TIME, CONSCIOUSNESS AND SCIENTIFIC EXPLANATION

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I am conscious of being only an individual struggling weakly against the stream of time.

Ludwig Boltzmann

For HJD

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SUMMARY

To date, there is no universal and coherent theory concerning the nature or the function of time. Furthermore, important and unresolved controversies raging within both philosophy and the natural sciences apparently indicate that there is little hope of constructing a single, unified theory. Even so-called "folk" theories of time, embedded within different cultural traditions, show no common elements, and therefore can not provide a pre-theoretical description of time, towards which an explanatory framework could be constructed. This lack of consensus indicates that the concept as it is currently being used is ill defined, and, at the very least, needs to be considerably revised. The conceptual disarray surrounding time has aided and abetted the arguments of certain thinkers, especially Ricoeur, working within the phenomenological tradition who make *de principe* claims that there can not be a single theory of time.

My intention is not to try and to produce a concept of time that was capable of unifying all these different elements. Rather, Ricoeur's arguments and those of others working in the phenomenological tradition dissatisfied me. I believed that their arguments were informed by a myopic, muddled and positively 19th Century understanding of the scientific project. Hence, my aim is to show that Ricoeur's claim will not stand up to scrutiny, and that there are no principled arguments against the possibility of a unified theory of time. We examine the major arguments against unification in general, and also with particular reference to theories of time, such as Husserlian phenomenology, conventionalism, instrumentalism, anti-reductive positions in general, as well as the specific problem of reducing subjective experience to objective description. We demonstrate that none of these objections constitutes a watertight *a priori* argument against a unified theory of time.

Furthermore, we demonstrate that recent developments in the philosophy of science and the philosophy of mind have made such a unified theory a plausible goal. We argue that post-positivist philosophy of science, with its emphasis on research programmes, the co-evolution of theories and super-empirical rational support, opens the way for new types of evidence to be brought to bear on questions about time. Also, recent developments in the brain sciences mean that a neurologically plausible and fully naturalised analysis of our experience of time is being developed.

Although much work in this direction has begun, we argue that it is fragmented, partly through the limitations of our current knowledge, but more particularly through an inadequate background of coherent philosophical thought. This has lead both philosophers and scientists to attempt grand metaphysical answers to muddled philosophical questions which threaten the progress which natural science and the philosophy of science have offered in the second half of the twentieth century.

CHAPTER 1
THE DISUNIFIED THEORIES OF TIME

In any attempt to bridge the domains of experience belonging to the spiritual and physical sides of our nature, time occupies the key position.
Arthur Eddington

Well, there has probably been more nonsense written by philosophers on the subject of time, from Plato onwards, than on any other topic.
Paul Davies

1.1 INTRODUCTION

When setting out to write about the concept of time, it has become obligatory for would-be authors to quote St. Augustine,

> What then is time? I know well enough what it is, provided nobody asks me; but if I am asked what it is and try to explain, I am baffled. (Augustine 397, 264)

The significance of the Augustine quote lies not its succinct encapsulation of a philosophical problem, but in its age. A millennium and a half have gone by since the good saint wrote his *Confessions*, and yet still philosophers and scientists alike are unable to adequately define the concept, nor even agree on a single property that it may possess. Time has been described as linear and cyclic, relative and absolute, continuous and discrete, finite and infinite, as being a property of the real world and as being an imposition of the mind on the world. Some, infuriated by the concept's intractability, have resorted to extreme measures. Notoriously, McTaggart's philosophical musings led him to conclude that time did not exist at all. (McTaggart 1908) It is this lack of coherent definition that provides the initiating problem for this thesis.

In this opening chapter we examine the diversity of extant theories of time. To date, there is no universal and coherent theory concerning the nature or the function of time. Furthermore, important and unresolved controversies raging within both philosophy and the natural sciences apparently indicate that there is little hope of constructing a single, unified theory. Even so-called "folk" theories of time, embedded within different cultural traditions, show no common elements, and therefore can not provide a pre-theoretical description of time, towards which an explanatory framework could be constructed. This lack of consensus indicates that the concept as it is currently being used is ill defined, and, at the very least, needs to be considerably revised. The conceptual disarray surrounding time has aided and abetted the arguments of certain thinkers emanating from the phenomenological tradition who make *de principe* claims that there can not be a single theory of time. These thinkers have cited, for example, the failure to reduce time as experienced to theories of "cosmological time" as support for their claims.

1.2 THE PHILOSOPHIES OF TIME

There is, to date, no universally accepted theory of time. The most casual survey of the diverse literature, which constitutes time's philosophical treatment, reveals a manifold of apparently unconnected material. So much so that it is not evident that the same concept is being discussed. Hence it is not a new observation that time as a concept has played many differing rôles in a wide range of theories. So, for example, Adam writes of the various theorists, who have used the term,

> It is hard to believe that these theorists have made the same "phenomenon" central to their work. Between them they associate time with death, ageing, growth and history, with order, structure, synchronisation, and control. They view time as a sense, a measure, a category, a parameter, and an idea. (Adam 1990, 15)

Many authors have claimed that the concept of time is of central importance to the subject of Philosophy, and the roll call of philosophers who have struggled to develop theories of time reads as a list of its greatest thinkers, from the pre-Socratics onwards: Aristotle, Augustine, Leibniz, Kant, Hegel, Heidegger. It is disquieting, therefore, amongst this profession which has prided itself on conceptual analysis that there appears to be the least consensus over the meaning of the concept. There is little or no commonality of reference between many of the term's philosophical (or even technical) uses. It is as if Heidegger, McTaggart and Reichenbach were writing on completely different subjects. Their respective bibliographies carry few, if any, shared references.

There have been different philosophical responses to this disunity of the concept. Firstly, there are those who have tried to do some groundwork towards a unified concept. For example, Kenneth Denbigh (1981) argues that time as it is currently understood is not an unitary concept and draws out three distinct strands: time as it is to conscious awareness, time in theoretical physics, and time in thermodynamics and evolutionary sciences. He argues that is these three factors which will need to be reconciled in some fashion if we are to achieve a unified theory of time. Secondly, in frustration at the inadequacies of previous attempts to ground an explanation of temporality in some original, primitive time, anti-foundationalist philosophers, Jacques Derrida (1967) and Paul Ricoeur (1984 and 1988) have chosen pluridimensional accounts as the preferred option, arguing that the different accounts are not reducible to one another. Thirdly, there have been those philosophers like Heidegger and Husserl who regard any scientific theories of time as secondary and derivative of our pretheoretical experience of temporality. Finally, as has already been mentioned, some philosophical approaches, typified by the work of McTaggart, have led certain thinkers to pronounce that theories of time are internally and irrevocably incoherent, and that therefore time is unreal.

That a philosopher might be driven to this extreme conclusion is not surprising. The philosophical discussions of time are exhausting, if not exhaustive. Every possible stance seems at one time or another to have been occupied. Without claiming to be a complete description of the plethora of theories, the philosophical discussion of time can be described as falling into four main categories. Firstly, what we will call the Aristotle-Augustine debate. The question here is whether time is a real property of the external world, or an ordering structure imposed on the world by the mind. (This debate is dealt with in more detail below when Ricoeur's thesis in *Timeand Narrative* is discussed.) The second great debate is staged between Leibniz and Newton about the structure of time, that is, as to whether time is relative or absolute in relation to events. Einstein notwithstanding, the issue is still hotly debated, though the current tendency is for philosophical authors to claim that the absolute versus relative debate is badly conceived. (See, for example, Newton-Smith 1980 and Earman 1989.) Subsidiary to this debate there is a gamut of issues which concern themselves with the topology of time; each of these tributary arguments involves dichotomous positions: Is time finite or infinite, is it linear or cyclic, is it discrete or continuous? Related to the question of topology, but of such importance that it has merited a field in its own right, is the third major debate that focuses on the direction of time. Hans Reichenbach's book *The Direction of Time* (1956) is the seminal text in this field. (For a contemporary treatment, see Horwich 1987) Finally, the fourth main area of debate concerns tense. Briefly, this is the question of whether indexical components in a sentence can effect its truth value. Are there ontological differences between the past, present and future such that, for example, statements about the past have a determinate truth-value (i.e. are real), whereas statements about the future do not (i.e. are unreal); or perhaps are past, present and future all ontologically equivalent? (There are several intermediate ontological positions that can be adopted.) Subsidiary to this debate is the question of whether temporal relation are primitive, or whether they are reducible to some other kind of relation, such as causality. (For discussion see, e.g., LePoidevin and MacBeath 1993 or Mellor 1981.)

Newton-Smith has eloquently noted that the confusion surrounding "time" is further compounded for,

> [T]ime is not just an abstract beast but also it is a most promiscuous beast who regularly couples with elusive partners. (Newton-Smith1980, 3)

He lists a host of other concepts that have been used in an attempt to define time including motion, change, entropy, agency and consciousness. He notes the problem with using these terms to explain time is twofold: firstly, like time, they are terms that are from theories "underdetermined by data." Secondly, each of these terms depends on the concept of time for its own definition. So, for example, time can only be explained in terms of causality, and causality can only be explained in terms of time.

Despite their longevity, all of these philosophical debates remain for the most part unresolved. Furthermore certain philosophers, with a death wish for their profession, have suggested that the solutions to these debates will not be found in philosophy. They argue that these are empirical, not philosophical questions; that it is only by grounding philosophical explanation upon empirical theory that we can ever begin to approach an authentic explanation of time. However, as is to be expected in this most contrary of professions, certain other philosophers deny that time, as it appears in scientific theories, can tell us anything about the concept of time. (See Chapter 2) The chances for a unified theory of time seem remote, given that such basic methodological arguments are far from being resolved.

In the twentieth century, the initiative for the study of time has been wrested away from the philosophers, and lies firmly in the grasp of the scientists. A review of the UK best seller lists catalogues the ascendancy of Professors Hawking, Penrose, Davies and Prigogine, each hawking their own brand of metaphysics. The ascendancy of the scientist has brought with it a certain arrogance of tone. For example, Paul Davies typically remarks,

> Well, there has probably been more nonsense written by philosophers on the subject of time, from Plato onwards, than on any other topic. (1995, 252)

(One wonders why Davies, therefore, prefers to call himself a professor of natural philosophy, rather than a physicist.) Aspersions aside, this annexation by the "scientists" of the traditional territory of the "philosophers" has not occurred without some resentful ripostes. In Chapter 2 of this thesis we will examine the exemplary arguments of Husserl. In developing the phenomenological method, he managed to construct a philosophical position that entirely negated the influence that the empirical theories of natural science could have on the understanding of time.

1.3 DISUNIFIED THEORIES OF TIME IN THE NATURAL SCIENCES

Though it may be true that philosophers in their time have written their share of nonsense about time, it would not be true to say that scientists have always written good sense. Indeed the history of science is just as littered with degenerated and degenerate theories as any other subject.

The initiative for theories about time has undoubtedly been wrested from the hands of the philosophers, but not without cost. In order to claim its theoretical successes, for example, space-time physics has had to deny one of the most basic phenomena of human experience - that time is perceived as asymmetrical. It has also had to ignore lawlike time-asymmetrical behaviour in other natural sciences, in particular the Second Law of Thermodynamics. As with philosophy, important and unresolved controversies raging within the natural sciences seem to indicate that there is little hope of constructing a single, unified theory of time.

In a letter addressed to Michele Besso's recently bereaved relatives, Einstein wrote,

> For we convinced physicists, the distinction between past, present and future is only an illusion, however persistent. (Einstein, 21 March 1955, in Speziali, 1972)

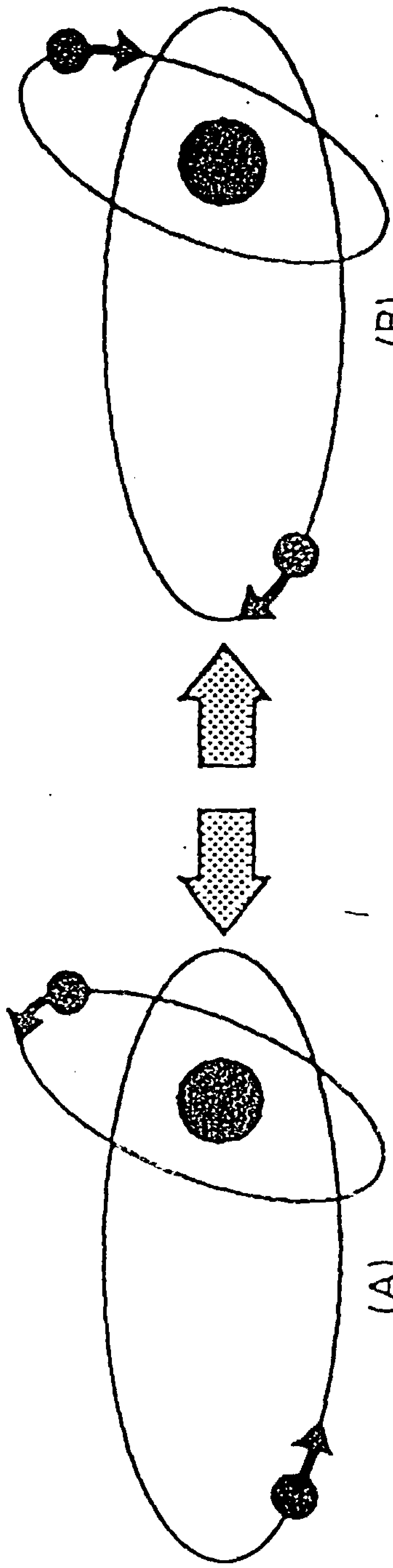
In making such an assertion, Einstein was dismissing an problem which would be of central concern to many of his scientific colleagues, namely, is there a direction of time?

1.3.1 TIME REVERSAL INVARIANCE

Time-asymmetry has posed one of the most pernicious conundrum in science, for since the time of Newton there had been an incongruence between scientific theory and the human experience of time. Briefly, explanation in classical physics is invariant with respect to time inversion. That is, events in time can be reversed without requiring a different form of explanation. Within Newtonian theory a reversed sequence would make just as much sense. (Prigogine 1980) (See Figure 1.1)

It was the influence of Newton's theories that first popularised the representation of time as a parameter, universal and absolute. Also in Einstein's theories, time acts as a parameter, though now relativised to an observer. Time functions as a *t*-co-ordinate that fits in alongside side the other three spatial dimensions to comprise space-time. In the theories of relativity, time as such has no intrinsic direction. Relativity theory provided its proponents with ammunition against the phenomenological experience of time-asymmetry. The phenomenological subject, they argue, perceives "now" as a unity of existence. Whereas according to the theory of relativity all of the things which the subject observes as now have already happened, because of the time it takes light to reach the retina. In effect, we observe only past events. This has encouraged certain physicists to claim that the subjective experience of the "now" is only an illusion. If the experience of "now" is an illusion, this casts into doubt other subjective judgements concerning time, such as time-asymmetry. Some philosophers have added their support to similar claims. For example, Price (1996) argues that there is a need to distinguish which properties of time are genuinely properties of reality from those perceived properties which are peculiarities resulting from the anthropocentric perspective through which we humans observe the world.

Figure 1.1: The reversibility of Newton's mechanics. (From Coveney 1989)



The reversibility of Newton's mechanics. There is no distinction between time running forwards and backwards. Consider two planets orbiting the Sun as in A. If we film them and run the film in reverse, we obtain B. But how can we alone distinguish which is really going forward in time?
[Adapted from P. V. Coveney. *La Recherche* 20, 190 (1989).]

> Our present view of time and the temporal structure of the world is still constrained and distorted by the contingencies of our viewpoint. (Price 1996, 5)

It has been noted that the ordinary or "folk" experience of time as an irreversible phenomenon seems to directly contradict scientific theory. As we have seen Einstein and other theorists dismissed this contradiction. The phenomenon of temporal irreversibility was dismissed as a curiosity of human consciousness, and therefore only appropriate to a phenomenological level of description. The feeling that we have a past, present and a future is dismissed as a characteristic of the human observer, not existing in scientific reality and therefore not important in terms of scientific explanation.

No serious attempt was deemed necessary to reconcile the perceived arrow of time with other levels of description. An implicit dualism remained for the most part unexamined. It seems to betray a position, which would put the laws of perception and cognition beyond of the laws of science.

1.3.2 ENTROPY AND IRREVERSIBILITY

However a challenge to time reversal invariance comes from within the ranks of science. During the 19th Century the time-reversible theoretical framework of classical mechanics was thrown into dissaray when Sadi Camot (1824) developed his theory of heat transfer in steam engines. His work was to develop into the study of thermodynamics. The physicist Ludwig Boltzmann seized upon the time-asymmetrical behaviour displayed by thermodynamical processes. Boltzmann believed that he could define the direction of time by linking it to another anisotropic process, and increasing entropy offered such a possible process. (Boltzmann 1896-98)

Hereafter it was no longer possible for science to easily dismiss temporal asymmetry as a quirk of human observation for now the putative arrow of time had support from scientific theory. Boltzmann's theories received a highly critical response and themselves offered theoretical loopholes, which other physicists used to defend their fundamentally time-symmetrical theories. For example, Arthur Eddington (1929) argued that the irreversibility of time was an illusion, a subjective impression that originated in exceptional (statistically improbable) conditions in our part of the universe.

The disagreements between scientists over time still remain unresolved. Paul Davies argues that the theories that claim that nature imposes time-asymmetry on everything, including individual particles misleading. There is no time-asymmetry quality to be found. He writes of Boltzmann's *H*-theorem:

> [T]ime asymmetry is only a type of description, relevant to the macroscopic world-view of the physicist, rather than an extra-physical ingredient to be added to the laws of mechanics. (Davies 1974, 4)

However there are equally adamant advocates for time-asymmetry. Whitrow (1980) argues that the Second Law gives substance to the claim that there is some objective temporal trend in nature, and the experience of directionality is not a curiosity arising from the subjective standpoint.

1.3.3 FAR FROM EQUILIBRIUM THERMODYNAMICS AND IRREVERSIBILITY

Prigogine's work on far from equilibrium thermodynamics has added credence to Boltzmann's claim that the direction of time can be defined as the direction of increasing entropy, and also provides a potential bridge between the laws of fundamental science and those governing phenomenological experience. Prigogine argues that in some self-organising, non-equilibrium processes which are inherent in some classes of dynamical system, we find time-asymmetrical activity which is compatible with our phenomenological experience. That is, he posits laws in ontogeny, phylogeny and thermodynamics where later events are not transitive with earlier ones. Prigogine does not incorporate time into his theories merely as a parameter. Rather time is an operator. Prigogine argues that in biological and physiological systems it is as important when things happen as where they happen. Temporal organisms, Prigogine argues, depend on temporal organisation to maintain their integrity. We shall see that Prigogine's theories will provide an important link in any attempt to construct a unified theory of time. (See Chapter 5) The import of his theory lies in the challenge to the assertion, for example, found in Whitrow, that human beings are the only biological life forms that experience time or organise their lives in time. He will challenge the viability of the nature - human dichotomy. Prigogine suggests that we should think of ourselves as "an evolved form of dissipative structure" and that this provides the basis of our being able to distinguish between past and future. As he himself phrases it, biological systems, as well as humans, have a history.

In summary, Prigogine's argument are: irreversible phenomena are as real as reversible ones; irreversible processes play a fundamental constructive role in the physical world; and, irreversibility is deep rooted in thermodynamics, and irreversible processes are also found at the most primitive level.

1.3.4 FUNDAMENTAL TIME-ASYMMETRY AND PENROSE'S "NEW PHYSICS."

Prigogine's work is not the only theory position which aims to bridge the gap between time-symmetrical physics and time as experienced. Roger Penrose in *The Emperor's New Mind* (1989) argues that we need a new theory of quantum gravity, which will be fundamentally time-asymmetrical. Furthermore he also argues that we do not yet have a physics which is adequate to explaining the operations of the mind-brain. We must await this new physics for our explanation of time-perception, as well as for explanations of other deep philosophical problems such as free will. Nevertheless Penrose's position is as yet nothing but a promissory note, with no substantial new theory behind it.

1.3.5 SUMMARY

In brief, it is evident that the concept of time plays different and sometimes contradictory roles in the natural sciences. The source of this conflict is that the symmetrical, isotropic time relations of Einsteinian physics are not commensurable with all forms of scientific explanation, for at certain levels of description irreversible phenomena seem to occur - such as the diffusion of heat. If there is going to be some unified scientific explanation, the incongruence between the seemingly more primitive time-symmetrical laws of physics, and the irreversible, asymmetrical behaviour observed in some chemical and biological systems will need to be explained.

1.4 PRETHEORETICAL TIMES

There is a further difficulty however in trying to align scientific explanation with time as experienced. Even so-called "folk" theories of time embedded within different cultural traditions which would provide the background for phenomenological accounts of the experience of time show no common elements, and therefore can not provide a pre-theoretical description of time, towards which an explanatory framework could be constructed.

A society's description of time is not independent of the overarching epistemological framework within which it is situated. Sociologist Emile Durkheim, in his *Elementary Forms of Religious Life* (1947) argued that time is an essentially social concept, unique to the human species. He claimed that ideas of space, time, number and other general categories are not natural concepts, universal and unchanging. Time is a social institution in so far as it is socially organised. Hence, it takes its form from the structure of society, and changes as the society changes, or is different between different societies. Though we will not agree that a sense of time is unique to the human species (see Chapter 5), it is the case that time has had different structures and functions within different forms of society. Indeed attitudes towards time can differentiate societies. Historian E.P. Thompson (1967) has argued that it is the orientation around clock time which is the main distinguishing fact, separating the capitalist form of society from other forms.

1.4.1 NEWTON AND THE TIME LINE

Much has been written about the historical emergence of our (i.e. Western) folk understanding of time. (See, for example, Whitrow 1988.) If this time were to be represented, then one expect it to be drawn as a single line, divided into equal intervals, heading of into an indefinite past and future. The origins of this form of representation are deeply embedded within the Judaeo-Christian-Moslem tradition and the development of the merchant navy, combined with the rise of the mercantile classes and monetary economy in the proto-capitalist period. (See Dixon, 1993) However the influence of the Newtonian research programme is prevails in this representation of time as a geometrical straight line - the time line. The concept of a linear and teleological history, and the idea of time as the basis of that history, gained popularity in the last century and developed through this. This linear concept of time underpins work in different disciplines; for example, geology (the world as a dynamical and changing environment), astrophysics (the universe as a having its own life beginning with the Big Bang and ending with its Heat Death), biology (humanity emerges out of a long process of evolutionary development) and most importantly philosophy (Hegel and Marx, Kuhn).

Newton's predecessor as Lucasian professor at Cambridge, Isaac Barrow, first described the time line in 1760.

> Time has length alone, is simple in all its parts and can be looked upon as constituted from a simple addition of successive instants or a continuous flow of one instant. (Quoted in Whitrow 1988, 128.)

However, prior to this relatively recent linear form of representation, time was predominantly represented as cyclical. Indeed this representation of time is still apparent in the face of "analogue" clocks. Other forms of religion, which have not had the underlying teleology of the Judaeo-Christian-Moslem traditions, have reinforced a cyclical understanding of time. For example the Hindu, the Zoroastrian and the Mayan cultures believed that the underlying structure of time was one of an eternal recurrence, emphasising the cosmic rhythm of an essentially unchanging world. There are still some cultures who, in living memory, have preserved this cyclical notion of time, for example the Mursi (Turton and Ruggles 1978) of Ethiopia, the Saultaux of Canada (Hallowell 1937) and the Ainu, Hokkaido (Ohnuki-Tierney 1973). Common to all these cultures is the organic quality of time, linked as it is to sidereal and natural cycles, such as seasonal changes, cropping, animal migration and tides. Rather than using calendrical dates to remember events, past events are identified in terms of their relation to other events. Precise datability is replaced by a social consensus about location in time.

However, as Nguyen has noted:

> [T]he specifically western temporal regime which had emerged with the invention of the clock in Medieval Europe became the universal standard of time measurement. Indeed its hegemonic development signified the irreversible destruction of all other temporal regimes in the world, the last vestiges of which remain only in the form of historical and anthropological curiosities. (Nguyen 1992, 33)

Thrift (1990) has described "the making of a capitalist time consciousness," where there is a shift in emphasis from task orientation to time orientation.

> In industrial societies time has become the measure of work where work was the measure of time in earlier historical periods. (Adam 1990, 112)

The increasing importance of time measurement and clocks is well recorded. (See Landes 1983 for a fascinating and detailed history of horology) In a moment reminiscent of Heidegger's *Being and Time*, Lefebvre writes that,

> The dominance of the paradigm of capitalist time consciousness is such that it no longer even leaves us able to think about lived time. (1991, 95-6)

Although the Western capitalist notion of time appears to be the ascendant cultural form, there is no consensus between different cultural traditions as to whether time is linear or cyclical. But this is not the only area of dispute. Cultures differ as to whether they treat time as concrete or abstract. There is also disagreement as to whether time is continuous or discrete, and also whether it can be used instrumentally. (Whitrow 1988) A survey of the so-called "folk" theories of time, embedded within different cultural traditions, shows no universal elements, which might provide the basis for a research programme.

1.4.2 TIME IN ORDINARY LANGUAGE

Even when forgetful of cultural difference, the Western understanding of time, more so than any other concept, has been distorted by the vagaries and metaphoricities of ordinary language and "folk" theory. The language most frequently used to describe or explain time has often served more to obscure than to elucidate the concept.

The metaphor of time as a river, perhaps the oldest of temporal metaphors, is also the most enduring, and least easy to shake off. Philosopher J.C.C. Smart was particularly vociferous against the metaphor of time "flowing," arguing that it leads to all sorts of metaphysical confusion.

> Talk of the flow of time or the advance of consciousness is a dangerous metaphor that must not be taken seriously. (Smart quoted in Davies 1995, 253)

Never the less, despite his warnings, the metaphor pervades both our ordinary language and cultural heritage.

> There is a sort of river of things passing into being, and Time is a violent torrent; no sooner is brought to sight than it is swept by and another takes its place, and this too will be swept away. (Marcus Aurelius, Med 4: 43)

> Time, like an ever-rolling stream,
> Bears all its sons away. (Watts, 1790)

We have already described the Western representation of time as being dominated by the time-line. This representation has also come under attack. Henri Bergson (1896) was referring to this representation when he argued that our understanding of the concept of time had become contaminated by our representation of space. Thus, when time is represented as a line, intersected by break lines, each designating a fixed duration, be it a second, month or year, it gives the impression that time, like space, may be measured with a ruler or tape measure. This, claims Bergson, is misleading.

Another misleading phrase in common parlance is the "sense of time." The term is frequently used in everyday language. However the term "sense" is used in the same way that one uses, for example, a sense of direction or even, a sense of things to come. That is, the term is understood metaphorically, and does not imply that we have a sense of time in the sense way that we have a sense of audition or olfaction. Sense in this latter sense usually refers to the putative five senses, where there is a direct physical or chemical stimulus acting upon the body. Unsurprisingly, there have been those like Ernst Mach, who nevertheless have claimed that time is a sensation experienced like sounds and flavours. On the whole, the evidence is against this suggestion. In Chapter 5, however we shall be examining some of the theories that attempt to explain the sense of time in terms of an internal clock model as in, for example, the work of Treisman (1990).

It is clear that using everyday language as a starting point will not be a helpful departure point from which to try and construct a theory of time. Our ordinary language descriptions of time have long led thinkers astray who should have known better. Our folk theories are just as riddled with conflicting theories as philosophical and scientific theories.

1.5 ARGUMENTS FOR DISUNITY

So far we have detailed the many, diverse, and sometimes conflicting uses of the term time. We have looked at its different rôle in theories in philosophy, the natural sciences, and within different cultures. Overall there has been little common agreement about the term. This lack of consensus indicates that the concept as it is currently being used is ill-defined, and, at the very least, needs to be considerably revised.

However it is not sufficient to note that there are different accounts of time operating within different disciplines. The awareness of these differences and the apparent incommensurabilities between these accounts has enabled certain philosophers to argue and conclude that there can not be a single, unified theory of time. The conceptual disarray surrounding time has shored up the arguments of certain thinkers working within the phenomenological tradition who make *de principe* claims that there can not be a single theory of time. These thinkers have cited, for example, the failure to reduce time as experienced to theories of "cosmological time" as evidence for their claims. We will now briefly examine their arguments.

Upon reading the writings of Wood (1989) and Ricoeur (1984-88), what becomes immediately apparent is their steadfast belief that science's only real concern with time is as a means of measurement. Interpreting the entirety of the scientific corpus in this way enables them to characterise the problem of procuring a unified theory of time as one of reconciling the "cosmological instant" with "the dialectic of the threefold present" of lived experience.

Their belief that science's sole interest in time is as a means of measurement originates, not in an informed and topical familiarity with scientific theory, but in an unhealthy and widespread obsession that considers Aristotle to be the be all and end all of scientific investigation. Their attitude to science is best typified by a remark of Heidegger's in *The Concept of Time* when he describes Einstein's theory of relativity as, "An old proposition of Aristotle's." (Heidegger 1924b, 3) (Though Kuhn also shares some of Heidegger's sentiments when he writes: "[I]n some important respect, though by no means in all, Einstein's general theory of relativity is closer to Aristotle's than either of them is to Newton's." (Kuhn 1970, 207))

An example of this somewhat idiosyncratic attitude towards science can be seen in Volume 3 of Paul Ricoeur's *Time and Narrative* (1988). His thesis is,

> The *aporia* of temporality.....lies precisely in the difficulty in holding on to both ends of this chain, the time of the soul and that of the world. This is why we must go to the very end of this impasse and admit that a psychological theory and a cosmological theory mutually occlude each other to the very extent that they imply each other. (Ricoeur 1988, 14)

In brief, he is arguing that one can not adequately reduce either of these two accounts of time to the other, without losing some explanatory power. He makes an additional claim that, by itself, neither account is able to ground adequately a universal account of time. Ricoeur is quoted as saying that,

> We are not capable of producing a concept of time that is at once cosmological, biological, historical and individual. (Quoted in Virilio 1991, 103)

That is, there can be no unified theory of time, because the theories are incommensurable. However the problem of incommensurability that Ricoeur encounters might be a consequence of the two protagonists he chooses to represent the respective cosmological and psychological positions. For the cosmological account of time he does not choose a Hawking or a Penrose, or Einstein, or Boltzmann, or even Newton. Rather, as we have already indicated, he harks back some two millennia to Aristotle's account of time in Book Δ of the *Physics*. Equally perversely, to represent the psychological explanation of time-perception, he disregards the modern work of Fraisse or Treisman, preferring Augustine of Hippo's fourth century theories concerning the distension of the soul (*distensi animi.*)

The dispute between Aristotle and Augustine is described thus. Aristotle, in the Physics, struggles to define time. He identifies time as being *ti tes kineseos*, that is, something to do with movement, without making time synonymous with it. Aristotle notes that there appears to be no perception of time, without there being a perception of movement. (It is a sorry reflection on philosophy that this debate is still up and running. See for example, Shoemaker 1969.) Time then, Aristotle muses, is linked to movement, that is, change.

> For time is just this - the number of motion with respect to before and after. (The Physics, Book Δ, 219b)

And it is here that we find the origins of the connection between time and the measurement of activity. Even when he modernises the debate, substituting Kant and Husserl, or "ordinary time" and Heidegger, for Aristotle and Augustine, his perspective on cosmological time is so narrowly drawn as to make any scientific explanation seem insignificant. Again, the cosmological account is limited to measurement and chronometry. (Ricoeur 1984-88) If the post-Husserlian scholars are attempting to ground an argument against the possibility of a unified theory of time solely upon the inadequacies of science characterised as chronometry, then clearly they are setting science up as a straw man.

Augustine's phenomenological project fails, Ricoeur argues, because he is unsuccessful in substituting a psychological theory of time for a cosmological one. Psychology legitimately supplements cosmology, however it does not replace a cosmological theory of time. Unlike Aristotle, where the soul (i.e. mind-brain) has no power to produce time, Augustine tries to derive the principle of extension and measurement of time from the distension of the soul. *Distensi animi* is, for Augustine, the condition of possibility for the measurement of time and as such measurement is secondary. Augustine's position can be likened to McTaggart's A-series, where past, present and future are defined in terms of the indexicality of a particular person and their consciousness.

In summary, the debate between Aristotle and Augustine is one between time originating in the world (nature, universe) and time originating in the mind (consciousness, the soul.) Ricoeur argues that though they both explain time, neither account is primitive.

This position is untenable, being constrained by an antiquated and myopic understanding of the scientific project. Our conclusions are similar to those of Adam (1990) who argued that the conception of time which is used in the social sciences (which for my purposes also includes philosophy) is antiquated, and has not taken into consideration recent changes in the way time is understood in the natural sciences. She argued that social scientists are still working with Newtonian and Cartesian theories. This failure to incorporate new information has resulted in the persistence of the incommensurabilities between natural time and social time. (Adam's interest in time is as a sociologist.)

Similarly the position taken by Ricoeur has failed to incorporate advances in the social sciences. His theories are based on a type of science that has long since disappeared, if it ever existed at all. He has failed to take into account advances both on the theoretical and empirical level. Firstly, his conception of how the natural sciences regard their own activity, is nineteenth century. Science, it would seem, is dismissed as a naive, foundationalist and positivistic enterprise. The shift to an anti-foundationalist and empirically driven epistemology is either not recognised or not understood. Secondly their description of the role that time plays in scientific theory is a hotchpotch of Newtonian and Aristotelian physics, and shows scant awareness of twentieth century theory. Without taking into account both these factors, any criticisms of the natural scientific programme must surely be inefficacious and invalid.

