature that in any way sort of tied in with his basic observations.
00:26:08 00:26:10 00:26:15	MUSIC IN C/U BYGREN HOLDING DEVELOPING POLAROID TO CAMERA	 V/O: The e-mail was sent by Olov Bygren. He was studying the population records of an obscure town in northern Sweden, Overkalix.
00:26:18 00:26:21 00:26:23 00:26:24 00:26:33	MICROFICHE RECORDS GVs OVERKALIX BYGREN AT MICROFICHE READER RIPE BARLEY	 What made these records unique was their detail. They recorded births and deaths over hundreds of years but they also had accurate details of the harvests.
00:26:37 00:26:39 00:26:43 00:26:45	BYGREN IN LIBRARY PANORAMA OVER OVERKALIX GVs OVERKALIX B/W PERIOD STILL LOCAL FARMERS & C/Us INDIVIDUALS	 More significantly Overkalix's isolated location on the Arctic Circle meant that it was particularly vulnerable to famine.
00:26:53 00:27:02 00:27:07	MUSIC OUT TO CAMERA/CAPTION IN *Prof Lars Olov Bygren* *University of Umea* CAPTION OUT	BYGREN: In the 19th century this was a very isolated area. They could not have help from outside. As it was so poor they really had a hard time when there was a famine and they really had a good, good time when the harvest were good.
00:27:12	MUSIC IN	

00:27:14 00:27:19	BYGREN AT MICROFICHE READER BYGREN IN LIBRARY	V/O: Bygren appeared to be seeing links between generations that confounded his expectations.
00:27:21 00:27:25	TO CAMERA	BYGREN: I sent Marcus Pembrey an e-mail telling him that we had some, some data which could interest him.
00:27:30 00:27:33 00:27:35 00:27:39 00:27:45 00:27:47	BYGREN AT MICROFICHE READER PEMBREY AT LAPTOP TO CAMERA C/U MICROFICHE PEMBREY & BYGREN WANDERING AROUND OVERKALIX	PEMBREY: I was terribly excited to get this completely out of the blue. And for the first time it seemed that there was some data that we could then start to explore, so that was the beginning of our collaboration.
00:27:48 00:27:56	B/W ARCHIVE STILL FARMERS HARVESTING POTATOES	V/O: Overkalix offered Pembrey a unique opportunity to see if the events that happened in one generation could affect another decades later.
00:28:00 00:28:02 00:28:07	MUSIC OUT VIEWS FROM SHIP TO NEW YORK WATERFRONT & MONTAGE OF TOURISTS	While Pembrey and Bygren sifted through their Overkalix data, someone else had stumbled on another group of people that caught them by surprise.

00:28:18 00:28:21 00:28:22 00:28:24	MUSIC IN C/U YEHUDA HOLDING DEVELOPING POLAROID C/U YEHUDA MS YEHUDA WITH NEW YORK BUILDINGS BEHIND	 Rachel Yehuda is a psychologist. She's interested in how people respond to stress.
00:28:26 00:28:27 00:28:30	VIEW TO SKY MS YEHUDA TO CAMERA	 YEHUDA: Well trans-generational effects were not on my radar screen at all until we opened up a clinic for the treatment of Holocaust survivors.
00:28:34 00:28:39 00:28:43	B/W ARCHIVE FOOTAGE HOLOCAUST VICTIMS IN CONCENTRATION CAMPS YEHUDA WORKING IN LAB	 V/O: While treating the Holocaust survivors for stress, she was surprised that many of the children of the survivors were themselves suffering stress effects.
00:28:48 00:28:56 00:29:01	 TO CAMERA/CAPTION IN *Prof Rachel Yehuda* *Mount Sinai School of Medicine* CAPTION OUT	YEHUDA: About five children of Holocaust survivors were calling us for every Holocaust survivor and what these children said was that they were casualties of the Holocaust too, that they had been affected by the Holocaust indirectly.
00:29:03 00:29:05	MUSIC IN B/W ARCHIVE HOLOCAUST VICTIMS IN CONCENTRATION CAMPS	 V/O: She was convinced that the stress in

  00:29:15	 MUSIC OUT DISSOLVE	the children was caused by continual re-telling of the stories by their parents.
 00:29:16	 TO CAMERA	YEHUDA: Our studies had really convinced me that it was the later experiences of the child, as the child was growing up bombarded with years and years of symptoms from the parents, that accounted for the effect that we observed.
00:29:34 00:29:36 00:29:38 00:29:40 00:29:42 00:29:47	MUSIC IN C/U SECKL HOLDING DEVELOPING POLAROID SECKL REVEALS FACE MS SECKL SECKL INTERVIEWING PREGNANT WOMAN C/U RATS	 V/O: However in Edinburgh Jonathan Seckl was interested in stress exposure in pregnant women and wondered if stress effects could be transmitted to their children. He started some experiments with pregnant rats to see if exposing them to stress hormones had any effect on their offspring.
00:29:57 00:30:02	MUSIC OUT MONTAGE LAB EXPERIMENTS WITH SECKL TO CAMERA	SECKL: And we found that the next generation for the rest of the lifespan of those animals themselves had altered stress responses, and showed behaviour that looked like anxiety.
00:30:06 00:30:07	MUSIC IN MONTAGE LAB EXPERIMENTS	 V/O: To see if this was affecting the genes

00:30:16	ANIMATION MOUSE EXPERIMENT	themselves, he decided to breed them and see if the stress effects could be found in generations never exposed to the stress hormone.
00:30:19		SECKL: And their daughters and sons also got the propensity for abnormal stress responses.
00:30:25 00:30:30 00:30:32	MUSIC OUT MONTAGE LAB EXPERIMENTS CGI GENE SWITCH ANIMATION SWITCH IN MICE	V/O: For Seckl the only explanation was that a stressful event was throwing a switch on a gene which was then being inherited.
00:30:35 00:30:39	ARCHIVE FOOTAGE 9/11 DISASTER	His work might have stopped there until world events took a hand.
00:30:44 00:30:46 00:31:00 00:31:02	MUSIC IN WS BURNING TOWERS WITH CHILD ON BOAT IN NY HARBOUR	When on 9/11 the planes crashed and the towers came down Yehuda and Seckl were critically aware of the potential for the impact to be far-reaching, even affecting generations yet to be born. Ailsa Gilliam was working in a building next to the Towers.
00:31:05 00:31:08	 TO CAMERA/CAPTION IN *Ailsa Gilliam*	GILLIAM: As I left my building coming out through the doors, there was a lot of ash floating through the air and some

00:31:12	CAPTION OUT ARCHIVE FOOTAGE 9/11TOWERS BURNING	office papers. So I knew that if I looked up I may see something that I didn't want to see.
00:31:19 00:31:20 00:31:21 00:31:25 00:31:27 00:31:29 00:31:31	 TO CAMERA TOWERS BURNING TO CAMERA AILSA WITH SON ON BOAT TO CAMERA	Just the thought that people had died close to me, I broke down. I got very upset. I wanted to get out of the environment. Being pregnant I did not want to open myself up to more emotional uncertainty and emotional distress.
00:31:39 00:31:41 00:31:47	WS BURNING TOWERS 9/11 YEHUDA AT WATERFRONT	V/O: After the events of 9/11 unfolded, Yehuda and Seckl teamed up to study women like Ailsa who were pregnant at the time.
00:31:50 00:31:57 00:32:07	 TO CAMERA AILSA WITH SON	YEHUDA: There were a lot of different opportunities to examine what the effects of 9/11 would be on the children who might be born to parents who developed post-traumatic stress disorder in response to 9/11, and particularly those who had been exposed in utero.
00:32:10 00:32:21 00:32:23	ARCHIVE 9/11 FOOTAGE TOWER COLLAPSING REACTIONS OF ON-LOOKERS	V/O: When exposed to a stressful event a person produces cortisol, the hormone that helps regulate the body's response to that stress. If cortisol levels are too

		low, a person finds coping with stress very difficult and are prone to PTSDs, post-traumatic stress disorder.
00:32:40 00:32:42	 AILSA WITH SON	But could this effect be transmitted to their offspring?
00:32:45 00:32:46	 TO CAMERA	SECKL: They found nearly two hundred women of whom a number had actually been in the Twin Towers.
00:32:51 00:32:55 00:32:58	MUSIC IN AILSA & SON LOOKING AT STATUE OF LIBERTY FROM BOAT MONTAGE AILSA & SON	 Half of them developed post-traumatic stress disorder. We then looked at those women and found they had abnormal cortisol in their saliva. The most striking finding was so did their babies.
00:33:11 00:33:13 00:33:25	TO CAMERA MUSIC OUT AILSA & SON ON BOAT TO CAMERA/MUSIC IN	The argument in the Holocaust survivors had been that their children showed abnormal stress hormones because they themselves had been stressed by listening to the tales recounted by their parents of their awful exposure during the 1940's. That could not be the case with the 9/11 survivors. These babies were one year old.
00:33:33 00:33:35 00:33:41	AILSA'S SON PLAYING MUSIC OUT TO CAMERA	 YEHUDA: Nor only did infants have lower cortisol levels but they were different depending on how pregnant the mother was on 9/11.