1.6 CONCLUSION

So far we have concentrated on the rôle that time plays in a wide range of theories: philosophical, scientific and in folk theories. Though the concept of time is of central importance to all these theories, it is not clear whether each theory is referring to the same concept. There are many contradictory uses of the term. The main area of contention is as to whether time is a real objective property of the world, or whether it is an organisational structure imposed on the world by the human subject. If the latter option were to be subscribed to, then the likes of Einstein and Husserl ironically could be in agreement, as both regard the phenomenon of temporal-asymmetry as being a product of human consciousness, and though their respective ontologies would have little else in common.

In this thesis, I intend to take up the gauntlet thrown down by Ricoeur. He has made an *a priori* claim that there can not be a unified account of time that can incorporate time as we experience it and time as it appears in scientific theory. It is not my intention to construct or to even begin to construct a unified theory of time. Rather my path will take the *via negativa*. In the next chapters, I analyse all of the major arguments against a unified account of time, and, more generally, against the unity of science. I will show that none of these arguments constitutes an insurmountable hurdle to the project, even less so an *a prioi* argument. Furthermore, drawing upon recent arguments from the philosophy of science and the philosophy of mind, backed up by empirical studies, I argue that there is the possibility of a thorough reconciliation of the subjective and objective aspects of time. For the first time, a theoretically integrated concept seems possible.

CHAPTER 2
PHENOMENOLOGY AND ITS CRITIQUE OF NATURALISM IN PHILOSOPHY

I am convinced that the philosophers have had a harmful effect on scientific thinking in removing certain fundamental concepts from the domain of empiricism, where they are under our control, to the intangible heights of the a priori.
Albert Einstein

2.1 INTRODUCTION

A crucial place to start examining anti-reductionist and anti-unificationist arguments with regard to time is the work of German philosopher, Edmund Husserl (1859-1938). His disagreement with Frege over Husserl's book *The Philosophy of Arithmetic* (1891) is regarded as a crucial bifurcation point in the development of Western philosophy, defining the so-called separate strands of anglo-american and continental philosophy. However, despite Husserl's work being usually classed as continental philosophy, Husserl can be regarded as a precursor to several anti-reductionist schools of thought in both the continental and the anglophone tradition. His influence is as cogent on Hubert Dreyfus as it is on Paul Ricoeur.

In the following chapter, the work of Husserl will be examined in two stages. In Section 2.3, we will deal with his criticisms of naturalism in philosophy, which he believed had brought the discipline to crisis point. This will be done by examining two essays written in the first half and towards the end of his career, respectively *Philosophy as rigorous science* (written in 1910-11) and *Philosophy and the crisis of European man* (written in 1936). In both, he advocates phenomenology as the only way forward for philosophy. In Section 2.4, we shall examine his lectures on *The Phenomenology of Time-Consciousness*, which represent an actual example of a phenomenological analysis.

Using these three essays, and drawing on material from the rest of the Husserlian corpus, we will focus on the following aspects of his position. Firstly, the claim that first hand or "lived" experience is not reducible to any natural scientific explanation, that is, publicly available objective description. It will be shown that this assertion is based on a further claim, that scientific knowledge is a secondary, derivative and inauthentic type of knowledge that relies on the intentional act of the subject to constitute and validate it.

As a consequence of this stance, Husserl is able to make several additional claims. Firstly, that phenomenology is the only possible foundation for epistemology. It alone can reveal the eternal truths of thought, and is thus the only possible philosophy. We shall see, for example in his work on time-consciousness that Husserl eliminates all other potential candidates for a philosophy of time on methodological grounds. However we shall question whether Husserl's position is sustainable. Also, having looked carefully at his analysis and results, we ask whether he has gone any way to establishing his claim. That is, show how time is constituted by a transcendental subjectivity.

Secondly we will examine and later challenge Husserl's claim that phenomenology is the only discipline appropriate for the study of experience. On the basis of his study of time-consciousness, we will ask how far is phenomenology successful as a study of the experience of temporality, and whether Husserl can legitimately eliminate, for example, psychological data from his account.

Finally, we will examine the methodological difficulties presented by phenomenology. We shall see that, due to its rejection of natural scientific method, phenomenology requires its own methodology and its own grounds for validation. The adoption of this position, if it is tenable, would have damning and terminal consequences for the project of developing a unified theory of time. However, it shall be argued that the adoption of this isolated position by phenomenology will leave it vulnerable and uncorroborated, whereas the natural scientific approach has meanwhile developed its own sophisticated non-foundationalist and co-evolutionary strategies for validation (See Chapter 4). In conclusion, we shall be asking, Can phenomenology justify its candidature as the philosophy of time? Can phenomenology validate its own methods and results? Are its claims that there can not be a unified theory of time justified?

2.2 THE BASIC PROBLEMS OF PHENOMENOLOGY

Before dealing with the specific essays mentioned, it is necessary to give an brief, overall account of the Husserlian project in order to provide a background for his arguments. Because they assume much of the methodological thought that has gone on and will go on elsewhere, such as in the *Cartesian Meditations* (1929), *Ideas* (1913) and *The Formal and Transcendental Logic* (1928-29).

Although Hegel had already coined the term "Phänomenologie", that is, "phenomenology", for his *Phänomenologie des Geistes* (1807), the school of thought founded by Husserl can not be regarded as originating out of the Hegelian project. Nor does it appear to be related to Peirce's namesake. Furthermore it is wholly distinct from the philosophical position of phenomenalism, that is, the doctrine that human knowledge is confined to the appearances of the senses, although a superficial comparison of the two might suggest some similarities.

Husserl in *Ideas* defined phenomenology as "the theory of the essential nature of transcendentally purified consciousness." (Husserl 1913, 177) Elsewhere the project has been more lucidly described as "an analytical method devoted to describing the qualitative constants of human experience." (Wood 1989, 40) Though this description does not adequately capture the metaphysical underpinnings of the project, for what is patent in the Husserlian definition is that phenomenology is a theory of essences (*essentia*), as opposed to a theory of existence (*existentia*). (Though Husserl would claim that he was very much interested in questions of existence.) This distinction between essences and existence indicates an epistemological stance, which holds there are universal rules of logic, which are applicable to all forms of thought. Husserl's adage was "To the things themselves." By this he meant accessing the essences of things which are common to all of us, regardless of the differences between our empirical experience of the world. In order to illustrate what he means by essences, he uses the example of the colour, red. Thus, we may be presented with two different objects, for example, a strawberry and a ruby. When asked what colour they both are, despite differences in hue, intensity and chroma, both objects could be described as "red." Given that their colour is not exactly the same, how do we identify them both as red? Husserl argues that this essence of redness is not something that could be reconstructed from experience, for our experience of the external world is always of particular objects. Husserl argues that one can never abstract universals from particular existing examples. In the second logical investigation, dedicated to the theories of abstraction found in the British empiricists (Locke, Berkeley and Hume), Husserl seems to suggest that such theories tend to presuppose what they are trying to explain. So where do we get this general concept of redness unless, as Husserl believes we do, we all have access to the same universal essences? In general he phenomenological project sets out to draw up the inventory of all these pure essences of thought. As such, phenomenology claims it is unique amongst all disciplines as it is able to reveal laws of experience which are absolutely valid, everywhere and in all circumstances. Hence in *The Phenomenology of Internal Time-*

Consciousness, Husserl presents us with a set of *a priori* laws which always structure our consciousness of time. (See Section 2.4 below)

Immediately phenomenology is setting itself and its subject matter (essences) apart from the subject matter of the empirical sciences. And grounded in this separation are the explanations both for why phenomenology requires its own methodology and also the justification for its claim to be a foundational or first philosophy. Inherent in the move is the belief that consciousness or experience is not a physical entity in the world, like a brain or a sensory organ, and hence it demands a radically different type of philosophical treatment. Husserl claims that he adopts an “transcendental attitude” to consciousness, as opposed to the “naïve attitude” adopted by the natural sciences.

In the *Cartesian Meditations*, Husserl claims for phenomenology the status of first philosophy as it makes essences manifest. These essences and the transcendental subject, who intends these essences, are arrived at by careful reflection on our conscious life. An example of this is shown in his book on time-consciousness. He argues that it is essences which constitute the conditions of possibility of all our experience and our language, and hence all our knowledge of existentia, the external world. Hence essences or ideas are said to be prior to any existential realisation - an idealist manoeuvre, which has trivialising consequences for the entirety of the natural sciences and the significance for philosophy. This shall be discussed further below. (Section 2.3)

In characterising Husserl's philosophy, two major and connected features will be of particular importance to this thesis, namely, the intentionality of mental states and the methodological device of the *epoché*. The latter is a means to draw ones attention to the former.

2.2.1 THE *EPOCHÉ*

The *epoché*, synonymously known as "phenomenological reduction," "bracketing out" and "transcendental suspension of belief," is a methodological device to draw attention away from both the world as such and also our everyday and unphilosophical (in the Husserlian sense, see below) stance towards the world. It enables the phenomenologist to concentrate instead on experience as it is lived. *Epoché* also has the function of avoiding what Husserl calls “metabasis,” that is, the use of knowledge taken from a “positive science” in founding a theory of knowledge.

The *epoché* has both a purgative and constructive stage. The purgative stage has two outcomes. Firstly it detaches the observer from the naturalistic stance, that is, our everyday disposition, which takes the world and its content for granted (or as given), without attending to the intentional acts which constitute the world *qua* world. Husserl's account of experience stresses the active function of the subject in endowing the world with meaning, rather than it having a simply receptive role. The distinction between the synonyms "subject matter" and "object," the former implying the positive imposition of the subject's cognition over the world, emphasises the difference between the active and passive models. In emphasising the sense-bestowing action of the subject, attention is drawn away from the "inauthentic" public domain of given objects that is characterised by the ease of speech in everyday language. This "bracketing off" of the world as represented in ordinary language will be shown to be problematic for the phenomenological project. The first stage of the *epoché* does not solely suspend the naturalistic stance, it also debars any reliance on judgements about the "world as such" beyond that experienced as phenomena. This is a post-Kantian move with a twist. Like Kant, Husserl moots the subject's cognitive activity as being the only gauge of its own truth. No allusion can be made to any external benchmark as providing a criterion by which to evaluate the truth of thought. Hence for Husserl, "objectivity could never be the measure of subjectivity.....The objectively real is that which corresponds with true thought." (Lauer 1965, 16) But Husserl goes beyond Kant. Kant posited a noumenal world, which was real and out there, albeit inaccessible, as perception and cognition are necessarily structured by the Categories. Husserl discards with the need to reference extrinsic considerations (such as "external reality"). All the information he requires to elucidate the constitutive elements of experience are disclosed adequately in phenomena. Phenomenology thus involves the adoption of a non-committal position towards the existence of an objective, in the scientific sense, material world autonomous of intentional consciousness. Phenomenology is as such an idealist undertaking.

Once the *epoché* has "bracketed out" thought of inauthentic and existential considerations, the phenomenologist's task can then concentrate on revealing its particular subject matter, that is the intentional or meaning giving acts by which the transcendental subject creates its world and focussing on immanent structures of that consciousness. The implementation of the *epoché* and its outcome shall be demonstrated in the discussion of *The Phenomenology of Internal Time-Consciousness*, below.

2.2.2 INTENTIONALITY

Husserl's analysis, he claims, leads us back to the intentional act which constitutes the *object as such*. The grasping of an object has two elements: *noesis* and *noema*. *Noesis* is the act of consciousness, which is correlated with the *noema*, which is the object of the consciousness, in the sense of the object as it appears to consciousness. (It does not imply the existence of an object, independent of consciousness.) *Noesis* is intentionality or directedness (goal orientated activity.) Here the influence of Brentano is apparent in positing the essentially intentional character of all acts of consciousness. Consciousness is always consciousness of something - it is always about something. The two elements, *noema* and *noesis*, are *de jure* inseparable.

The important task of philosophy is then, for Husserl, bringing to light the essentially intentional grounds of our beliefs about the world. Wood (1989, 39) describes the Husserlian project as a search for the primordiality, intuition and immediacy that Husserl believed all previous philosophy had lacked. Rather than an "objective", derived and inauthentic account of experience, Husserl wanted to replace it with a subjectively intuitable, original, authentic account. As will be shown, Husserl in his two essays, *Philosophy as rigorous science* and *Philosophy and the crisis of European man*, argued that the legitimacy of all scientific concepts and principles depends upon this intuitive process.

2.3 TWO ESSAYS BY HUSSERL - *PHILOSOPHY AS RIGOROUS SCIENCE* (1910-11) AND *PHILOSOPHY AND THE CRISIS OF EUROPEAN MAN* (1936)

We shall now look at Husserl's criticisms of the natural science approach. The two essays *Philosophy as rigorous science* and *Philosophy and the crisis of European man* (hereafter *Crisis*) respectively come early and the end of Husserl's writing career. However the opinions expressed in these essays are markedly consistent given the quarter of a century gap between them, even taking into account the major shifts in Husserl's philosophy around 1910. Both are concerned with the crisis, as Husserl judged it, facing philosophy. Principally we are concerned with the criticisms that Husserl voiced against empiricism and its inappropriateness as a philosophical method. However Husserl did not restrict the scope of his criticism. Indeed his dissatisfaction with most, if not all, of his historical precursors is made evident. Hegel's speculative *Weltanschauung* philosophy as well as Dilthey's historical relativism are rejected. Similarly the later essay and its accompanying addenda give voice to his dissatisfaction with his own pupils, most famously Heidegger, as well as the members of the Vienna circle whose "mathematical positivism" Husserl deemed to be sham philosophy. Husserl is not unique in his dismissal of his philosophical peers, as like so many before him, and certainly many more to come, claimed that he would found philosophy afresh and create his own definite “First Philosophy.” Once again, in concordance with many of his peers, Husserl would hearken back to the supposed "Golden Age of Philosophy" of Ancient Greece to establish his arguments.

The crisis then which Husserl judges European philosophy to be facing can only be understood in the context of Husserl's interpretation of the history of thought. In the *Crisis*, Husserl argues that the very identity of Europe is intrinsically linked with the birth and development of philosophical thought. And only European thought, stemming from the Greek tradition, is truly philosophical. European thought is characterised by a shift from the human who is pre-theoretically living in a world and being conditioned by it, to a theoretical stance whereby they are actively investigating that world. For this reason, claims Husserl, only Western thought can be classed as truly scientific in its nature. (Husserl 1936, 171) The Greeks are a turning point in that, for Husserl, they represent the move in thought from the pre-theoretical to the theoretical. This is exemplified by a shift from mythological explanation to *theoria*. Thought predating that of the Greek philosophers is described as the

> natural primordial attitude [....] characterised as life naïve, straightforwardly directed at the world, the world always being in a sense consciously present as an universal horizon without, however, being thematic as such. (Husserl, 1936, 281)

The transformation that heralds the beginning of philosophy takes place when the subject transcends its *Umwelt*, that is, removes itself from naïve engagement in worldly activity, and adopts instead the attitude of an impassive observer. A thirst for knowledge supersedes myths, superstition and unquestioning acceptance of authoritative position.

> Man becomes a non-participating observer, a surveyor of the world. He becomes a philosopher. (Husserl, 1936, 285)

But, and this is the crux of the Husserlian analysis, at the same time as the philosopher adopts this detached and now theoretical stance towards the world, she simultaneously and necessarily has already assumed the ongoing *Lebenswelt.* The *Lebenswelt* has always been there, and hence is the ground [*Boden*] or condition of possibility of theory. In brief, what Husserl is claiming is that we are necessarily involved in the world before we are able to objectify the world *qua* world. And it is this pre-theoretical involvement which is the focus of phenomenological analysis, and which will demand its own unique analytical tools.

2.3.1 THE NATURALISTIC PARADOX

Due to its unique subject matter, Husserl claims that philosophy has never been able to live up to its claim to be a rigorous science. (It will be recalled that Husserl defined philosophy in *Logical Investigation* as the "science of the trivial.") Historically however, rigorous sciences have been founded out of philosophy. Husserl, recalling his academic roots as a mathematician, acknowledges the great advances that have been made in the natural, social and mathematical sciences. However this leaves the empirical sciences with a problem. Husserl has already argued that the natural sciences necessarily presuppose involvement in the world as the condition of their possibility. This being their back-ground, Husserl alerts us to the need to have a genuinely scientific science of philosophy - for it is only when we have secured this that the scientific validity of any of the empirical sciences can be guaranteed. Phenomenology, for Husserl, is then the science of science, or the universal science. Later Heidegger would more famously reiterate this claim in his introduction to *Being and Time*, stating that,

> Ontological inquiry is indeed more primordial, as over against the ontical inquiry of the positive sciences. (Heidegger 1927, 31)

In the *Cartesian Meditations* Husserl returned to the Cartesian project of grounding a universal and rational science through reflective consideration of the thinking subject. The impact of the Cartesian revolution in thought can be portrayed as the inverse of the Copernican revolution in science. Whereas the earth lost her importance by no longer being regarded as the centre point of the universe, epistemologically the objective world becomes dependent on the thinking subject. The subject *qua* thinking substance asserts its importance as the ground for validating knowledge of the external world. Scientific knowledge is active (not "a passive acceptance of alien matter to the mind.") This has repercussions for science as it complicates the pre-Cartesian understanding of objective scientific observation. Though, as has already been pointed out in Chapter 1, researchers involved in the empirical sciences frequently and necessarily pay no attention to such transcendental problems!

However, by Husserl's exacting standards, the empirical sciences scarcely live up to their name. Science as such, for Husserl, is characterised by its aspiration to and achievement of pure, apodeictic evidence, that is, cognition free of conjecture and construction. But the empirical sciences, precisely because they rely on empirical evidence, can not produce apodeictic truths. Science therefore is at odds with its own *telos*. Its aim is the objectively and the eternally true, but its methodology by its nature denies what it presupposes. Through such reasoning Husserl thus is able to reject naturalism and psychologism as bases for not being sufficiently "scientific." Psychology as a science, by its empirical nature, can not reveal anything essential about thought. It can not produce universal truths about the structure of our apperception. So Husserl sets up science as having idealist aspirations, but it is inevitably doomed to failure by its own idealist criteria because of its necessarily empirical methodology. Whether this is an accurate and pertinent portrayal of the scientific enterprise will be questioned later.

So, as has been seen, the foundations of science had to be analysed by a more rigorous thinking - phenomenology. "Scientific philosophy" for Husserl is constituted by an infallible grasp of ideal essences, via intuition. At the same time, Husserl is making a methodological claim, namely, that there can not be a universal methodology for scientific analysis. That the methodology of the natural sciences has reaped success for the natural sciences is not sufficient reason to believe their success can be extended to the scientific study of experience. He has already presented *a priori* arguments for why such methods will not succeed. He also insists that the method should be determined by the object of analysis, not predetermined and prejudiced by the success of certain methodologies in the natural sciences. And the methods of the natural sciences are wholly inappropriate. The naturalist stance, according to Husserl, is defined by its tenet that only the physical is real [*wirklich*] and therefore the object of its attention. (Psychology is naturalistic in so far as it objectifies the mental.) As such it is unable to treat upon non-physical realities, as Husserl identifies them, for example, the lived experience of the subject. Phenomenology, he claims, is the only "method" capable of studying consciousness, as it does not treat consciousness as a physical entity. Through the process of *epoché*, all hitherto physical or external reference are bracketed out thus enabling the phenomenologist to concentrate exclusively on the appearance to consciousness, that is phenomena, in itself. It is via the essences intuited by experience that we grasp the objective validity of conscious experience. So that we can see immediately that there is a contrast between the kind of knowledge that is acquired through lived experience via phenomenology, and the "natural attitude."

Briefly, it is the *epoché* that distinguishes phenomenological observation from other types of naturalistic observation. As we have seen, Husserl wants to bracket out firstly, the possibility of any reference to reality as such as a standard for validating the content of experience and secondly, our naturalistic stance towards experience. Furthermore, *epoché* means that we can not access reality as such, as we have no access to it independently of the world of meaning, which we intend. Additionally the methodology of the natural sciences is ruled out as a basis for a philosophical theory of knowledge as it is oblivious to the foundations of its own truth. That is, it ignores the constitutive activity of giving meaning. In contrast, the phenomenological method investigates that which other sciences take for granted (that is, the essence of their object of investigation.)

For Husserl, philosophical science has to be a strict science - it cannot restrict itself to empirical observation. The world that this strict science studies is not the objective world of nature - that is, the domain of the natural sciences. It can not be objective in the same way as the natural sciences. Rather its object is the *Umwelt* - the "immediately surrounding world" of the mental subject. Natural scientific methods are not the appropriate means with which to analyse mind, as scientific method is a product of mind. Mind requires its own methodology. Husserl's methodological claim that there is no universal scientific method paves the way for certain philosophers such as Paul Ricoeur and David Wood to claim that there are two incommensurable discourses of time - the cosmological and the phenomenological.

After a somewhat sparse and esoteric summary of phenomenology, we shall now examine in detail the methods and analysis used and conclusions reached by Husserl in the *Phenomenology of Internal Time-Consciousness* (hereafter *PITC*). The purpose of this is manifold. It will serve as a practical example of the phenomenological approach when applied to a specific problem. *PITC* is one of very few actual analyses published by Husserl. (Though other attempts exist in his prolific notes.) The most of his books concentrate on the theory of phenomenology. This practical example will also allow us in the next chapters to compare Husserl's treatment of a philosophy of time against other candidates, and to assess how far his method is successful and can be defended against the naturalistic approaches he eschews.

2.4 *THE PHENOMENOLOGY OF INTERNAL TIME-CONSCIOUSNESS*

The writings, which we now know as the *Phenomenology of Internal Time-Consciousness*, were originally a lecture series, first given in the academic year 1904-05. Husserl repeated these lectures with some modifications in subsequent years, up until 1911. However it was not until 1928 that they were published under the editorship of Husserl's student, Martin Heidegger.

The argument of the book can be roughly divided into three stages. After a few opening remarks, Husserl first criticises Brentano's theories of time consciousness. Whether Husserl intended these remarks to be included in the published version is a subject of contention. At the time of his lectures, Husserl was unaware that Brentano himself had distanced himself from his own theories. Husserl later found this out and retracted his comments, but his editor may not have been aware of this. (It is rumoured that Husserl was not well pleased with Heidegger's editing job, and that this might have been one of the reasons. See Wood 1989, 66) The second part of the argument is constituted by a phenomenological analysis of a "temporal object," exemplified in the structure of a single tone. (See 2.4.1.) The third and final part uses this analysis as the foundation of a general theory of time-consciousness, which ultimately is grounded in what Husserl calls "flux."

PITC begins by stating the subject matter of its analysis. The book's self-declared aim is "a phenomenological analysis of time consciousness." (1905-10, 22) Husserl claims to show how temporal objectivity is constituted in what he terms "subjective time-consciousness." That is, he is going to offer an account of time as it is experienced, without any reference to extrinsic factors. For example, this would rule out psychophysical approaches to time experience or time perception where a subject's judgement about the duration of a certain interval of time is compared with chronometrically defined periods. (There will be further in depth discussion of psychophysical approaches in Chapter 5)

However it might be a mistake to describe *PITC* as merely undertaking a piece of introspective and descriptive psychology. The philosophical agenda has important consequences for empirical investigations of time, whether directly or indirectly philosophical. In accord with the general phenomenological project outlined above, *PITC* claims for itself the privilege of the first and only candidate for a philosophy of time. If Husserl's claim is true then this has terminal consequences for all the other contenders. The works of McTaggart, Reichenbach, Grünbaum and a great many other eminent thinkers will be exposed as philosophically naïve usurpers. Whether Husserl's claims are justified will be examined later.

The claim of phenomenology to be *the* philosophy of time is entirely consistent with Husserl's overall disposition towards the sciences. That is, the question of how time is constituted can only be clarified [*Erklärung*] in terms of the essential structures of our experience. His claim that phenomenology is the only approach that can yield the true essence of time flies in the face of the research of many of his contemporaries. Though the study of time is necessarily as old as the study of philosophy itself, it was only at the end of the nineteenth century, with the birth of psychology and the cognitive sciences, that systematic empirical studies were undertaken on time perception. However, it is clear from the outset that Husserl holds such approaches with a lofty contempt. In the opening paragraph, Husserl rejects all of his contemporaries' work on time consciousness, instead favouring the works of Saint Augustine (397), some fifteen hundred years previous.

> Chapters 13-18 of book XI of the Confessions must even today be thoroughly studied by everyone concerned with the problem of time. For no one in this knowledge-proud modern generation has made more masterful or significant progress in these matters than this great thinker... (Husserl 1905-10, 21)

We believe this claim, extreme as it appears, was made in all earnestness. It is towards these "knowledge-proud" types (Dondes 1868, James 1890, Guyau 1890, Nichols 1891) that Husserl's criticisms are aimed, as well as the work of Brentano, explicitly discussed in the first section of *PITC*. Husserl would eschew incorporating any of their findings into his own work. Nor would he find inspiration in the emergent theories of Boltzmann or Einstein, unlike the writings of his contemporary, French philosopher Henri Bergson (e.g. *Matière et mémoire*, 1896, revised 1908), who incorporated material from the ascending general and special theories of relativity into his theories of time. Husserl would argue that such studies do not help clarify the "essence" of time, as they presuppose a certain scientific conception of time already.

Husserl claims a privileged status for phenomenology because naturalistic theories presume an objective time. Phenomenological analysis can actually offer an account of the intentional constitution of the supposed objective time. The phenomenological approach can thus be said to be prior to all these other theories of time because it explains the foundations on which these theories are constructed. That is, it deals with the primitive forms of time. It is on this pretext that phenomenology claims to be First Philosophy.

After this preliminary stage Husserl then moves on to the phenomenological analysis of the temporal object. It is in these first stages of the argument proper that empirical data is bracketed out from the philosophical enquiry. Husserl begins the phenomenological analysis by immediately excluding [*Ausschaltung*] all detail concerning objective time. That is to say that no transcendental supposition about the existence of a world time extrinsic to our time consciousness is being made. (This exclusion move, as has already been discussed, is the theoretical precursor to the phenomenological *epoché* introduced in *Ideas*. The later terminology will be used.) The focus is wholly on time and its structures as they appear in lived experience. Inherent in this move is the adoption of a quasi-Cartesian stance, adapted from the "Method of Doubt," whereby one's cognition is treated as an absolute datum that can not be called into question, whereas the positing of any external world beyond ones consciousness can be.

> Just as a real thing or the real world is not a phenomenological datum, so also world-time, real time, the time of nature in the sense of natural science including psychology as the natural science of the psychical, is not such a datum. (Husserl 1905-10, 23)

So, from the outset, Husserl has prohibited any reference to the world, which might be external to our experience. He accepts only time and duration as they appear to us as absolute data. These appearances, he argues, do not necessarily presume the existence of a world time. However they do presume the existence of the immanent time-of-the-flow-of-consciousness, which he argues provides the conditions of possibility for what is posited as "real Objective time." Whether Husserl actually believed that there was such a thing as real time or world time external to and independent of the intentional act of a subject's time-consciousness has been the matter of much debate. On the one hand, Sokolowski (1964, 75n) argues that at the time of writing *PITC*, Husserl has not yet made the move to an idealist position. Husserl therefore admits the existence of a world time independent of time-consciousness, but as this can not be the subject matter for phenomenological treatment, he pays the topic no further attention. Wood (1989, 60), on the other hand, typically places much emphasis in Husserl's use of phantom scare quotes. Wood argues that already in *PITC* Husserl is ontologically non-committal about the existence of an external world time, that is independent of the intentional (or as Wood stresses, constitutive) acts of our lived experience: "The relationship of constitution can not hold between independently existing things." (Wood 1989, 61) What is certain, though, is that by the time Husserl was writing *Ideas* (1913), which must have overlapped or at least abutted the period in which he was giving the lectures on time-consciousness, he had definitely shifted to an idealist position. (Husserl's position can be described as "transcendental idealism," as distinct from Berkeley's "empirical idealism.")

The prohibition of any resort to extrinsic reference independent of consciousness has important methodological consequences. It also leaves a question or two begging to be asked. As has been seen, by defining the philosophical task as he does, Husserl has used this to eliminate all other (i.e. non-phenomenological) candidates for a philosophy of time. Given the position he has staked out, other ways of addressing and contributing to the question of time, such as cosmological or psychological, can be acknowledged but they can not claim to be philosophical approaches, nor can they form the basis of any philosophical approach. However the question still needs to be raised as to how justified is Husserl's claim that phenomenology is the only true form of philosophy, and therefore on what grounds will he validate this claim, given no extrinsic evidence can impinge on his philosophical project? We will return to this issue.

Turning our attention back to the text, in order to illustrate how the *epoché* operates, Husserl uses the example of space. A phenomenological analysis requires that all transcendent interpretation is subtracted from the phenomenologically given content. Hence we are asked to eliminate all we know about three dimensions, perspective and distance as these are transcendental schema extrinsic to appearance. What is left is appearance in itself, stripped of interpretation. So, his aim is to isolate how the world appears to us, when stripped of its theoretical framework. He claims that once the *epoché* has taken place, then phenomenology would produce the following description,

> Roughly described, this is a two-fold, continuous multiplicity. We discover relations such as juxtaposition, superimposition, inter-penetration, unbroken lines which fully enclose a portion of the field, and so on. (Husserl 1905-10, 24)

Hence we are not able to make judgements of the types that the table is one metre from the chair, or that the clock is above the fire, as these judgements rely on interpreting the visual field within an objective spatial structure that is not given in the phenomenological field.

When the experience of time consciousness is scrutinised, certain necessary, that is, *a priori*, structures become apparent. For example, Husserl's analysis reveals that temporal succession is one thing that we can not doubt. Even though sometimes the order of things becomes confused, even if there is an inversion of the proper order, there is still a succession of events to our appearances. A la Kant, temporal succession is then an essential structure of our consciousness of temporality, that is, a primitive relation.

Husserl then proceeds with his treatment of time and temporality. He begins his analysis by a double exclusion. His first move is to establish the priority of phenomenological time, "the primordial temporal field," over Objective time. Objective time is derivative of lived experience. Objective time is the time Husserl identifies with the time of chronology in which all things and events, physical or mental, can be dated. That is, they "have their definite temporal positions, which can be measured by chronometers." (Husserl 1905-10, 26) Objective time is a transcendent concept [*Transzendenzen*], which belongs to what he terms the empirical order, along with Objective space and the Objective world of real things and events. Phenomenology, as we have seen, makes no such presuppositions about the reality of things and events beyond experience. As such it can tell us nothing about the Objective world. The psychophysical approach to time would not be acceptable as an analysis of internal-consciousness as it relates to objective time. In the Husserliana notes supplementing the *Vorlesungen zur Phänomenologie des innern Zeitbewusstseins*, (1893-1917) Husserl claimed that he would one day carry out a phenomenological investigation of "objective" time. Interestingly, this task was never undertaken.

Husserl also separates the phenomenological study of time-consciousness from other methods, for example psychological. He disregards psychological approaches that presuppose stimuli that produce sensations in us.

> We are indifferent to the question of empirical genesis. What interests us are lived experiences as regards their objective sense and their descriptive content. (Husserl 1905-10, 28)

The study does not concentrate upon temporal determination in the objective physical sense. So he is excluding as different from phenomenology, for example, psychophysical experiments which attempt to correlate variations in subjective judgement about duration to determined chronometric intervals along with certain variables, for example body temperature, drug induced states or sleep deprivation.

> Psychological apperception, which views lived experiences as psychical states of empirical persons...is something wholly other than the phenomenological. (Husserl 1905-10, 28)

So Husserl has no interest in psychological temporal determination. Rather, as phenomenology is only concerned with reality or Objectivity so far as it is intended or represented, his focus will fall upon how we intend the Objective. That is, his stated project is to access what he believes are the *a priori* truths that belong to the moments that constitute objectivity.

2.4.1 THE CONSTITUTION OF THE TEMPORAL OBJECT

After a discussion of Brentano's theory of the origin of time-consciousness, which Husserl finds steeped in "psychologisms," *PITC* proceeds with its own phenomenological analysis. In order to discuss how time itself is intended, Husserl claims that it is first necessary to explain how what he calls temporal objects are constituted. By temporal object, he means a single event or thing that endures over a certain period of time, for example a melody, or even a single note or tone. It is the latter, which he uses to phenomenologically analyse the structure of the experience of time-consciousness and to show how the temporally distinct elements that comprise the apperception of a tone are unified into a single temporal object. He chooses it because of its simplicity. It has minimum sensory content that facilitates the exposition of the barest structural content of time-consciousness. Temporal objects, to be understood as such, have to be encompassed within a "temporally comprehensive act of cognition." (Husserl 1905-10, 40) For example, when a piece of music is heard, it can only be understood as a piece of music, and not just a succession of sounds, because we have an understanding of it as a temporally enduring object: "a unitary apprehension." Though each note of the melody is presented in succession, we recognise them as a single temporal object. However, at any given moment, we only hear a short phase of a melody, or of a single tone. For Husserl, the crucial question is how a moment that is no longer *now* can be incorporated into a unified act: "Is it possible to combine these successive, expiring [*ablaufenden*], representative data into one now-moment?" (Husserl 1905-10, 42) Once the question of the temporal object has been tackled, Husserl will then be able to tackle a phenomenological analysis of time proper.

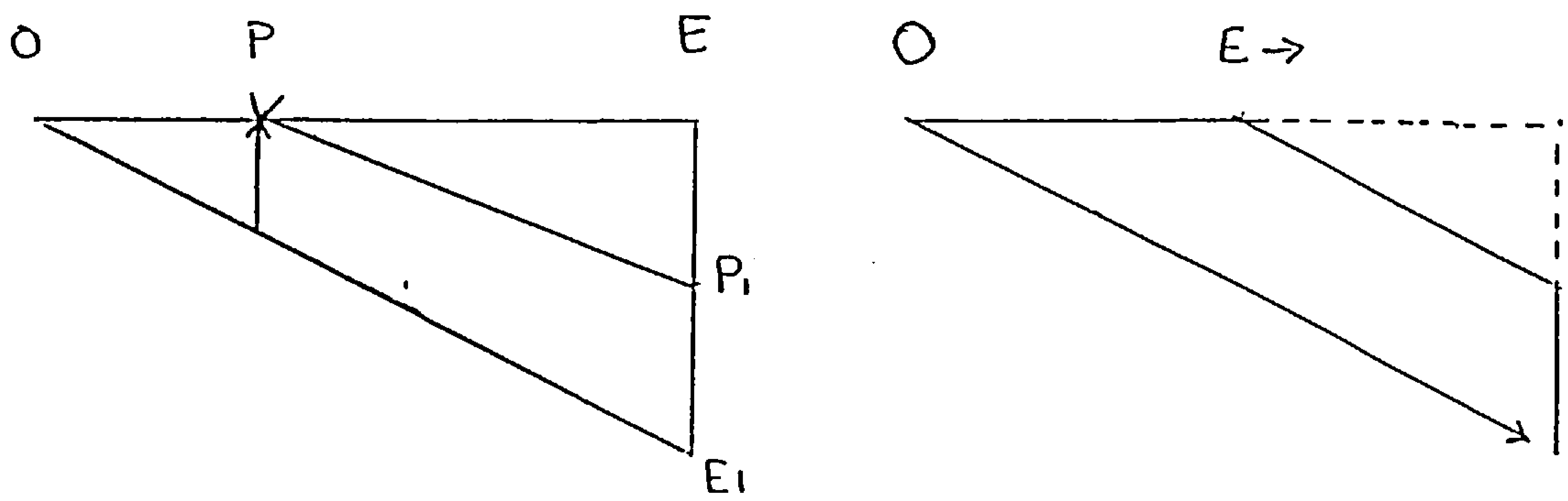
Husserl uses a spatial example to illustrate how we experience the past,

> To my consciousness, points of temporal duration recede, as points of a stationary object recede when I "go away from the object." The object retains its place: even so does the sound retain its time. Its temporal point is unmoved, but the sound vanishes into the remoteness of consciousness; the distance from the generative now becomes ever greater. The sound itself is the same, but "in the way that" it appears, the sound is continually different. (Husserl 1905-10, 45)

Husserl is trying to describe by analogy the continuous action of *ablaufen*, which is so essential to his theory of how we experience the binding together of a temporal event. In order to elucidate further the experience of *Ablaufsphänomene*, Husserl in Section 10 illustrates the structure of time-consciousness with a diagram - the diagram of time. (See Figure 2.1)

The diagram illustrates the constitution of a temporal object, that is, unitary apprehension over a temporally extended content of consciousness. The main purpose of the diagram is to explain how a moment that has chronologically passed can still be retained in present consciousness as part of a unitary temporal object. Other solutions to this question have been suggested such as the "specious present" of James (1890), but Husserl raises several objections to such approaches. (Husserl 1905-10, 41) If we examine the upper portion of the diagram, the line *OE* represents what Husserl calls a "series of now points." Ordinarily this line might be described as a time line, where *O* marks the beginning in time of a certain temporal event, and *E* marks the current point in that event. However it would be a mistake to interpret the diagram in this way, as it will be recalled that chronology as the representation of a single objective time into which all events, physical or mental, has been explicitly rejected. Line *OE* does not represent a series of perceptions that occur in time, but a series of now-points perceived by the subject as successive. It is of no consequence for the phenomenologist that the order as perceived

Figure 2.1: The Diagram of Time. (From Husserl 1905-10, 49)



OE Series of now-points

OE' Sinking-down [Herabsinken]

EE' Continuum of phases (now-point with horizon of the past)

E↦ Series of nows which possibly will be filled with other objects may differ from the objective order. Indeed even to speak of the disparity between experienced time and objective time is an illegitimate move within phenomenological rules. What is of interest to Husserl is the ineliminability of succession from the perception of phenomena. There is no guarantee that the real order of events is being reproduced because of the effect of intentionality, the function of bestowing meaning. Whether we are able to ignore the phenomenon of reconstructing temporal order is a question that will be returned to. However, for the mean time, we can note that it is an area in which incisive advances have been made recently in the fields of cognitive science. The line *EE'* represents the retention of the previous moments or now-points within the current now, so that the temporal object is bound together in a unified act. Without this binding effect, there could be no temporal objects, only a series of disjointed moments. We will reassess Husserl's work in the light of these advances in Chapter 5.

So having established that subjective succession is not to be identified with an objective series of events, Husserl outlines the purpose of the running-off phenomena [*Ablaufsphänomene.*]

> With regard to the running-off phenomena, we know that it is a continuity of constant transformations which form an inseparable unit, not separable into parts which could be by themselves nor divisible into phases, points of the continuity, which could be by themselves. (Husserl 1905-10, 48)

So there is a dual movement of the *Ablaufsphänomene*, both into the subjective past on the *OE* line and deeper into the current moment (*EE'*). So that as each moment passes it becomes further and further into the past, and also deeper and deeper into the present (*T*). The line OE represents this deepening [*Herabsinken*]. (In the upper part of the diagram, he has not yet included protention, that is, anticipation of the rest of the temporal object into his schema.) Husserl distinguishes between the two distinct and mutually exclusive elements that constitute an adequate experience of the temporal object: perception (indicated by the point *E* on the diagram, and sometimes referred to as primal modification) and primary remembrance (*EE '*).

> [A] temporal object is perceived (or intentionally known) as long as it is still produced in continuous, newly appearing primal impressions. (Husserl 1905-10, 61)

So primal impression and retentional modification both play a rôle in consciousness. The structure which makes up the tone compromises of two elements, the now and retention. Crucially both elements are, says Husserl, immediate and non-reproductive. Primary remembrance or retentional modification is a type of memory that occurs whilst the temporal event is still ongoing. Primary retention is non-representational because it is still present. Husserl distinguishes it from secondary remembrance where the temporal event is no longer enduring, but the memory is now present in mind. Husserl prefers to call this re-production or re-constitution in order to emphasise the simulation of the original event, its non-presence, rather than the continuing presence of retention. Husserl further claims that it is only because we have a retentional grasp of the past that we can understand our secondary, reproductive memory as being of the past. That is, without retention we would be unable to distinguish memory from imagination.

In the lower part of the diagram, the concept of protention is illustrated to complete the explanation. Protention or expectation is recognised as an important part in the apperception of a temporal object. For example, when we listen to a piece of music, we have certain expectancies of what is to come and would perhaps be surprised if the music ceased suddenly part way through a bar.

Finally in this discussion of the constitution of the temporal object, Husserl distinguishes between protention and recollection which we would normally term, memory. Recollection is a re-production of an apperception once the temporal object is no longer ongoing. Unlike retention, which is faithful to the order of subjective succession, recollection can offer no such degree of accuracy. For in recollecting, the order of succession can be altered and elements of detail may be added or lost. However, how Husserl can observe this alteration in the order of subjective memory without reference to some other criterion other than itself is difficult to fathom. If belief in the external world, including an Objective time-order, has been suspended, then there can be no way of checking to see if the order of events as reproduced in memory differs from the original apperception of the order of events. For it is the very order of that original apperception that has become disordered in its remembrance.

So far, Husserl has argued that phenomenology is the philosophy of time, because phenomenology is the only method that can offer an account of the constitution of time. His analysis can be carried out without any reference to the world, external to our experience. He has ruled out all other types of analysis, from cosmological theories about time to psychological theories, because they presuppose an objective time. In his phenomenological analysis of the temporal object, however, we have seen that his method runs in to problems, precisely because he cannot refer to any extrinsic data to support his theories.

2.4.2 THE ABSOLUTE, TEMPORALLY CONSTITUTIVE FLUX OF CONSCIOUSNESS

Husserl, having analysed the structure of the temporal object finally moves on to discuss the origins of time-consciousness, that is, the constitution of time. However, before he does this he presents the reader with some general comments about the *a priori* characteristics of our experience of time. In Section 33, Husserl reasserts that time has the following essential characteristics,

> 1) that there is a fixed temporal order of an infinite two-dimensional series.
> 2) that 2 different times can never be conjoint
> 3) that their relation is a non-simultaneous one
> 4) that there is transitivity, that is, to every time there belongs an earlier and a later time. (Husserl 1905-10, 29)

Here he does no more than reiterate the statements he made earlier in Section 2 of the book, where he promised that he would,

> try to clarify the *a priori* of time by investigating time consciousness, by bringing its essential constitution to light. (Husserl 1905-10, 29)

So it is a disappointment when Husserl simply restates these *a priori* characteristics without significant further clarification. For example, his analysis of transitivity is restricted to: "[I]f A is earlier than B, then B is later than A." (Husserl 1905-10, 97) This is patently not ground breaking material. Husserl himself describes these *a priori* characteristics as "self-evident laws." (Husserl 1905-10, 24) But if they are so self-evident, it seems unlikely that he could claim this as an important result of his analysis. Indeed it appears his philosophy *is* the "science of the trivial." He is trying to establish the essential constitutive laws of experience, that is the most basic temporal elements, by which all phenomena are determined. Husserl wants us to accept that through phenomenological analysis we can access these primitives which, because of their *a priori* characteristics, are necessarily pre-theoretical. Whether this form of analysis can justifiably sustain this claim will be discussed in detail, but it is noted here as one of the problematical claims in *PITC*, and with phenomenology in general.

Having described the constitution of temporal objects, Husserl now looks at the origin of the constitution. This analysis should reveal the most primitive source of temporality - what Husserl calls "the absolute flux of consciousness." In order to describe this deep underlying structure, Husserl struggles with the connotation and denotation of ordinary language. Husserl states that he wanted to eliminate the inauthentic language of public speech, but is constrained by it as the only mode of expression. His most explicit attempt at defining this flux concludes with a defeatist note.

> It is absolute subjectivity and has the absolute properties of something to be denoted metaphysically as 'flux', as a point of actuality, primal source point and a continuity of movements. For all this names are lacking. (Husserl 1905-10, 100)

One tactic he is able to adopt is metaphor as a form of indirect communication, but this does not come without its drawbacks. The metaphoric use of the term "flux" [*Fluß*] brings with it many connotations, which Husserl does not want his concept to inherit. Consequently Husserl's description is almost totally characterised by statements of elimination – a *via negativa*, as it were. Accordingly, flux is not a process, in the sense of something that proceeds in time. Consequently flux can not be described in the same manner as a temporal object, for flux is the *a priori* condition of all temporal objects. Similarly, as this is the ultimate source of temporality, Husserl tries to rule out what he calls the "absurd property" (logically speaking) of the flux flowing more or less quickly. For if flux could flow more or less quickly, then there would have to be another more primitive metric of time which determined this change of speed. In order to prevent this regress Husserl has to say that flux does not manifest any temporal properties. In order to skirt around the problem of when these constitutive acts occur, he attempts to remove the description out of the temporal domain.

> Temporally constitutive phenomena are, in principle, objectivities other than those constituted in time. (Husserl 1905-10, 101)

> Subjective time is constituted by an absolute timelessness. (1905-10,150)

This obscure reference to a primordial "flux" is as far as Husserl's analysis can proceed. It will be recalled that Husserl had said that he is not concerned with questions of empirical genesis. That is, if such a flux were to exist, Husserl could not explain its origins in scientific terms, for example locating it within a theory of thermodynamics. It will be recalled that this is because to have a natural scientific theory in which objective time plays a part presumes its intentional constitution. Husserl steadfastly aims to construct an idealist first philosophy.

> Pre-objectified time, which pertains to sensation, necessarily, founds the unique possibility of an Objectification of temporal positions. (Husserl 1905-10, 97)

2.5 SOME PROBLEMS WITH PHENOMENOLOGICAL ANALYSIS

Phenomenology as a project in its own right has been criticised at length by many different schools of thought, by Marxists, existentialists, the Oxfordites. However our interest in Husserl's *The Phenomenology of Internal Time-Consciousness* has been his objections to naturalism in philosophy, and particularly how they apply to other theories of time. We saw that Husserl has claimed that first hand or lived experience is not reducible to any form of natural scientific explanation. He has claimed that scientific knowledge is secondary and inauthentic, and relies on intentional act of subject to validate it. That is, scientific knowledge needs "prior" validation. Phenomenology is the only candidate for a philosophy of time, given that natural sciences are unable to deliver universal truth and laws. Hence, phenomenology is the only rigorously scientific basis for epistemology, as it is able to deliver universal truth and laws. Phenomenology is the only discipline appropriate for the study of time-consciousness and experience in general, as all other approaches presuppose an objective time. As there can be no universal methodology in the natural sciences, there can be no unified theory of time.

However we have identified several problems with Husserl's analysis that seriously undermine his claims, and hence his objections to the project of a unified theory of time. Firstly, there are no criteria by which Husserl's can validate his phenomenological analysis.

This is a consequence of phenomenology's self-imposed hermetism. Certainly the results of his investigations are not going to be independently repeatable and verified as, for example, in natural and social scientific experimentation. Nor can phenomenology rely on producing the same sorts of proofs, which are available to mathematicians and logicians. Science, on the other hand, has realised the difficulties of trying to establish a foundational epistemology, and has developed its own highly sophisticated methods of validation, or partial corroboration. We shall discuss these in Chapter 3. But phenomenology, by its very own definition has ensured that none of these mechanisms are available to validate its claims. Secondly, it seems unlikely that Husserl can simply ignore any scientific input to its project? In the next chapter we will argue that *a priori* approaches, in general, are in the wane. Thirdly, Husserl does not successfully answer his own question, namely, how is time itself constituted? Husserl at no time claims that he is going to give any account of Objective time. This is because any talk about real time, as well as talk about the real world or real things, is explicitly excluded as not part of a phenomenological datum. So it may seem unfair to criticise him precisely for his inability to deliver on this subject. Indeed in the opening pages of *PITC* he claims that nothing can be found out about Objective time via phenomenological analysis, as these are all transcendencies [*Tranzendenzen*]. Objective time is only bracketed in order to see how this becomes something transcendent for us. But given this, can the phenomenological *epoché* deliver? Or is Husserl being naïve in believing he can access these constitutional acts without his observations being distorted by theory. Philosophers broadly sympathetic with his aims have even questioned Husserl's claims. For example, Derrida (1967) challenges whether Husserl can abstract autonomy of intention from the signification found in language.

There are clearly many problems inherent with the phenomenological programme, and Husserl's claims remain unproven. He has not been able to construct a sustainable argument for his *a priori* philosophy of time. Furthermore, many of the difficulties have been compounded by the very aspect of phenomenology that is meant to bestow it with the status of first philosophy – its hermetism. It seems that phenomenology, in its defining act, dooms itself.

CHAPTER 3
TIME, PHILOSOPHY AND SCIENCE

> *For the entire universe, the two directions of time are thus impossible to distinguish, and the same holds for space; there is neither above nor below*
> Ludwig Boltzmann

> *The problem of time can not be solved by an appeal to intuitive knowledge.... If there is a solution to the philosophical problem of time, it is written down in the equations of mathematical physics.*
> Hans Reichenbach

3.1 INTRODUCTION

In the last chapter, we examined Husserl's *a priori* argument that our subjective experience of time-consciousness cannot be given an objective and natural scientific explanation.

In this chapter we will examine some different *a priori* arguments within the philosophy of science, which are similarly concerned with the question of whether empirical evidence can be brought to bear on questions about the structure of time. They also address the question of the ontological status of time – are we entitled to say that time exists? Or is the concept of time just a useful fiction that helps to bind together certain scientific observations and laws.

We will argue that evidence from the fundamental sciences can be brought to bear on philosophical questions about time. This has become possible through recent developments in the philosophy of science. There has been a shift away from both foundational questions of meaning, and an emphasis on individual laws and their evidence based on observation. This has been replaced by an emphasis on research programmes, bootstrapping, the coevolution of theories and super-empirical virtues.

3.2 THE DECLINE OF THE *A PRIORI* APPROACH

In the previous chapter, we saw that Husserl characterised the founding act of European thought as a rejection of mystical and religious types of explanation, in favour of a naturalised approach. He claims that it was out of this demystification of the world that Western philosophy and science were born. At this early stage of development, the pre-

Socratic and Socratic philosophers had not yet made a demarcation between philosophy and science. For example, it is unclear whether Heraclitus and Empedocles were proposing philosophical or scientific theories when they attempted to identify a primary substance that constituted the world. By the time of Aristotle we can see the differences between the two disciplines beginning to emerge in the division of content between *The Physics* and *The Meta-physics*. *The Physics* addressed questions that Aristotle believed could be answered by empirical investigation, rather than metaphysical musing. He includes a section on time in Book δ.

Since his time, the domain of metaphysical philosophy has been in decline. History has witnessed an academy of disciplines breaking away from metaphysics and establishing themselves as independent subjects. As we have seen physics and also mechanics were some of the earliest subjects to break off and establish themselves in their own right. They were soon followed by other *natural* sciences. The nineteenth century saw the birth of the so-called human sciences with the foundation of the independent disciplines of economics (Ricardo), sociology (Durkheim) and psychology (Wundt). With an emphasis on observation and controlled experiments, these disciplines wanted to emulate the methods, and hopefully the success, of the natural sciences. This century has witnessed the empirical sciences successfully encroaching on one of the last bastions of philosophical investigation – the mind. Evidence and theories from neurophysiology and cognitive sciences are being brought to bear on philosophical argument. Virtually all aspects of the world and our mental life seem receptive to naturalistic explanation. And certain scientific discoveries are forcing philosophy to re-examine its construal of reason, explanation and understanding. (I will return to this issue in Chapter 5.) In response to these advancements in knowledge, there has been a gradual shift in philosophical fashion away from *a priori* arguments, in favour of empirically informed theorising. The areas over which philosophy exclusively reigns are becoming increasingly restricted, so the question might be raised as to whether there is any rôle left for philosophers to play. Or has the Queen of Metaphysics been well and truly dethroned, and her lineage traced to its vulgar origins in common experience? (Kant 1769-80, 8)

Undoubtedly the rôle of philosophy and its relation to science has changed. The modern view is that philosophy is no longer an extension of science, in the sense of it being *meta*-physics – beyond what physics is able to explain. It serves rather as a critical observer of the empirical sciences, clarifying their conceptual relationships, describing the methods employed and exploring the ground for rationalising these methods. These grounds do not mere include consistency with observational data, but other forms of rational support such as judging theories on their simplicity or explanatory power. We shall return to this in this and the following chapter.

3.3 TEMPUS ORDINE GEOMETRICA DEMONSTRATA

The idea that scientific observation and experimental data can be brought to bear on questions about the nature of time is relatively recent. Although Aristotle included time in *The Physics*, those who succeeded him, Augustine, Spinoza, Leibniz, and Kant, firmly established time as an item for metaphysical investigation. In Chapter 2 we saw that this opinion still prevails in twentieth century thought, though not all its holders come from the phenomenological school of thought. Looking at some of the reasons for the persistence of this opinion will be useful. Understanding why an *a priori* theory might be possible will elucidate the particular difficulties of trying to construct an empirically informed theory of time.

The belief that it is possible to construct an *a priori* concept of time is a long held one. The persistence of this belief can be explained by the apparent and longstanding success of the *a priori* model of space furnished by Euclidean geometry. For many centuries, geometry was to provide the paradigmatic example of a theory which was "*a priori*" – that is, knowable by pure reason and invulnerable to empirical evidence. Starting with a finite number of axioms and postulates, which are first principles and "true", it was believed that the whole of the rest of geometry could be logically deduced therefrom. (We now know that Euclidean geometry is incomplete.) The ten axioms and postulates were for the main part self-evidently simple, and yet abundantly productive of secondary theorems. As a result of its fertility, Euclidean geometry became the epitome of rational theory, and other disciplines aspired to its self-contained and rational rigour. (Sklar 1974, 9-156.) Many thinkers tried to equal its simplicity and rigour in their own theories, dissatisfied with the contingency of theories that were founded upon observation, both because of the fallibility of human perception and the problem of inducing general laws from particular instances. Geometry in contrast seemed immune to such flaws. For centuries, many thinkers believed that an equivalent degree of certainty was possible in other fields of enquiry. They tried to emulate geometry's success. The most beautiful example of the application of the geometric method is, of course, Spinoza's *Ethics* (1661-75). Geometry provided the "prototype of a demonstrable science." (Reichenbach) We now know that it is a matter of historical contingency that Euclidean geometry was the only geometry available for so long. Two millennia would pass by before Lobachevsky, Riemann and Gauss would posit their alternatives. The development of alternative systems prompted thinkers to speculate that space might have different properties from those permitted by Euclidean geometry.

Prior to the development of alternative geometries, Euclidean geometry provided the model for any aspiring scientific or philosophical theory. It was the exemplary theoretical systematisation of a vast body of truth. It furnished the model of a science, which could lay claim to completeness and veracity. Given the persisting success of Euclidean theory, it is not surprising, therefore, that it was believed a similar theory could be constructed for time. Likewise it might start with a small number of true and self-evident first principles, from which all the other truths about time could be deduced. After all, does it not seem that there are certain self-evident and simple truths about time? Perhaps the single most self-evident assumption concerning time is that it has a direction. That is, events which are not simultaneous stand in a relation of temporal order to each other, so that one event is always said to be *earlier* than the other, whilst the other is always said to be *later*. The iteration of this concept of temporal order is so deeply embedded in ordinary language and in the way we describe the world that it is almost a truism. As we have shown, the asymmetry of time is virtually all that Husserl is able to surmise in *The Phenomenology of Internal Time-Consciousness*. (See Chapter 2) However, our suspicions about the steadfastness of any so-called self-evident truths about time should have awakened when Aristotle and Augustine's theories held so little (if anything) in common. (See Chapter 1)

The dispute between Leibniz and Newton is a significant moment, as for the first time, it plays a central rôle in a scientific theory. Their argument focused on different and conflicting theories of time. Leibniz denied that time is independent of the events that occur in the world. It was an abstract relation. He argued that talk about time could be reduced to talk about temporal relations between events. According to the Leibniz theory, if there were no events *at all* there would be no time, and one therefore could not talk about empty time. He argued, however, that though time has no independent existence, temporal relations between events were real. On the other hand, Newton thought that time and space were something over and above the events that take place in them. Sklar points out that Newton was often inconsistent in what exactly he thought time and space were, sometimes referring to them as if they were a substance and sometimes referring to them as if they were a property, or even an attribute of God. (Sklar 1992, 22) Newton's theory of time is sometimes referred to as the "container theory of time." Crudely, this theory treats time as some kind of placeholder, into which all events can be located in their temporal order. The Newton-Leibniz disagreement is also significant in its lack of resolution. Recent works by Earmann (1989), Sklar (1974) and Newton-Smith (1980) testify to the vitality of the debate. Newton-Smith blames the evasiveness of a solution on the underdetermination of theory by data. (Newton-Smith 1980) This is significant as it highlights the most important aspect of Newtonian theory, that is, space and time play a crucial rôle in scientific theory, whose existence is assumed in order to explain the behaviour of phenomena at the observational and experiment level. (Sklar 1992, 23) For Newton, they are very real features of the world, but they are unobservable in themselves. The only available evidence for their existence comes indirectly through the success of the theory that they underpin. Significantly, also for our thesis, their disagreement represents an important juncture in the history of the concept of time. Time here becomes an item for scientific investigation rather than metaphysical speculation, raising question about its ontological status.

3.4 INSTRUMENTALISM

As we have just noted above, Newton posited the existence of space and time in his theory though neither can be observed. However the issue of the ontological status of theoretical entities is not cut and dried. Does the positing of a theoretical entity in a successful theory necessarily mean that the entity actually exists? Various philosophical solutions have been proposed to circumvent this problem. One response is the instrumentalist position. An instrumentalist claims that theoretical, unobservable entities should be credited with being no more than useful fictions. The positing of theoretical, unobservable entities helps connect one series of observations with another. Only those entities that are observable can be said to exist. The position is highly conservative, only admitting observation as a reason for positing a theoretical entity's existence.

Historically the instrumentalist position may have been adopted out of vital necessity, that is, cultural necessity rather than necessity of the philosophical kind. For example, Osiander famously tried to save his bacon during the time of the Inquisition, by arguing that Copernicus was not actually claiming that the earth orbited the sun. Rather that this caprice was a mere mathematical model, useful for predicting the orbit of the planets, and made no claim about reality whatsoever. Osiander wrote in the Introduction of Copernicus's *On the Revolutions of the Heavenly Spheres*,

> [T]hese hypotheses need not be true nor even probable; if they provide a calculus consistent with the observations that alone is sufficient. (Osiander quoted in Rosen 1959, 250)

In its most naïve manifestation, instrumentalism could accommodate explanations that were superstitious, magical or mythical, if they fitted in with observations. So, providing it fits in with the observable data, it would be just as acceptable to explain earthquakes in terms of a big underworld fish, flipping its tail, as it is to explain quakes in terms of plate tectonics. An instrumentalist makes no commitment to saying what the world is really like, which is in many ways the antithesis of the overall aim of science. It falls short of being a "bold" conjecture in Popper's language. In fact, it conjectures nothing at all. It does not amplify our knowledge of the world.

However, certain unobservable theoretical entities, which are meant to be useful fictions, sometimes turn out to actually exist. Chalmers (1976) illustrates this point with the example of benzene.

> The idea that the molecular structure of some compounds, benzene for instance, should consist of closed rings was first proposed by Kekulé. Kekulé himself had a somewhat instrumentalist attitude towards his theory and regarded his ring structures as useful theoretical fictions. On this view, it must be regarded as a remarkable coincidence that these theoretical fictions can be seen almost "directly" through electron microscopes. (Chalmers 1976, 117)

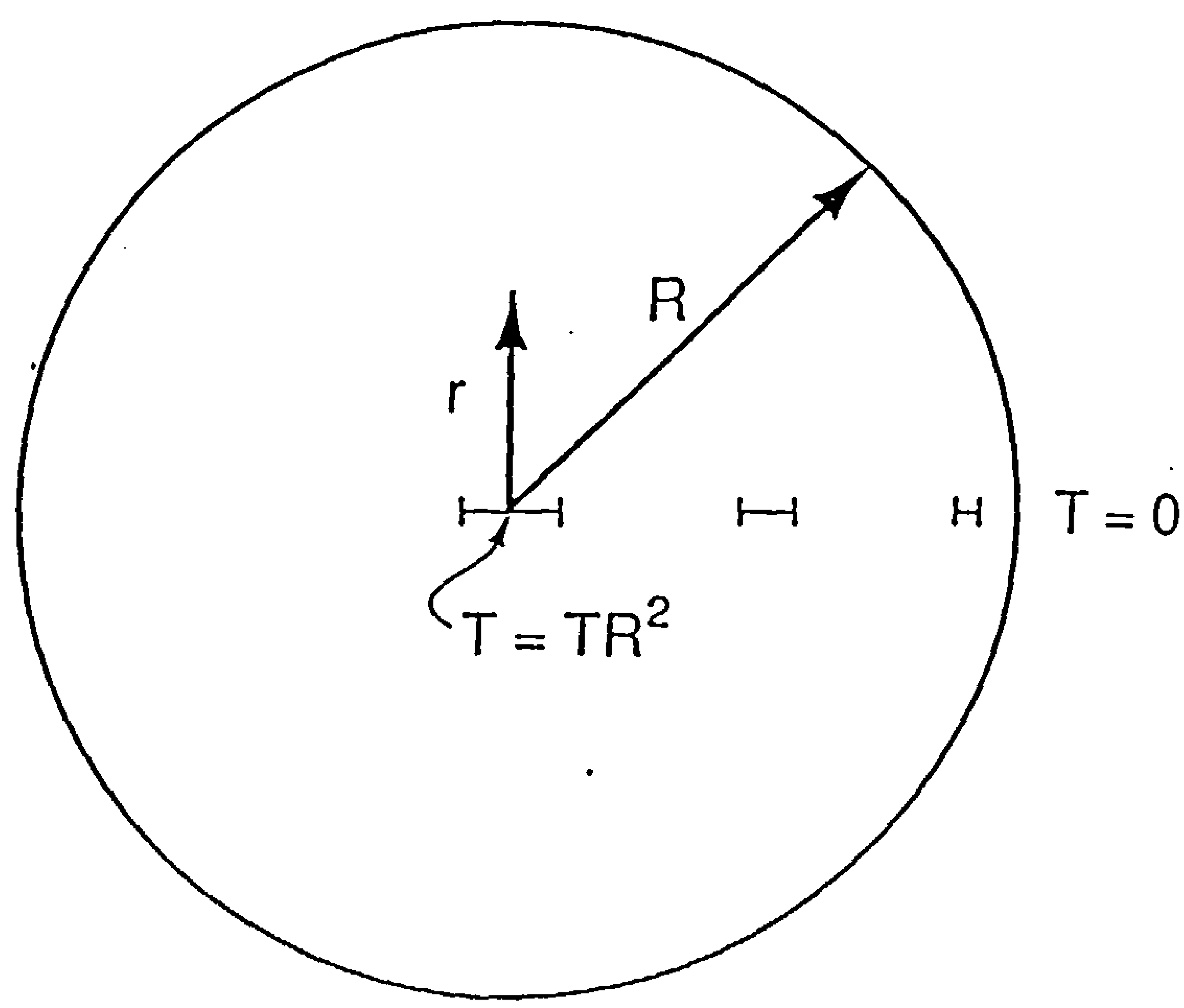
The fact that good theories can sometimes pre-empt scientific discovery suggests that there may be other reasons, other than observation, for believing an entity to exist. In later sections we will also show that observation is not so straightforward.

3.5 Conventionalism

The conventionalist offers a different approach to dealing with unobservables. Before specifically addressing the question of time, I will continue with the historical example provided by geometry. It will be recalled from Section 3.2 that Euclidean geometry for a long while was regarded as providing *the* topology for space. Then, during the 1820s, Bolyai and Lobatschevsky developed competing alternative non-plane geometries, in response to the infamous problem of "the axiom of the parallels". (Reichenbach (1958, 3) notes that Gauss is said to have tackled this problem earlier, but did not publish his work.) The development of the alternative geometries raised two new problems. First, which of these geometries was the actual geometry of the world? For all these new geometries appeared to be as internally consistent as Euclid's. Secondly, what sort of evidence could be brought to bear on this question? Previously it had been believed that the true geometry of the world could be posited by the operation of pure reason, without any reference to observation in or experiments on the world. Henri Poincaré devised an argument that would leave the *a priori* approach to geometry intact. Poincaré argued that the geometry that we choose to adopt is a matter of convention or decision. It is possible to adopt mathematically any of these geometrical systems without being right or wrong. To illustrate his argument he uses the thought experiment detailed in Figure 3.1 which shows that no experiment could determine any particular geometry as the true geometry of space providing that one is willing to make the necessary changes to other parts of ones theory. (Sklar 1974, 91-91) Poincaré concluded therefore that it is a matter of convention which pure geometry was used to describe spatial relations between bodies. And moreover, he claimed that Euclidean geometry would be the normal choice because it is the simplest. (Though this is a matter contended by his critics, Eddington (1920) and Reichenbach (1958).)

Conventionalism is appropriate only in cases where items can only be known via inference, and not by any sort of direct observation or experiment. However Poincaré's claim is that space and time can only ever be known conventionally, which implies that there could never be any empirical evidence which could be brought to bear on questions about their structure. This rules out two possibilities. Firstly, that some experiment or observation might be made in the future that could inform our hypotheses. That is, that knowledge of space and time is indifferent to empirical evidence. Conventionalism would not be appropriate if some decisive experiment or observation could be made. And how can Poincaré say definitively that so such observation can ever be made. For many items in the world which were once unobservable, are now observable. Furthermore, many uncontroversial features of the world are unobservable, for example, gravity. We never directly observe gravity, only bodies which theory tells us gravity acts upon. Secondly, that some future paradigm shift might result in our re-evaluating our current experience of the

Figure 3.1: Poincaré Parable (Sklar 1992)



Poincaré's parable. Two-dimensional inhabitants are confined to the interior of an ordinary disk on the Euclidean plane. They are equipped with measuring rods that change length with temperature in a linear way. The temperature at the center of the disk is TR^2, where R is the radius of the disk and T is a constant. At any point on the disk the temperature is $T(R^2 - r^2)$ where r is the distance of the point in question from the center of the disk. At the rim of the disk, then the temperature goes to zero and the measuring rods shrink to zero length. It is easy to show that if the inhabitants take their measuring rods as having constant length, they will come to the conclusion that they live on a nonflat Lobachevskian plane that has constant negative curvature and extends to infinity.

world so that we could see its temporal or spatial structures. Certainly this has happened many times in science. For example, the shift from terracentric to heliocentric theories of the planetary motion meant that we were able to see the structure of our solar system.