00:33:44 00:33:53	MUSIC IN PAN TO AILSA ON BENCH TO CAMERA	SECKL: The main effect was only seen with those mothers with PTSD who were pregnant in the last third of pregnancy. Mothers with equal levels of PTSD who were pregnant in the first and second thirds of pregnancy at 9/11, there was very little effect on the baby's cortisol.
00:34:00 00:34:10	MUSIC OUT AILSA PLAYING WITH SON TO CAMERA	 YEHUDA: It suggested to us that it couldn't just be about genetics, that there was something that was being transmitted in the late stages of pregnancy where the mother's symptoms were having some effect on the development of the offspring's cortisol system.
00:34:16 00:34:19	MUSIC IN AILSA'S SON PLAYING INTERCUT WITH 9/11 SCENES OF PANIC	 V/O: It appeared that epigenetics might be responsible, that an event had altered the stress response in the children.
00:34:27	TO CAMERA MUSIC OUT	YEHUDA: What these findings did was suggest to us that we need to be looking where we hadn't even considered looking before.
00:34:34 00:34:35	AILSA WITH SON	 V/O: To know for certain that this was an

		epigenetic effect, they'll need to be sure that their observations weren't simply due to high levels of stress hormones in the womb.
00:34:46 00:34:48	 TO CAMERA	SECKL: Now - and here is the bit where we have to speculate - the animal work would suggest that this might then persist into the next generation.
00:34:55 00:34:57 00:35:04	MUSIC IN WOMAN HOLDING UP DEVELOPING POLAROID WOMAN WITH FAMILY	 V/O: If they find the same stress effects in the children's children of 9/11 then it will be clear that a genetic memory of a stressful event can travel through the generations.
00:35:10 00:35:12	 TO CAMERA MUSIC OUT	SECKL: It's the key thing next to find out, but the 9/11 population will be very, very important for us to be able to follow what is a single discreet event.
00:35:18 00:35:19 00:35:21 00:35:27	YEHUDA CONDUCTING LAB TESTS SECKL EXAMINING PREGNANT WOMEN MS AILSA	 V/O: The work of Yehuda and Seckl offers tantalising evidence of proof of inherited epigenetic effects in humans, but they need data that extends beyond just one generation.
00:35:32 00:35:35 00:35:39	MUSIC IN WS OVERKALIX PEMBREY & BYGREN IN GRAVEYARD	 The only way forward was to look back to the past. In Sweden Pembrey and Bygren had data that provided the

		chance to study the effects of famine through many generations.
00:35:47 00:35:53 00:35:54 00:36:01	MONTAGE OF HEADSTONES BYGREN IN LIBRARY MUSIC OUT C/U BYGREN AT MICROFICHE READER	Olov Bygren was looking to see if poor nutrition had an effect on health when he stumbled on something curious.
00:36:04 00:36:09 00:36:10 00:36:11 00:36:14 00:36:20	MUSIC IN B/W ARCHIVE FOOTAGE HARVESTING POTATOES B/W STILL ARCHIVE FAMILY PHOTO ZOOM INTO GRANDFATHER PAN TO GRANDCHILD FIELDS OF CORN	It appeared that a famine could effect people almost a hundred years later even if they never suffered a famine themselves. He wanted to know how this might be possible, so he asked Marcus Pembrey.
00:36:27 00:36:32	MUSIC OUT TO CAMERA	PEMBREY: Olly first reported that the food supply of the ancestors was affecting the longevity or morality rate of the grandchildren so I was very excited. I responded immediately.
00:36:41 00:36:42	BYGREN AT MICROFICHE READER & C/U FILES	V/O: Pembrey had a hunch that the incidence of one disease, diabetes, might be an indicator that epigenetics was involved.

00:36:50	TO CAMERA	PEMBREY: Specifically I wanted to know the results of the diabetes because this was the one that I thought might involve the imprinting.
00:36:57 00:36:59 00:37:03 00:37:09	BYGREN AT MICROFICHE READER BYGREN IN LIBRARY C/U LIBRARY RECORDS	 V/O: So Olov trawled the records for any deaths due to diabetes and then looked back to see if there was anything unusual about the diet of their grandparents.
00:37:12 00:37:14 00:37:34	 TO CAMERA MUSIC IN C/U MICROFICHE PAGES	PEMBREY: A few months later he e-mailed me to say that indeed they had shown a strong association between the food supply of the father's father and the chance of diabetes being mentioned on the death certificate of the grandchild. So of course I was really rather excited by that because it really did look as if there was some trans-generational effect going on there.
00:37:40 00:37:48 00:37:53	B/W ARCHIVE FAMILY PHOTOS WITH MICROFICHE OVERLAY MICROFICHE OUT	 V/O: It looked as if there were clear links through the generations between grandparents and grandchildren. They found that the life expectancy of grandchildren was being directly affected by the diet of the grandparent.
00:37:54	MONTAGE GRAVEYARD	

00:37:56 00:37:58 00:38:01	& HEADSTONES WS OVERKALIX C/Us HEADSTONES & CROSSES	It appeared that Overkalix held the key to finding the first evidence of epigenetic inheritance in humans.
00:38:05 00:38:07 00:38:10	B/W STILL OLD LADY & OTHER FAMILY MEMBERS MUSIC OUT TO CAMERA	PEMBREY: It really did look as if there was some new mechanism transmitting environmental exposure information from one generation to the next.
00:38:12 00:38:15 00:38:25	MUSIC IN PEMBREY & BYGREN IN GRAVEYARD LOOKING AT HEADSTONES	V/O: Because these ideas were so heretical, Pembrey knew that the results could be dismissed as nothing more than a curiosity. They needed to get an understanding of how this was happening. How could the grandparent capture the information that was affecting the grandchildren?
00:38:37 00:38:40	MUSIC OUT TO CAMERA	PEMBREY: We wanted to tease out when you could trigger in the ancestor a trans-generational response.
00:38:45 00:38:52	C/Us BYGREN & PEMBREY CHECKING DATA C/U FICHE FILES	V/O: So he and Bygren went back to the data and looked again. The more they looked, the more patterns started to

		appear.
00:38:54 00:38:55 00:39:01 00:39:04 00:39:09 00:39:17 00:39:21	MUSIC IN B/W ARCHIVE FOOTAGE PLOUGHING FIELDS B/W ARCHIVE CORN FADE TO MODERN DAY FIELD MUSIC OUT/TO CAMERA B/W ARCHIVE PHOTOS LOCALS MUSIC IN	 PEMBREY: We were able to look at the food supply every year in the grandfather and the grandmother from the moment they were conceived right through until the age of twenty. We found that there are only certain periods in the ancestor's development when they can trigger this trans-generational response. They're what one might call sensitive periods of development.
00:39:23 00:39:24 00:39:29 00:39:33 00:39:36 00:39:38 00:39:42	B/W ARCHIVE FOOTAGE CORN GROWING B/W ARCHIVE STILL GRANDPARENTS C/U GRANDMOTHER ARCHIVE STILL OF FOETUS C/U GRANDFATHER ARCHIVE C/U BOY ARCHIVE	 V/O: They discovered that when a famine was able to trigger an effect was different for the grandmother than the grandfather. The grandmother appeared susceptible while she herself was still in the womb. While the grandfather was affected just before puberty.
00:39:44 00:39:45 00:39:48	LAKE SCENE FADE FROM B/W TO COLOUR MUSIC OUT/TO CAMERA	 PEMBREY: And the timing of these sensitive periods was telling us that it was tied in with the formation of the eggs and

00:39:52	MUSIC IN	the sperm.
  00:39:55 00:39:59 00:40:00	C/U B/W STILL ARCHIVE BOY FADE TO C/U SPERM & EGG B/W ARCHIVE CORN INTERCUT WITH SWIMMING SPERM & EGG & CELL DIVISION	 V/O: This was critical because now they knew how it was happening. Environmental information was being imprinted on the egg and sperm at the time of their formation.
00:40:08 00:40:14 00:40:16 00:40:18 00:40:23 00:40:24	MEADOW FLOWERS & LAKE VIEWS BYGREN AT MICROFICHE READER MONTAGE GRAVE HEADSTONES C/U BYGREN WS BYGREN IN LIBRARY	At last a clear picture of an inherited environmental effect was beginning to emerge. All they needed to do now was to compile their findings. Bygren drew up a rough diagram and sent it to Pembrey.
00:40:28 00:40:29 00:40:31 00:40:40 00:40:42	MUSIC OUT PEMBREY AT LAPTOP C/U DIAGRAM TO CAMERA C/U DIAGRAM TO CAMERA	 PEMBREY: Hand drawn, this is what Olly sent me, you know he was too excited to wait for the thing to be drawn out properly. You know he sent me the data and in fact I was recovering from having something done on my heart so he sent it saying you know I hope this helps you get better quickly you know because it was so exciting.
00:40:47	MUSIC IN C/U DIAGRAM	

00:40:48 00:40:49	 C/U PEMBREY & C/Us DIAGRAM	V/O: When Pembrey plotted out the diagram he was immediately struck by its significance.
00:40:55 00:41:01 00:41:09	 TO CAMERA B/W ARCHIVE STILL LOCALS & OVERLAY DIAGRAM	PEMBREY: Once I had plotted out the full extent of those results, it was so beautiful and such a clear pattern I knew then quite definitely that we were dealing with a trans-generational response. It was so coherent and that's important in science, that the effect was coherent in some way was tying in when eggs and sperm were being formed.
00:41:20		V/O: The diagram showed a significant link between generations, between the diet in one and the life expectancy of another.
00:41:30	TO CAMERA	BYGREN: When you think that you have found something important for the understanding of diseases itself, you can imagine that this is something really special.
00:41:44 00:41:47 00:41:49	BYGREN IN LIBRARY TO CAMERA	 PEMBREY: It's up there with, I'm a sort of fair weather supporter of Liverpool, it's up there with Liverpool winning the Champions' League.
00:41:53 00:41:55	BYGREN IN LIBRARY TO CAMERA	 BYGREN: You can only have it once in your lifetime.
00:41:58	TO CAMERA WITH	PEMBREY:

	DIAGRAM	This is going to become a famous diagram, I'm convinced about that. I get so excited every time I see it. It's just amazing.
00:42:05	C/U DIAGRAM TO CAMERA	Every time I look at it, I find it really exciting. It's fantastic.
00:42:10 00:42:12 00:42:14 00:42:17 00:42:18 00:42:26 00:42:30 00:42:33	PEMBREY HOLDING UP DEVELOPING POLAROID BYGREN HOLDING UP DEVELOPING POLAROID B/W ARCHIVE FOOTAGE THRESHING CORN ARCHIVE FOOTAGE PROJECTED ONTO BODIES OF CHILDREN C/U EGG & SPERM B/W ARCHIVE OF HARVEST PROJECTED ONTO BODIES OF CHILDREN	 V/O: Pembrey and Bygren have the first conclusive proof of an environmental effect being inherited in humans. The impact of a famine being captured by the genes in the eggs and sperm and the memory of this event was being carried forward to effect the grandchildren generations later.
00:42:42 00:42:47	MUSIC OUT TO CAMERA	PEMBREY: We are changing the view of what inheritance is. You can't in life in ordinary development and living separate out the gene from the environmental effect. They're so intertwined.
00:42:57 00:42:58	B/W ARCHIVE WAR SCENES PROJECTED ONTO BODIES OF CHILDREN	 V/O: Pembrey and Bygren's work showed clearly that what our grandparents ate