Certain scientists are reluctant to admit that important elements of their theories about the world are based on conventions, agreed by the members of a scientific community, rather than on facts. It directly challenges the founding tenet of scientific practice, namely that all claims to knowledge can be determined by independent means, that is, by reference to empirical facts. In terms of time, a conventionalist might argue that, because of underdetermination by data, it is impossible to make a judgement about the structure of time. Therefore all that is possible is an empirically arbitrary decision about its topology and its metric. We are unable to make meaningful statements about the nature of real time. A weak version of conventionalism holds that it is justifiable to introduce conventions into physical theory just in case it proves fruitful in subsequent research. Indeed some element of convention seems very much to be an ineliminable part of any scientific research programmes. Lakatos (1980) argues that there is a rôle for convention in his description of a research programme, in so far as the scientists involved in a certain research programme decide to accept its metaphysical core. For example, in the research programmes based around the special and the general theories of relativity, we might include amongst the conventions the claim that nothing travels faster than the speed of light in a given direction, and the prohibition of super-luminal particles such as the hypothetical tachyon.

There have been several other attempts to circumvent the problem of underdetermination by data when it comes to selecting a structure of time. For example, Newton-Smith (1980) in *The Structure of Time* argues in favour of what he calls the *standard topology of time*: an open interval of real numbers based on a topology that is linear, unified, non-ending, non-beginning, and that can be either discrete or continuous. His justification for adopting such a topology is not based on convention, but based on a form of *a priori* argument, which claims that the standard topology is applicable in more possible worlds than any other topology. But Newton-Smith's argument is careful to state that his argument is only appropriate in situations where we have no knowledge of the constitution of the world. Should decisive empirical evidence come to the fore that would inform his choice, Newton-Smith would relinquish his claim.

3.6 REICHENBACH AND COORDINATIVE DEFINITIONS

As we have already seen, Reichenbach was unhappy with the conventionalist position. He argued that what appeared to be competing alternative geometries were not alternatives at all, but merely different expressions of the same theory. Conventionalism defined the meaning of theoretical terms by fiat, where meaning could neither be posited *a prioi* (by analytic statement) nor known by empirical investigation. Reichenbach offered an alternative account of the semantics of theoretical terms. He argued that if we wish to use a term in science, we must first define it. Reichenbach distinguishes between two sorts of definition – conceptual and coordinative. By a conceptual definition he means a definition which involves reducing a concept to other concepts. Hence, using spatial measurement as an example, we can conceptually define a standard unit of length as a distance which when transported along another distance, supplies the measure of this distance. Typically this unit would be represented in the form of a measuring rod or a ruler. However he argues that physical knowledge is additionally characterised by its co-ordination to real objects in the world. (Reichenbach 1958, 14) Hence, if one wanted to measure a distance, then one would have to determine in advance, which unit of length is to be used by definition. This unit is defined with reference to a physically given length. For example, a metre was defined at the time of the First French Republic as one ten millionth part of one-fourth part of the terrestrial meridian as calculated by Delambre and Mechain. In 1799, this standard metre was reified in a platinum rod encased in the National Archive of Paris. So the characteristic of coordinative definition is the co-ordination of a concept to a physical object, in this case a fraction of the terrestial meridian. There is an element of fiat in Reichenbach, but it is not wholly conventionalist. Using a second example, temperature, he argues that the scale of measurement that we choose is arbitrary, and is a subjective matter. However the indication of the temperature of a physical object is not a subjective matter. It measures an objective feature of the world. Hence the scale is given a coordinative definition, that is, to measure the temperature. The coordinative definition lends an objective and physical meaning to the measurements.

> A statement about the boiling point of water is no longer regarded as an absolute statement, but as a statement about a relation between the boiling water and the length of the mercury column. (Reichenbach 1958, 37)

How can the use of coordinative definitions help us in reference to time? Reichenbach argues that it becomes useful when we ask the question, how do we know that two events have the same duration when they do not start and stop at the same time? Usually we judge that they are of the same duration by reference to a third party, namely a clock. However there are several concrete problems associated with this. The first problem is one of the accuracy of measurement. For example, imagine I were sitting in the Grandstand at Silverstone during the British Grand Prix. If I tried to measure the lap time of the cars with my wristwatch which only has second hand, given the information available, I would have to conclude that, say, the top twelve cars were lapping at the same speed. However the FIA's TAG Heuer timing system, which is accurate to within a thousandth of a second, could discriminate between times that would determine pole position and the cars on the sixth row. Hence judgements about *isochrony* are dependent on the accuracy of the measuring instruments and the resolution of scale. As Newton-Smith points out, in such cases, we are not able to say that events are isochronous. All we are able to say is that the lengths of the measured events are indiscriminable, relative to a certain temporal scale. Thus, in order to say that two events are the same duration, we go beyond the available data. (Newton-Smith 1980, 143-175). Bouchareine alternatively articulates the problem,

> Precision is the relationship of measured value to the value of its uncertainty. One could say that precision is its inverse: relative uncertainty. (Bouchereine 1978, 643)

The second problem is how do we know that the clock always measures the time at regular intervals, that is, remains isochronic? We know experimentally that a clock carried on a jet aeroplane, or taken to a high altitude, will not keep synchronised with its twin clock on the ground. It runs faster as a result of gravitation time dilation. (Coveney and Highfield 1990, 95) Another famous problem concerns measuring rods and whether we could know if they had changed length when transported. For everyday purposes we assume that our clock is metrically adequate, that is, it keeps good time. But how do we *know* this?

The third problem is the choice of clock. That is, what is the best sort of clock to use? Both common sense and tradition indicate that the best choice of clock is a regular occurrence. Hence the relative movements of the earth, sun and moon were the ancient standards of time measurement. But,

> The earth [as a clock] becomes inaccurate with the introduction of atomic clocks in 1948 by the US National Bureau of Standards. This new level of accuracy was able to show that irregularities in the earth behaviour brought about by tides, subterranean volcanic activity and melting icecaps meant that the length of day fluctuates by milliseconds throughout the year. Sufficiently so, for its value as an interval of measurement to be inadequate for technological purposes. (Dixon 1993, 3)

The ideal clock would provide a reliable sequence of isochronic events. Given that we have to choose some regularity as the basis for *The Clock*, what clock can we then use to measure *The Clock*'s isochronic accuracy? It would involve choosing another clock. But again the problem of its accuracy would arise. In order to avoid an infinite regress of clocks checking clocks, Reichenbach argued that it was meaningless to ask if a clock was really isochronic. Rather we decide that a clock is isochronic by convention.

> The equality of successive time intervals is not a matter of knowledge, but of definition ... This determination can again be made by reference to a physical phenomenon; a physical process, such as the rotation of the earth, is taken as a measure of uniformity by definition. All definitions are equally admissible. (Reichenbach 1958, 116-7)

Here Reichenbach makes a double move. The choice of interval is a matter of convention: one could equally choose the swing of a pendulum, or the emptying of a clepsydra. Reichenbach notes that physics employs three independent methods for defining the units of time: definition by natural clocks, definition by the laws of mechanics, and definition using the motion of light. By making this stipulation, Reichenbach introduces the idea of a coordinative definition into the metrication of time. The coordinative definition specifies that a certain physical process provides the basis for a clock. It stipulates that that particular process measures one unit of time. It is also stipulated that all occurrences of the chosen process are equal to one unit. So given that there is no *a priori* way to choose the ideal clock, and also the problem of empirically verifying the accuracy of any chosen clock, then the only way to give the metric meaning is by stipulated definition. (Newton-Smith 1980, 161-164)

But is it true to say that the choice of metric is merely one of convention? There are reasons for choosing a particular clock which mean the choice is not purely one of convention. Firstly, as has already been observed, it is a matter of fact that we do not choose an irregular event and stipulate that it is isochronic. Though in some possible world, it is conceivable that a society might choose some non-isochronic event to be their basic unit of time, for example the reigns of monarchs. In fact, during the period of the Ancien Régime, Virilio reports that units of length were co-ordinated to the physiology of the human body, often the body of the king himself. (Virilio 1984, 37-8) (A somewhat literal interpretation of the old Aristotelian adage, "Man is the measure of all things.") Secondly, the choice of best clock is not made independently physical theory. The standard metre is no longer co-ordinated by the earth's meridian. The Michelson-Morley experiments on "aether winds" showed that the speed of light was constant for a round trip, and provided a new standard for defining length, i.e. the length of a wave of atomic radiation. We now have the following definitions of units of measurement. A metre is "the length crossed by light in a vacuum in 1/299,792,458 of a second." And a second is "the duration of the 9,192,631,770 periods of radiation corresponding to the transition between the two hyperfine levels of Caesium 133 atom in its fundamental state." (Detail from Bouchareine, 1978) As scientific theories change and technologies progress, certain coordinative definitions are replaced by more precise definitions. Changes in theory are reflected by changes in the choice of clock.

However, redefining units of measure by co-ordinating them with some physical process is not without difficulty. For example, is it possible to find some ultimate standard, which is free from the conventional element in Reichenbach? Namely, that uniformity of metric is posited by definition? So for example, can we make an experiment that would define the one way speed of light? (The constancy of the speed of light in one direction being a non-trivial convention in relativity theory.) Michelson and Morley paved the way for the speed to be measured for a round trip. Fizeau and Foucault performed experiments to determine light's speed. However great controversy continues to surround questions of measuring the one way speed of light, as to whether it is an empirical fact or merely determined by convention? Salmon argues that all attempts to measure *c* are doomed to failure because when we try to measure the one way speed, this either ends up involving a round trip measurement or some other dubious assumption. (Salmon 1977) As we have seen above the one-way speed of light is a matter of convention. The usual way of judging that two events at a distance are simultaneous has involved using light signals to synchronise them: standard signal synchrony. However the assumption here is that the speed of light signal is constant. So if we send out a light signal from A to B and back, we assume the time that it took for the light to travel to B is the same time as it takes for it to return. Thus the time taken for the trip AB is equal to the time for trip BA, which is the time taken for trip ABA/2. But how do we know the speed of light is constant. Might the speed of light on its journey slow down, taking less time to do the trip AB than the return trip BA? (See Figure 3.2) Despite the invention of several thought experiments, such as Ellis and Bowman's slow clock transport, the question is still unresolved, and the one way speed of light is still defined by convention. However, this does not mean to say that this will always be the case, should an appropriate method of measurement be found. For the time being, a degree of conventionality in scientific theory is a necessary fiat. However Poincaré's claim that certain items in science will always be defined by convention looks tenuous.

3.7 Observables

A significant problem when addressing questions about the nature of time is its intangible quality. It differs from most other objects of scientific investigation in ways that cause significant problems for the investigator. Firstly it is unobservable, in a manner that most other phenomena are not. Secondly, it is much more tenuously associated with observable phenomena than, for example, gravity. Finally, as several writers have pointed out, many of the concepts with which it is associated are as equally as elusive as time itself: entropy, causation, explanation. All in all, this has created a dearth of empirical information to inform hypotheses about its structure. The fact that time is not observable raises questions about whether we are entitled therefore to say that it exists. There are substantial problems with the traditional foundationalist programme that only permits corroboration based upon observational evidence. If such views were sustainable they would constitute a substantial objection; namely we could not start to talk about the existence of time.

However the problems caused by time being unobservable may be a red herring, and not a problem at all for my analysis. Concern about positing the existence of unobservable phenomena is often traced back to the philosopher and physicist Ernst Mach (1883), who argued that only objects of sense experience have any legitimate rôle in science. Thus any theoretical entity that can not be directly observed is only to be given instrumental status. (It is important here to note briefly that the terms "unobservable" and "theoretical" are by no means synonymous with one another.) Mach's argument about the instrumentality of unobservables proved a major influence on the logical positivists - and their edict that statements are only meaningful insofar as we can verify them.

Van Fraassen (1980) has made a sophisticated version of the argument against unobservables more recently. He argues that there are certain unobservables which one may legitimately infer because we can "see" them indirectly by the way they act on material objects. So one may infer the existence of an aeroplane if one were to observe a vapour trail in the sky, because, under the right conditions, one could see the aeroplane, perhaps through a telescope or with one's own eyes, if one were spatially close enough. But one may not infer the existence of a micro-particle in a chamber, because there are no conditions under which the said particle could be observed. According to this line of argument it would be wrong to posit the existence of space and time, as there are no possible conditions under which they can be seen. Van Fraassen argues therefore that one can only accept them as purely theoretical entities on the grounds of explanatory adequacy, and that this does not commit us to any belief about space and time *per se*. Thusthe acceptance of a theory should make no ontological commitments beyond the observational level.

However it has been pointed out that the reasons for why a thing may or may not be observable are highly idiosyncratic. P.M. Churchland, responding to van Fraassen, gives the following reasons why certain entities will be unobserved.

> First they may go unobserved because, relative to our sensory apparatus, they fail to enjoy an appropriate spatial or temporal *position*. They may exist in the Upper Jurassic period, for example, or they may reside in the Andromeda Galaxy. Second they may go unobserved because, relative to our natural sensory apparatus, they fail to enjoy the appropriate spatial or temporal *dimensions*. Hey may be to small or to brief, or too large, or too protracted. Third, they may fail to enjoy the appropriate *enegy*, being too weak or too powerful, to permit useful discrimination. Fourth and fifth, they may fail to have an appropriate *wavelength*, or an appropriate *mass*. Sixth, they may fail to "feel" the relevant fundamental *forces* our sensory apparatus exploits, as with our inability to observe the background neutrino flux, despite the fact that its energy density exceeds that of light itself. (Churchland 1989, 143. *His italic.*)

Churchland does not think that the contingencies of human based observation are a satisfactory basis for making decisions about what can or cannot be said to exist. We shall return to the issue of anthropomorphic limitations and observation below. Unfortunately time does not fall under any of the reasons given above. Nevertheless, there are other reasons, other than human observation, for believing that certain entities exist.

What other criteria could be brought to bear on the ontological status of unobservables? Churchland argues that there are certain super-empirical virtues which can fortify a theoretical entities claim to exist. These depend on certain qualities of the theory the entity appears in; namely its degree of simplicity, coherence, and explanatory power (See 4.8.3). Newton-Smith (1980) also adopts a realist stance to the question of the structure of time. Despite its intractability, he likens the problem to that of the structure of the nucleus. In order to choose between theories, he argues that it is best to posit that explanation which explains the most about events in time.

Others have argued that the fact that we are able to manipulate these so-called theoretical entities to produce independent results in experimental contexts supports a realist position. Hacking (1993, 162) uses the example of the electron to illustrate his point. We are able to manipulate the electron in an experimental context, and the fact that we are able to do this that leads us to believe that electron exists. He argues that though we might be incorrect in our theoretical description of them, we can not be wrong about their existence. (Though we do not go along with Hacking in that he finds no support for theoretical entities which are not manipulable, as this would exclude space and time.) Again examples from real research add strength to the realist position. We have already said that sometimes theory pre-empts reality. Harry Kroto, when modelling the putative V60 carbon molecule, used a sixty-sided "buckyball" as the basis of his model. When the actual V60 molecule was observed it drew an uncanny resemblance to its model. To summarise, relying solely on human observation as a foundation for differentiating ontological status creates a gamut of difficulties. We have already given several reasons why certain entities might remain unperceived by human eye. By placing the emphasis on observation alone, the many varieties of evidence are ignored. Observation is only one part of scientific activity. Research programmes also involve, for example, experiments, thought experiments, novel predictions, etc. Also the position appears to adopt a somewhat naïve conception of what is observed, failing to take into account the theoretical element in all observation. (See 3.8)

3.7.1 HUMAN OBSERVERS AND THEIR PROBLEMS

Churchland has already argued that the contingencies of human physiology can result in an idiosyncratic definition of the observable and the unobservable. It is important though to note that Churchland does more than dismiss these contingencies of human observation. He goes on to explain these limits of human observation, in terms of theories about human perception, its thresholds and constraints. Hence, the reasons why we perceive the world in certain ways and why there are limitations on our perception and observation can be explained. Churchland's approach is in stark contrast to the dismissive attitude by Einstein. It will be recalled that Einstein thought it unimportant to explain why humans experienced the world as time-symmetrical. Rather than trying to accommodate this phenomenological anomaly within his theories, he dismissed it offhand as not being an issue worthy of address. However this response is not adequate. In order to be a fully subscribed scientific realist, any "curiosity" of human consciousness must be just as susceptible to a naturalistic explanation, as any other phenomena observed. Theories about perception, cognition and consciousness must cohere with the rest of the body of scientific theory. As we argued in Chapter 2, the fact that perceptual phenomena are observed introspectively does not make them necessarily immune to scientific explanation. However the sort of arguments that Churchland is able to propose have only been made possible with recent advances in our understanding in neurophysiological and neuroscience. In Chapter 5 we shall be exploring this issue in more depth.

However, there are several other issues that need to be addressed here about fundamental theories and the limits of human observation. These issues bear directly upon issues to do with time and observation.

3.7.2 ANTHROPIC PRINCIPLES

The anthropic principle adds an interesting dimension to the question of how the fact that we are human effects what we observe. It was first proposed by Brandon Carter in 1973 and has two forms, weak and strong.

1. Strong Anthropic Principle: "The universe *must* be such as to admit the creation of observers within ii at some stage." (Carter 1974, 291-8)

2. Weak Anthropic Principle: "Intelligent life can exist only in certain regions of a universe given physical laws." (Hawking 1981, 425)

The Strong Anthropic Principle requires us to explain laws and conditions of the Universe in terms of life and consciousness, whereas the Weak Anthropic Principle explains life and consciousness in terms of the laws and conditions of the universe. As a form of explanation, they are more akin to teleological explanation than the sort of scientific explanation usually employed. The principles have been used in one form or another by certain writers to "explain" certain facts about the universe, such as the cosmological constant being very close to zero. (Davies 1982, Hawking 1982, Barrow and Tipler 1986, Barrow 1988.) For example, Barrow and Tipler in *The Anthropic Cosmological Principle* (1986) argue that the reason that the cosmological constant is so low is that it has to be that low for human life to obtain. Hence, the cosmological constant's low value is important in explaining why life is possible in the Universe (Ray 1991, 192). Barrow writes,

> It is a sobering thought that the global and possibly infinite structure of the Universe is so linked to the conditions necessary for the evolution of life on a planet like Earth. (Barrow 1988, 355)

However, for our purposes, it is important to note one of the comments about the rôle of the human observer, and the influence she may have on observations. Carter alerts us to,

> the risks and errors in the interpretation of astronomical and cosmological data information unless due account is taken of the biological restraints under which the information was acquired. (Carter 1983, 347-63)

This issue becomes highly pertinent when we address the question of the direction of time.

3.7.3 THE DIRECTION OF TIME AND LOCALISED CONDITIONS OF OBSERVATION

Carter is alerting us to the possibility that our situation in the universe may have the effect of restricting what we can observe. We have already argued that one of the main problems that a prospective unified theory of time would have to cope with is the question of temporal asymmetry. (See Chapter 1) From the human point of view, time is usually described as asymmetric, and many processes in nature appear to be, in fact, irreversible. However, in elementary physics the fundamental laws posited are time reversal invariant. Much has been made of this discrepancy, and none more so (and with such tragic consequences) than by Ludwig Boltzmann. Boltzmann made many unsuccessful attempts throughout his life to argue that the direction of time was the same as the direction of increasing entropy. However his arguments to prove the Second Law of Thermodynamics fell foul of substantial criticisms by Poincaré, Zermelo and Loschmidt. In an attempt to bypass their objections and explain our experience of increasing entropy and hence the direction of time, Boltzmann's final offer was the quiescent Universe response. Boltzmann in his *Gas Lectures* (1896-8, 447) argued that the Universe was in a quiescent state, very close to overall thermodynamical equilibrium. If, according to his theory, the most part of the Universe is at or very near equilibrium, how is it that we live in an area that is far from equilibrium? Boltzmann's response is that the universe that we inhabit is extremely large. This being the case, it is probable that some small areas of the universe will be in a far from equilibrium state. Hence our experience of the world, with its apparent tendency towards increased entropy, is explained by our being precisely in one of these far from equilibrium pockets of the universe. That is to say, our experience of increasing entropy is a purely local phenomenon, and not applicable more generally to the Universe as a whole. It is not unusual that we should find ourselves in one such far from equilibrium pocket, Boltzmann argues. (Boltzmann 1896-98) For, it is only possible for highly complex and intelligent beings such as ourselves to evolve in far from equilibrium conditions. In order to have observers of entropy increase, there needs to be a constant flow of energy to sustain them, that is a far from equilibrium condition. So the existence of the observer presupposes the conditions which they will observe. Boltzmann is using the weaker anthropic principle to explain why we live in such a part of the Universe, and by extension, why therefore we experience the direction of time.

The theory of localised time-asymmetry is intuitively difficult to grasp. Us folk hold the belief that past and future are fundamentally dissimilar. This is in contrast to our beliefs about space, which we deem to be only numerically distinct, that is, the same whatever direction is being regarded. This has not always been the case. Aristotle's flat earth cosmology described space as anisotropic because fire always seemed to rise, whereas objects tended to fall back to the ground. But Boltzmann (and Reichenbach whose theories of branch systems refined those of Boltzmann) argued that the direction of time, perceived as moving from the past to the future, is a function only of particular systems, and in relation to a present indexical defined within that sub-system. Boltzmann wrote,

> [F]or the entire universe, the two directions of time are thus impossible to distinguish, and the same holds for space. (Boltzmann 1896-98, 77)

Just as up and down are relative to an event or observer, so that what is up for the Australian is down for the English, and so time-direction is dependent upon its own branch systems. However, Coveney and Highfield write of Boltzmann's theory,

> It is an ingenious argument. Unfortunately, it is undermined because we never do observe other portions of the universe possessing an inverted arrow of time, where bulls grow younger and have the power to clear up demolished china shops. Indeed ... modern astronomy and cosmology show that the entire universe is expanding and so it can not be in thermodynamic equilibrium. (Coveney and Highfield 1990, 175)

Nevertheless this example alerts us to the dangers of generalising about laws of nature, from the viewpoint of our local and particular situation. If we take on board the idea that our concepts of time have evolved to meet our local needs, since we can only exist far from equilibrium, these concepts are unlikely to yield "philosophical solutions" to more general questions. There is a need to sort out how much time-asymmetry is objective and how much is a result of our peculiarly human point of view. Could it be that our views about the anisotropic nature of time are as scientifically unfounded as those of Aristotle?

This tension between subjective time-asymmetry and objective time-symmetry might be one that can not be eradicated. The difficulties raised by the question of the direction of time have led certain philosophers to construe a pluri-dimensional theory of time. For example Deleuze tackles the problem in his book, *Logique du Sens*. The title in English is *Logic of Sense*, but the French word *sens* can equally be translated as *meaning* or *direction*. In this book, Deleuze proposes a two-fold account of time, represented by the concepts *Chronos* and *Aion*. Drawing heavily on Boltzmann's work he writes,

> Here we rediscover the opposition between Chronos and Aion. Chronos is the present, which alone exists. It makes of the past and future its two orientated

> dimensions, so that one always goes from the past to the future – but only to the degree that the presents follow one another inside partial worlds or partial systems. Aion is the past-future, which in an infinite subdivision of the abstract moment endlessly decomposes itself in both directions at once and forever side steps the present. For no present can be fixed in a Universe which is taken to be the system of all systems, or the abnormal set. (Deleuze 1969, 77)

Here we see the difference between Deleuze's Chronos and Aion. Chronos being the localised time asymmetric system as perceived by the observer within a particular sub-system. Whereas Aion is the overall and universal system in which there is neither fixed present nor unidirectionality. His work analyses the relation between different levels of description. The ideal mathematical realm of Aion, like Einstein's theories, acknowledges no difference between past and future. The actual and complex world of the observer however is oriented. We shall return to this in 4.10 with reference to the work of Prigogine. (For more detail, see Dixon 1993)

However to summarise, there may be specific problems associated with human observers, which may necessarily place restrictions on how we can observe the world, such as the local conditions that need to obtain for us to exist at all. This needs to be born in mind and to alert us to the dangers of making generalisations about time based upon our possibly myopic experience.

3.8 THEORY AND OBSERVATION

We have argued that there are substantial problems with the traditional foundationalist programme that only permits evidence based upon observation. If such views had been sustainable, there would have been a substantial objection to the project. I would have only been able to talk about time in an instrumentalist way. In sections 3.4, 3.5, 3.6 and 3.7, it was argued that the meaning of a theoretical term does not depend upon observation statements, convention or coordinative definition. This was the traditional view that held that the truth of an observation statement could be established without reference to theory.

However Feyerabend (1958) and others began to question the distinction between observation and theory, arguing that it was not theoretical statements that were dependent on observation for their meaning. Rather all statements were theoretical, and the observable-theoretical distinction was untenable. About the same time Quine was formulating what was to become known as the Quine-Duhem thesis which questioned the distinction between analytic and synthetic statements, and their relation to empirical evidence. Quine in his seminal paper *Two dogmas of empiricism* shifted the emphasis away from talking about individual laws. The previous orthodoxy described a scientific law as a universal description of an observable entity, which is true, falsifiable, and synthetic (i.e. can be denied without self-contradiction), acquiring its meaning from observation statements. Quine adopted Duhem's definition of a theory as a correlative device grouping together scientific laws. He wrote,

> Our statements about the external world face the tribunal of sense experience not individually, but only as a corporate body.(Quine1953, 41)

That is, a hypothesis can not be tested independently of the theoretical network that it is part of. So any individual statement is made in the context of its embedding theory. What was proposed to replace orthodox account of meaning has come to be known as the *network theory of meaning*. The meaning of a term is not stated explicitly in the traditional manner: x is y. Rather, concepts derive their meaning in part from the rôle they play in that theory.

Given that observation statements are formulated in the language of some theory, then the statements and definitions will be as precise and informative as the theory whose language they are framed in. And one only achieves precision in meaning from a coherently structured theory. Hence the anti-foundationalist stance is that observation is not epistemologically pure, for what we observe always depends upon non-observational theory, and the distinction between theoretical and observational statement becomes blurred (Hesse 1966). There are no basic observation statements upon which to ultimately ground a theory. Earlier arguments about observables did not take into account the theoretical nature of all entities.

The shift away from foundational questions of meaning not only enables us to be able to theorise about the nature of unobserved entities, such as time; it also opens up the opportunity for new types of evidence to be brought to bear on theoretical concepts.

3.9 SHIFT TO RESEARCH PROGRAMMES

I will now go on to show how progress in the philosophy of science allows empirical evidence to bear on highly "theoretical" concepts such as time. An important element in this has been the emphasis on the overall structure of research programmes, rather than individual laws.

Both Kuhn and Lakatos regard scientific activity as being involved with something greater than just individual laws and theories. Kuhn uses the term paradigm, or later, disciplinary matrix to denote the network of activities involved. Lakatos adopts the term research programme to designate the full range of scientific activities. We adopt his language here. Typically a research programme would include standard derivations, problems, models, methodological conventions and values. Galison (1987) argues that there are three parts to a research programme that are partially autonomous: experiment, observation, and theory.

Firstly, we shall describe in more detail the structure of a research programme with particular reference to Lakatos. Lakatos's work is described by Zahar (1989) as developing a "rational heuristic" for assessing research programmes, in comparison with Popper's "psychology of invention" which seems to depend on chance discovery rather than structured development. Lakatos argues that scientific research programmes have three main components: a metaphysical core, a negative heuristic and a positive heuristic. The metaphysical core comprises of the basic theoretical postulates of the programme. Being definitive of the programme, these are to be regarded by the researcher as unfalsifiable. However, around this core there is a "protective belt" of auxiliary hypotheses, observation statements and initial conditions. If an anomaly appears within the theory, then it is to these that the scientist will look as the source of the fault. The metaphysical core will admit of no alteration. The negative heuristic is simply the assertion of these guidelines. That is, the metaphysical core must remain intact in the face of anomalies. It is irrefutable. If the metaphysical core is modified, then one is no longer working within the same research programme, as it is the core that provides the programme with its defining characteristics. The positive heuristic provides guidelines for developing further research within the programme - a "research policy." It defines the problems that have yet to be solved and lays the ground for new auxiliary hypotheses in the protective belt. It also tries to foresee and offer advice on how to handle anomalies within the theory. It only applies to the "refutable variants" of the programme, and not the metaphysical core. The belt should provide new theories explaining previously observed phenomena and also make novel predictions. For example, Einstein's prediction that a light ray from a distant star would bend in the curved space close to the sun was a novel prediction of his general theory of relativity. Eddington confirmed this on the basis of observations in 1919. (Ray 1991, 1)

The positive heuristic provides criteria for choosing between competing research programmes, and also for judging whether a particular research is progressing or in regress. Lakatos writes,

> A research programme is said to be progressing as long as its theoretical growth anticipates its empirical growth, that is, as long as it keeps predicting novel facts with some success...; it is stagnating if its theoretical growth lags behind its empirical growth, that is, as long as it gives only post hoc explanations either of chance discoveries or of facts anticipated by and discovered in a rival programme..... If a research programme progressively explains more than a rival, it 'supersedes' it, and the rival can be eliminated. (Lakatos 1978, 112)

That is to say, if a research programme makes a novel prediction that is corroborated, such as, predicting the existence of new planet that is subsequently observed, then it is growing. For example, Newton's gravitation theory received corroboration when the existence of the planet Neptune was predicted, and then observed by Galle. However if a programme fails to make novel predictions which are corroborated, and is constantly having to make *ad hoc* adjustments to its auxiliary principles, then these are symptoms of degeneration. When comparing competing theories within a research programme, Lakatos offers three criteria for rejecting an old theory, T_O, in favour of a new one, T_N. Firstly, T_N should offer excess empirical content, that is, predict novel phenomena that are improbable or impossible within the T_O. Secondly, T_N should be able to explain the empirical content of T_O, if anything remains of it, stating limiting assumptions and boundary conditions. Finally, some of the new theory should have been corroborated, that is, have some empirical evidence of its success. Lakatos does say that it is possible for a degenerated theory to be resurrected, for example, the particle theory of light.

Importantly, a research programme does not fail (or succeed) when an individual hypothesis is falsified. Rather the emphasis is on the success of the research programme over time. Lakatos's work *Falsification and Methodology of Scientific Research Programmes* is an attempt to improve on the shortcomings of Popper's falsificationist account of scientific activity. It will be recalled that Popper claimed that all that was necessary in order to refute a scientific theory was a single anomalous observation. Lakatos's account of science is more sophisticated than that offered by Popper. Lakatos argues that when a theory incorrectly predicts a phenomenon, the scientist does not jettison the whole theory, as is suggested in Popper's model. Indeed, this approach is unrealistic, because of the complexity of theories, it is inevitable that they will not be born perfect into the world, but will require further modification and development as they mature. If every scientific theory were abandoned the first time a problem emerged, the prospects for development would be slim.

3.9.1 BOOTSTRAPPING

The emphasis on research programmes, which contain collections of related theories, as opposed to emphasis being placed on a single theory enables us to employ the bootstrapping strategy of confirmation, whereby certain hypotheses in a research programme can be confirmed or falsified by evidence from other hypotheses. This type of approach was first suggested by Carnap in *Testability and Meaning* (1936 & 1937) when he writes of confirming hypothesis by deducing instances of them by means of other hypotheses in the same theory. (Glymour 1980, 123)

Bootstrapping is different from the hypothetico-deductive account of confirmation because it offers an account of evidential relevance, that is a way of determining which *parts* of a theory a given bit of evidence confirms or falsifies. So it is a method of relative confirmation, acknowledging that any given piece of evidence bears upon different parts of a theory unequally. The bootstrapping strategy aims to side step some of the problems arising out of Quinean holism, which we discussed in the previous section. Glymour argues,

> No working scientist acts as though the entire sweep of scientific theory faces the tribunal of experience as a single undifferentiated whole.....[o]n the contrary, much of the scientist's business is to construct arguments that aim to show that a particular piece of experiment or observation bears on a particular piece of theory." (Glymour 1980, 3)

Bootstrapping strategy would not be acceptable to a radical holist, because the holist upholds that evidence can only be brought to bear on the entire body of our beliefs. The bootstrap strategist argues otherwise, that evidence does not bear uniformly on all of theory. Observations are relevant to some hypotheses in a theory and not to others. Glymour (1980, 133) cites the example of Kepler's theories concerning the orbits of the planets. Evidence produced by the observation of a single planet's orbit can be used to test Kepler's first and second laws, which deal with the orbit of single planets. However evidence produced by observations made of a single planet cannot be brought to bear on Kepler's third laws which relates the features of the orbits of two moving bodies. Hence variety of evidence can serve to separate hypotheses within the same theory.