00:43:04 00:43:07 00:43:09 00:43:10 00:43:13 00:43:17 00:43:20 00:43:24	 B/W ARCHIVE CHILDREN EATING ARCHIVE FIRE SCENES MUSIC IN C/U SKINNER HOLDING DEVELOPING POLAROID LS SKINNER ZOOM INTO C/U US CORNFIELDS CROP SPRAYING MUSIC OUT C/U SKINNER IN LAB	could affect our health. Increasingly it appeared as if all sorts of environmental events were capable of affecting the genes. And in Washington State, Mike Skinner stumbled on some results with profound implications. He triggered an effect with commonly-used pesticides and fungicides. He exposed a pregnant rat with a high dose of one of these pesticides and then looked for effects in her offspring.
00:43:31 00:43:36	ANIMATION MOUSE EXPERIMENT TO CAMERA	SKINNER: And so I treated the animals, the pregnant mother with these compounds and then we started seeing between six months to a year a whole host of other diseases that we didn't expect and this ranged between tumours such as breast and skin tumours, prostate disease, kidney disease, and immune dysfunctions.
00:43:53	ANIMATION OF MOUSE EXPERIMENT	V/O: He bred these rats to see if these effects persisted into subsequent generations.
00:44:01		SKINNER: The next step was for us to go to the next generation and go to the third generation out in the same disease state occurs so after we did several

00:44:13 00:44:15	TO CAMERA MUSIC IN	repeats and got the third generation showing it and then a fourth generation, we sat back and realised that the phenomenon was real.
00:44:17	SKINNER IN LAB WITH COLLEAGUE	We started seeing these major diseases occur in approximately 85% of all the animals of every single generation.
00:44:25	HELICOPTER CROP SPRAYING	V/O: His discoveries were a revelation.
00:44:29 00:44:30 00:44:34 00:44:39 00:44:45 00:44:47 00:44:48 00:44:49 00:44:59 00:45:08	 TO CAMERA HELICOPTER CROP SPRAYING TO CAMERA SKINNER HOLDING DEVELOPING POLAROID C/U SKINNER MS SKINNER & FAMILY TO CAMERA SKINNER IN LAB WITH COLLEAGUE TO CAMERA	SKINNER: We knew that if an individual was exposed to an environmental toxin that they can get a disease state potentially. The new phenomenon is that when environmental toxin no longer affects just the individual exposed but two or three generations down the line. I knew that epigenetics existed. I knew that it was a controlling factor for DNA activity whether genes are silenced or not but to say that epigenetics would have a major role in disease development - so I had no concept for that. The fact that this could have such a huge impact and could explain a whole host of things we couldn't explain before, took a while to actually sink in.
00:45:10 00:45:13	TRACTOR CROP SPRAYING ANIMATION MOUSE EXPERIMENT	 V/O: The exposure of a single animal to a toxin was causing a whole range of

 00:45:23 00:45:25 00:45:27	 FAMILY GROUP BY EROS STATUE BLACK FAMILY GROUP C/U CAUCASIAN GIRL PAN TO SISTER	diseases in almost every individual of the following generations. And because epigenetic effects have been observed in humans, this may have implications for us too.
00:45:30 00:45:32	MUSIC OUT LARGE FAMILY GROUP TO CAMERA	SKINNER: What this means then is what your grandmother was exposed to when she was pregnant could cause a disease in you, even though you've had no exposure and you're going to pass it on to your great-grandchildren.
00:45:40 00:45:46 00:45:53	MUSIC IN MONTAGE OF FEATURED SCIENTISTS HOLDING DEVELOPING POLAROIDS MONTAGE C/Us SCIENTISTS B/W ARCHIVE FOOTAGE PROJECTED ON BODIES OF CHILDREN	 V/O: The work of these scientists is at last throwing a spotlight onto the mysterious hidden world of epigenetics. They appear to show that the lives of our ancestors have a capacity to affect us directly.
00:46:04 00:46:05 00:46:11 00:46:13 00:46:19	 TO CAMERA FAMILY GROUP – LONDON C/U FATHER PAN TO REST OF OFFSPRING THEN MOTHER TO CAMERA	SECKL: These results are provocative. Some find them, them difficult to accept. But it's quite clear now that a number of laboratories are finding similar findings in the various systems that they are interested in. So the

		phenomena are there.
00:46:24 00:46:26 00:46:29 00:46:32 00:46:35	FAMILY GROUP C/U BABY C/U MOTHER C/U SISTER	V/O: Epigenetics has the capacity to reach into every aspect of our lives and links our past present and future in previously unimagined ways.
00:46:37 00:46:40	PAN TO FATHER TO CAMERA	SKINNER: I think this will be the next revolution in molecular biology. This is, this really could be a paradigm shift we did not expect. It could explain a lot of things.
00:46:45 00:46:58	TO CAMERA B/W ARCHIVE IMAGES PROJECTED ONTO BODIES OF CHILDREN	REIK: There are many diseases, very common diseases such as Alzheimers disease of the brain, diabetes which are very difficult to explain currently genetically. Maybe a lot of these kind of very common diseases are actually caused by epigenetic switches.
00:47:06 00:47:07 00:47:08 00:47:09 00:47:10 00:47:13 00:47:15	C/U MOTHER C/U DAUGHTER C/U BROTHER C/U REIK FAMILY GROUP OTHER FAMILY GROUP	V/O: We are just at the beginning. There is much that is unknown but what is clear is that it will change the way we think about ourselves for ever.
00:47:17 00:47:18 00:47:20 00:47:24	C/U PEMBREY HOLDING DEVELOPING POLAROID MS PEMBREY & WIFE MUSIC OUT TO CAMERA	PEMBREY: I've thought of nothing else really for the last five years. It is said the first time you know one had a photograph of the earth you know this sort of delicate thing sailing through, sailing

00:48:05		through the universe you know, it had a huge effect on the sort of save the planet type of feeling. You know I'm sure that's part of why the future generation think in a planetary way, because they've actually seen that picture you know and this might be the same. It, it, it may get to a point where they realise that you live your life as a sort of - I don't know, as a sort of guardian of your genome. It seems to me you've got to be careful of it because it's not just you. You can't be selfish because you can't say well I'll smoke or I'll do whatever it is because I'm prepared to die early. You're also looking after it for your children and grandchildren.
00:48:29	CREDITS IN	

APPENDIX 2

TRANSCRIPT OF *HOPEFUL MONSTERS: AN EXPERIMENT*

000000	TITLE	
		WILLIAMSON: I have an edited...
00:00:19	DON WILLIAMSON shuffles across his sitting room to the T.V.	...version of the video
		I think this is it...'Don Williamson', yes
		...I'll need the set on, of course.
	He inserts a video tape and heads back to his chair. The tape starts to play.	
00:01:14		V/O STEPHEN FRY: So, for a bit of fun with genetics now. What do you get if you cross a
00:01:21	The TV set - An episode of *Qi*	caterpillar with a butterfly?
		ALAN DAVIES: A Butterpillar.
		STEPHEN FRY.: Oh! Should have said the other one
	WILLIAMSON raises the volume with his remote control.	ALAN DAVIES: Catterfly!
	The TV set.	STEPHEN FRY: Oh!
		ALAN DAVIES: I feel such a fool!
		JIMMY CARR: Can I just say, I'm reading a book at the moment and I haven't finished it about a very very hungry caterpillar…I think I kind of might know where it's going but I don't want to spoil it.
		CLIVE ANDERSON: So are you saying that there's some

		species that reproduces half way through its life-cycle…
		STEPHEN FRY No, there is a theory which a man has put forward which is that actually they are different species. I know this sounds insane -
	WILLIAMSON watches, smiles. The TV set.	JIMMY CARR: Oh, what he’s done there is he’s not understood. That’s what’s happened…and I mean fair enough because it is complicated and you might not… Was it Alan that put this forward?
		STEPHEN FRY: His name, I’ll tell you his name. His name is Donald Williamson, formerly of the University of Liverpool. It’s called Hybridogenesis apparently. Now it does seem pretty off the wall to say that they are two different species, but he has some-
		CLIVE ANDERSON: That’s a fantastic idea though
	WILLIAMSON watching.	STEPHEN FRY: It is an amazing idea
		JIMMY CARR: You know but sometimes you see an old guy like in St. Tropez like with a really beautiful young girl and you think well maybe…could be a similar thing-maybe the caterpillar’s had a lot of money.
	The TV set.	JAN RAVENS: There’s no such thing as an ugly rich bloke.
		STEPHEN FRY His star witness in this is a starfish called Luidia Sarsi. It starts life as a small larva with a tiny starfish inside. As the larva grows the starfish migrates to the outside and the larva settles on the seabed and they separate. This is normal. But in this one something remarkable happens. Instead of degenerating the larva

00:03:20	WILLIAMSON watching.	swims off and lives for several months as an independent animal. It’s as if the caterpillar and the butterfly were alive at the same time. And he reasons…the point is that for millions upon millions of years particularly in the sea, sperm and seed have been mixed hundreds and thousands if not millions of different species and just once every million years they happen to create a double species. He thinks it’s not impossible. We’re not sure whether we believe it but we’re intrigued by its possibilities. Now to something completely disconnected
00:03:28 00:04:03	A large room, lit from translucent windows. In front of us DON hauls himself onto a low walkway of wooden slats. The sound of flowing water is loud. Title: “Ten years Earlier…” DON makes his way to the far end of the room where he reaches a deep metal tank, bends over it and looks inside. A star fish DON examines the starfish clinging to the side of the tank beneath the water, then, standing back as if to make room for someone behind the camera… ROBERT appears from off-screen, brandishing a net on a pole with which he fishes out a large starfish and hands it to Don. DON injects the animal with a small syringe before returning it to the water. It sinks down to the bottom of the tank (about four feet). The two men repeat	WILLIAMSON: That big one down there looks promising. WILLIAMSON: You’ve got it. WILLIAMSON: Oh, it’s having a crab for dinner. WILLIAMSON: Small, edible crab…and I’m

00:05:00 00:06:13	the process with three more starfish, DON looks directly at the camera ROBERT leaves and we watch Don make his way much more slowly between the tanks of water, stepping down from the walkway and shuffling past some smaller aquaria in which are hermit crabs and an octopus. He lifts a small white bucket and carries it before him into "the room".	interrupting its dinner to induce it to spawn. Inject about 1cc…and put it back. WILLIAMSON (to ROBERT): Thank you. WILLIAMSON: Very much trial and error. Find a soft bit, there we go. I hope that within an hour they will turn their thoughts to reproduction. Before they spawn, they will crawl up to the surface, or near the surface, and – which is very convenient, and we can then put them on the rack and hold a can underneath to collect the drips. That's all for now. ROBERT: Okay, so what's the next step for us? WILLIAMSON: Next step, go back to the room and do things to the heart urchins
00:07:44 00:08:00	DON places the white bucket on the side of a large basin. He takes two urchins out of the bucket and places them on clear watch glasses that are on the side of the basin. There are others of these watch glasses that have been previously left here. DON moves an urchin from the top of one of the watch glasses on to the top of another one, picks up the watch glass on its own, turns around and takes it	WILLIAMSON: We can examine them now, see if we have eggs or sperm.

	over to the work surface opposite the basin. He places the watch glass under the microscope and sits down to view it. CU of DON'S hand as he tries to view the contents of the	
00:08:18	watch glass under the microscope.	WILLIAMSON: Water with ciliates. But no sperm, or eggs.
	DON turns the microscope off and gets up. He shuffles back over to the large basin and throws the contents of the watch glass into it. He takes an urchin off one of the other dishes, picks up the watch glass and takes this over to the microscope, placing the watch glass under it. He sits down and views the contents of the watch glass using the microscope.	ROBERT:
00:09:00	CU of watch glass under microscope.	Can you see anything in that one Don? WILLIAMSON: Err, again ciliates, and sand grains. But neither eggs nor sperm. ROBERT: What does 'ciliates' mean?
	CU of DON looking down microscope.	WILLIAMSON: Ciliated protozoa, single-celled animals. ROBERT: That's a bad sign is it? From our point of view?
	CU of watch glass under microscope.	WILLIAMSON: They tend to accumulate if there's anything rotting or going a bit off. Like flies do on land.
	CUs of his face peering down microscope, his hand adjusting the focus.	ROBERT: So our sea urchins are probably dead then?
	DON takes the watch glass off	WILLIAMSON:

00:09:46	the microscope and turns the microscope off. He washes the watchglasses under the tap and lays them beside the sink to dry	Yes.
00:09:57	DON empties the white bucket of sea urchins into one of the big tanks we saw previously, which contains the starfish. DON's face is reflected on the surface of the water in the tank.	
00:10:13 00:10:41 00:11:30	DON dials a number using a wall-mounted phone and holds the receiver to his ear. We see ROBERT in the 'tank room' whilst hearing DON's side of the telephone conversation. We cut back to DON on the telephone, then back to ROBERT, who is decanting the starfish out of the large tank using a net and into a large white bucket on the side of the tank. Back to DON on the telephone. Cut back to ROBERT, who puts down the net and takes the white bucket from the side of the tank. He disappears off camera. We hear gulls in the background. Cut back to DON on the telephone. He ends the call and places the receiver back.	WILLIAMSON: Hello Mike, I thought you would have gone – Don. We would like more *Marthasterias*. This batch seems to not be in spawning condition WILLIAMSON: Ah well, today nothing worked. Our stock of *Echinocardium* were all dead. So that was, err, useless. And the stock of starfish which were collected by divers last week were all healthy, but although we injected six of them, none of them spawned. I think we can get more heart urchins. We can dig in the sand at low water at Darby Haven. But I think we'll probably have to go for another species of starfish. Mmm, the ones that have produced have been medium to big. But the really biggest ones have not produced and the really smallest ones have not produced. It could be just luck. WILLIAMSON: Right, thank you. Thank you, bye.

00:11:53	We follow ROBERT (holding the white bucket) walking past the outside ponds, through the front gate, and down a steep flight of stairs which leads to the sea. He pours the contents of the bucket (including the starfish) into the water.	
00:12:30	DON is flicking through a book ('The Handbook of Echinoderms of the British Isles'). We cut between CUs of his face and the pages.	WILLIAMSON: Hmm.
00:13:03	Cut to outside, a man is walking along a jetty towards a boat. Another man, onboard the boat, is clothed in diving gear. The boat departs from the jetty. We see another man is also clothed in diving gear.	DEREK: Take her out, you want to take her out Dave? MIKE Derek? DEREK Yeah?
00:14:13 00:15:05	We are back in the room with DON and a photographer taking pictures of DON seated at his microscope. The photographer lowers the camera.	PHOTOGRAPHER: Would it be possible, could you manage just to get in a little bit closer to the microscope. WILLIAMSON: How near? PHOTOGRAPHER; Little bit closer I think, would you normally work closer? WILLIAMSON: No. About there. PHOTOGRAPHER: I know it's a little bit awkward for you. That's better. Okay…at the camera now. That's good. And again. Cheer up a little bit. That's fine, thank you very much indeed. Right, how long are you actually working at the biological station for? WILLIAMSON: Well, I've got to the stage when I can only work with somebody to work

		me. Robert is going to London tomorrow, he'll be back in about ten days and do another three or four weeks work and that will be all for this session. But we might be able to resume it next Spring. PHOTOGRAPHER: Next Spring, right. Okay, could we have Robert in one of the photographs do you think?
00:15:49	View from the bench: ROBERT's arm blocks the shot. We see the photographer in the background.	ROBERT (off camera): I'm totally incidental to this process, I'm merely a pair of hands. WILLIAMSON: Well, I can't work without a good pair of hands so you're essential. PHOTOGRAPHER: So do you actually live on the island, do you? WILLIAMSON: Yes in Port Erin. I've lived here for forty, fifty years. Port Erin and Port St Mary. I've worked here all my working life. PHOTOGRAPHER:
	Camera has been picked up by ROBERT again. We see the photographer and DON talking.	I don't know how I haven't come across you before? WILLIAMSON: Well, I've been here…
00:16:22	CU of DON, then a girl. DON puts his glasses on. We see another man in the room, who DON is speaking to. CU of book.	WILLIAMSON: This is my one and only book…That looks like a caterpillar. I would take it for a caterpillar, but it is the larva of a wood wasp, which is nothing like a butterfly or a moth. In fact, the wood wasp is related to the stinging wasp and bees and wasps, and bees and wasps have grubs, not caterpillars.
00:17:02	Camera pans up from book to DON's face.	REPORTER: Yeah. WILLIAMSON: So we get these forms turning up in different groups. Under conventional
	Shot of the man holding a notepad, interviewing DON.	theory, this is quite inexplicable, if the larva and the adult evolved

00:17:30 00:17:56	DON flicks through the book. The reporter looks at the book off screen on Don’s lap. CUs of book and REPORTER’s face.	together. And I puzzled over this for years and years and years and I used to lecture to students and point out there’s several other anomalies, like this, of species apparently having the wrong larvae which couldn’t adequately be explained. In many, many invertebrate animals the larva does not simply ‘develop into’ the juvenile. In the case of *Luidia sarsii*, the starfish juvenile started to form within this very big larva, it eventually migrated to the outside and then dropped off. REPORTER: Right. WILLIAMSON: But the larva repairs any damage done by the juvenile and it goes on swimming. And there are two recorded cases of this taking place in an aquarium and the larva has gone on swimming for a further three months after the juvenile has separated from it. REPORTER: Right.
00:18:35 00:19:00	CU of REPORTER scribbling notes on his notepad. REPORTER’s face. CU of DON’s face.	WILLIAMSON: I think it could not possibly have evolved by natural selection and gradual changes over thousands and millions of years. REPORTER: Yeah. WILLIAMSON: And I say this as a clear indication that the larval…form…and the juvenile form originated as two distinct genomes. REPORTER: Yep. WILLIAMSON: And the fact that they can exist simultaneously, side by side, is fully consistent with my view. There’s the larval genome and the juvenile and adult genome. And they were distinct
00:19:32	REPORTER’s face.	

		right from the start and have remained so, to this day. It was never taken seriously that larvae can be swapped from one group to another. But this seemed to be the only explanation for the facts as I saw them.
	DON’s face.	REPORTER: Yep.
00:19:59	REPORTER’s face. DON’s face.	WILLIAMSON: And I eventually hit upon the idea, the most convenient way to get a larva from one group to another is by hybridising. A member of one group will hybridise with a member of the other and the larva will be transferred…hopefully.
00:20:29	Shot of both DON and REPORTER.	REPORTER: Is there, is there any particular reason why a species would hybridise? Is that, I mean…
	CU of notepad. CU of REPORTER writing. CU of girl. Shot of DON	WILLIAMSON: Well, they normally don’t, as is well known. We’re talking for the most part about species which…release their eggs and sperm into the sea and they mix. And, the egg of one species is always more readily fertilised by the sperm of the same species.
00:21:00		REPORTER: Yeah.
  00:21:30	Pan to CU of REPORTER. Pan to shot of DON.	WILLIAMSON: But I’m theorising that once in a million years, or even once in ten million years. The first sperm that that egg would come up against would be the sperm of another species and we’d get a successful hybridisation. And it’s only necessary, millions and millions of times in between, one species would breed with the same species. But just once in ten million years…you get a hybrid.
		REPORTER: Through sheer random chance?
		WILLIAMSON: Yes, from sheer random chance, as