Glymour also argues that scientists may use hypotheses in theories for the determination of the value of quantities, which have not been measured. That is, as a testing procedure which localises confirmation. Earman and Glymour (1988) argue that the scientist is able to use background knowledge and some of her hypotheses in arguing for and against other hypotheses. Whilst in practical and real terms, the strategy can be highly complex, an example is given to illustrate how such a strategy has been employed. Glymour (1980, 112-4) works through an example from a published psychology paper which includes the set of linear equations and their consequences shown in Figure 3.3. The A's and B's are quantities which can be estimated experimentally. Assume this has been done. What the diagram in Figure 3.4 shows is that not only are we able to calculate a value for E_1 from A_1 (Equation 1: $E_1 = A_1$); we can also calculate a value for E_1 using the known values for B_1, B_3 and A_3. Hence the data about A's and B's facilitates the calculation of E_1 in more than one way. And because both paths of calculation should arrive at the same result, E1, this provides us with a method of checking that the data is consistent with the theory. But, the other important feature of the bootstrap method of confirmation is that it allows us to calculate values, such as those for E_1 and G_1, which cannot be arrived at experimentally. There are of course limitations to this method. Glymour argues that there are certain theoretical quantities that cannot be tested from recognised pieces of evidence, because there is no means of computing them from the other evidence that has been accrued. (Glymour 1980, 143) There remain other values, such as E_2 and G_2, which can not be calculated, because of the structure of the theory and the set of initial data. Glymour concludes, "It is clear, then, that scientists may use hypotheses in their theories for the determination of values of quantities that are not in fact measured or estimated by standard

Figure 3.4: Diagram (Glymour 1990)

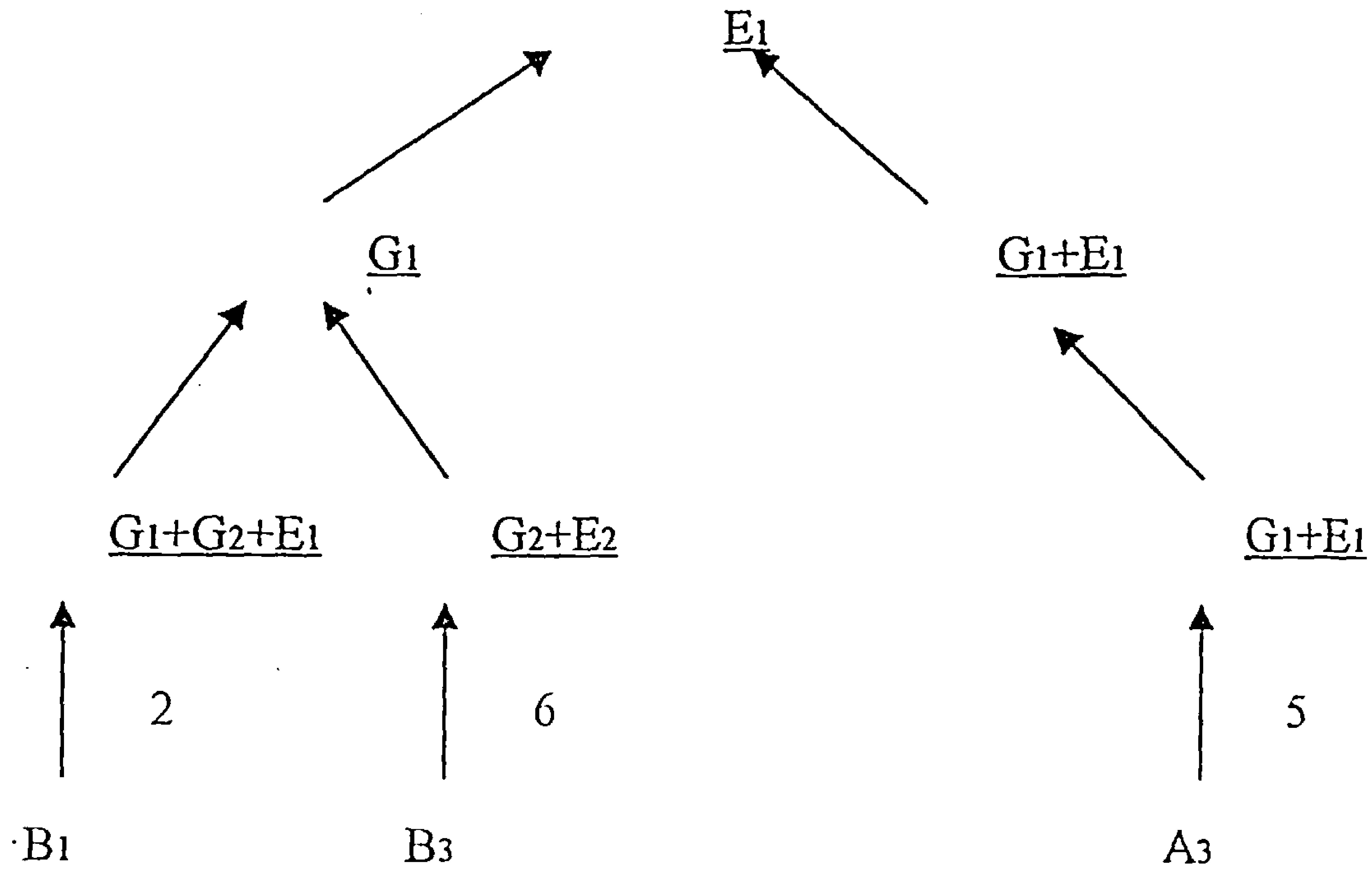


Figure 3.3 Equations

1	A_1	=	E_1
2	B_1	=	$G_1+G_2+E_2$
3	A_2	=	E_1+E_2
4	B_2	=	G_1+G_2
5	A_3	=	G_1+E_1
6	B_3	=	G_2+E_2

statistical data." (Glymour 1980, 114) That is, one is able to go beyond the available empirical data.

We now have a strategy, which allows one to compute the values, or confirm hypotheses about unobserved, or unobservable parts of a theory. This has only been possible once the strict verificationist theories of significance have been jettisoned. As such it opens up the possibility of using theories about observable asymmetrical processes in time to enable us to reach conclusions about time itself, and its structure (topology and metric.)

3.9.2 COEVOLUTION OF THEORIES

A second research strategy made possible by a dynamic theory of theory development is co-evolution. The co-evolution of theories is an important aspect of the contemporary unity of science hypothesis as it enables different theories, operating at different levels of description, to develop by mutually correcting and informing one another. Eventually, it is hoped that this co-evolution will result in he theories converging to the point where reduction becomes feasible. Co-evolution stresses the gradual development of theories over a period of time. P.S. Churchland writes,

> The logical empiricists, in focusing exclusively on the final products of a long history of theoretical evolution, overlooked the dynamics of theoretical evolution. This is a serious oversight, since it is typically in a theory's evolution that the major reductive links are forged, and the major revisions—categorical and ontological – are wrought. (P.S. Churchland 1986, 286)

That is the logical positivists overlooked the epistemological significance of theoretical evolution. We shall discuss co-evolution in brief here, as it is an issue that we will return to in both Chapters 4 and 5. Wimsatt (1976) describes co-evolution as a relation between different levels of theory in a potentially reductive relationship. Each level informs and corrects the other, mutually suggesting modifications, developments and experiments. Initially the two theories may be undeveloped and the higher level theory may not be reducible to the lower level. However as the two theories co-evolve, the aim is that they will eventually converge to the point that a reduction may be possible. The co-evolutionary approach to research programmes becomes acceptable once it is accepted that empirical theories do not arrive fully matured into the world. The adoption of this approach has important consequences for those working within certain disciplines who claim that their discipline has developed an explanatory system which can exist autonomously of other levels of explanation. They can hence argue from splendid isolation either that there can be no grand unification of theories, or alternatively, that there is no need for intertheoretic integration. We have already argued in Chapter 2 that this is a weak position, and that one of the main failures of the phenomenological programme is its isolationist position. One of the strengths of an anti-foundationalist approach in the philosophy of science is that other factors, apart from observation, can be brought to bear on the validity of a research programme. One of these factors is corroboration from other theories. The converse of this symbiotic relationship is that theories can highlight anomalies in and offer new solutions for one another. For example, Hooker (1981) discusses the concurrent developments of thermodynamics and statistical mechanics, and how the two research programmes inter-regulated each other.

> First, the mathematical development of statistical mechanics has been heavily influenced precisely by the attempt to construct a basis for the corresponding thermodynamical properties and laws. For example, it was the discrepancies between the Boltzmann entropy and thermodynamical entropy that led to the development of the Gibbs entropies and the attempt to match mean statistical qualities to thermodynamical equilibrium values which led to the development of ergodic theory. Conversely, however, thermodynamics is itself undergoing a process of enrichment through the injection "back" into thermodynamics, the differences among them forming a basis for the solution of the Gibbs paradox. More generally, work is now afoot to transform thermodynamics into a generally statistical theory... (Hooker 1981, 49)

Broadly speaking, co-evolution can take place between different levels of explanation, as in the example above. However I believe it can also occur between different research programmes, where talk of levels of explanation is less appropriate. For instance, in current work being undertaken on olfaction and flavour perception, we can witness the complementary development of the neurophysiological exploration of the biological and neurological mechanisms, which underlie human olfaction, and the computer-engineering project to build a commercially viable "electronic nose." The original model for an electronic nose system, proposed by Persaud and Dodd (1982), drew upon very early and elementary knowledge of the workings of the human olfactory pathway for its inspiration. Currently, engineering knowledge, acquired in the need to build working system, is being re-imported into neurophysiology to help model and understand the workings of the human system. The success of this interactive and complementary relationship has resulted in the specific establishment of interdisciplinary research teams. (See Appendix)

Importantly the outcome of a successful co-evolution of theories is the eventual unification of two theories via inter-theoretic reduction, which previously were considered to range over quite different domains.

3.10 CONCLUSION

My aim so far has been to demonstrate that there are no sustainable principled arguments against an empirical, as opposed to a an *a priori* theory of time. If there were we would have to abandon all hope for a unified theory of time. We identified several potential hurdles, but nevertheless we have argued that there has been a shift away from purely philosophical theories of time, to empirically based theories that receive rational support from philosophy. The attraction of the *a priori* theory of time, modelled on Euclidean geometry, has waned. Similarly there has been discontentment with regarding conventionalism as anything other than a stopgap strategy. Though our theories of time are still underdetermined by data, there are no convincing reasons to believe that this will always be the case. Finally we have rejected the foundationalist approach to ontology based upon observational evidence. This would prevent us from taking anything other than an instrumentalist stance towards time. We argued that human observatory powers were too idiosyncratic to be the sole arbiter of existence. Furthermore the instrumentalist position disregarded the activities involved in real scientific research, as well as the varieties of evidence hat can be brought to bear on its problems.

In the second half, I argued that there has been a certain "relaxation" of method in the philosophy of science. The historic and social context of research activity has been acknowledged, as have the not wholly rational reasons that underlie decision making within research programmes. However this relaxation has opened up new possibilities and techniques, such as co-evolution, bootstrapping and an emphasis on different types of evidence, such as the super-empirical virtues.

Perhaps it is best not to regard these innovations as a relaxation, but a move to a more complex and ultimately richer appreciation of scientific activity and growth.

CHAPTER 4
THEORETIC INTEGRATION

The problems of unity of science and of time are so intimately connected that we can not treat one without the other.
Ilya Prigogine

Nature doesn't consult you; it doesn't give a damn for your wishes or whether its laws please you. You must accept it as it is.
Dostoevsky.

4.1 INTRODUCTION

In Chapter 1, we claimed that there is, to date, no single unified theory of time. Furthermore, important thinkers have claimed that there could never be one. However we are arguing that there are no sustainable *a priori* arguments why this should be the case. Some of the objectors cite the apparently heterogeneous uses of the concept within different theories. Ricoeur, for example, claims that the failure to reduce time as experienced to theories of cosmological time, is a corroboration of his disunity hypothesis. Of course, Ricoeur's claim was based upon his studies of Aristotle and Augustine. Nevertheless, in this chapter, we examine whether there are any general reasons why such a reduction cannot take place. In the next chapter, we will specifically address the issue of reducing mental states to brain states, with particular discussion of time-consciousness.

The classical account of unity of science, as described by Putnam and Oppenheim falls foul of many of the same problems facing the overall positivist project, as discussed in the previous chapter. In particular, the epistemological significance of theoretical evolution was overlooked. It may be necessary for two theories to co-evolve side by side, informing and correcting each other, before they can become sufficiently close for a reduction of one to the other to take place. (See 3.9.2) In this chapter, we will argue that the classical account of unity of science has had to adapt itself to accommodate the post-positivist account of theory evolution. Nevertheless, these accommodations do not weaken the unity of science hypothesis. On the contrary, it makes it more plausible.

4.2 HISTORICAL BACKGROUND TO THE UNITY OF SCIENCE

The unity of science has long been posited as a theoretical desideratum, by philosophers and scientists alike. It has been an ideal driving scientific activity, unmindful of metaphysical claims about the monist or pluralist nature of reality. Historically, unity of science is associated with the work of Moritz Schlick, the Vienna Circle and logical positivism. Neurath's 1938 *Encyclopædia* collected a series of articles by the group's members, which advocated the unity of science as a general programme. Their project typified the mood of great confidence in the natural sciences that dominated the early decades of the twentieth century. Great progress was being made in the form of Einstein's theory of relativity and developments in quantum theory. Moreover, not for the first time this encouraged some scientists to believe that they were on the brink of constructing a complete and final theory of "life, the universe and everything" (as they say.) The fervour stirred up by the successes of the natural sciences spilled over into the human and social sciences. We saw in Chapter 2, this enthusiasm for positivism was not without its critics. (Heidegger 1924a, Husserl 1936b) In the next section we will examine the classic account of the unity of science, given in Oppenheim and Putnam's 1958 paper *Unity of Science as Working Hypothesis.* It will be shown that the traditional account of unity of science based upon strict deduction of laws and identification of terms can not adequately capture the relationship between the different levels of description. And thus arguments against the unity of "time" based on these views lose their power.

4.3 DIFFERENT USES OF THE TERM REDUCTIONISM

We have already noted that the unity of science is a theoretical desideratum, for scientist and philosopher alike. Were it achieved, it is claimed that it would bring along with it a bag of theoretical goodies, including simplicity, coherence, internal consistency. (See 4.8.3) However, there is a disagreement as to what is meant by unity of science. The traditional account of unity of science sees unification as being realised through a hierarchy of intertheoretic reductions, which would all ultimately reduce to a single, final level of explanation. However, though the terms *reduction, reductionist* and *reductionism* are commonly used in philosophy, they can mean many different things. (Stöckler 1991 and Rose 1997) There are four main issues raised by reductionism, but this list is by no means exhaustive: ideological, methodological, ontological and explanatory.

4.3.1 IDEOLOGICAL REDUCTIONISM

One of the most enduring arguments over reductionism has been on ideological, or perhaps more accurately on theological, grounds. The objection finds force in the fact that something of cultural value or meaning is reduced to some form of explanation involving other things adjudged to have less cultural value or meaning. Hence, it is regarded as objectionable that, for example, something like *love* could be explained in terms of pheromones and other chemicals; or that *thought* might be *nothing but* "grey gook" (as John Searle phrase called it.) This form of reductionism was vehemently attacked by Robert Nozick in his *Philosophical Explanations* (1981), who wrote,

> Reductionism is not simply a theoretical mistake; it is a moral failing. (Nozick 1981, 631)

However Nozick's objection seems to rest on an equivocation of the meaning of the term *reduce*. He associates the technical term with the negative connotations of reduce, as if explanatory reduction resulted in the item being explained losing its import or becoming something inferior. Of course, technical reduction implies no such change in value or worth. Indeed, some of its supporters have argued that the effect of reduction can be quite the opposite. For example, Dennett in *Consciousness Explained*, writes,

> When we understand – when there is no more mystery – consciousness will be different, but there will still be beauty, and more room than ever for awe. (Dennett 1991, 25)

Nor does reductionism imply that higher level entities are as simple to understand as the more fundamental entities that constitute them. (As if elementary particles physics were somehow simple.) Paul Churchland writes,

> A reductionist is bound to say that we are composed of simple things. But he is not bound to say that we, or our environment, are simple things. (Churchland 1992, 134)

4.3.2 METHODOLOGICAL REDUCTIONISM

The advocate of methodological reductionist rejects the use of subject specific methods of explanation, and the methodological autonomy of the biological, human, and social sciences. We do not subscribe to this type of reductionism. Its underlying motivation is that there is only one genuine scientific method, which is appropriate to all research programmes. That is the method of elementary particle physics, or whatever theory is claiming the status of being the fundamental level of explanation. Methodological reductionism is not to be confused with Popper's attempt to identify certain scientific methods which should be common to all enquiry, that is, falsification and corroboration of hypotheses.

4.3.3 ONTOLOGICAL REDUCTIONISM

Although there are still notable dissenters (notably Eccles, in Popper and Eccles 1977), most contemporary philosophers would like to describe themselves as materialist or physicalists. They are committed to ontological unity, albeit in many different forms and without any consensus about what being a materialist entails. However, provisionally, we define ontological reductionism as the theory that, given the explanatory adequacy of physics (Lewis, 1971), all phenomena in nature are covered by the laws of physics. The ontological reductionist believes that here are no new elements, special principles or additional *fundamental* laws at higher theoretical levels. This would prohibit the positing of any phenomena which can not, in principle, be explained in terms of physics, and then therefore require a special explanatory theory of their own, such as *élan vital*, epiphenomenalism or "mind stuff." (See Chapter 5 for further discussion.) Ontological reductionism or unification can occur by two methods, reduction and identification, or elimination. An example of identification and reduction would be the following statement cited in gas theory where it is said that:

temperature = mean molecular velocity

Here an identity statement indicates that one single phenomenon is being described by two theories. Intertheoretic reduction wields Ockham's razor, for where we previously had two theories seemingly describing different phenomena, we find that they are both describing an identical phenomenon. Ontological unification can also occur via the elimination of one theory by another. (Technically speaking, this is not strictly a reduction, but it does reduce the number of ways things are spoken about.) The examples typically cited come from the history of science where two theories of a single domain are in competition, an old theory and a new theory. If the old theory is considered so far wide of the mark as to be not worth preserving, no attempt at rehabilitating that theory in terms of the new dominant theory is made. It is despatched to the bone yard for much degenerated research programmes. Such theories include the caloric theory of heat transfer and the phlogiston theory of combustion.

4.3.4 EXPLANATORY REDUCTIONISM

As we have noted, most philosophers subscribe to some version of ontological simplicity, and the days positing of "mind stuff" as well as to "physical stuff" have gone. However, a commitment to ontological unity does not necessarily imply that there will be a commitment to explanatory unity. As a principle, explanatory unity seems uncontroversial enough: all theories, or levels of explanation, should be ultimately reducible to one fundamental theory, that is, everything in principle can be explained by a single theory. In concrete terms, this is often couched in terms the laws of physics being sufficient in providing a complete description and explanation of all the processes observed in nature. For example, Weinberg (1993) describes the *currently favoured* reductionist programme as an attempt to connect all laws through the physics of elementary particles. (Whether this in practice is desirable is a separate issue. There may be pragmatic reasons why you do not wish to explain the meaning of *Waiting for Godot* in terms of protons and neutrons.)

The claim that "everything is, in principle, completely describable and explicable in terms of the physical sciences" is disputed. Warner (1994) points out that what qualifies as a physical science is in itself contentious. Purists might only admit physics and those other sciences, which are reducible to it. However, depending on the contingencies of their position, other philosophers can be more lenient, admitting chemistry and biology, or even social sciences to the acceptable list, whether or not, *de facto* or *de principe*, they are reducible to physics. However, as we shall see in 4.7 and 5.2.3, there are those who believe that certain disciplines are "special" and have explanatory methods which are autonomous of and irreducible to the physical sciences. For example, Thomas Nagel (1974) famously questioned whether a scientific approach, no matter how narrowly or broadly defined, was capable of generating the type of theory that could adequately explain the relationship between our experiences as conscious subjects and their physical instantiation. Similarly Davidson (anomalous monism) and Fodor ("special sciences") have raised objections to explanatory unification. Noticeably these most famous of objections have arisen out of the so-called "mind-body" debate. However calls for explanatory laws which are autonomous of the laws of physics are not restricted to the philosophy of mind. Practitioners have made them across all disciplines as diverse as biology and sociology. This is not surprising when they are faced with having to formalise the relationship between mental events and their physical instantiation, and between society and the individual, between living entities and inert matter. In 5.2.3 we will examine whether holding onto ontological reductionism whilst rejecting explanatory reductionism is a consistent and tenable position.

4.4 MICROREDUCTION AND MACROREDUCTION

Before commencing with the discussion of the unity of science hypothesis, it is necessary to further clarify what type of reduction is being written about. The discussion so far has concentrated on microreduction. Reduction can take two forms, micro (local) reduction or macro (global) reduction. However, the form of reduction most commonly discussed in the natural sciences is microreduction. The microreductionist is motivated by the belief that complex behaviours observed in nature can only be analysed and explained by considering them in terms of their simpler constituent parts. For example, in order to explain changes in temperature in gas, a microreductive explanation would refer to the gas molecules that collectively constitute the gas. It would be concluded that the mean molecular velocity of the molecules is temperature. Inversely macroreduction explains the laws and terms of the reduced theory in terms of their position within a larger theoretical framework and instances of this type of reduction can be commonly, but not exclusively, found in the social sciences. An example of macroreduction is Durkheim's theory of suicide, which explains the individual's propensity to kill their self, not in terms of individual motives, personal crises, etc., but in terms of the relations of society as a whole (Durkheim 1897.) Similarly, Marx is often described as an economic reductionist as he argued that all social relations could be understood in terms of the mode of production prevailing in a particular society. (Marx 1867) In this chapter, we will be primarily addressing questions about microreduction.

4.5 DIACHRONIC AND SYNCHRONIC REDUCTION

At this stage, it needs to be noted that the microreduction of one theory to another occurs in two distinct ways. It is argued that the microreduction of old theories by newer theories plays an important rôle in the progress of science. Nagel refers to this as growth by incorporation.

> The phenomenon of a relatively autonomous theory becoming... reduced to some other more inclusive theory is an undeniable and recurrent feature of the history of modern science. (Nagel 1961, 336-7)

This process of incorporation was termed "homogeneous reduction" by Nagel (1961) because it is a reduction carried out within the same field of investigation. The new theory is said to employ approximately the same terms as the old theory. The example, which Nagel claims fits this model, is the incorporation of Galileo's laws of falling objects into Newtonian laws of mechanics. (Nagel 1961, 339) Here an older and partial explanation, T_o, is incorporated into a newer and more general theory, T_n. Thus,

(T_n + bridge laws) logically entails (T_o)

Examples of reductions of this kind have been used as evidence for progress in science in so far that T_n is said to be more "fertile" than T_o. . The new theory explains everything that the old theory did, and more. Certain theorists argue that it is incumbent on T_n to say why T_o is subject to certain boundary conditions which define the limits of the domain in which T_o is successful. Where a newer theory is replacing an older theory, Nickles (1973) has called this diachronic. Recently, certain theorists have questioned whether most diachronic reductions can be truly classed as reductions at all. This issue will be addressed in section 4.8.2.

However, the type of reduction of particular interest for this argument is that which take place between different levels of theory, for example the reduction of chemistry to physics, or psychology to neurophysiology. Nagel called these types of reduction "heterogeneous" as reduction takes place between different fields of investigation, which have different terminologies. Hence, the reduced theory may contain terms such as "temperature" which does not appear in the reducing theory. The laws governing the term temperature in classical dynamics would be subsumed under the laws of molecular velocity in statistical mechanics. Nagel argued that such reduction could only take place between disciplines that had been formalised. Hence, the laws of the reduced theory could be deduced from the reducing theory. (Nagel 1961, 345-366) As the reduction takes place between coexisting levels of explanation, we adopt Nickles terminology, "synchronic."

4.6 "UNITY OF SCIENCE AS A WORKING HYPOTHESIS."

The classic account of the unity of science is given in Oppenheim and Putnam's 1958 paper *Unity of science as working hypothesis.* They argue that the integration of scientific knowledge should be an ideal goal of researchers, and note that there is already a trend towards unifying branches of knowledge in the physical sciences. The formalisation of the relation between reduced and reducing theory, as described by Oppenheim and Putnam, has been continuously criticised and reformulated, by both their sympathisers and their opponents.

The Oppenheim and Putnam articulation of the unity of science hypothesis has two components. Firstly, unity of science should incorporate unity of language, that is, the terms of the reduced theory are identified with the terms of its reducing theory. Secondly, unity of science involves the unity of laws whereby the laws of one branch are substituted for the laws of its reducing branch. The unity of science hypothesis rests on the assumption that there is a single theoretical base, which would be final or fundamental, to which, directly, or indirectly, all other levels of theory would be reduced. It posits a hierarchy of theoretical levels. Each theory would be reduced by the theory "below" it in the hierarchy, until the fundamental reducing theory was reached. The Oppenheim and Putnam formulation of the unity of science hypothesis requires that reductions between theories are cumulative. The relation between theories in the hierarchy is transitive, so that given three theories, T_1, T_2 and T_3, if T_1 reduces T_2 and

T_2 reduces T_3 then T_1 reduces T_3.

In order to facilitate unity of science via microreduction there must be several distinct levels. These should be characterised by their own terminology, and would be regarded as "natural" levels. The number of levels in the theoretical hierarchy must be finite (no unity would be possible if the number of reductions were not finite.) Each level must be reducible to the level directly beneath it. Each level must be discrete, that is, it must not share any terms or theoretical entities with its higher or lower level neighbour. Finally, there must be a single fundamental level to which, directly, or indirectly (that is, via other micro-reductions), every "higher" level will ultimately be reducible. To illustrate both what is meant by levels and the relationship between levels ("the nestedness of theories"), Oppenheim and Putnam offer a provisional hierarchy of the different phenomena under investigation at each level,

6	Social groups
5	Multicellular living things
4	Cells
3	Molecules
2	Atoms
1	Elementary particles

Level 6 would be reduced to level 5, and so on, until the fundamental theory was reached. The plausibility of such an oversimplified schema does not detract from the underlying principle which Oppenheim and Putnam are trying to illustrate. It is not simply a question of directly reducing time as experienced to theories of cosmological time. There will be many intermediate stages.

The formal Oppenheim-Putnam definition of a reduction of one branch of science to another branch of science, for example, chemistry to physics is as follows:

Given two branches of science, B^2 and B^1, B^2 is said to be reduced to B^1 iff,

(1) the accepted theories of B^1 at a given time t are said to be T^1

(2) *the accepted theories of B^2 at a given time t are said to be T^2*

Then given 2 theories, T^2 and T^1, T^2 is said to be reduced to T^1 iff,

(1) *the vocabulary of T^2 contains terms which are not included in the vocabulary of T^1*

(2) *the observational data explainable by T^2 are explainable by T^1*

(3) *T^1 is at least as well systematised as T^2*

It can be seen that reduction is primarily a relation between theories, and only by derivation is reduction a relation between terms in theories.

This formulisation is consistent with the DN (deductive nomological) model of explanation because when a reduction has been successfully carried out it meets the following two formal conditions. Firstly, the laws of the reduced theory are logically deducible from the more basic laws of the reducing theory, thereby achieving a smooth reduction. Secondly, the terms of the reduced theory should be linked to the terms of the reducing theories by bridging laws that enable identity statements to be made. A frequently cited example of a successful inter-theoretic reduction is the aforementioned reduction of the theory of thermodynamics to kinetic theory, which produced the identity statement that, in gas, temperature is mean molecular velocity.

Motivating the unity of science hypothesis as described by Oppenheim and Putnam is the belief that there is a systematic relationship between all levels of description. All levels can ultimately be reduced to one fundamental set of laws and terms; in other words, explanatory unification. However, Oppenheim and Putnam are not making the more sensational claim that we can or should directly explain, for example, the behaviour of one particular human being in terms of the sub atomic particles that constitute her. Nor are they necessarily implying that we should stop talking about ourselves in terms of beliefs and desires in favour of talk about interactions between quarks and neutrinos. However, as we shall see, the consequences of a unity of science approach can have revisionary repercussions for specific theories. (See section 4.8.2)

> It is not absurd to suppose that psychological laws may eventually be explained in terms of the behaviour of individual neurons in the brain; that the behaviour of individual cells - including neurons - may eventually be explained in terms of their biochemical constitution; and that the behaviour of molecules - including the macro-molecules that make up living cells - may be eventually explained in terms of atomic physics. If this is achieved, then psychological laws will have, in principle, been reduced to the laws of atomic physics, although it would nevertheless be hopelessly impractical to try to derive the behaviour of a single human being directly from his constitution in terms of elementary particles. (Oppenheim and Putnam 1958, 7)

The goal of the unity of science remains very much a working hypothesis, and still upholds as a desideratum of scientific research. (Frosch 1997) However, since the publication of Oppenheim and Putnam's manifesto, unity of science based on a microreductive strategy has been subjected to much criticism and revision. We have already quoted Nozick's moral objection to reductionism. However, the most candid critics are the "neo-dualists" in the mind-body debate, in particular, Fodor in his 1974 paper *Special sciences (or: the disunity of science as a working hypothesis).* In particular Fodor is attacking type-type identity theories.

4.7 TYPE-TYPE IDENTITY THEORY

The first point of reference for a description of type-type identity theory is usually J.C.C. Smart's 1967 paper, "Sensations and Brain Processes." Similar positions were also advocated by U.T. Place (1957) and H.. Feigl (1960). Smart argued that there could be strict identity relations between types of brain and central nervous system activity, and types of sensations or feelings. Discussing type-type identity theory, he wrote,

> All it claims is that insofar as a sensation statement is something, that something is in fact a brain process. (Smart 1967, 163)

These sensations and brain processes are strictly identical in the same way that lightning *is* an electrical discharge. Type-type identity theory does not require that properties of type S (psychological) have the same sense or meaning (*Sinn*) as properties of type P (neurophysiological), though they have the same reference (*Bedeutung*.) For example,

(1) Kim knows that exposing food to heat will make it warm.

(2) Kim does not know that increasing food's mean molecular velocity will make it warm.

However, this does not allow us to conclude that,

∴

(3) Heat ≠ mean molecular velocity

The fact that the truth-value of a propositional attitude is determined by information possessed by the subject, and not the veracity of that information, clouds the issue. In the example above we can see that the substitution of co-referring values has given rise to the intensional fallacy. Smart expresses the intensional fallacy thus,

> [W]e can easily jump from 'we are not aware of X being Y' to 'we are aware of X not being Y.' (Smart 1994, 19)

Smart's position has some supporters. Lewis (1966) argues psychological ascriptions have the same reference as brain ascriptions, but they need not have the same sense; what is true of brain states is not necessarily true of mental states. Rorty (1965) makes a similar claim, neurological statements need not entail the same content as phenomenological report statements, nor need they express the same thing. Likewise, Putnam (1971) points out that concepts are not properties. A property identity statement would be, Temperature is the same as mean molecular energy.

However, we are not able to say,

The concept of temperature is the same as the concept of mean molecular energy.

Smart's theories opened the way for the eliminitivist position. He argued he could not guarantee the translatability of propositions of type S into propositions of type P, because the integrity of P-type propositions could not be guaranteed. Finally, P-types do not have a causal relation with S-types. They are not two distinct events. For example, it is fallacious to say the increase in molecular mean velocity *causes* the food to heat up, because molecular mean velocity *is nothing but* heat. Heat is not something over and above. In terms of unity of science, Smart's theory suggested that types of brain processes were identifiable with types of sensation. Hence, we might say,

C fibre stimulation = having the sensation of pain

The work of Smart, Place and Feigl was innovative in its claims, but was restricted by availability of experimental data and knowledge of the brain. Their work exemplifies the enthusiasm of their era, when cognitive science, neurophysiology and the artificial intelligence research programmes were still in the early stages of development. Nevertheless, their work establishes a philosophical precedent for countering anti-reductionist arguments with empirical examples. In essence, their programme tries to show that psychological states can be explained in terms of neurophysiology.

4.8 ARGUMENTS AGAINST UNITY OF SCIENCE

The most enduring arguments against the unity of science hypothesis arise out of the mind-body debate. The mind-body problem debate has been well documented. (See for example Warner and Szubka eds., 1994 for the main arguments.)

4.8.1 MULTIPLE REALISABILITY

Fodor famously voiced his objection to the unity of science hypothesis and attacked type-type identity theory in his 1974 paper, *Special Sciences: Disunity of science as working hypothesis.* His argument can be summarised thus: if all sciences are to be successfully reduced to physics, then for any natural kind of the reduced science, there is some *physical* natural kind which is coextensive with the reduced natural kind. Fodor does not think that this is possible because of multiple realisability. The example he makes is taken from the "special" science of economics. He cites "monetary exchange" as a natural kind in economics that has no physical natural kind with which it can be coextensive. Formally,

$$S_1x \quad \leftrightarrows \quad P_1x \lor P_2x \lor P_3x \lor \ldots\ldots\ldots \lor P_nx$$

Where *S* is a token in the special sciences, and *P* are physical tokens. Multiple realisation is, for example, where two or more tokens of cognitive type are respectively identical, that is the cognitive tokens have the same functional state. Each cognitive type is identical with some physical type, however the tokens of the physical type are not identical. So, for the example of "monetary exchange", there is no single physical instantiation of exchange value. Exchange value can be physically instantiated in many forms: in gold, in plastic, in paper, in beads, in salt. Fodor's criticism is an expansion of Putnam's argument in "The Nature of Mental States" (1967) Putnam's argument was directed against Smart, Place and Feigl. This influential paper was instrumental in causing a decrease in interest for type-type identity theory. In brief, his argument was that cognitive kinds are not coextensional with physical kinds. And, by extension, cognitive generalisations are not reducible to physical generalisations, meaning there can be no explanatory unification. In an oft-cited passage, Putnam wrote,

> Consider what a brain-state theorist has to do to make good his claims. He has to specify a physical-chemical state such that any organism (not just a mammal) is in pain if and only if (a) it possesses a brain of a suitable physical-chemical structure: and (b) its brain is in that physical-chemical state. This means that the physical-chemical state in question must be a possible state of a mammalian brain, a reptilian brain, a mollusc's brain (octopuses are mollusca, and certainly feel pain), etc. At the same time, it must not be a possible (physically possible) state of the brain of any physically possible creature that can not feel pain. Even if such a state can be found, it must be nomologically certain that it will also be a state of the brain of any extraterritorial life that may be found that will be capable of feeling pain before we can even ascertain the supposition that it may be pain. (Putnam, 1967, 56)

4.8.2 RESPONSES TO THE MULTIPLE REALISABILITY OBJECTION

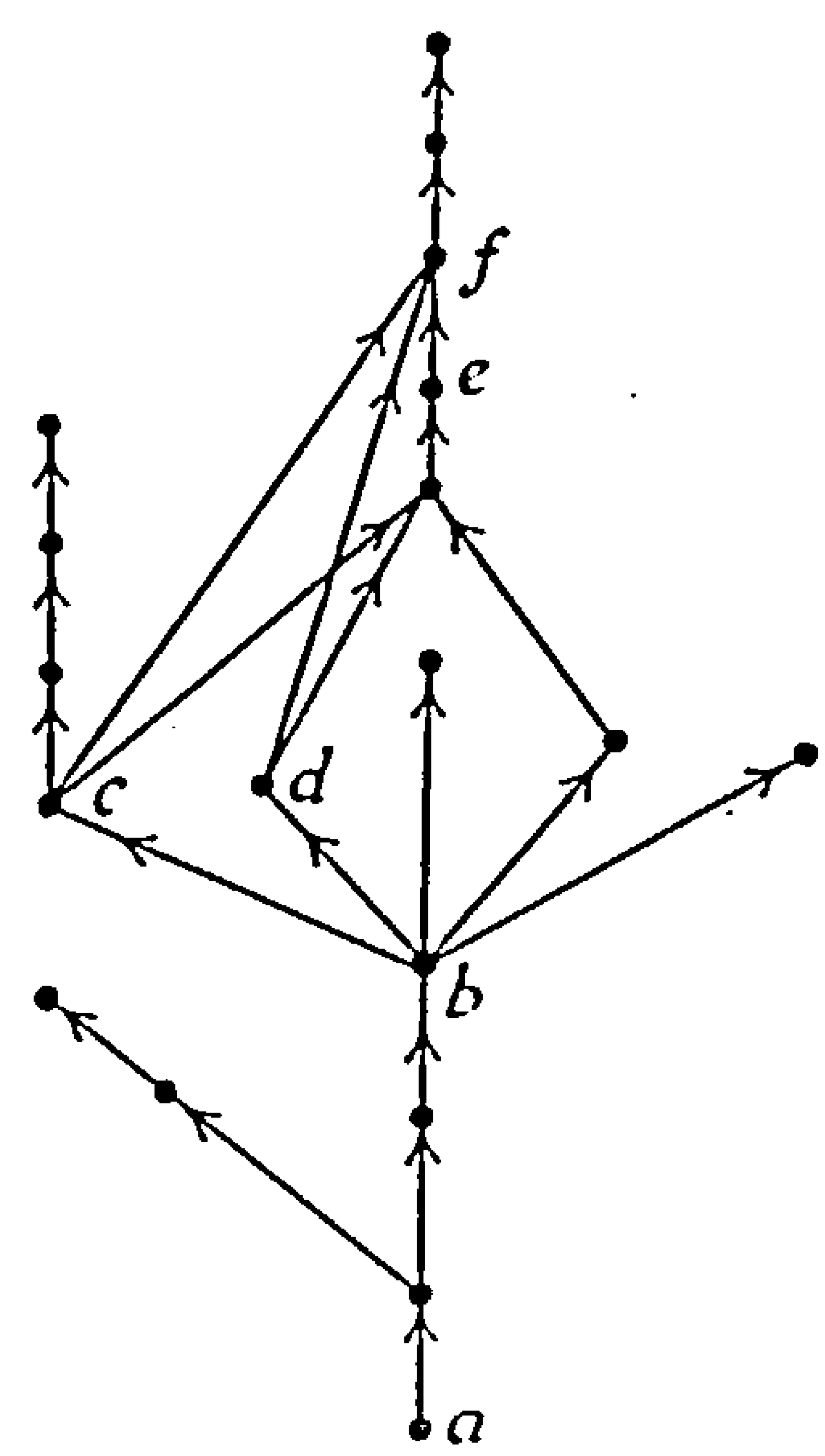
The upshot of both Putnam and Fodor's arguments is that the "special sciences" can not be reduced to the physical sciences, as there can be no universally quantified biconditional connectability between the two. However, Causey (1977) is not convinced by the Fodor's disunity of science hypothesis. Causey makes two objections. Firstly, Fodor's argument relies on the inability to establish bridge laws (in terms of identity statements between universally quantifiable biconditionals) between the *natural kinds* of the special and the *natural kinds* of the physical sciences. However, Fodor offers no examples of what might be a natural kind in psychology. Furthermore, Fodor apparently believes that we will always individuate psychological states in a way which will not correspond with physical states. Apparently, he does not subscribe to the coevolutionary view of theory progression. Given that psychology is a relatively new science, far from maturity, it seems a little too early for Fodor to claim that the psychological predicates that are currently in use will never change. Secondly, if S_1x is coextensional with P_1x in one case, and is coextensional with P_2x in another case, why should we consider S_1x to be a natural kind? Why, for example, in the case of monetary exchange should we not differentiate between gold (S_1x), plastic (S_2x), and paper (S_3x) instantiations at the special science levels. Causey subscribes to the revisionist theory of reduction. He believes that the difficulties raised by the multiple realisability objection suggest that we should modify the special sciences to take account of such detail, rather than reject the unity of science hypothesis. He argues that, compared with the natural sciences, the special sciences are relatively undeveloped. Their theories are more likely to be at fault.

In the case of monetary exchange, Causey points out that exchange value can be expressed in virtually any medium. However, the role of money is culturally defined, that is, plays a certain role in a certain social context. As such, the manner in which money is multiply instantiated is unimportant. Causey believes that Fodor's argument is unreasonable and misleading, in sofaras it focuses on this. The important factor to focus on is the economic *laws* governing money, and not the many different forms money takes. It will be recalled that the reduction is primarily a relation between theories and their laws, and not individual terms within theories. Reduction, as we have argued, is primarily a relation between the laws of theory, and only derivatively between the terms of the theory.

Further criticism of Fodor's multiple realisation objection comes from Putnam. Putnam does not agree that the multiple realisability argument enables one to adopt a general anti-reductionist position. Kim (1993) develops Putnam's argument, proposing what he calls "species-specific biconditional laws." Such laws state that, given a certain species, organism or system, it can be described as being in a certain mental state iff it is in a certain physical state. Formally,

$$Si \rightarrow (M \Leftrightarrow Pi)$$

Figure 4.1: Diagram of a possible array of theories. (Causey 1977)



which, relative to species or system, Si, specifies a physical state, Pi, which is both necessary and *sufficient* for the occurrence of mental state, M. Kim argues,

> Multiple realisation is consistent with the species-specific strong connectibility. (Kim, 1993, 274)

Kim's position enables local reductions of mental states, but not global reductions. (Only species independent would enable us to have global reduction.)

> Multiple realisability of the mental has no anti-reductionist implications of great significance; on the contrary, it entails, or at least is consistent with, the local reducibility of psychology, local relative to species, or physical structure types. (Kim 1993, 275)

Local reduction is the normal case in the sciences. The Churchlands (1990) use an adaptation of this argument to circumvent the multiple realisability problem. They concede that there can not be global reductions of psychological events, if there can be a diversity of physical instantiations. However, citing examples from the history of science, they argue irreducibility does not follow,

> Temperature, we claimed earlier, is identical with mean molecular kinetic energy. But, strictly speaking, this is true only for a gas, where the molecules are free to move in a ballistic fashion. In a solid, where particles oscillate back and forth, their energy is constantly switching between a kinetic and a potential mode. In a high-temperature plasma, there are no molecules at all to consider, since everything has been ripped into sub-atomic parts. Here temperature is a complex mix of various energies. And in a vacuum, where there is no mass at all, temperature consists in the wave-length distribution - the black-body curve - of EM waves passing through it.

> What these examples show us is that reductions can be domain specific.....(They all count as "temperature," since they interact, and they all obey the same laws of equilibrium and disequilibrium.) None of this moves us to say that classical thermodynamics is an autonomous, irreducible science, forever safe from the ambitions of the underlying microphysical story. (P.M. and P.S. Churchland 1990, 52-3)

Fodor's multiple realisation objection may be damning for the original formulation of type-type identity theory, however as we have just shown this only provides an objection for global reduction and does not cover local reduction. Furthermore, Fodor's argument rests on the claim that natural kinds in the special sciences are not reducible to natural kinds in the reducing theory. However the reductionist position does not of itself rely on a theory of natural kinds. Fodor's formulation of his argument in terms of natural kinds is offers ammunition for his opponents. For, given two theories, one relatively successful and highly formulised, and a second highly contentious and currently under attack from new theories encroaching on its explanatory territory. If then, the latter of these two theories fails to reduce to the former, where are we more *likely* to believe the fault lies? This scenario is, of course, analogous with the arguments of the Churchlands, who cite failure to reduce as a reason to reject folk psychology. Not everyone goes down the eliminative path, but such a scenario suggests, at least, that there is a case for revising the taxonomies and theories of the special sciences before rejecting unity of science. That is, in order to implement the unity of science, we might have to employ different psychological predicates than those we currently use, in order to identify the physical and behavioural sciences (Causey 1977.)

4.8.3 DUPRÉ'S THE DISORDER OF THINGS

Other criticisms of the unity of science hypothesis fall foul of the same problems. Dupré in his book the *Disorder of Things: Metaphysical Foundations of the Disunity of Science* (1993) also brands those who subscribe to the unity of science hypothesis as essentialists, hung up on identifying natural kinds. "It is still widely believed that science is the search for fundamental kinds defined by real essences." (Dupré 1993, 60) By Dupré perhaps! In the book *The Disorder of Things*, he illustrates his argument with some fascinating examples from biology, showing that there is a multiplicity of ways to classify living organisms. Whilst much of what he says may be true, he can not use his evidence to argue against the unity of science hypothesis if the unity of science hypothesis does not succeed or fall upon the existence of natural kinds. Oppenheim and Putnam might have stipulated that the levels of description had to be "natural." However Causey (1977, 135-7) pointed out the obvious, namely that modern science is not organised in terms of such clear-cut levels. Science presents a complex array of domains and theories, with very many genera of structures. There are no "natural" levels, only theoretical ones. However, the complexity of the real world does not present *de principe* an insurmountable difficulty for the unity of science programme. (See figure 4.1)

Dupré's objection to the unity of science hypothesis falls prey to some of the misunderstandings that we have already discussed. For example, he argues for a pluralistic ontology of the sciences, but his objection to the unity of science project seems to be guided by an ideological motivation, similar to that of Nagel in 4.3.1. That is, he interprets those who argue for the ontological priority of elementary particles as making a value judgement about the superiority of physics, when compared with other levels of explanation. It is true that some physicists speak as if physics were a superior discipline, however ontological priority does not involve any judgement about a certain level of descriptions value. It means only that, for the time being, element particle physics is the lowest level theory and therefore the most fundamental theory available.

He also seems to be confused about the nature of the relationship between the levels. Dupré argues that there are many different causal entities at many different levels of description. Hence, lower levels events may be determined by what is happening at a higher level. For example, our desire to drink beer might cause us to walk to the pub. He argues that there is a complex interdependency of entities at many different levels shows implausibility of reductionism. He argues that reductionism might explain, but can not predict the behaviour of complex systems. This, in general, can only be explained in terms of an autonomous understanding of the phenomenon at a higher level. Firstly, as we stated in 4.7, the relation between the reducing theory and the reduced theory is not a causal one. P.M. Churchland points this out in an amusing example. He is responding a similar point in Searle's *The Rediscovery of the Mind.*

> Searle's robust persistence in thinking of mental states as ontologically distinct from, yet causally produced by, brain states reminds me of a comparable persistence in a comparable domain. It appears in the Introduction to *Betty Crocker's Microwave Cooking* [....] Before turning to the recipes, the authors attempt a brief explanation of how such new-fangled devices manage to produce heat in the foodstuffs we put inside them. 'The magnetron tube converts regular electricity into microwaves... When the [microwaves] encounter any matter containing moisture - specifically food - they are absorbed into it.... The microwaves agitate and vibrate the moisture at such a great rate that friction is created; the friction, in turn, creates heat and the heat causes the food to cook.'
>
> The decisive failure of comprehension begins to appear halfway through the last sentence. Instead of asserting that the induced motion of the moisture molecules already constitutes heat, and gracefully ending their explanation there, the authors benightedly continue to discuss heat as if it were an ontologically distinct property. (P.M. Churchland 1994, 14)

Dupré's objections to reductionism can be sometimes bewildering. He claims that,

> It is surely imaginable, for instance, that people with identical physical states, including states of the brain, might be thinking different things. (Dupré 1993, 166)

I can not suggest any neurologically plausible scenario where this could be the case. Even the staunchest of anti-reductionists subscribe to the primacy of the physical; that is, physical identity entails mental identity.

4.9 ORTHODOXY CHALLENGED

However, the unity of science project, based upon type-type identity is flawed, suffering from the same problems that inhabit the logical positivist account of reduction. The shift away from logical positivist philosophy of science is well documented. Scriven (1962) and Harré (1970) attacked the DN model of explanation. At the same time, the independence of theory and supporting evidence was problematised. The so-called Quine-Duhem thesis championed epistemological holism, claiming that observation statements were deeply implicated in the empirical theories they were meant to prove. It was argued that there on no basic observation statements upon which to ultimately ground a theory. (Quine 1953) Lakatos attacked Popper's falsificationist account of scientific progress arguing that one contradictory observation can not falsify an entire research programme. (Lakatos 1978, 22)

Observation of supporting evidence was no longer regarded as a sufficient condition for accepting a theory. In order to be accepted it was argued that a theory also needed corroboration from other surrounding accepted theories. There was a shift away from proof and refutation to strategies of bootstrapping and coevolution. (See section 3.9)

4.9.1 FROM GROWTH BY INCORPORATION TO SCIENTIFIC REVOLUTIONS

Similar shifts were taking place with regards to reduction and the unity of science. The model of "growth by incorporation" was challenged. Kuhn's influential text *The Structure of Scientific Revolutions*, first published in 1962, attacked the orthodox account of progress in science. (Similar criticisms had been made previously by French philosophers of science, for example Bachelard (1934 and 1953), however their work was apparently unread by the anglophone philosophical community.) Kuhn's concern was predominantly with diachronic reduction, that is, the replacement of an old theory with a new theory. He argued that science is not cumulative in the straightforward way originally conceived within the logical empiricist programme. Rather than smooth, or slightly bumpy, reductions of old theories to new theories, Kuhn argued that the change that took place was of a revolutionary nature. Furthermore, he claimed that these changes in scientific research programmes were often made for less than scientific reasons. Emphasising the cultural and sociological background in which scientific places took place, Kuhn claimed that what occurred was the revolutionary overthrow of one "paradigm" by another. There was no smooth incorporation of old knowledge. Rather the process of change was discontinuous. (A similar argument is made by Toulmin 1961) The ramifications of such theories have consequences that extend to synchronic as well as diachronic reduction.

We have already stated the classical formula for reduction based upon the DN model of explanation (Nagel 1961, 339) that,

(T_n + bridge laws) logically entails (T_o)

When T_n replaces T_o, Nagel's formula establishes a set of identity relations whereby terms in the old theory are said to be synonymous with some of the terms in the new theory. Furthermore, remembering Oppenheim and Putnam's stipulation that reduction takes place between theories, and not merely between terms in theories, the key set of principles of T_o should be mapped onto sentences which are consistent with, and consequential to the overall theories of T_n. Hence, in principle, T_n can be said to replace T_o, although, in fact, there may be pragmatic reasons for still using the terms of the old theory. If T_n can explain all the phenomena explained by T_o as well as or better than T_o, and is also more powerful in its overall explanatory scope, then, given that the formal conditions have also been met, a smooth reduction is said to have taken place.

4.9.2 FROM DIACHRONIC REDUCTION TO ELIMINATION

However, whether such smooth reductions ever take place is a contentious. Feyerabend (1962) and later Hooker (1981) have argued that there is not a single example in the history of science of a diachronic reduction that conforms to the growth by incorporation model. Churchland (1979) and Hooker (1981) have proposed what they believe is a more realist account of what occurs when an old theory is "reduced" by a new theory. They claim that intertheoretic reductions are rarely smooth, and do not readily the tender identity statements that provide bridge laws between the two theories. Frequently the older, reduced theory is found to be incorrect to some degree. Thus to implement a reduction, some reconstruction and correction needs to take place before it can be deduced from the reducing theory. (For an interesting discussion of historical cases see Churchland 1979, 80-86 and Hooker 1981) In such cases, what takes place is not strictly a deduction of T_o, but of an analogue reconstruction of the older theory. In this case the analogue, T_a, is expressed in terms of T_n, and is isomorphic with the old theory. Formally speaking, what takes place here is not a reduction at all as T_n only explains T_a. When the old theory requires substantial reconstruction and correction to bring it into line with the reducing theory, this brings into question the identity between the two theories as the identity relations which would have provided bridge laws are no longer in tact.

In certain cases, there may be good and pragmatic reasons for trying to rehabilitate the terms of an older theory into the new. For example, Feyerabend (1962) discusses the example of the reduction of classical mechanics, CM, to the Special Theory of Relativity, STR. Though this is not a smooth reduction, by reconstructing an analogue of CM within the terms of STR, one is able to demonstrate why STR can supersede CM, and also show why CM worked within certain limits. However, for example, there is no theoretical utility in striving to accommodate chyle and morph within a theory of blood corpuscles, nor malevolent demons into a modern day theory of mental illness. As we noted in 4.3.4 in these cases where the old theory is so inadequate or false that one cannot justify any attempt to try and establish a correlation between it and a reducing theory. In such cases where the old theory is so totally off-course that we would do far better just to eliminate it, than try to reduce an implausible ontology. (Feyerabend 1963)

4.9.3 SUPER-EMPIRICAL VIRTUES

The grounds for discarding one theory in favour of another are not yet formalised, and may never be so. However, without recourse to *a priori* arguments, the empiricist is ultimately forced to say that only time will sift out the good theories from the bad. (Lakatos gives several examples of research programmes that have been discarded, to be resurrected later, for example the particle theory of light.) Nevertheless, in the absence of formal guidelines several "super empirical virtues" have been proposed: augmented explanatory power, corroboration, fecundity, intertheoretic endorsement, integrity, coherence, and simplicity. All of these criteria offer rational support for accepting one theory in favour of another, in the absence of empirical data. Very briefly, I will summarise these criteria.

Augmented explanatory power - T_n should have more explanatory power than T_o. It should be able to explain as much as the old theory and/or increase the domain over which successful predictions can be made (e.g. the relation of Special Theory of Relativity to classical mechanics.)

Corroboration ensures the continuing success of the theory, that is, by observations confirming its predictions. A research programme is said to be fecund if it is successful in generating new research programmes, that is, suggesting possible areas of future research to complete the theory, or to develop it further.

A theory gains *intertheoretic endorsement* when there is actual or potential compatibility with the theories of other scientific disciplines. This argument has been inverted in the case against folk psychology. The Churchlands and Stich have argued that because folk psychology is not compatible with current knowledge then it should be rejected as a viable theory.

The *integrity* of a theory is reflected in the manner in which its shortcomings are manifested. If its failings are systematic, perhaps ranging over a specific domain, then this can identify a distinct area where more work needs to done. However, if the theory, in order to remain plausible, requires a myriad of *ad hoc* modifications, "sellotaping " it together, as it were, this would suggest the theory was in crisis (as Kuhn might put it.)

Coherence depends on the internal consistency of a theory, namely that it should not make contradictory claims.

Simplicity, as Putnam (1992) notes, has been an infamously elusive concept to define. However a highly informal formulation might be: given two competing theories which are equally successful, if it is easier to make predictions of equal success with one theory, rather than another, use the simpler method.

In addition to these super-empirical criteria, there are of course other less rational reasons for adopting one research programme instead of another, such as likelihood of receiving funding, peer pressure, political climate, religious belief, and theoretical fashion (See Kuhn 1970).

4.9.4 SUMMARY

We have been illustrating the shift away from the logical positivist DN model of explanation. A further modification was suggested by Salmon (1971). He argued that not all scientific laws are universal generalisations. (Indeed maybe no scientific law is.) Salmon therefore suggested that the DN model should be modified to include covering laws that are statistical and probabilistic. This may pose a problem for those who favour the neatness and completeness of the DN model, with its symmetry of explanation and prediction. However, the DN model is, in reality, nothing more than a theoretical desideratum, applying only to complete and formalised theories. Unfortunately, scientific theories are seldom, if ever, complete, and there may be strong *de facto* arguments for why they never will be so.

4.10 RENEWED INTEREST IN THE UNITY OF SCIENCE

We stated earlier that the unity of science hypothesis was first popularised during a period of great innovation, when the steady onslaught of scientific discovery seemed relentless. However this period of high hopes for the completability of science foundered when presented with the consequences of its latest discoveries: for example, the superpositions of quantum physics, and the awareness of complexity and sensitive dependence upon initial conditions. Similarly, difficulties arising within the theory of elementary particle physics has led current thinkers to suggest that there might be a more basic ontology, beyond quarks and electrons. However, the recent popularisation of complexity, chaos and self-organisation theories has led to a resurgence of interest in the unity of science hypothesis (Prigogine 1980, Davies 1988). Theories of self-organising systems and complexity were extensively developed in the 1960's. Although their impact on public consciousness has only been made in the last ten years, with the work of Gleich, Prigogine and Stengers, Kaufmann, Waldrup and Stewart. Philosophically the work of Prigogine on dissipative systems had an extensive influence on philosopher Gilles Deleuze; see for example his *Différence et répétition* (1968) and *Logigue du sens* (1969). Until very recently, the unavailabilty of Deleuze's work in English and the resistance of the Anglo-American academy to all things continental have meant that his works have remained largely unread this side of the channel. In the UK, attention was drawn to the importance of these "new sciences" for philosophy by Popper, who saw that Prigogine's work might offer hope to the beleaguered reductionist programme in science. He wrote that,

> Prigogine's work may be looked upon as a piece of exciting physicalist reduction, at least in the sense that it takes the first steps towards the a physical understanding of the evolution of higher structures...(Popper 1982, 174)

Prigogine (1971, 12) appears to support the reductionist programme when he writes: "The concept of stability really reconciles the unity of laws with the existence of well defined levels of description." Prigogine's theories offer the possibility of bridging the gap between inert matter and living entities. But others do not agree with the thesis that theories of dissipative systems and self-organisation lend support to the reductionist programme. Paul Davies writes that the new sciences are "synthetic and holistic rather than analytic and reductionist." (Davies 1988, 2) Gleick agrees with Davies,

> Chaos is anti-reductionist. This new science makes a strong claim about the

> world: namely, that when it comes to the most interesting questions, questions about order and disorder, decay and creativity, pattern formation and life itself, the whole can not be explained in terms of the parts.
>
> There are fundamental laws about complex systems, but they are new kinds of laws. They are laws of structure and organisation and scale, and they simply vanish when you focus on the individual constituents of a complex system - just as the psychology of a lynch mob vanishes when you interview the individual participants. (Gleick paraphrased in Weinberg 1993, 48)

He, and also Penrose (1989), argue that there remains to be discovered a new and radical form of science which has principles which are over and above those of physics, and which are ultimately irreducible. However, as Stöckler (1991, 247) points out, many of the examples of "irreducible phenomena" which Davies uses to make his point come from the (lunatic) fringes of scientific activity, for example, Sheldrake's highly contentious theory of morphic resonance. One suspects that Davies's reluctance to accept a reductionist *weltanschauung* is motivated by his spiritual beliefs. One finds a similar attitude in the work of Stuart Kaufmann (1995), whose book title manifests its spiritual motivation: *At home in the universe.* The book concludes with a section entitled "Reinventing the Sacred." It is as if certain scientist are no longer content to conquer the earth with their theories, and are hell-bent on laying a claim on the celestial. It is as though they are uncomfortable with the truths about the world that science is producing for them, and are seeking consolation. For Weinberg describes the reductionist position as "chilling and impersonal...not because we like it, but because that is the way the world works." (1993, 41) Nevertheless, as we have already said, pessimism and reductionism need not go hand in hand. (See 4.3.1) Other reasons for resisting reductionist theories can be more to do with the politics of obtaining funding. Weinberg claims that certain physicists would dispute that elementary particle physics is more *fundamental* than their particular area of physics, as it might mean that funding bodies would judge elementary particle physics to be more important. This might mean the diversion of funding away from their research programmes. Perhaps, in the case of Penrose, there is a certain mileage to be had in claiming that there needs to be a "New Physics", rather than unglamourously labouring away with the old one. (The cultural context of the popularisation of these scientific theories is not without relevance. It takes place against a background of New Age mysticism in the late eighties and early nineties.)

Nevertheless implicit in many anti-reductionist positions, is the belief that traditional reductionist theory that attempts to explain entities by reducing their behaviour to some more fundamental form of behaviour is inadequate. That is, the laws of physics, elementary particle physics in particular, are only adequate to explain the most simple of interactions, and can not comprehend the behaviour of complex structures. And it is here that the work of Prigogine and others can suggest how links can be made. He does not call for new laws of nature to explain such phenomena. Nor does he see the need any new principles or assumptions.

4.11 DE FACTO LIMITATIONS OF REDUCTION

It is notable that Oppenheim and Putnam titled their paper, *Unity of science as working hypothesis*, suggesting that the goal of unity of science should guide research, rather than being the end of research. They probably realised that there may be real and practical limits on reductive explanation. Rose, in a recent article, explains some of those limitations.

> The world is a complicated place, full of multiple simultaneous events, processes, causes and effects. Science needs to simplify, to design experiments in which nature is caged, parameters held constant and variables changed one at a time. In studying an enzyme reaction for instance, one might hold the acidity of the solution constant and change the temperature – and derive simple equations describing the consequences. But if acidity and temperature change simultaneously, as may indeed happen in "real life," the equations will not work and we lose our ability to predict. (Rose 1997, 16)

Rose touches upon an important issue. Under ideal experimental conditions, it is possible to isolate an individual variable. It is possible to observe them, measure them and make predictions about their behaviour *ceteris paribus*. However, in "real life" situations, where there may be many known and unknown variables interacting, it becomes impossible to predict with complete accuracy what will occur. This especially becomes the case when one is dealing with processes that involve very large numbers of molecules. Even if complete knowledge were available for the fundamental level, it would be *de facto* impractical to compute a prediction. Prigogine and Glansdorff discuss the relation between the fundamental level of description, and its relation to the types of systems studied in chemistry and biology. They write,

> Even if we could conceive computers big enough to study the molecular dynamics of say 10^{23} molecules in a macroscopic system, the knowledge of their position and their velocities would be of little interest as we would never be able to repeat an experiment involving the same initial state. (Glansdorff and Prigogine 1971, xi)

It is simply not practical to describe the behaviour of thermodynamic systems in terms of their constituent molecules, in the same way that it is sometimes not appropriate to explain human behaviour in the language of neurophysiology. Rather, new laws are constructed based on generalised behaviour at a particular level.

> For instance, to predict the temperature evolution of some piece of metal it is sufficient to solve the Fourier equation with approximate initial and boundary conditions. The temperature at every point is an average taken over a large number of molecules. The agreement between the predictions of the Fourier equations and experiment shows that a more detailed study of the evolution in terms of mechanical quantities is not required. (Glansdorff and Prigogine 1971, xi)

However, these laws are not fundamental laws, in the same way that the laws of a complete and final theory would be. They do not explain the behaviour of the processes they cover, but they do have predictive efficacy. The existence of these laws does not in any way challenge ontological unity or explanatory unity as, in principle, a full explanation in terms of a fundamental theory is available. Rather, in most cases, it is impractical and unnecessary to do so. Analysis using only fundamental laws can be unwieldy, and in some causes practically impossible. These new non-fundamental laws could be described as *phenomenological* laws, with a small "p," which are abstracted from the observed behaviour of the system. Putnam expresses it thus,

> Complex systems require a simplified characterisation which nevertheless saves the essential features of the system. (Putnam 1975, 296)

The positing of these non-fundamental laws has caused problems for the reductionist, as the higher level laws are approximations apposite to that level of description. Nevertheless, once they are recognised as approximations or abstractions, much of the difficulty falls away. As they are not true descriptions, but approximate descriptions, there can be no precise bridge laws and boundary conditions to connect them to the fundamental level. This differentiation between types of explanation adds further complication to the orthodox model of inter-theoretic reduction. There will be no neat formal relations between the different levels. Nevertheless, this is in no way a contradiction of the overall unity of science hypothesis. However, there are real limitations to reduction, to the point that there may not be *de facto* reductions at all. At the same time, no new fundamental laws need to be posited.

Hence, the pragmatic value of, for instance, the thermodynamical level of description is that it provides a theory that is able to explain the behaviour of the system. The theory even has its own set of laws, albeit statistical and non-fundamental. Which means, for pragmatic reasons, we may have to adopt new methods to analyse these types of system, but this does not mean that we are positing any new ontological entities or fundamental laws, which are *de principe* irreducible.

4.12 Provisional ontology

There is still dispute as to what is the fundamental level of explanation. Oppenheim and Putnam suggested it might be elementary particle physics. This may be the case, but nonetheless the theories and theoretical entities posited in these research programmes are still contended. The content of the theories continues to change. The theories are incomplete, and not yet fully matured. At the same time, theories at "higher levels" are also provisional. In the case of complexity and self-organisation, these theories are still in the early stages of their formulation, and are much less developed and formalised than physics. Given the interim nature of both research programmes, we have not yet reached a stage where we are able to formulate precise bridge laws and limiting conditions between the two. However, our current lack of information does not preclude such laws and conditions being established in the future. Nor does it prevent us from speculating how any future reduction might be brought about. In Chapter 3, we discussed the coevolution of theories, in which theories at different levels of description develop in parallel, informing and correcting one another, suggesting that such a strategy would eventually facilitate inter-theoretic reduction.

4.13 Conclusions

In the last chapter, we discussed the shift away from a positivistic towards an anti-foundationalist account of scientific activity. The philosophy of science has had to accommodate a more realistic account of the practical and theoretical activities involved within scientific research programmes. In this chapter, we have demonstrated how the traditional unity of science has also had to adapt, faced with the complexities of a post-positivist philosophy of science. Despite the original unity of science hypothesis having to adapt, nevertheless the underlying motivation remains the same. None of the argument which we have discussed has challenged Oppenheim and Putnam's fundamental claim that everything *in principle* is describable by the laws of physics. They themselves were aware of the impracticalities of actually trying to do this. However, certain modifications have needed to be made. Firstly, they stipulated that ideally there should be distinct levels that are "natural." In 4.9.3, we argued that such clearly defined levels do not exist at the practical level. Nevertheless, this does not pose an insurmountable problem for the overall project. Secondly, Oppenheim and Putnam also required that the laws of the reduced theory should be logically deducible from the reducing theory. However, this stipulation has had to be relaxed in order to accommodate statistical and probabilistic laws. Thirdly, the hypothesis has also had to accommodate that certain terms can only be reduced locally and not globally. Fourthly, there are different types of explanation. There is fundamental level of explanation, which determines the boundary conditions and initial conditions of the entire system. There is no form of behaviour that is not governed by these explanatory principles. But there are also other levels of explanation which are apposite to the form of behaviour being described, providing a simplified characterisation, which nevertheless captures the important features of that level. However they are not fundamental explanations, and do not therefore threaten explanatory unification. Fifthly, the question of what is this fundamental level of explanation is not yet settled. Finally, Prigogine's work has highlighted the practical difficulties involved in explaining and predicting the behaviour of a complex system in terms of the fundamental level.

However, as we have seen, none of these difficulties challenges the essence of the unity of science hypothesis, and it remains an ideal still driving scientific investigation. Moreover, we now have a unity of science hypothesis that may not be complete and formalised, but which takes into account the contingencies of scientific research, and is thus, more resilient to the complexity of real scientific explanation.

Finally, in this chapter, we have tackled some of the general objections to explanatory unification. In the next chapter, we will tackle specific arguments that claim that we cannot or should not describe or explain subjective experience in objective language. We will do this with particular reference to time-consciousness.

CHAPTER 5

TIME AND CONSCIOUSNESS

> *Pluritas ponenda praeter necessitatem non est.*
> William of Ockham

> *Seek simplicity and distrust it.*
> Anon

5.1 INTRODUCTION

The problem of the relation between time as it appears in natural scientific theory and the subjective experience of time still continues to perplex. For example, Paul Davies writes at the end of his populist book, *About Time*,

> Galileo, Newton and Einstein all chose time as the central conceptual pillar of physical reality, and yet, when we stare into our own minds to find the foundations of temporal experience, it seems to crumble away, leaving only mystery and paradox. (Davies 1995, 274)

Newton-Smith at the end of his 1980 study, *The Structure of Time*, concludes the book with the remark,

> [T]here are depths yet to be plumbed; in particular, the perhaps most puzzling aspect of time, the relation between time and consciousness, remains. (Newton-Smith 1980, 242)

Although the relationship between time and consciousness does indeed remain a puzzle, the veil of mystery surrounding the workings of the mind-brain is beginning to draw back. In the last three chapters we concentrated on issues concerning the study of time and the natural sciences. Finally, in this chapter, we turn to the relationship between time and our conscious experience of it. Human time-perception and cognition is not a "curiosity of consciousness," unworthy even of explanation. It poses an important, if pernicious, problem in terms of its relation to other theories of time. If there is an opportunity for a truly unified theory of time, we must be able to show that it will be possible to integrate the natural scientific approach to time with modern psychological theories of time-perception and time-cognition.