	Shot of REPORTER.	you say. And that's all that is required for my theory.
00:22:00	CU of book. CU of REPORTER writing CU of the book	REPORTER: But I always thought species couldn't interbreed in that way under such sort of different. I mean, from my point of view, I would have thought the DNA would have been sufficiently dissimilar to prevent that happening.
00:22:33	CU of REPORTER, then of REPORTER's notepad. Tilt up to REPORTER'S face. CU of book. CU of REPORTER's face. Pan to DON's face.	WILLIAMSON: Well, years ago, well ten years ago. I got eggs from a sea squirt, which is a group vaguely related to the vertebrates, and of course we are vertebrates. And I fertilised these eggs with the sperm of a sea urchin, a very different group with very different larvae. The sea squirt would normally have a tadpole larva…and the sea urchin has a larva that swims with its own cilia…very fine, undulating whiskers, if you like. Well, nothing like a tadpole.
00:23:00		REPORTER: Right.
00:23:30	REPORTER's face, both REPORTER and DON in shot, REPORTER's face. DON puts glasses on and closes the book.	WILLIAMSON: Well, in spite of many failures, one of my attempted hybridisations worked, and I get 3000 eggs…these are ascidian eggs…sea squirt eggs that would normally hatch as tadpoles, they all hatched as little ciliated larvae and in fact very definitely sea urchin larvae from a sea squirt egg.
00:24:03 00:24:30	REPORTER's face, then to CU of his notepad. DON's face.	WILLIAMSON: The sceptics say this was a fluke. It couldn't really have worked, although I have…since explained in detail all my experimental methods and I took great precautions to avoid mixing cultures and all this sort of thing. So, I claim that I have a theory of evolution, which I'm not knocking Darwin. Darwin also had a theory of evolution, and a very good one, and he could explain about three quarters of evolution. But in the middle of the nineteenth century, when he published his famous book, very little was known about marine larvae, or

	REPORTER's face.	about larvae in general.
	DON's face.	REPORTER: Yep. WILLIAMSON: And had Darwin the knowledge that we have today, he would have written his book very differently. In fact, he would have incorporated my book in his.
00:25:00	REPORTER's face, then notebook.	REPORTER: [Laughs]
	DON's face.	WILLIAMSON: I'm not blaming Darwin, it was just the knowledge of his day. He was explaining the evolution of adults. I am trying to explain the evolution of larvae.
00:25:30		REPORTER: So what would be next, what would you hope to achieve next with the theory?
00:25:48 00:26:00	REPORTER's face. DON's face.	WILLIAMSON: Well, the majority of biologists ignore my views, but there is a minority that support them, and some very enthusiastically, and I'm very grateful to them…and they are spread about the world, so I, in fact there are more in the United States and Korea and Japan than in the UK.
	REPORTER's face.	REPORTER: Right.
	DON's face.	WILLIAMSON: I am satisfied in my own mind, but other people aren't, so what we want is more genuine hybrids. And Robert and I are trying to produce some right now.
		REPORTER: Great. Well, I hope one day to be able to say I've met someone whose theories were as important as Darwin's.
		WILLIAMSON: Well, I don't expect you will

		but…mine is a PS to Darwin. REPORTER: [Laughs]. Right. Well thank you very much and very good luck with it. Thanks very much for agreeing to be interviewed and photographed today. WILLIAMSON: You're welcome. REPORTER: Thank you.
00:26:29	We are on a boat with two men. We see the divers (that we saw earlier) on the surface of the water ~30 metres away from the boat.	MAN 1: There they are. MAN 2: Divers up.
00:27:24 00:28:00 00:28:30	A car engine is started. CU of man's hand. CU of DON's face. Shot looking out of the windscreen of the car, ROBERT can be seen in the rear view mirror. The car drives along the sea front and through the town	ROBERT: Don, where did you study originally? WILLIAMSON: I went to Newcastle University which was then part of the University of Durham, in 1940. And my university studies were interrupted by the war. I did two years and then I went into the navy for the next two years. But I got ill in the navy and I was invalided out, so in 1945 I came back to college in Newcastle, finished my original degree, and went to do a PhD. After that I got a job with Liverpool University but not at Liverpool, in the Isle of Man where they have, and still have, a marine biological station. ROBERT: Was it unusual in your family? I mean, to do science.

00:29:00		WILLIAMSON: Well, from the biological point of view, my father was a very keen amateur naturalist, so the- that side of it I got from him. ROBERT: What was your father? WILLIAMSON: He was a school teacher, a rule school teacher, a village school master in North Northumberland. And he used
00:29:30	The car reaches a junction at the top of a hill and prepares to turn left.	to…He was very keen on birds and very keen on pondlife in general, freshwater life. And he used to keep freshwater aquaria and that was all the sort of thing that interested him. Of course, my original intention was to teach, like my father, but I would have been a teacher of biology. But when I contracted tuberculosis during the war, one of the – well it was the
00:30:10	The car turns right	doctor at the sanatorium – were discussing what I would do when I was fit and I said I intended to teach. He says, “not a good idea…there’s always the possibility that your disease will become active again and
00:30:30		you could then be potentially infecting school children”. And of course I had to acknowledge the truth of this. But I suppose if I had been a school teacher, it’s very unlikely that I would have come up with my own evolutionary theory.
00:30:56	DON’s face. Zoom out to reveal that he is in car.	
	Sandy beach at low tide. DON in car. Sandy beach at low tide.	WILLIAMSON: It’s rather…not very low spring tide tonight but it should be low enough. And you don’t have to go right to the water’s edge. Anywhere where it’s sandy and digable. You look for very small indentations in the sand. You dig up a fork full of sand and if you’re lucky there will be one or two heart urchins in it.
00:31:37	ROBERT comes into frame holding a pitchfork. DON in car. ROBERT walking down the	You’ve plenty of time, half an hour before low tide.

	sandy beach with the pitchfork and a big white bucket. A Time Cut	
00:32:04	ROBERT back at car showing DON what he has in the bucket. ROBERT closes car door. ROBERT is driving, we see the road ahead through the windscreen with ROBERT in the rear view mirror. The car enters a suburban housing estate and parks in front of a garage door.	WILLIAMSON: Right, ok.
00:32:43	Shot of a bungalow.	WILLIAMSON: My father was a compulsive reader.
00:33:00	DON sitting down in a room of the bungalow. Shot of bookshelf, CU of the book.	There were not many books of, his own books on general science although he had a copy of Darwin's 'Voyage of a Naturalist' which I have inherited and it's up there.
00:33:30	DON gets up and gets the book down from the shelf. He sits down again.	WILLIAMSON: I suspect it was a college set book that he had to read. I don't know. I think he thoroughly enjoyed reading it anyway and he was…He knew about evolution…he loved to talk to the children…it was not in any syllabus, but he would talk about the evolution
00:34:00	Zoom in towards DON. Camera follows DON's gaze then Tilt down to the book. CU of DON's face	and the way animals in particular – to a certain extent plants but particularly animals – had evolved. And here we have Dad's 'Voyage of a Naturalist' with 'questions on the book'.
00:34:30	CU of the book.	Well, at random: 'describe the habits of the condor'; 'describe methods of coal mining in Chile and conditions of mines in general in that country, explain why the condition of the
00:35:00	Tilt up to DON's face.	agricultural labourer there is worse than that of miners'; 'describe the inhabitants of New Zealand'; 'give an
00:35:30		account of the inhabitants of Tierra del Fuego'; 'how does Darwin account for the diversity and the

	DON flicks through the book.	quantity of vegetation in South America?'. And the answers are on page 44 to 45 but what the answers are I do not know, but there's that sort of thing. And there are marginal marks and occasional marginal comments throughout the book.
00:36:03 00:36:25 00:36:53	A white tiled annex at the marine labs. DON is filling a white bucket with water from a large basin. ROBERT comes into shot and picks some water up in the bucket he is holding and places some starfish into it. ROBERT exits with two buckets, through a small alleyway between two buildings, and into the tank room. DON is following behind. DON releases the starfish into one of the tanks. ROBERT injects the starfish.	ROBERT: The divers did well. WILLIAMSON: Yes. WILLIAMSON: We'll see which if any release eggs and sperm.
00:37:24	ROBERT lays out empty watch glasses on to the work surface in the other room. He covers each with a plastic mesh. We hear the sound of running water, then cut to a shot of 2 white buckets being filled with water by ROBERT. He is removing urchins from them at the same time. Cut to the urchins being placed on the mesh over the empty watch glasses.	

00:38:13 00:38:35 00:39:00 00:39:30 00:40:00	CU of one of the urchins. ROBERT then injects it. MS of ROBERT injecting the rest of the urchins. CU of the urchins DON shuffles into the room. CU of DON's face Cutaway of DON's hand, then shot of his face. DON places one of the watch glasses (minus netting and urchin) under a microscope CU of microscope DON looking down microscope ROBERT looking at the rest of the urchins still on the watch glasses. CU of one of the urchins DON places another watch glass under the microscope and looks down it. CU of 'big blob of sperm' DON looking down microscope DON takes the watch glass from under the microscope and places it on the workbench opposite.	WILLIAMSON: Something. ROBERT: Something came out? WILLIAMSON: Yes, err, white stuff WILLIAMSON: Oh hundreds of eggs…no shortage of eggs from now on. Marvellous. ROBERT: There's something white in there WILLIAMSON: It's a big blob of sperm, very concentrated. That's marvellous. WILLIAMSON: If only the starfish will do their stuff, the *Echinocardium* have done their's (without the apostrophe?). That sperm, very concentrated. So it goes over here with the rest of the males
00:40:00	Three different CUs of a starfish in one of the tanks. DON peering down into one of the tanks CU of a starfish DON and ROBERT both	ROBERT: Don, this one's releasing something, I'm sure of it…should I take it out?