The possibility of such unification has only become practicable in the last couple of decades with the advances made in brain science that has seen the rapid development of the fields of neurophysiology and neuro-computational modelling. Over the same time period, certain philosophers have had the eclecticism to study and use the evidence and information emerging from these relatively new areas of investigation to challenge the orthodox views within the philosophy of mind. Despite the relative newness and incompleteness of the theories emerging from these disciplines, some philosophers are proposing powerful, novel but empirically grounded explanations of phenomena in domains where the orthodoxy was claimed that science could make no impact. Time-consciousness, as we saw in Chapter 2, is one of those areas. It is not the aim of this chapter (nor is the aim of the thesis as a whole) to propose a unified theory of time. Rather, drawing upon advances in the brain sciences as well as philosophy of science and the philosophy of mind, I aim to show that such a theory has become a tangible goal.

In the following chapter we will demonstrate how recent developments in the brain sciences of psychology, neuroscience and neurophysiology are overhauling orthodox views in the philosophy of mind, overcoming objections to the naturalising the explanation of the mind. (Some of the arguments surrounding early attempts to reduce theories about mental states to theories about physical states have been dealt with in Chapter 4.) We will argue that whilst virtually all philosophers recognise that the mind is instantiated in physical stuff, not yet all believe that scientific explanation is adequate or necessary to tell the full story about the mind-brain. Some argue that there is something special about subjective introspection that can not be captured by scientific explanation. That is, first person experience can not be captured in third person description. Fodor will claim that we already have a successful and autonomous theory of mind that works very well. We answer their criticisms. We will then progress to the positive task. Drawing upon research on human time-perception and time estimation, we will demonstrate that progress towards a unified theory is already being made.

5.2 Arguments against the naturalisation of psychology

In this chapter we will sow that questions about human time-perception and time-consciousness are ultimately empirical questions informed by philosophy, which can only be settled with reference to theories, observations, and evidence generated by the empirical sciences. Much orthodox philosophy of mind has remained anomalous in its staunch resistance to an empirical approach. Although there has been some progress since Descartes posited his two substances, a sophisticated form of dualist explanation still exists. Although there are still a few dissenters like Eccles (1977), most contemporary philosophers would like to describe themselves as materialist or physicalist, albeit in many different forms and without any overall consensus about what being materialist entails. However, for clarity of argument, we define materialism as the theory that, given the explanatory adequacy of physics (Lewis, 1971), all mental states are physical states. Defining materialism thus leaves open the question of the explanatory relation between the mental and the physical, and significantly does not commit oneself to reductionism. However, we have already argued that some sophisticated form of intertheoretic integration is necessary, in order to have a naturalised and unified theory of time-consciousness or time-perception. Non-reductive materialism is not an option available to us. We therefore must be able to show that everything is, in principle, completely describable and explicable in terms of the physical sciences, broadly construed to include the emergent brain sciences. As Warner (1994) points out, what constitutes a physical science is contentious. Purists might only admit physics and those other sciences that are reducible to it. However, depending on the contingencies of their position, other philosophers are more lenient, admitting chemistry and biology, or even social sciences to the acceptable list, whether or not, *defacto* or *deprincipe*, they are reducible to physics.

5.2.1 THE ARGUMENT FROM LACK OF IMAGINATION

However, to reiterate, being a materialist only acknowledges the primacy of the physical. It does not commit one to physical reductionism. Nagel questioned whether the sciences as such, no matter how narrowly or broadly defined, are capable of generating the type of theory that could adequately explain the relationship between our experience as conscious subjects and its physical instantiation. He wrote,

> We at present lack the conception of a complete analysis of the subjective, phenomenological features of mental reality in terms of an objective physical

> basis, and there is no reason to believe such a thing is possible. (Nagel 1994, 67)

Indeed, given the relative incompleteness of the brain sciences, it seems difficult to conceive how theories of psychology, with their component elements of beliefs, desires, dreams, and reason, could ever be explained in terms of the axons, ganglia, dendrites and neurotransmitters, which are the tools of the neurophysiologist. Nagel again,

> [U]ntil we discover a way to stand theoretically astride the boundary between objective spatio-temporal physical reality and the subjective contents of experience, we can not claim to be in possession of the basic intellectual tools needed for a comprehensive understanding of conscious life. This may be unattainable, but without it we can not have a general cosmology. (1994, 68)

That is, Nagel is claiming that our current scientific theories are inadequate for the task of explaining the relationship between the mind and its subjective experiences and their instantiation in the brain. Similarly, McGinn (1982) doubts whether in fact we will be able to understand mental states in terms of physical theories, despite their shared ontology. Strawson (1994) does not think that current types of scientific theory are adequate for the task. It is not the reducing theory that is at fault, but the theory that is being reduced. In response to this apparent impasse, it has become popular for some writers to argue that a radically new theory of the mind is required (e.g. Penrose 1994) to bridge the gap between mind and brain. But it is not necessary for us to await the development of new theories and techniques. The types of theories that we already possess, given time to develop and expand, will be adequate to the task. As we shall see, even at this early stage, the brain sciences are already generating potent theories to explain human perception and cognition.

The Churchlands (1994) have replied to Nagel's doubts about the possibility of a naturalised understanding of perception and consciousness. It is often the case, they claim, that we can not imagine how a reduction might be instantiated. This seems to be especially the case when we examine the richness of our subjective experience. However, they argue, we should not let our current inability to imagine how a reduction might take place become an *a priori* argument against it ever being able to take place. The history of science has many examples of such unforeseen reductions. They write,

> For who would have imagined, before James Clark Maxwell, that the theory of charged pith balls and wobbling compass needles could prove adequate to explain all the phenomena of light? (Churchland and Churchland 1994, 49)

Often the reduction is highly surprising, and difficult to imagine in advance. Even more so considering that a successful reduction often involves a partial or total reconstruction of the old theory's taxonomy within that of the new, reducing theory. (See Chapter 4)

5.2.2 THE SPECIAL QUALITY OF SUBJECTIVE EXPERIENCE

The arguments arising from Nagel's infamous 1974 paper, *What is it like to be a bat?*, have been over-rehearsed in the philosophical literature. Nevertheless his claim that there is something special about subjective introspection that can not be captured in scientific explanation is highly intuitive, and demands response. Nagel argues that currently we have no reducing theory or explanation, which can successfully tackle the "mind-brain problem." He argues that there is a special quality to mental states which means they can not be straight forwardly reduced to another level of description. Nagel claims that any attempt to identify our experience of sensations with physical descriptions will not capture the unique and subjective character of our experience.

> Every subjective phenomena is essentially connected with a single point of view, and it seems inevitable that an objective, physical theory will abandon that point of view. (1974, 393)

Nagel is not claiming that qualia are the private experience of the possessor, and hence inarticulable in a public language. Rather that phenomenological facts can only be objective to the extent that other people are sufficiently similar to the perceiver to share them. Hence, when I report that: "I can smell fresh baked bread," you (assuming you are a fully osmotic human being) can extrapolate from your own experience and imagine what I am now experiencing. You have an idea what it is like to smell fresh baked bread. However, when it comes to small, winged, visually challenged rodents who guide themselves through the air by echolocation, it is virtually impossible for us to imagine from our experience what it is like to be a bat. No amount of physical or neurophysiological description will allow us to share the bat's experience. Nagel argues that what an experience is *like* is only available from the subjective point of view. That is, we *know* quaia in a radically different way than we *know* physical facts in the scientific way. So, to summarise his argument,

(1) Quale types are only knowable from a single and subjective perspective.
(2) Neurophysiological types are knowable from many different perspectives.
∴
(3) Quale types ≠ neurophysiological types.

According to Churchland, the first premise is mistaken. (P.M. Churchland 1992, 58) What if, taking a more concrete example, neuroscientists were to discover that the qualitative experience of seeing red is having a certain activation vector across the neurons of the n^{th} lay

er of the occipital cortex? Could we not then say the qualitative experience of seeing red is the same as having a qualitative experience of having a certain activation vector across the neurons of the n^{th} layer of the occipital cortex? Though the person in question might not *know* that she is experiencing a certain activation vector. For we would not accept the argument,

(1) Jane knows that she experiences red.
(2) Jane does not know that she experiences a certain activation vector across the neurons of the n^{th} layer of the occipital cortex.
∴
(3) Red ≠ a certain activation vector across the neurons of the n^{th} layer of the occipital cortex.

This is another obvious example of the intensional fallacy that was discussed in Chapter 4. Nagel's argument falls foul of the fallacy.

Jackson (1982) made a stronger argument against the reducibility of sensations to physical descriptions. Briefly, his argument is that no matter how much knowledge we have of our sensations, and regardless of the amount of neurophysiological knowledge we can have about brain states, we can never have access to knowledge about qualia as someone else experiences them. (This is different from Nagel who said, were the similarity between individuals close enough, then we could extrapolate from our experience to imagine what it is like for them.) Jackson illustrates his point by the following thought experiment. Mary is a neuroscientist who, up until now, has lived her life in a monochrome environment. Her only access to information about the outside world has been brought to her via a black and white television set. In the course of time, she has learnt all that there is to know about neurophysiology. She has a particular knowledge of visual perception, including colours. What then would happen when Mary is then released from the room? Jackson's argument can be characterised thus,

(1) Mary knows everything there is to know about brain states and their properties.

(2) It is not the case that Mary knows everything there is to know about sensations and their properties.

∴

(3) Sensations and their properties ≠ brain states and their properties. (P.S.Churchland 1989, 331)

Jackson's argument might be rewritten thus. Imagine that Mary is a toxicologist and knows all there is objectively to know about the chemical structure and action of poisons, especially cyanide. She is particularly interested in the effects that it has on the nervous system, especially how it kills people. However, up to now, the only access that she has had about cyanide's effects has been from experiments on third parties and from books. However, one afternoon she discovers a bottle of cyanide on her laboratory bench and, having read Jackson's article the night before, now believes that her knowledge of toxicology is now severely lacking. In the name of scientific progress, she drinks it. Does Mary know anything more about the lethal effects of cyanide? Clearly not.

Seriously, if we look at the formal version of the argument we can see that there is an equivocation between the two uses of the word "know." Mary has not in fact increased the stock of things known to her, only the manner in which these things are known. Thus the Churchlands rewrite Jackson's argument,

(1) Mary has a mature and complete scientific theory of the neurophysiological functioning of visual perception.

(2) It is not the case that Mary has had experience of a certain activation vector across the neurons of the n^{th} layer of the occipital cortex.

Rewritten in this way, it is clear that the sequitur is false,

∴

(3) Sensations and their properties ≠ brain states and their properties.

That is, we can conclude from Jackson's example that there are many possible representations of the same kind of knowledge. However this does not commit us to saying that there two, or more, kinds of things known. In the same way suppose that there is a fire in a building, to borrow and adapt an example from Pylyshyn (1980.) This can be known in a vast variety ways: by feeling oneself burning, by choking on smoke, by hearing someone shouting "Fire!", by checking the fire detector panel and concluding that there is a fire in Zone 3. However these are all different ways of knowing the same thing, namely, there is a fire in the building. There are many different ways to know a thing, but it does not mean therefore that more than one thing is known.

The weaknesses in both the Nagel and Jackson arguments arise from a confusion of the different kinds of description. It is a consequence of our, as yet, under developed comprehension of exactly what we are apprehending when we report seeing red. It has been argued that talk about qualia as some form of emergent and irreducible property would be eliminated were we to adopt the descriptions of a future, mature neuroscience. Rorty writes,

> If we got into the habit of using neurological properties...then our experience would be of things with neurological properties, not of anything else, for example, intensity. (Rorty 1965, 229)

It is an inadequate understanding of what we are experiencing that is encouraging us to multiply entities beyond necessity. That is to say that the following argument is at fault, (from P.M. Churchland 1989, 58),

(1) The qualia of my sensations are directly known by me, by introspection, as elements of my conscious self.

(2) The properties of my brain states are not directly known by me, by introspection, as elements of my conscious self.

∴

(3) The qualia of my sensations ≠ the properties of my brain states.

The qualia objection is founded on a misapprehension about different ways of representing the same information. As we have shown, there are different ways of representing and hence describing the same thing. Because a sensation can be described from a subjective point of view does not mean something different, or extra, is being described. As Rorty, and other eliminative materialists have pointed out, it is the fact that two language of description are available that confuses matters. There is nothing special about what we describe as our subjective experiences, which some future scientific description could not be substituted for.

> Some things indeed are inarticulably phenomenal in character, because they are the targets of our basic discriminatory modalities. But that in no way makes them immune to an illuminating intertheoretic reduction. History already teaches us the contrary. (Churchland and Churchland 1994, 49)

5.2.3 THE AUTONOMY OF FUNCTIONALIST PSYCHOLOGY

Certain supporters of functionalist psychology have argued for a special autonomous status for psychological explanation. They claim that it is neither important nor necessary for psychology to cohere with the rest of the scientific corpus. If this position were tenable it would have significant repercussions for our theory. For ultimately the position involves denying that questions about the mind are ultimately to be settled by reference to empirical theories, the arguments for and against the autonomy of psychology have been rehearsed many times. (See, for example, Greenwood's *The Future of Folk Psychology* (1991b) for a collection of the main papers.) My intention is not to re-examine the intricacies of this complex debate, for that would demand a thesis in itself. Rather we only aim to demonstrate that the adoption of our position enables us propose a more coherent and fertile research programme, in accordance with some programme laid out in Chapters 3 and 4.

Fodor (1975), Pylyshyn (1980) and early Putnam (1967) are the main proponents of functionalist psychology, who are claiming the autonomy of psychological explanation. Though there are subtle differences between their positions, the main gist of their argument is that information collated from the brain sciences is largely irrelevant when we come to ask questions about human reasoning. It is argued that we already have a perfectly adequate model for human reason, inspired by the Von Neumann architecture for information processing. This model is characterised by its distinction between the functional and structural levels, otherwise characterised as a software – hardware division. (See Figure 5.1) Reasoning comprises two elements; the semantic level of mental representations which is controlled by the rules and principles of the syntactical level. It is argued that it is the causal rôles between mental representations that are important in these types of explanation. Thus if you wanted to know why Joan went to the pub, you would explain this in terms of my desire for a pint of beer and my belief that the pub sold beer. It would not be necessary for you to know intricate details about my neuron populations and activation vectors in my brain. Hence the functionalist is able to make the claim that psychological explanation can stand alone and aloof. An analogy can be drawn with a computer and its software. The physical details about the machine upon which the software is running are irrelevant to the user, in the same way that a software programme could run on a Mac, an IBM clone and a Sparc workstation, all of which have different architectural structures. However, at the functional level the programme remains the same. The important fact to note here is that a software programme can be *multiply instantiated* on a different range of machines.

One can see why this analogy would be attractive to a functionalist, as it apparently adds credence to the autonomy of psychology position, and supports arguments based on multiple realisability. In 4.9.1, we answered some of Fodor's reservations to the unity of science project. It will be recalled that Fodor's main objection to the reductionist programme was based on the multiple realisability. That is, although two tokens of a cognitive type might be identical, and that both of these tokens will be identical with a physical type, the two physical types need not be identical. However Fodor's arguments were primarily targeted at a specific type of reduction, namely that based upon type-type identity statements. We have shown that successful reduction is not contingent upon the success of reduction thus construed. Fodor's position concerning the autonomy of psychology relies in part upon a special version of the argument from multiple realisability. Fodor argues that the multiple realisability objection demonstrates that a psychological

FUNCTION	
SEMANTIC	content
SYNTAX	algorithms, control principles, logical rules
STRUCTURE	
MECHANISM	architecture/structural implementation

Fig. 5.1: The Fodor and Pylyshyn model of cognition

term is not reducible to a single physical description. Consequently, if a single cognitive term is not uniquely identical to a physical term, then, according to the traditional central state definition of reduction, there can be no bridge laws between the two levels of description. However, we have argued that the success of the unity of science project does not depend on this form of one-to-one mapping.

Unfortunately, the arguments for the autonomy of psychology do not rest solely on the unavailability of bridge laws. Fodor also claims that we understand mental states in terms of the logical relations between mental representations, that is, the functional organisation of internal states. These internal states in and of themselves they form a semantically coherent system. Fodor argues that these logical relations can not be reduced to causal relations. The relations between internal states have an abstract functional character. Harking back to the computer analogy, he is therefore able to claim that the concrete, physical implementation of thought is largely irrelevant to his theory. So the use of the term, for example, "intelligent" does not apply exclusively to humans, or only to animals with neuronal structures, or, to say, carbon-based life forms. The term could also be used to describe the behaviour of computers, silicon-based aliens from outer space, angels, ghosts and any other, as yet, unencountered or unimagined entities. It is not necessary to know about the hardware, because the functional organisation is quite separate.

There are several objections to the functionalist position. Firstly, we have already argued in 4.9.2 that multiple realisability argument raised by Fodor does not constitute an objection against the unity of science hypothesis, as most reductions in the natural sciences are relative to a domain. Just as temperature is multiply realised in different theories in the natural sciences, so too are terms in the psychological taxonomy. Intelligence can be potentially both carbon or silicon based. The sense of direction is multiply realised in different animals, being different for a bat, a pigeon and a human, but nevertheless, each in its own right can be explained and reduced to some general mechanism relative to its species. Secondly, the three level model which has influenced the functionalist position may offer an interesting way in which to think about the brain, however as an empirical theory of information in the brain it is unhelpful. Pylyshyn, in particular, proposed the symbolic manipulation model of reasoning as a general theory of cognition, which use the sentential structure as its basic kinematic element (Pylyshyn 1980). However, as a model it does not correspond well with empirical theories about the brain is organised. Churchland (1986, 349-400) objects that the function/structure (or software/hardware distinction) is relative only to the level of strata being described. It is a relative distinction, and not the absolute distinction that functionalist appears to be trying to claim. Additionally, it is clear that the three levels model is oversimplistic. Evidence from the brain sciences suggest that there are not just three, but many, different levels at which the brain operates. Furthermore, ascertaining the number of levels, how they are distinguished and what they do is a matter for empirical research and can not be established in advance. (See Figure 5.2) In response to Pylyshyn's claim that the engineering structure is unimportant, she argues that scientists are still at the early stages of discovering how the brain operates. Therefore, it is simply too early to decide whether and when structural implementation is irrelevant.

Fodor defends functionalism by claiming that "It's the only theory we've got." Though, for the time being this may be true, this does not justify his claim for the autonomy of psychology. Others have agreed that it is the best available theory, but have only ascribed it instrumental status (Dennett 1987) Functionalist psychology may indeed by the best theory we will ever have, but this claim can not be made *a priri*. Whether it is the only theory that we currently have is also doubtful. Neuroscientists are already doing research on learning, memory and intelligence, and offering new, alternative and powerful explanations for our behaviour. Yet to be a defender of the autonomy of psychology thesis, one would have to bloody-mindedly ignore these new theories and evidence. This would involve an act of extreme philosophical perversity, akin to the Creationists who still believe that God created the world in seven calendar days, despite the overwhelming geological, cosmological, and biological evidence that suggests otherwise. Like Husserl, in Chapter 2, this isolationist position means that functionalist can not benefit from the advances being made in other disciplines. It does not even cohere well with other areas of psychology, never mind the rest of the scientific corpus.

5.2.4 SUPERVENIENCE AND ANOMALOUS MONISM

We have seen that the claims for autonomy of psychology have relied partially on the argument that the failure of type-type identity theories means that there can not be bridge laws to enable reduction between the two levels of description. In this next section we shall ask whether it is possible to claim to be both a materialist and at the same time claim nomological autonomy for psychological events. Such a position depends on the tenability of weak supervenience. Supervenience is presented as a materialist theory insofar as it

LEVEL	*DETAILS OF RESEARCH*	*REFERENCE*
CELLULAR	Modification in presynaptic neurotransmitter release in habituation	**Hawkins and Kandel, 1984**
SYNAPTIC CLUSTERS	Modification of synapse numbers and synaptic morphology correlated with plasticity of behaviour	**Lee *et al.*, 1980**
DENDRITIC BRANCHING	Effect of maze training on dendritic branching in the occipital cortex	**Greenough, Juraska, & Volkmar, 1979**
MULTICELLULAR	Hippocampus unit behaviour during classical conditioning	**Berger, Latham and Thomson, 1980**
CELL ASSEMBLY	Response averaging techniques in paleocortex (olfactory bulb)	**Freeman, 1979**
CIRCUIT	Seasonal changes in the "songster" nuclei of the canary brain	**Nottebohm, 1981**
SPECIFIC AREAS OF NEURAL TISSUE	Effect of neural tissue atrophy or cerebral blood flow and metabolism on memory performance using neuro-imaging techniques	**Jernigan, 1984** **Volpe et al., 1983**
NEUROLOGICAL	Human memory and amnesia	**Weiskrantz, 1978** **Squire and Cohen, 1984**
ETHOLOGICAL	Flower recognition in bees	**Gould, 1985**
PSYCHOLOGICAL	Memory capacities and skill in humans	**Norman, 1973** **Tulving, 1983**

Fig. 5.2: Organizational levels of the brain for memory and learning, defined in terms of research method used. Information provided by Churchland, P.S., 1986, pp. 359-360.

acknowledges the primacy of the physical without necessarily committing oneself to physical reductionism. Teller has offered a general definition of supervenience,

> Truths of kind S supervene upon are determined by truths of kind P iff any two cases which agree as to truths of kind P also agree as to truths of kind S. (Teller 1983, 145)

There are three types of supervenience relations: weak, strong and global. Strong and global supervenience are arguably identical as both are committed to kind-to-kind correlation. (Kim 1993, Teller 1983) Briefly,

> S strongly supervenes on P just in case necessarily for each x and each property F in S, if x has F then there is a property in P such that x has G, and necessarily if any y has G, then it has F. (Kim 1993, 65)

> S globally supervenes on P just in case worlds that are indiscernible with respect to P.... are also S-indiscernible.(Kim 1993, 68)

In both cases, physical properties (P) entirely determine the supervening properties (S). If we examine the definition of strong supervenience, we can see that it entails that every mental property has a coextensional physical property. Therefore this kind-to-kind correlation is consistent with type-type identity theories, and provides bridge laws to facilitate smooth intertheoretic reduction. Weak supervenience, on the other hand, does not imply the existence of a physical correlate for mental properties. It is consistent with multiple realisability, and hence the autonomy of the supervening properties.

> S weakly supervenes on P iff necessarily for any x & y, if x & y share all the same properties in P then x & y are all properties in S - that is indiscernability with respect to P entails indiscernability with respect to S. (Kim, 1993, 58)

Weak supervenience does not capture determination and hence is consistent with the autonomy of psychology. It is the position adopted by Davidson to justify anomalous monism. (1980). Davidson uses it to achieve two aims. Firstly, to show that psychological events depend on physical events, that is, to claim that his position is materialist. Secondly, to deny that there are any laws which can connect psychological and physical properties, that is, an anti-reductionist stance. However, is it possible to claim psychophysical dependency without having psychophysical laws, or at least, psychophysical entailments? Kim argues (1993, 267) that the Davidson's position is untenable. One can not subscribe to the one without subscribing to the other. There are, Kim claims, only two tenable positions. Either anti-physicalist dualism, that is, one can not claim to be materialist; or, reductionism-eliminativism, which is not consistent with the autonomy of psychology.

Kim claims it unlikely that physical predicates will entail psychological predicates, for example a physical predicate that entails being bored. However he does not accept the functionalist argument from multiple realisability that psychological states can be realised in many divergent organisms and/or physical structures therefore no one state, S, is likely to have a uniform physical state, P, as its correlate. Kim responds that multiple realisation implies nothing about the general impossibility of psychophysical laws. It only rules out completely biconditional laws of the type,

S iff P

where P is a single physical state. Kim argues that a supervening property can have different supervenience bases. This position does not appear inconsistent with that of P.S. Churchland. That is, a supervening property does not necessarily depend on particular base, for example "goodness" does not require x to be virtuous and benevolent. Other qualities might entail the same judgement.

So, to summarise, the main tenets of anomalous monism can be characterised thus: firstly, there are no psychological or psychophysical laws. All strict laws are expressed in the language of the physical (anomalous-ness of the mental); secondly, mental events interact with physical events; and, thirdly, one event, c, can be said to cause another event, e, only if there is a strict causal law which entails c causes e (nomological character of causality.) Davidson defines a non-strict law as one which contains a *ceteis paribus* qualifier or is *de facto.* Usually in the special sciences Davidson claims we use non-strict laws. Anomalous monism claims that mental events enter into causal relations with physical events. Mental events do not involve the sort of general laws that govern physical events, and consequently no laws can be drawn up between them or between mental events and physical events. Therefore we can have no lawlike connection between the physical and the mental. It claims to be materialist in so far as all events have physical properties, and some events have mental properties. The mental has no effect on the causal relations that interconnect events. In short: mentality has no causal or explanatory rôle. Kim replies that if this is the case then it is difficult to understand why we would then need to recognise mentality as a feature of the world. At its extreme it is "a doctrine virtually indiscriminable from eliminitivism." (270) Non-reductive materialism is not a plausible position, which leaves some form of naturalised psychology as the only viable alternative.

In this and the previous sections on qualia and functionalist psychology, I have discussed several objections to a naturalised theory of mind. Whilst inevitably I have done great injustices to the subtleties of the respective authors' arguments, it was not my intention to focus on the intricate details of the debate. My aim is to show that there are no conclusive reasons why evidence from the emerging brain sciences can not be brought to bear on arguments in the philosophy of mind. There is no *a priori* arguments that there is something special about subjective experience that makes it irreducible. There are no arguments that conclusively support the autonomy of psychology. Finally one can not consistently claim to be a materialist, whilst advocating the anomalous nature of psychological explanation.

5.3 NATURALISED PSYCHOLOGY

Inevitably the evidence and theories emerging from the brain sciences are of a provisional nature. However, it is significant that already powerful arguments against certain positions, such as those described, within the philosophy of mind are already being articulated. Although, given their relative immaturity, these arguments and the evidence they are based upon are incomplete and inconclusive. Nevertheless a pattern seems to be emerging which has been common to other areas of philosophy, namely orthodox views being replaced by empirically grounded theories. In Chapter 3 we showed how the philosophical discussion of time and space had shifted from an *a priori* approach, to one informed by the current leading theories. It is apparent that a similar shift is occurring in the philosophy of mind and in epistemology. Kitchener (1985) has argued for a naturalised theory of epistemology and human cognition. He calls this genetic epistemology. Kitchener's workis in contrast to the orthodox approach to epistemology. This sought to construct formal rules for human reasoning, learning, knowledge, and belief acquisition, modelling itself upon the methods employed in formal logic, probability theory and statistics. Essentially this was an *a prioi* approach in the sense that it is logically prior to empirical science, and analysis can be conducted prior to any particular facts. (Morley and Hunt 1991) McTaggart's *The Unreality of Time* is typical of such an approach. However, in line with other developments in philosophy, there has developed an alternative approach to questions epistemological. This approach is typified by its emphasis on an interdisciplinary approach, bringing evidence from such as psychology, the brain sciences, anthropology and sociology to bear on their theories. Kitchener's argument is that the adequacy of a particular epistemic account can not be established by *a priori* epistemology alone. It requires corroboration from other disciplines. His theories are informed by the work of Piaget, who defined intelligence as the extent to which an organism is able to adapt its internal systems to cope with some intractable elements of the environment. As an organism adapts to its environment, it develops certain mental structures. These structures are continually modified and reorganised, resulting in a gradual accrual of "knowledge." There are three important features of this account of epistemology. Firstly, the definition of intelligent behaviour is can be used equally to apply to all creatures on an evolutionary scale, from molluscs upwards. Intelligence is not strictly reserved for human behaviour. In the past, there has been a tendency to dismiss apparently intelligent behaviour by other animals as conditioning. This is wholly due to the way that intelligence was defined. This leads to the second point. Kitchener offers an alternative to the orthodox account of epistemology, which, because of its emphasis on formal rules and the linguistic analysis of sentences, is limited to human behaviour alone. We shall see that a similar claim is made about other animals and time consciousness. (See 5.3.2) Finally, it offers an evolutionary account of the development of minds, and those minds relationship with the world. "Knowledge would have no validity if the structures of the mind failed to match up to the structures of physical reality." (Rychlak 1981, 671) If we include time-consciousness as an epistemological structure, then we can move some way to explaining its rôle. That is, the development of time-consciousness is part and parcel of our overall adaptation to and learning about the environment. Michon has proposed such an explanation of time,

> Time is the conscious experiential product of the processes that allow the human organism to adaptively organise itself so that its behaviour remains tuned to the sequential, (that is, order) relations in the environment. (Michon 1985, 20)

My discussion so far has concentrated on the more general issue of a naturalised psychology and epistemology. Now we shall address more specifically the question of the psychology of time. It is very difficult to predict what a complete and mature theory of time-consciousness would look like. However, drawing upon the research in other areas, we can make a few tentative suggestions regarding what we might expect.

5.3.1 TIME IS NOT A SIMPLE, UNITARY EXPERIENCE

Firstly, the term time-consciousness suggests a unitary conscious experience. However it is likely that this term conceals a collection of many different functions, which all together contribute to our overall experience of time-consciousness. Certainly recent evidence from other research programmes on human perception teaches us to be suspicious of any such assumption. Similar assumptions have been relinquished in research programmes studying other aspects of human experience. For example, Peter Smith discusses the example of vision. He argues that initially visual perception might have been described "as a relatively straightforward matter, involving the triggering of a play of uninterrupted images before the mind's eye." (Smith 1989, 21-2) However, this traditional and simple assumption has been challenged by evidence produced out of pathological psychological and neurophysiologcal studies. These studies have demonstrated that vision has a far more complex and modular cognitive substrate. What appears in non-pathological cases as a single and indivisible phenomenon, is revealed under investigation to be constituted by a set of specific sub-computations in different parts of the brain. Cases of visual disturbance or abnormalities are well recorded. Smith describes the case of a person who had sustained specific neural damage, resulting in visual agnosia. When tested, the patient was shown to have sharp visual acuity. He was able to make good line by line drawings of a face, and he was able to identify objects from silhouettes, proving he had a good grasp of local forms. However, the patient was unable to either recognise faces or locations. When he looked at himself in the mirror, he reported seeing his own face either as a set of unrelated details, or as a "global", schematic face. He was unable to integrate the two sets of information together in order to recognise either himself or anyone else. Another case cited by Smith details a woman who was unable to perceive movement. This left her unable to do many ordinary tasks, like poring herself a cup of tea. She described the fluid pouring from the spout as solid, and reported she could not perceive the level rising in the cup. These strange pathological studies reveal some of the different elements that comprise vision, and show that vision is not a single, simple operation in the brain. This thesis is further corroborated by reports from other scientific studies. For example, there are cases where people have been diagnosed as blind, yet they claim that they can still see (blindness denial). Interestingly, another piece of research has examined a group of subjects who have been diagnosed as blind, and furthermore report themselves as blind. Yet, in experiments, they have demonstrated an improbable acuity for locating objects placed within their reach, or for correctly reporting how many objects were held up before them to count (blindsight.) Clearly, there is more to sight that meets the eye.

Work carried out in AI may also offer insights into the workings of the visual system. In designing an artificial analogue for the eye, Nilsson, Raphael *et al* broke down the task of visual object recognition into several, separate computational tasks. These are, for example, recognising the difference between light and dark, the identification of the target object's edges, and using vertices as a way of computing the three-dimensional position of an object. (Raphael 1976, Nilsson 1984) Other complex factors that may play a rôle in mammalian visual recognition are anticipation, context, familiarity, and memory. The example of vision should make us wary about time-consciousness, and approaches, which seek to reduce time-consciousness to some single underlying process. It is unlikely that such a simple explanation will be available. Even the review of time-perception and estimation should act as a warning that any explanation will have to take into account a wide range of variables and their interactions, such as arousal, attention, memory, sleep, and motivation.

5.3.2 TIME-CONSCIOUSNESS IS NOT UNIQUE TO HUMANS

Secondly, it is unlikely that time-consciousness is unique to humans, although some authors have suggested as much. Whitrow, drawing upon evidence from Koehler (1957) and Walker (1983), concludes,

> There is evidence that our sense of these distinctions [of past, present and future] is one of the most important mental faculties distinguishing man from all other living creatures. For we have good reason to believe that all animals except man live in a continual present. (Whitrow 1988, 7)

Similar claims have been made about the uniqueness of human competencies, such as language ability, using tools and taking medicines, only to be disproved. It is more likely to be true that humans have the most sophisticated time-consciousness currently known, in the same way that we appear to have the most sophisticated form of language. Experiment show that pigeons and other animals can be trained to estimate intervals of time with accuracy. (Friedman 1990) If we accept that other animals share some of the same temporal abilities as human beings, it opens up the opportunity of conducting research on mechanisms which are perhaps simpler than our own, as a way of building up understanding. If we however claim that time-consciousness is unique to humans, no such path is available.

5.3.3 NO SINGLE MASTER CLOCK OR TIME

Thirdly, we know that temporal behaviour and rhythm can be observed at every level of description, from the cyclic behaviour of the individual cell upwards. Nevertheless, in seeking to explain this synchronicity of bodily processes we should be wary of hypotheses which posit some master clock or single timer which is co-ordinating it all. Such a hypothesis is reminiscent of the old homunculi theories of consciousness, which are seductive in their simplicity, but actually explain nothing.