    00:40:32 00:40:57 00:41:34 00:42:00	peering over the tank. ROBERT picks up a net. DON peering over tank alone ROBERT lifts starfish out of the tank with the net. He hands it to DON (off-camera). CU of starfish in DON's hand. Followed by CU of DON's face. Shot of starfish upside down in a plastic drinking cup. CU of starfish on the side of the tank DON peering down into tank CU of starfish in the cup, DON takes it off the cup and pulls off some tube feet. ROBERT watches him do so. The starfish is put back in the tank, where it sinks to the bottom. DON smiles at the camera (CU), then makes his way out of the tank room (MS).	WILLIAMSON: Yes.
  00:42:30 00:43:00	DON pours the contents of the plastic cup into a watch glass and places it under a microscope. DON peers down microscope. CU of down the microscope – the eggs. DON empties a watch glass into a bowl. He carries it across the room and empties the contents in to a filter on a retort stand.	WILLIAMSON: Eggs of *Marthasterias glacialis*…large starfish. And we have thousands, probably hundreds of thousands of eggs. WILLIAMSON: So that's *Echinocardium* sperm from two males.

00:43:32	DON looks at his watch. ROBERT and DON peer over the dripping coming from the filter.	WILLIAMSON: And the sperm is slowly dripping through ROBERT: So the idea is to keep the eggs immersed in concentrated sperm for, ten minutes? WILLIAMSON: At least ten minutes, try twenty.
	CU of dish being placed under vessel to catch the drips.	
00:43:55	DON waits. ROBERT looking out of the window. Shot of a boat passing along the water outside, POV shot. ROBERT looking out of window. Shot of two cyclists riding past outside, POV shot.	
00:44:30	ROBERT looks down and empties the dish that has been collecting the drips back into the vessel above. DON looking at diagrams in a book. CU of DON's watch. DON sitting down at the microscope	WILLIAMSON: So we're just coming up to twenty minutes that the eggs have been in the sperm. WILLIAMSON: And we can start the washing through process ROBERT: Would you like to do that or would you like me? WILLIAMSON: Err, well you're nearer. And you
00:45:00	DON looks directly at camera.	have a steadier hand, or two steadier hands….But just a little at a time. Before squirting through the filter to get the eggs in the glass bowl. ROBERT: What's the idea of this stage? WILLIAMSON:
00:45:30	WS of the room. DON watches ROBERT as he does the 'washing-through process'.	Getting rid of excess sperm. First of all, you can't see the eggs when they are in the very concentrated sperm

		suspension. Also, the sperm eventually dies and it just fouls the water.
	Robert squirts water through filter	
	CU of the liquid running through the filter	
	CU of filter with a bowl underneath collecting fluid.	
	CU - ROBERT swaps the bowl for an empty one and removes the filter from the clamp holding it. He sprays the filter.	
	MS of the room.	ROBERT: You think that will be it?
	ROBERT passes the bowl to DON.	WILLIAMSON: Err, yes. It's err, we can examine them now.
00:46:30	CU of the bowl on the microscope. DON looks down the microscope.	WILLIAMSON: Although there are two or three at the surface, floating. There are two or three thousand at the bottom, sunk. Which is encouraging.
00:47:00	CU of microscope view: eggs	
	CU of DON looking down the microscope.	ROBERT: I suppose I also need to produce the samples for the DNA testing?
	CU of bowl under microscope. Shot of DON.	WILLIAMSON: We already have tube feet from the female starfish, so they are duly pickled.
		ROBERT: So we just want the, bit of the male?
		WILLIAMSON: Bit of the male.
	CU of an urchin.	ROBERT: This is the bit I don't like, having to kill the animal.
	CU of ROBERT's hands using forceps to pick apart the urchin.	WILLIAMSON: I share your feelings.
	CU of ROBERT's face.	WILLIAMSON: 11:45 and I have one cell division.

	Back to CU of urchin being picked apart.	Also, two or three that have divided very unequally and I don’t think these will come to anything.
00:48:00		WILLIAMSON: But I also have at least one equal division and I’m very hopeful for it.
	ROBERT empties the contents of the urchin into the basin nearby. CU of DON looking down the microscope.	WILLIAMSON: Whether they divide equally or unequally, some of them might be possible. If they reach a many-celled state, I hope that they will be capable of forming a blastula and hatch in the late-blastula stage.
	CU of what’s down the microscope.	ROBERT: How long will that take do you think?
	ROBERT taking apart the urchin.	WILLIAMSON: Round about twenty-four hours, but “round-about” is as precise as I can
00:49:00	ROBERT places a bit of the urchin in a tube.	say. I’ve had hybrids hatch in eighteen hours, others failed to hatch within twenty-four hours but they still have hatched later.
		ROBERT: And Don, this morning, what would you like me to do with the starfish that are in the tank?
		WILLIAMSON: Return them to the sea, thank you. They’re just taking up tank space. So the best thing for them is to return them to the sea.
	Shot of ROBERT writing in a book, recording what has happened.	
	Shot of DON’s face	
00:49:26	Shot of book again, being closed.	

00:50:00	Outside, looking out to sea. ROBERT walks to the wall in shot. Sky shot, dark clouds. The moon covered in dark clouds. A daytime shot of the harbour, gulls heard overhead. Shot of the marine station, followed by one further away. A fishing boat can be heard and seen.	
00:50:24	Sky. Gulls wheel about. DON and ROBERT shuffle towards the wooden gate that gives access to the side of the marine station. Reaching this gate, they pass through. On the other side they re-bolt it and then, continue to shuffle slowly past the side of the building. In the foreground are concrete, water-filled pools and the sound of flowing water is audible	
00:50:51 00:51:30	Shot of the harbour through a window. Pan round to DON sitting at the microscope. ROBERT labelling tubes, DON in the background. DON seated at the microscope, camera looking over his shoulder. DON looking down microscope, ROBERT in the background. CU of bowl under microscope. Back to view of DON looking down microscope with ROBERT in background. ROBERT looks down the microscope.	WILLIAMSON: We have something swimming. WILLIAMSON: A rather misshapen blob. WILLIAMSON: Have you got it? ROBERT:

	CU of what's down the microscope.	Umm, I can't, slowly turning… WILLIAMSON: Slowly turn it. ROBERT: Is it in the centre? Oh yes. Oh yes. WILLIAMSON: So something has hatched. ROBERT: So that's a hybrid? That's a hybrid larva? WILLIAMSON: Well, I would expect it to be.
00:52:00	DON looking down microscope, ROBERT in the background.	WILLIAMSON: I'm agreeably surprised that there are…I've seen up to ten already, moving, which is more than I expected. And I think others are capable of hatching. So we're in business.
	Back to CU of what's down the microscope.	WILLIAMSON: It's good. Have you means of photographing such things if I could fish one or two out?
	DON looking down microscope.	ROBERT: This is the dish so we could put them in those little cells.
	CU of what's down the microscope.	WILLIAMSON: Yes. WILLIAMSON: We have larvae!
	Microscope view, using a pipette tube to suck up one of the hybrids.	
	ROBERT uses the pipette to squirt the hybrids into a 'cell'.	
	DON looking down microscope.	

00:52:37	CU of hands preparing a slide Pan up to MONTAGNES face. Shot of MONTAGNES's hands pipetting liquid, then looking down the microscope. MONTAGNES adds liquid to a slide, which is under the microscope.	MONTAGNES: That's going to support the coverslip, above the slide so that the organisms don't get killed…squished. MONTAGNES: Just get this up over the lip of glass here. You can just see some little white dots in there barely, but those are all your little guys swimming around. They're all there, I'm just getting rid of excess water. And then we take the slide and do that. They're sandwiched there.
00:54:00	MONTAGNES gets up and goes to put the slide under his microscope. Shot of computer screen (linked to microscope). We see a hybrid move across the screen.	MONTAGNES: There we go. Like that one? WILLIAMSON: That's fine. A hollow ball of cells. ROBERT: And that was created from the egg of a starfish fertilised by the sperm of a sea urchin.
00:54:30	DON talking to ROBERT (off-screen) MONTAGNES looking down his microscope.	WILLIAMSON: Right! Now we do not know what form these hybrid larvae will take…whether they will be paternal, maternal or a mixture. That's a wait-and-see. WILLIAMSON: Now that one I think is...has gastrulated or it could be just a distorted blastula. It's certainly nothing like spherical. A kidney-shaped blastula – new to science.
00:55:00	CU of computer screen with hybrid on the screen. DON shuffles off, out of the	MONTAGNES: I can get better shots. WILLIAMSON: Yes…and I'll get back to picking out more larvae, a long and tedious process.

00:55:28	room. MONTAGNES looking down microscope. CU of computer screen with blastula on the screen.	MONTAGNES: There you go. MONTAGNES: Beautiful. Yeah, it's got long cilia on it. They extend up to about there. You can just barely see them there. Shall I look for another one? ROBERT: Yes please.
00:55:47	View from an airplane An air stewardess closes an over-head locker Shot of the plane's wheels from inside the plane, the wheels are touching the runway	
00:56:26	ROBERT sits at the end of a corridor, on the phone. Closer shot of ROBERT on the phone, we see him sitting at a small table.	MARGULIS: Robert… ROBERT: Lynn, can you hear me? MARGULIS: I hear you perfectly, can you hear me? ROBERT: Lynn, I'm just trying to… MARGULIS: No, but Robert. Robert just keep talking about Williamson because I hear you perfectly. ROBERT: Alright, it's better now, now it's working. MARGULIS: Well listen you just keep talking all the time because I hear you perfectly, I've never lost the signal. ROBERT: When did you first hear about Don? MARGULIS: I heard about him in a way that must go into…that must go into your programme. And that is the

00:57:00	Even closer shot of ROBERT on the phone. ROBERT opens a book, we see a U of the front page where DON has written a ‘thank you’. ROBERT flicks through the book, written by DON.	following: I received a letter about five years ago or so, we can check it for sure, and the letter said, “I’m Don Williamson, I’m 68 years old. I’m from a short-lived family and I’m on a straight-line course for posthumous recognition”. Did you get me? ROBERT: I got you yeah.
	CU of ROBERT’s face. ROBERT flicking through the pages of DON’s book.	MARGULIS: And I said, I must read the rest of this letter because usually by that part of the letter I throw them out. And I read the rest of the letter and he said “I’m at the Isle of Man, the University of Liverpool marine station”. And he enclosed one of his early papers called ‘Incongruous Larvae’. He said he’s writing a book and he’s been rejected everywhere. And so, I mean, there are more letters than this, then we went back and forth and I got him the Chapman and Hall book. You know that book?
		ROBERT: Yeah, I’m looking at it now.
		MARGULIS: I worked very hard with the editor, the Chapman and Hall guy. He got Tauber, you know Tauber? Have you actually met Tauber?
		ROBERT: No, I never met him.
00:58:00		MARGULIS: Well Tauber was doing a programme on ‘Self’. And so when Tauber told me he was doing this programme of Self, he…have you seen the book on Self?
	Shot of ROBERT’s back. He gets up.	ROBERT: I read the book on Self.
		MARGULIS: Yes excellent. So, what happened was, Tauber went to England. Well, you know he went to Don’s lab. Tauber actually called me from Don’s lab and he said: “Lynn, I want to tell

	CU of ROBERT getting a book from his bookshelf.	you something. The man is not a charlatan. And he’s not a phoney in any way and he’s not incompetent.
	ROBERT sits down at the table agin with Tauber’s ‘Self’ book.	MARGULIS: He may be hard to understand but
	ROBERT flicks through Tauber’s book, camera is behind him looking over his shoulder. He stops on a chapter of the book written by DON.	he’s a totally serious scientist”. And I felt I needed to know that before I actually paid his way to come to the Self meeting. And he said “I want to just…I’m looking out the window,
	Shot of ROBERT on the phone. ROBERT looks up from the book.	and I’m looking at”…what is it, the Irish Sea, is that what you look at? ROBERT: You do, yeah.
	Shot of the sea.	MARGULIS: Well, he said, this is what Tauber said: “I’m looking out the window at
	Shot of a lighthouse by the sea.	the Irish Sea, I’ve been a guest
00:59:00	Another shot of the sea.	of…Williamson has been marvellous. He’s a totally interesting guy and I’m going to invite him. I wanted to tell you that, I’m going to invite him to my Self meeting. So he came to the
	Shot of birds over the sea (coast)	Self meeting. It was held in the Boston University Law School. It was a huge auditorium, way too large, and there were probably about forty-
	Back to ROBERT flicking through the Tauber book at the table.	five people there. And Williamson gave his spiel and it’s wonderful, his talk. He gave this seminar and afterward, you could hear a pin drop.
	CU of starting page of DON’s chapter of the Tauber book. ROBERT flicks through the pages.	Nobody said anything. And then one guy, it was Eric Davidson of Cal Tech. He’s a very important scientist. He’s a Cal Tech professor. Right?
	Shot of ROBERT reading book.	He gets up and he starts screaming at him saying. I mean, he was apoplectic. And I can’t remember the
01:00:00	Shot of the book, one of the pages, over ROBERT’s shoulder.	details, but I don’t think anybody can but it was, “this is such wrong stuff, it is so based on nothing and there’s no evidence…”…and you know, he got, and…and Tauber got up at the end
	Shot of ROBERT.	and he said, “Ladies and Gentlemen, either you’ve heard something that is entirely wrong and will go away without a ripple, or you can count yourself lucky because you are in …on the beginning of a new, a new
	Shot of the book from over ROBERT’s shoulder.	zoology”. And that, I think, was probably the last time I saw Don. I mean when he came for that Self

	Shot of ROBERT. Shot of the book from over ROBERT's shoulder. ROBERT gets up and puts the receiver down. Shot from behind, ROBERT turns off the camera.	meeting. ROBERT: Do you concur with that, with what Tauber said? MARGULIS: Yes, oh completely.
01:00:29 01:01:00	ROBERT setting up a video camera (in shot). He presses play. Shot of the back of ROBERT's head. Shot of a TV set playing what is on the tape. A man, STRATHMANN, is sitting down with books behind him. Shot of ROBERT writing in a notebook 'Richard Strathmann'. Shot of TV set with STRATHMANN. Shot of ROBERT watching. Shot of TV set again.	ROBERT: Richard, you were asked to review Don's ideas. What do you make of them? STRATHMANN: Err, it's a little hard. The question that immediately came to my mind: is this deception, or tunnel vision? You know if I were in Williamson's position, first of all, in good science, you should set up a hypothesis that's testable and then be willing to put it to the test. This is something the creationist's in the US don't want to do, which is why it's not really science. And it's something Williamson doesn't seem to really want to do. If he really wanted to do it and if I were in his position of trying to make my case, I'd first of all try to show that you couldn't, you really couldn't plausibly explain some of these incongruities between larva and adult…by convergent evolution. And he's never made that case well.

01:02:00	Shot of ROBERT	ROBERT: It’s an interesting question isn’t it, that balance between sticking to your conviction and accepting disappointing results?
01:03:00	Shot of TV set Shot of ROBERT	STRATHMANN: Yeah, and I would say that a new hypothesis really deserves a little leeway. It’s very…so I’m not, I’m not just jumping on Williamson because he has a weird idea and I don’t think we should ‘pooh pooh’ the idea that horizontal gene transfers are important. I think they could be and I think we now have the tools to look a lot more for that. It’s not going to be entirely easy to use them to distinguish Williamson’s hypothesis because you need to look at quite a few genes and you maybe need to know how they operate. I think that should be done, but Williamson has not made a…a good attempt to present his case.
	Shot of TV set Shot of ROBERT Shot of TV set, CU Shot of ROBERT	STRATHMANN: Well he has, if he wanted to pick something, a hobby-horse to ride, he has some good protections. And one is that he can say that you might never see this in the lab. I could be completely disproved in my experiments, but over half a billion years it only has to happen a few dozen times. And how can you disprove such a rare event? I mean that, that gives him a nice safety fortress to hide in. But there’s other problems, I wouldn’t…try to counter his arguments by saying, it simply can never happen. I’d be trying to look for evidence that it did.
01:04:00	Shot of TV set	STRATHMANN: And umm, what disturbs me when somebody like Williamson goes a little bit off of…I mean the book is sprinkled with the phrase ‘I believe’. This is like, this isn’t the Nicene or Apostolic creed, this is science. It’s not a matter of deep belief, it’s a

01:04:51	Back to ROBERT on the couch. He is leaning down to the floor, clearly changing tapes in the camera, etc Shot of TV set – a young DON on the screen Shot of ROBERT watching Shot of TV set	matter of testable propositions and beliefs are provisional. And it's that constant scepticism and self-doubt, not quite paranoia, but always wondering 'could this be right, is there another possible explanation?'. That's what makes science productive and possible and insightful..and umm…that's what's disturbing to me about the direction Williamson's going. WILLIAMSON: In the dim and distant past two animals, two different unrelated animals, neither of which had larvae, hybridised. The sperm of one fertilised the eggs of another, and whereas they would not normally hatch, on one occasion this happened. And the resulting fertilised egg had the genome, all the genes, to make both animal A and animal B. And what happened was, animal A was expressed as the larval form and animal B developed later as the adult form. It's very difficult to prove this – things that happened hundreds of millions of years ago. But there are lots and lots of examples that fit the theory. There is an adult form in so many many cases that is related to the larval form of an apparently distantly related animal.
01:06:00	Shot of ROBERT, followed by shot of ROBERT writing notes in his notebook	
01:06:24	View of plane wheels from inside a plane. The plane is leaving the runway. Shot of clouds Shot of plane wheels as the plane is in descent, grass fields and the runway are seen behind the plane wheels	ANNOUNCER: Good evening ladies and gentlemen, you're very welcome on board…???...for your flight to the Isle of Man and onwards to Belfast city. Your captain today is Captain Andrew O'Mallett. My name is Michelle and I shall be looking after you during your flight.

01:07:22	DON is his bungalow study-bedroom	ROBERT: So Don, can you let me in on what's happened since I was away.
  01:08:00	Shot of a computer screen, DON's diary. Followed by a shot of DON's face. Shot of computer screen. Shot of DON's face. CU of specific diary entry on computer screen Shot of DON's face reading off the screen	WILLIAMSON: Well, this is the story so far. By the 2^{nd} of August we had forty larvae, thirty of which were kidney-shaped bipinnarias. That is a normal starfish larva. I thought they were continuing to grow, but by the 9^{th} of August it became apparent that they were not growing, they were actually getting shorter. And I measured some on the 9^{th}, the longest was .26 millimetres, whereas we'd had one .4 millimetres five days before. And the next day I measured more. I got several about .2 by .16 millimetres. And others were completely spherical. And though there was food in the gut, the mouth and anus were probably closed. There was no, effectively no mouth or anus.
01:09:00	CU of DON reading off the screen	WILLIAMSON: They are now dying off rapidly. We only had about thirty to start with. We're down to, err. Well, yesterday, the 13^{th}, we were down to two, one of which was swimming, the other was apparently intact, looking like a good larva but no longer swimming.
		ROBERT: Did something else happen while I was away Don?
		WILLIAMSON: Mmm..?
	WS of DON sitting at his computer	ROBERT: Something else happened when I was away, The Examiner published the article
	Shot of DON's face, CU	WILLIAMSON: Ah yes, well. Here are copies.
		ROBERT: What do you think of the article?
	Camera follows DON reaching	WILLIAMSON: Well, considering that the reporter

	to pick something up from the bookshelf.	was admittedly not a biologist, he did quite a good job. It’s far from perfect.
01:10:00	DON holds up the copy of the article	WILLIAMSON: ‘Experiments continue as marine biologist challenges Darwin’s theory’. Well, my theory is an addition to Darwin’s theory, not a challenge to it.
	Shot of DON’s face CU of the article	
	Shot of DON Shot of article on DON’s lap	WILLIAMSON: ‘Darwin’s spectre might have risen from the grave as a fresh battle breaks out over evolution’. Well, that’s the opening sentence, and again it’s misleading. As I say, I’m adding a P.S., it’s not a challenge.
01:11:00	CU of DON’s face, followed by shot of DON holding the article.	WILLIAMSON: The picture, incidentally, illustrates err…’Mr Williamson pictured with research assistant, Robert Sternberg’.
	Don’s face, camera pans down to article. CU of article.	WILLIAMSON: I’m not very good at recognising my own photograph. I’m much more familiar with my mirror image. The microscope shows up quite well…It might be an idea to ask the computer to print out the story so far. And then we’ll go to the lab and see if there are any hybrids still surviving.
	Shot of DON’s face, article, then DON’s face, then article CU. DON’s face, then of article being put down. MS of DON. CU of computer printing.	
01:11:36	Shot of a microscope, pan to DON’s face. Shot of a computer screen showing what is under the microscope. DON fiddling with the watch glass under the microscope and watching the computer screen.	WILLIAMSON:

01:12:00	CU of computer screen Shot of microscope, CU of DON turning the dial on the microscope, CU of DON's face. CU of computer screen Shot of DON's face Shot of screen, then of DON's	I can get the hair and the air bubble showing up beautifully. But where the larva is…I wish we had dozens or hundreds rather than one. Here we are. We now have something that moves, so…I'm saying here we are, might be something… ROBERT: Try the ABC controls Don. WILLIAMSON: Ah. That's different. It's not completely spherical. But we can't
01:13:00	face. Shot of screen Shot of DON's face	make out any internal structure. It's just a ciliated larva. If it develops into anything recognisable it will of course take our ideas one step further forward. Obviously we are not trying to replicate actual hybrids that took place millions or hundreds of millions of years ago. The best we can do is show that hybrids between distantly related species are possible, and that
01:14:00	Shot of computer screen Shot of DON's face, then back to computer screen.	some of these turn out to produce recognisable a) larvae and b) juveniles. We have been using concentrated sperm and in some cases at least, the barriers between distantly related species seem to be broken down. WILLIAMSON: And I have now lost the swimming
01:15:00	Shot of DON, then computer screen (now empty). Shot of watch glass under microscope, then of DON turning the dials on the microscope. Shot of computer screen. WS of DON as he gets his glasses out.	larva. I have lost the little beast. WILLIAMSON: That again, is a bubble in the glass, and that doesn't move. ROBERT: That looks like it though doesn't it? Even though it's not moving much. WILLIAMSON: I suppose that's it. I hope we haven't cooked it. Well, let's take it down to the downstairs lab which is cooler anyway, and we'll see if we can find it, under one of those microscopes.
01:16:00	CU of DON	ROBERT: Ok, I'll bring it down.

		WILLIAMSON: Ok.
01:16:09 01:17:00 01:18:00	CU of dead larva down the microscope, then shot of DON looking down the microscope. CU of what's down microscope, then shot of DON looking down microscope. CU of ROBERT removing a folded bit of paper from his pocket. Shot of DON CU of paper. Shot of DON CU of DON taking off his glasses. Shot of the microscope as DON walks in front of it. Shot of DON.	WILLIAMSON: It's certainly stopped moving. A great disappointment. ROBERT: So we haven't achieved a hybridisation in this instance? WILLIAMSON: I'm afraid we have not. ROBERT: Don, when I was in London, Richard Strathmann sent me a poem about you. He said this should explain the situation. I don't know if you'll agree with it. WILLIAMSON: 'There once was a man from Port Erin, whose phlogenies were quite daring. Larvae flew between trees with the greatest of ease, Occam's razor was needed for paring'…Occam's razor…I have to be reminded what Occam's razor was. ROBERT: I think it's the principle of parsimony. You know, the simplest explanation is the most likely to be true. WILLIAMSON: Oh right. WILLIAMSON: Parsimony, in my mind, is a concept thought up by biologists for the convenience of biologists. It makes things simpler. But I don't think nature, well I don't believe in nature as a force with…that thinks in advance. Certainly Nature with a capital N is not sitting around thinking about the shortest ways to get from A to B. It does it by trial and error, and trial and error seldom produces the shortest way. So by

	CU of ROBERT putting the folded paper back into his pocket. WS of DON with ROBERT in the background. CU of DON. The camera follows him as he leaves the room.	definition, parsimony is unlikely in many cases. WILLIAMSON: I shall go and have a pee.
01:18:56 01:20:00	Through a window-pane, looking down at the water-filled ponds outside. Shots of documents, papers. Shot of DON searching for something. Shot of the papers he is rooting around in. CU of one of the papers. Shot of DON. DON is searching again Closer shot of DON searching Another shot of DON searching Shot of DON pulling out a	ROBERT: All these all papers by you Don? WILLIAMSON: Yes. An incomplete selection. Many of them, I still have dozens of the reprints. Others, I, I no longer have any. ROBERT: How many papers would you say you've written? WILLIAMSON: Not an enormous number. Certainly over seventy but, err…seventy plus. WILLIAMSON: Lots of rejects from journals but not the, the paper published in 1988 in 'Progress in Oceanography'. ROBERT: That was your first successful paper on the theory? WILLIAMSON: Yes. There again, after seven rejects by different journals the editor asked to publish it. ROBERT: Would be good to see a copy. WILLIAMSON: I've seen them within the last year but where I put them I don't know.

01:21:00	drawing from one of the stacks of papers, he places it down.	WILLIAMSON: That is my version of a chimera…of a hopeful monster. The head of a lion, the body of a she-goat, and the tail of a serpent. And it's in my best Greek. I'm not sure if I've got it right, but
	CU of DON's face, pans out. Shot of the drawing, followed by shot of DON's face	it's something like that. Homer, if I've got the right author, described a chimera and it was like that. He didn't draw it, but this is based on his description. I'm going on with the same concept of an animal with two or more different forms coming from
01:22:00	Camera pans down to the drawing. Shot of DON's face	the same egg. But they are, in this case, they are expressed simultaneously and in actual animals with larvae, they are expressed in sequence. Or one following the other. And I maintain that the only explanation of this is that the genes which prescribe the larva and the genes which prescribe the juvenile, were originally the genes of quite
	CU of DON DON flicks through some papers underneath the drawing Shot of DON	distinct species. ROBERT: Do you have any copies of the referees' comments from papers that you've tried to publish, and that have been rejected…just to see what they had to say? WILLIAMSON: Umm…

01:23:00 01:24:00 01:25:00 01:26:00	CU of a paper of DON's Camera pans up to DON's face CU of the referee's response. The paper is taken out of shot, shot of DON's face	WILLIAMSON: 'The National Academy of Science of the USA…'89, 1989'. 'This is a fascinating paper which makes exceptional claims about the ability of echinoderm sperm to fertilise ascidian eggs. If correct, this thesis will form a profound rethinking of metazoan evolution and genetics. This is a very big if. But on balance, I recommend publication of the paper if only for the reason that interesting experimental work will be done in attempts to refute the author's claims. As seen by modern embryologists and geneticists, this paper has a crazy premise. But come to think of it, so originally did most of the major scientific theories that we now accept'. But from another referee: 'Is this contribution of sufficient interest to justify publication in 'The Proceedings'? No. Is the overall quality of the paper suitable for the journal? No. Does the evidence justify the conclusions drawn? No. Is this paper clearly written for a diverse audience of scientists? No'. And he says, 'at the risk of overstating a truly simple point, this paper is not suitable for publication because it presents no actual evidence to support its claim. Mere verbal statements of such startling results are insufficient evidence. Minimal evidence of the claim requires sharp an unequivocal photographs of ciliated larvae in the process of hatching out of the ascidian chorions. It was my attempt to get further *Echinus* to repeat the experiment, in the hope of getting hatching eggs, just as he said. That was when I had my accident and went into hospital from the resulting stroke.
01:26:13	Shot of DON	ROBERT: When…just trying to think. When was the last time I was with you? WILLIAMSON: When was the last time? ROBERT That we were together?

	Shot of a trolley-cart at the airport, camera pans round to DON in a wheelchair. CU of DON, zooms out.	WILLIAMSON: I think three years ago. ROBERT: And this trip that we're on now, would you explain what it's about and where we're going.
01:27:00	CU of DON's face. Camera zooms out.	WILLIAMSON: Well, we're going to Italy, to Bellagio on Lake Como, for a conference on 'Human Brain in the Context of Natural History, Three Tousand Million Years of Evolution of Sensory Sytems'. And the idea is that this method of…evolution that I have called 'Larval Transfer', which might also be called 'Hybridogenesis', applies not only to larvae but also to some organ systems of animals or at least some animals. And it could perhaps directly be…responsible for
01:28:00		the evolution of human brains…but that is pure conjecture at the moment.
	Shot of the airport, passengers, etc Shot of DON	ROBERT: And how are you feeling Don, about giving the paper? WILLIAMSON: Well, it's been many years since I did give a paper or a public talk, but I'm reasonably confident that I can give a fairly intelli- I have difficulty with some words – intelligible presentation. I have to speak slowly so I hope they don't impose the half-hour rule too rigidly. ROBERT: Are the talks half an hour long?
01:29:00		WILLIAMSON: That's the rule. Which mine will go into half an hour but if I have too many word blocks or too many words that I cannot pronounce, it might take a bit longer. But I'm resigned to that.

01:29:20	CREDITS IN Shot through the windscreen of a car showing a road sign to Lake Como, then a tunnel, and then the end of the tunnel. Shot of Lake Como.	 WILLIAMSON: I think I am the only contributor to this conference on the human brain, who admits to being brain damaged. But my talk is not about brain damage. It's about a recently discovered type of evolution that has affected all animals and I call it 'Larval Transfer'. The human brain, of course, is a product of animal evolution. Well, Darwin assumed that larvae and adults evolved gradually from the same genetic stock. It took me thirty-five years to decide that this theory was untenable. Francis Balfour said much the same thing in volume two of his treatise. He, and Charles Darwin, both died in 1882. Balfour was thirty-one, Darwin was thirty–two. Balfour and I have independently come to the same views…
01:30:33	FADE TO BLACK	