5.3.4 THE AMBIGUOUS NATURE OF TIME-CONSCIOUSNESS

Finally, it is clear that time-consciousness does not fall conveniently into the traditional categories of perception or consciousness, though whether these categories have any scientific integrity must surely be brought into question. It has been argued that these two categories (perceptual recognition and propositional attitudes) are not in fact different and discrete types of mental events, which are characterised by their own explanatory theory, but rather they are "essentially the same kind of computational achievement." (P.M. Churchland 1992, 198) Moreover that all categories of mental events, including time-consciousness, are susceptible to explanation of this kind. Churchland has argued that the main difference between what we call perceptual recognition and explanatory understanding is the diversity of information that has to be processed. Perceptual recognition, he claims, is limited to sensory inputs, where explanatory understanding has to respond to a "wider variety of cognitive situations."((P.M. Churchland 1992, 198)) Churchland may have oversimplified the distinction. Even when it comes down to what is considered simple sense perception, our sensory-discrimination has been shown to be far more complex was initially imagined. (See Appendix, section 7.2) There have been some attempts to find a sense organ for time, but these have been unsuccessful. (See 5.4.1) Also there have been equally unsuccessful attempts to explain time in terms of sentence based cognition. For example Zwart argues,

> The only empirical research that is needed for the solution of a truly philosophical problem like the meaning of one of our fundamental concepts is research into the use of the term in question in everyday language. (Zwart 1976, 11)

But as we have seen the simplicity of ordinary language can be mistaken and misleading. (I shall not explore this claim here as there is further discussion of this issue in the appendix.) Others have argued that human time-consciousness is part and parcel of the domain of declarative knowledge, that is, high level cognition. Therefore if we wish to understand it we should employ the same sort of empirical methods that are applied to the understanding of problem solving, decision making, comprehension and explanation. However, we have already argued that these types of explanation that are being averted to are themselves coming under attack as empirical knowledge about the workings of human cognition increases. As with other aspects of human cognition, it is more likely that what has become labelled as time-consciousness, will turn out to be made up of a series of separate timing or rhythmic mechanics operating at all levels of the human body from the cell upwards. We know that some of the more "cognitive" aspects of time-consciousness are not unique to humans.

Bearing these four points in mind, I shall now proceed to show how we already are developing theories which explain certain aspects of time-consciousness.

5.4 EXPLAINING THE SUBJECTIVE EXPERIENCE OF TIME-CONSCIOUSNESS

Throughout this thesis I have referred back repeatedly to the problem of the direction of time. Initially it appeared to pose a great problem for psychological theories of time, for it appeared that our experience of the world as time-asymmetrical was at variance with the time reversal invariant laws of the most fundamental laws of physics. However, as we have seen, the problem of the direction of time arises in virtually every level of scientific explanation, as the theories in cosmology, thermodynamics, biology and biology all draw on time asymmetric behaviour. So it appears that time-asymmetry is not just a curiosity of consciousness, but is a real scientific problem that has to be addressed. If and when the problem is tackled at lower levels of description, it will be much easier to corroborate the human experience of time-asymmetry with the rest of scientific knowledge.

So, in this section, we will concentrate on another problem of time-consciousness, namely time-perception and time-estimation. We will show how the advancement of knowledge in the brain sciences is being brought to bear on these aspects of experience.

It is clear that many philosophers formed ideas that might be included in the historical corpus of the psychology of time, including Aristotle in *The Poetics* and, of course, Augustine and *distensi animi* in his *Confessions*. However, the earliest true psychological studies of time stem from 1865 onwards with the birth of the discipline of psychology itself. Mach, along with Vierordt, Wundt, Exner, Benussi and Titchener asked the question, what do we mean when we talk about a *sense* of time? Mach (1865), for example, believed that we had a time sense organ.

Interest in the psychology of time declined in the first part of the twentieth century despite the publication of works by Guyau 1890, James 1890, Nichols 1891 and Dondes 1868. It is difficult to suggest reasons for why this should be. The work of Boltzmann and Einstein had put questions about time firmly at the forefront of scientific research. Meanwhile, Husserl and Heidegger were doing much the same in philosophy. Despite research by Piaget, Bergson, Janet, Bachelard, Pieron, and Fraisse, psychological questions about time became marginal to the discipline as a whole. Finally, sfter 1951 the section on time was dropped from the influential *Handbook of Experimental Psychology* (Stevens, 1951).

In 1964, Adams argued that we should seek to explain time in terms of a more basic psychological mechanism. This strategy is similar to other approaches we have met in the natural sciences. It will be recalled that several attempts have been made to reduce psychological theories about time in terms of some other "more primitive" concept such as entropy or causality. Similar claims are being made here in psychology. That is, we should seek to explain the human experience of time in terms of a more primitive process. It is hard to believe that a more basic process or mechanism could be found as time underlies so many key psychological concepts. It occupies a fundamental position in cognitive representations of reality. For example, Slife (1993) has argued that time is often a background assumption in psychological explanation. It underlies the explanations of causality, change, process and behaviour. Indeed one psychologist Block has claimed that, "Non-temporal phenomena do not exist." In all these cases, the writers are not simple referring to events taking place in space-time. They are particularly referring to time as a structure that is asymmetrical, in terms of the differences of past and future. Perhaps the most deeply rooted assumption underlying psychological explanation is the primacy of the past, which fuels the hypothesis that present and future behaviour is dependent upon past experience. This hypothesis is drawn upon throughout the breadth of the discipline, for example, in psychotherapy where the patient tries to understand her abnormal behaviour in terms of her familial and social history. Similarly behaviourist theories of conditioning rely on learnt behaviour in the past controlling present and future behaviour.

The reductionist approach of Adam assumes that time-perception can be explained in terms of one single underlying factor. However as I have already suggested, it is more likely that our time-consciousness may be made up of several conceptually and functionally independent processes.

5.4.1 THE INTERNAL CLOCK HYPOTHESIS

Early hypotheses about time-consciousness claimed that there might be an organ of time, in the same way that we say that we have other sense organs. (See, for example, Mach 1865) However, as we said in Chapter 1, our "sense" or "perception" of time is almost certainly a metaphorical description, in the same way that we might describe someone as having a "sense of decency" or being able to "perceive a problem." As we have already seen, the term "sense" is used as a catch-all term to capture mental events which do not fall readily under the propositional attitude theory of behaviour. Other similar terms include "instinct" and "intuition," neither of which can be explained in terms of the sentential model. If the word "sense" is understood as referring to a bodily organ which responds to external stimuli, then humans certainly do not have a dedicated temporal sense organ which neurally codes energetic or chemical inputs in the same way that, for example, the olfactory pathway responds to olfactory information. However, we are able to identify specific areas of the brain in which specific types of temporal processing are located, such as the hippocampus, cerebellum and basal ganglia. (Edelmann 1989 and 1992) Time-consciousness is not a sense as commonly understood.

There have been attempts to partially explain some features of our time-consciousness in terms of particular low-level physiological phenomena, such as cellular oscillations and metabolic rhythms. In particular, much work has concentrated on the psychophysics of duration, which focuses on human time estimation and also our perception of time, as compared to chronometrically defined periods. Folk psychology tells us that "time flies when you are enjoying yourself" (time contraction), and "a watched pot never boils" (time dilation). Other curious phenomena also need to be explained, such as some people's ability to wake up just before the alarm clock rings. (In fact, a study by Zung and Wilson showed that subjects were able to wake themselves within ten minutes of a stipulated time.) A certain amount of overkill has taken place in this field. Eisler (1976) managed to locate 112 different studies on the human ability to estimate time periods on different scales. However, underpinning this descriptive level of human time estimation is the question of how these estimates come to be made, and here the genuinely explanatory research is being done. The development of psychology has followed the classical pattern of scientific research. That is, firstly measuring and accurately describing phenomena, and then moving on to try and explain them.

The first and most obvious hypothesis which was proposed to explain the human ability to estimate time was some kind of endogenous timing mechanism. The earliest search for a putative biologically based timing mechanism which might underlie this sense of time appears to be by Munsterberg (1890) who hypothesised a link between time-perception and heartbeat. In 1939, Schaefer and Gilliland posited a connection between time-perception and breathing. No direct relationship has been found. Other biological processes that have been hypothesised as potentially underlying the internal clock have been electroencephalogram (EEG) alpha rhythms. (See for example, Anliker 1963, and Durenan and Edstrom 1964, Treissman *et al.* 1984), body temperature (Pieron 1923, Hoagland 1933), and D2 receptor activity in the basal ganglia (Rammsayer 1993, 1994).

As a result of such studies it has been hypothesised that the body has one or several so-called "internal clocks," which underlie time-perception (Hancock, 1993). Typically such an internal clock has been modelled as consisting of a temporal oscillator (TO) and a calibrator or accumulator mechanism. The temporal oscillator generates pulses, which are then counted by the accumulator. Time-perception is based on the number of oscillations enumerated within a given time interval under "normal" operating conditions. (See Figure 5.3) The concept of an internal clock based on this model has proved itself to be useful in explaining distortions in human temporal perception. (Allan 1992)

Figure 5.3: A model for the temporal pacemaker (Treisman *et al.* 1990)

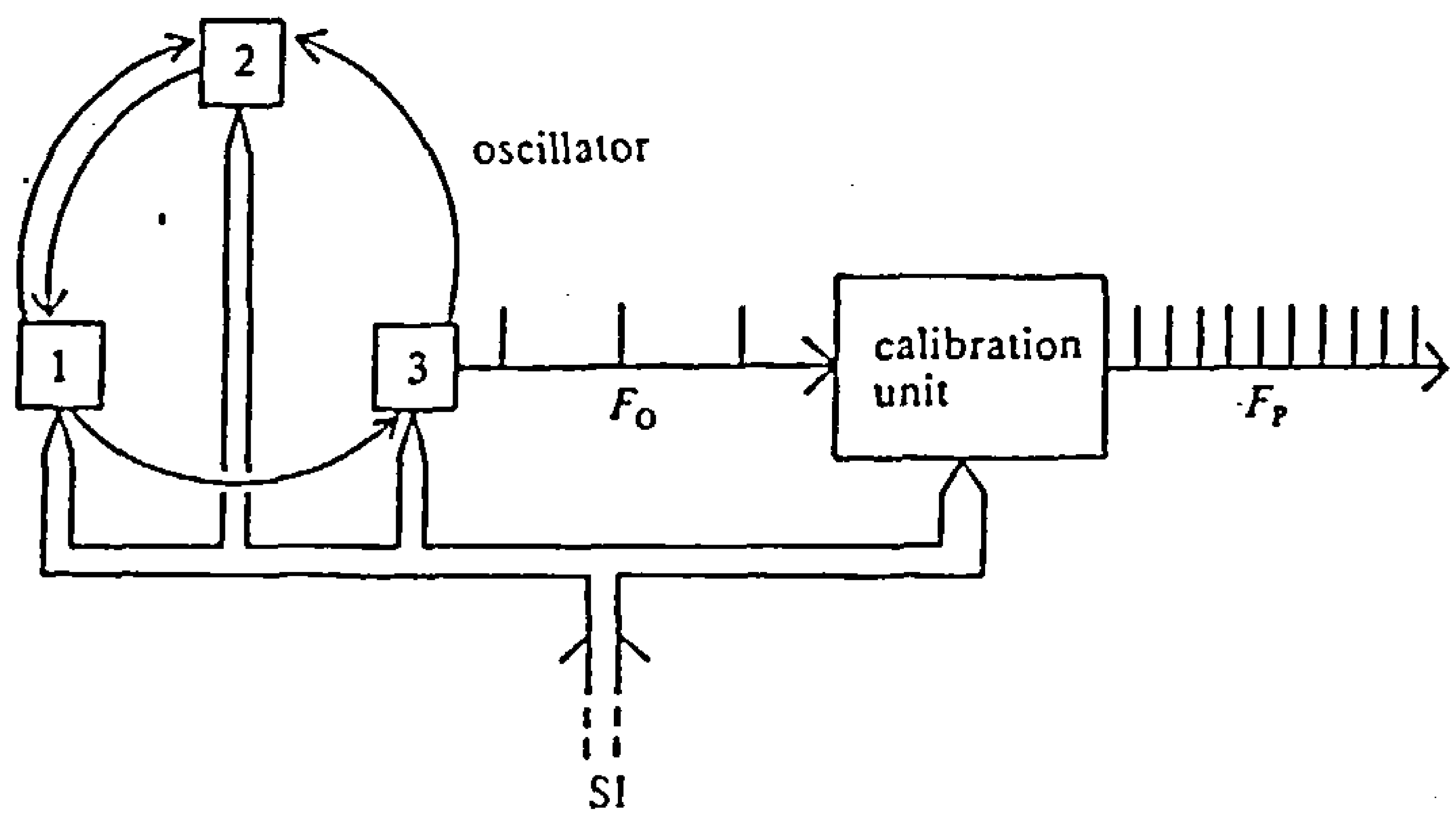


Figure 1. A model for the temporal pacemaker consisting of two components, a temporal oscillator (TO) and a calibration unit (CU), is shown. The TO is made up of units connected by paths that mediate excitatory and inhibitory interactions. Each unit may be affected by sufficiently strong sensory inputs, SI, which increase its specific arousal. The TO emits a regular series of pulses at a characteristic oscillator frequency $F_{c.O}$. These are transmitted to the CU, which in turn emits the final output of the pacemaker, a series of pulses at the pacemaker frequency F_P. This output provides timing information to the temporal processing mechanisms. Sensory inputs may act on the CU to increase F_P.

An internal or chemical clock was first proposed as an explanation to explain differences between the subjective perception of durations of time and chronometrically defined durations by the physiologist Hudson Hoagland in 1933. Hoagland observed that his wife, who was suffering from 'flu and was running a high temperature of 104 degrees Fahrenheit, had vastly overestimated the duration of his absence when he went out to a pharmacist. He knew that many chemical reaction and biological processes are speeded up when they occur at higher temperatures. He therefore conjectured that there was a master chemical clock that was temperature dependent, so that as body temperature increased, the number of oscillations would increase (as compared with "normal" body temperature), thus misleading the subject into believing that more time had elapsed than that defined chronometrically. Hoagland however could not identify any area of the brain where this clock might be located.

Treismann (1963) primarily developed the concept of an internal clock as an explanatory model. At the time, Treismann did not speculate on this model having any biological or neural basis. However research in psychopharmacology has offered support for the internal clock theory. For example it has been shown that the uses of certain psychoactive drugs such as lysergic acid diethylamide (LSD), metamphetamine and tetra hydro cannabinol (THC, cannabis) act as agonists on time-perception causing a reported overestimation of the amount of time that had passed (i.e., as if the internal clock had speeded upThe opposite antagonistic effect has been noted when subjects have ingested alcohol, tranquillisers such as haliperidol, or anaesthetics (Friedman 1990). The cause of the respective effects of these drugs is contentious. It has been argued for instance that the drugs effect the metabolism, and thus the internal clock (in a model reminiscent of Hoagland's original temperature modulated chemical clock.) Others have argued that these drugs alter the effective level of the neurotransmitter dopamine (DA), and that the rate of the internal clock is a function of the effective level of DA, increased DA levels having an agonist effect. However, overall the source and effect of the endogenous information that contributes to our sense of time is still contentious. (See for example the discussion in Friedman 1990, 14-15.) However, despite the observed effects of these drugs, there is no known neurological basis for the internal clock, though, for example, some have argued that the clock might be located in the superchiasmatic nucleus. (Block 1990) Although Treisman and the others may not have found an internal clock, their work has made progress towards explaining some of the phenomenological features of time-consciousness, such as time dilation ("time flying") and time contraction ("time dragging).

5.4.2 ZEITGEBERS

The lack of success in isolating a single variable determining the speed of a putative internal clock has led some to researchers to conclude that there is probably no central, neural pacemaker. Support for this thesis is provided by the work of Aschoff. Aschoff had argued (1964) that,

> There is apparently no organ and no function in the body which does not exhibit a similar daily rhythmicity. (1964, 1427)

However he also noted that humans, when deprived of exogenous external cues, or "*zeitgebers*", underwent a desynchronisation of some rhythms from others. For example, when deprived of *zeitgebers* (literally, time-givers) such as daily light and temperature changes, the human sleep and temperature cycles alter. (Most noticeably, the "normal" day increases to an average of about 25.5 hours when there is no sidereal cue.) The observation of the desynchronisation of different bodily rhythms led Aschoff (1984) to suggest that, rather than one internal master clock, humans have a number of oscillators that are kept in synch by *zeitgebers*, namely, by intrinsic changes in the environment. So it appears likely that we do not have a sense of time in the way that we have, for example, an olfactory sense. Sense being here defined as the neural coding of stimuli impinging on the body, which uses specific neurons that transduce energetic or chemical stimuli, and which use specific pathways to the brain. In other words, there are no direct environmental stimuli for time.

However at an automatic (sub-cognitive) level, the body does respond to certain forms of external stimuli that appear inextricably associated with time. And further more, as we have seen above, these external stimuli play an important part in regulating the rhythm of bodily functions. For example, the unpleasant experience of jetlag is caused when the body becomes out of synch with its environmental cues. Known as a circadian cycle or rhythm, the body, responding to external cues internally simulates the 24-hour cycle. It regulates and synchronises the sleep-wake cycle, the body temperature cycle, feeding patterns, as well as other hormonal and metabolic activity. (Friedman 1990) The circadian cycle also has effects on cognitive tasks, perception and motor performance.

However though circadian cycles may be useful for assessing how much time has passed on a relatively large scale (i.e.days), there is little evidence that it can help explain time-perception in the range of minutes and seconds. However there appears to be some correlation between sleep-wake cycles and time-perception. When isolated and deprived of *zeitgebers* or external cues, subjects who had the longest wake-sleep cycles, also had the longest subjective estimate of an hour's duration. (Aschoff 1985)

To briefly summarise, it is not useful to try and explain our time-perception in terms of some other more fundamental process. Nor does evidence suggest that there is a single underlying mechanism that could explain our time-consciousness, whether by a dedicated organ of time, or some from of internal clock. No neurological evidence supporting either of these mechanisms has yet been found. It seems likely that there is no single neural pacemaker. However it is clear that rhythms, oscillations and cycles permeate every aspect of our lives, and this manifests itself in regular behaviour from the single cell upwards. Also it is clear that the co-ordinated functioning of these at all levels is necessary to maintain normal behavioural and physiological function. Aschoff's work suggests that external environmental cues fulfil a vital function in maintaining synchronisation. So there is a complex explanatory relationship between the external world, the functioning of the body and our experience of time. Any integrated theory would have to incorporate all these factors. It begs the question of whether it will be possible to adopt one, single theoretical viewpoint that is capable of explaining the various factors affecting time-consciousness in psychological analysis. It has led one researcher to conclude,

> No existing model can handle the variety of experimental evidence on psychological time. (Block 1990, 1)

5.5 CONCLUSION

Any explanation of time-consciousness is going to be highly complicated. We have only examined the research on one small area, namely, time-perception and time explanation, and already we can see that there are many potential factors involved. A fuller account of time-consciousness would also have to cover memory, anticipation and clairvoyance, how we represent or make models of a temporal world, how we orient ourselves in time, and the development of time-consciousness throughout life. It would also have to take into account differences in culture and language. (Friedman 1990)

However what is apparent is that the detail these empirical studies provide is not irrelevant to our understanding. For the time being, the empirical evidence is highly fragmented. However, as knowledge about each of these areas increases we may be able to integrate the knowledge more easily. However it is not obvious that at the end of the research there will be a unitary time-consciousness, in the same way that vision is not a single, unified sense. Already certain naïve hypotheses, such as the master clock co-ordinating all bodily processes, are being rejected. It looks likely that the complexity of account will match the perplexity of our phenomenological experience.

Finally, an explanation of time-consciousness will have to connect our experience with our environment. It is apparent that we are sensitive to the environment at both the conscious and the sub-conscious level. By environment, we do not just mean natural features such as changes in heat and light, but also the social environment, such as work routine and time pressure. Ultimately there are bigger questions at stake, such as explaining our experience of time asymmetry. It is apparent that such an issue could not be tackled in isolation, and can only be answered when some very basic problems have been tackled at the most fundamental levels of explanation. For it is clear that time-consciousness is not something prior to the world, but is very much a product of the world. And the way to explain time-consciousness is through a truly unified approach.

CHAPTER 6
TOWARDS A UNIFIED THEORY OF TIME

The time is out of joint; O cursèd spite,
That ever I was born to set it right!
Hamlet

We began with the bafflement felt by St. Augustine when he tried to explain time. He expressed a certain consternation that something which is so fundamental should also be so abstruse. Examples from a wide range of theories and disciplines confirm that, to date, there is no single, unified theory of time. Indeed, as we have seen, there is virtually no consensus over any aspect of time. All the major questions surrounding time remain unanswered. Is time a real property of the world, or is it a structure imposed on the world by the mind? What is the structure of time? Is it linear or cyclic? Finite or infinite? Dense or discrete? Does time have a direction, or is our experience of time-asymmetry a quirk of consciousness, perhaps caused by the peculiar conditions that have to obtain for human life to exist? Is time relative or absolute? Does time actually exist at all, or is it caused by a conspiracy of clockmakers? Unfortunately, at the end of this thesis, these questions will remain unanswered. It was never my intention to tackle any of these important issues. However, the fact that all of these questions still remain unanswered was my departure point.

In Chapter 1, I showed the variety of conflicting ways that the term *time* is used in different types of theory. We found that substantial differences existed in the way time is conceptualised in different disciplines. These differences could be best illustrated by the contrast between time in three different theories: the special theory of relativity, thermodynamics and phenomenology. To be brief, time in the special theory of relativity is objective and reversible (time reversal invariant). Time in thermodynamics is objective but irreversible. Finally, time in phenomenological theory is subjective (or rather pre-subjective) and irreversible. Moreover, these differences are not confined to theories, operating at different levels of description. They also exist within the same research programme. For example, there is a debate in fundamental physics as to whether its laws really are time reversal invariant, or whether there are time-asymmetrical laws that pertain at an even deeper level.

We also showed that a similar degree of disarray obtains between so called "folk" theories about time. Though the linear form of representation appears to be the dominant cultural form, this is a relatively recent innovation. Previously the cyclical representation of time was dominant, and there are still cultures that prefer this form. (In the Appendix, we discussed the inadequacy of folk theories as a basis for scientific research programmes.)

Even in philosophy, there is no consensus. In fact, particularly in philosophy, one wonders whether the same concept is being discussed. We cited the examples of Reichenbach, McTaggart and Heidegger, and found that they held scarcely anything in common.

Paul Ricoeur has exploited the disarray surrounding the concept of time to make an extraordinary *a priori* claim, which would provide the focal point for our thesis.

> We are not capable of producing a concept of time that is at once cosmological, biological, historical and individual. (Ricouer, quoted in Virilio 1991, 103)

My intention was not to try and to produce a concept of time that was capable of unifying all these different elements. Rather, Ricoeur's arguments and those of others working in the phenomenological tradition, such as Wood, dissatisfied me. I believed that their arguments were informed by a myopic, muddled and positively 19th Century understanding of the scientific project. Hence, my aim is to show that Ricoeur's claim will not stand up to scrutiny, and that there are no principled arguments against the possibility of a unified theory of time. And furthermore, to demonstrate that recent developments in the philosophy of science and the philosophy of mind have made such a unified theory a plausible goal.

In the next four chapters, I analysed the major objections that stood in the way of a unified theory. In Chapter 2, we examined Husserl's arguments in *The Phenomenology of Internal Time-Consciousness.* Husserl made several claims. Firstly, the claim that first hand or "lived" experience is not reducible to any natural scientific explanation, that is, publicly available objective description. This assertion was based on his belief that scientific knowledge is a secondary, derivative and inauthentic type of knowledge that relies on the intentional act of the subject to constitute and validate it. Using this as his basis, Husserl was able to make several additional claims. Firstly, that phenomenology is the only possible foundation for epistemology. It alone can reveal the eternal truths of thought, and is thus the only possible philosophy. We saw that in *The Phenomenology of Internal Time-Consciousness,* Husserl used this claim to disqualify all other potential candidates for a philosophy of time. We challenged Husserl's claim that phenomenology is the only discipline appropriate for the study of experience. On the basis of his study of time-consciousness, we showed the limitations of his account of human temporality. We argued that his position was weakened by his inability to incorporate corroborating evidence from, for example, psychology. Finally, we examined the methodological difficulties presented by phenomenology. Due to its rejection of natural scientific method, phenomenology requires its own methodology and its own grounds for validation. However, the adoption of this isolated position left it vulnerable and uncorroborated, whereas the natural scientific approach has meanwhile developed its own sophisticated non-foundationalist and co-evolutionary strategies for validation. Phenomenology, almost by fiat, finds itself unable to validate its analyses. The philosophy of science, on the other hand and for the most part, has abandoned the project of foundationalist epistemology, and has developed its own highly sophisticated methods of corroboration. Philosophy of science has moved on. Phenomenology has not.

In Chapter 3, we examined some different *a priori* arguments within the philosophy of science. Like Husserl, but from a different perspective, they questioned whether empirical evidence can be brought to bear on questions about the structure of time, or whether such matters could only be settled by convention. We also asked whether we are justified in saying that time exists? Or is the concept of time just a useful fiction that helps to bind together certain scientific observations and laws. We argued that evidence from the fundamental sciences can be brought to bear on philosophical questions about time, and that the *a priori* approach is in decline. Recent developments in the philosophy of science have facilitated this empirical approach. In recent decades, there has been a shift away from both foundational questions of meaning, and an emphasis on individual laws and their evidence based on observation. This has been replaced by an emphasis on research programmes, bootstrapping, the coevolution of theories and super-empirical virtues. This has opened up opportunities for a wider variety of evidence to bear on our theories, and also means a whole set of new techniques can be adopted. Importantly, for us, it gives us a set of tools by which to bring empirical evidence to bear on questions of time.

In Chapter 4, we saw the limitations of the overall unity of science hypothesis as described by Oppenheim and Putnam. The classical account of unity of science, as described by Putnam and Oppenheim falls foul of many of the same problems facing the overall positivist project, as discussed in the previous chapter. In particular, the epistemological significance of theoretical evolution was overlooked. We saw how the hypothesis fell foul of the multiple realisability argument, and how Fodor has used this as an argument for disunity. The classical account of unity of science has had to adapt itself to accommodate the post-positivist account of theory evolution. Nevertheless there are no principled arguments, which challenge the metaphysical core, as it were, of the project. That is, mone of the arguments, which we have discussed, has challenged Oppenheim and Putnam's fundamental claim that everything *in principle* is describable by the laws of physics. They themselves were aware of the impracticalities of actually trying to do this. However, certain modifications have needed to be made. We argued that a more flexible understanding of the different levels of description was required, as real theories do not always have distinct levels that are "natural.". Oppenheim and Putnam also required that the laws of the reduced theory should be logically deducible from the reducing theory. However, this stipulation has had to be relaxed in order to accommodate statistical and probabilistic laws. The hypothesis has also had to be relaxed to accommodate the fact that certain terms can only be reduced locally and not globally. Also, it has had to recognise the differences between different types of explanation. There is fundamental level of explanation, which determines the boundary conditions and initial conditions of the entire system, and there is no form of behaviour that is not governed by its explanatory principles. But there are also other types of explanation which are apposite to the form of behaviour being described, providing a simplified characterisation, which nevertheless captures the important features of that level. However they are not fundamental explanations, and do not therefore threaten explanatory unification. We also saw that the question of what is *the* fundamental level of explanation is not yet settled. Finally, we highlighted the practical difficulties involved in explaining and predicting the behaviour of a complex system in terms of the fundamental level. However, as we have seen, none of these difficulties challenges the essence of the unity of science hypothesis, and it remains an ideal still driving scientific investigation. Moreover, we now have a unity of science hypothesis that may not be complete and formalised, but which takes into account the contingencies of scientific research, and is thus, more resilient to the complexity of real scientific explanation.

In Chapter 5, we examined the specific problem of reducing theories about mental states to theories about brain states. In particular, Nagel has argued that there is a special subjective quality of first hand experience that cannot be captured by an objective description. Also Fodor and others have claimed that we already have a successful and autonomous theory of mind, and that this means questions about mental states can be answered without reference to any empirical theory, such a neuroscience. Finally we tackled the issue of our subjective experience of time. We answered those critics who claimed that there could not be a naturalised psychology of time-consciousness. We did not do this by philosophical argument alone. We actually took an aspect of our time-consciousness and demonstrated that this subjective experience could be explained objectively. It is only by allowing a naturalised description of the mind, explained in terms of its relation to its environment, that we can truly start work on the ultimate grand unifying theory.

Although much work in this direction has begun, we have argued that it is fragmented, partly through the limitations of our current knowledge, but more particularly through an inadequate background of coherent philosophical thought. This has lead both philosophers and scientists to attempt grand metaphysical answers to muddled philosophical questions which threaten the progress which natural science and the philosophy of science have offered in the second half of the twentieth century.

APPENDIX
THE DEGENERATION OF THE "FOLK" OLFACTORY RESEARCH PROGRAMME

7.1 INTRODUCTION

There has been a persisting assumption in some philosophical quarters that a distinction can be made between our "ordinary" understanding of the world and the kind of knowledge that is generated by scientific investigation. Implicit in this assumption is the belief that "ordinary" knowledge is in some way basic or "given", and thus immune to theoretical change. This belief seems apparently justified for, though our scientific knowledge about the world in which we live has radically altered since the time of Aristotle's investigations, the manner in which we ordinarily observe and explain the world has remained, for the most part, unchanged. Athenians would still explain their actions in terms of desires and beliefs, and make observation statements that we could understand, such as "honey tastes sweet."

It has been argued that the two most significant changes in epistemology this century have undermined the deceptive simplicity of these types of explanation and observation. Firstly, the explanatory value of what has become known as Folk Psychology as a means of explaining human behaviour has been severely criticised, and new and potentially more fruitful paradigms of explanation are being proposed. Secondly, in the wake of important work by, for example, Hesse (1966), Kuhn (1970) and Feyerabend (1962), the theoretical nature of all our knowledge, including perceptual knowledge, has been exposed, in what represents a major shift away from foundationalist epistemology.

It will be shown that the naive conception of perceptual knowledge as being independent and untainted by theory is no longer tenable. It is as implicated within a theoretical system as any other form of knowledge. In particular, a strict dichotomy between descriptive and explanatory characterisations is shown to be untenable by any serious analysis of the development of scientific thought. The example of olfaction presented in this paper illustrates that the adequacy of perceptual judgements is dependent on the adequacy of the theoretical framework within which they are made. Indeed, an impoverished framework will hinder the full use being made of sensory information available to the observer.

7.1.1 WHY OLFACTION?

The example of the olfactory research programme has several advantages for the argument. Mainly, it is unencumbered by the philosophical tradition and weight of discussion under which, for example, the topic of visual perception currently labours. Furthermore, it is almost impossible, psychologically and culturally, to doubt the truth of everyday descriptions of visual perception. The argument of the paper allows us to evaluate a different sensory modality, which has remained relatively free of the burden of historical consideration.

The study of the chemical senses, and in particular olfaction, has received until recently relatively little attention when compared with the more familiar modalities of sight and hearing. This neglect is due to several contributory factors. (Doty, 1994) Firstly, although taste, smell and flavour play an enriching role in our everyday experience, the condition of anosmia (the partial or global insensitivity to odours) may have a less drastic and dehabilitating effect on our lives than the loss or impairment of sight or hearing. Secondly, and probably as a consequence of the former reason, there is a commonly held opinion that the chemical senses are unimportant to humans. Boring (1942, 437) commented that if Helmholtz had been a dog, we would have had hefty studies on the chemical senses, instead of his three volumed tome on the physiology of vision. A similar lack of interest led William James (1890) in his *Principles of Psychology* to comment "olfaction and the chemical senses need not be touched upon in this book, as almost nothing of psychological interest is known of them." Even scientists with a vested interest in olfactory research have concluded, "the sense of smell seems to be a non-vital or dying function in man" (Nef 1993, 266). Finally, unlike audition or vision, there appears to be no single important physical property that correlates with the quality of taste or odour. For example, Schiffman (1981) concludes that no single physico-chemical property is useful for predicting olfactory quality.

Science's concentration on vision and audition is reproduced in philosophical writing. A cursory glance at Merleau-Ponty's chapter on "Sense Experience" in the Phenomenology of Perception (1962) revealed not a single reference to the chemical senses. Its predominant focus is on vision. At the opposite end of the philosophical spectrum, as it were, the index to Patricia Churchland's *Neurophilosophy* (1986) has two independent references to olfaction, and only one to gustation, whilst having over fifty related to aspects of vision.

The argument will be in two stages. The first will expose the "theoretical" assumptions behind everyday olfactory judgements. The second will expose the inadequacy of this theoretical background when evaluated in light of recent systematic research. The conclusion will be that new theoretical understanding will suggest a wholly new "unfolk" set of perceptual categories.

Let us first sketch, albeit briefly, the main content of our folk theories about perception, particularly the understanding of taste and smell that is manifest in everyday language. There is difficulty for the framework of "ordinary beliefs" which is under scrutiny has been so deeply assimilated, that it is scarcely recognised as theory at all. Indeed, its nature is so insidious that several critics have argued that it is misleading to posit it as a theory and therefore should not be evaluated as such. (Wilkes, 1978) However it has almost become a philosophical platitude to argue that the concepts and theories of a particular paradigm can not be made explicit within that paradigm. And, in this case, when the framework that we are trying to isolate is ensconced in the structure and content of ordinary, everyday language, the problem is further magnified. However, as philosophers as diverse as Thomas Kuhn (1970), Hans Georg Gadamer (1965) and Michel Foucault (1966) have concurred, the emergence of a new and competing paradigm facilitates the evaluation of the old. And, as we have been arguing, a new paradigm (in this case, the consequence of recent scientific research on olfaction) can reveal what has become so deeply assimilated into the common parlance as to become transparent, qua theory.

7.2 THE FIVE FAMILIAR SENSORY MODALITIES OF FOLK PSYCHOLOGY

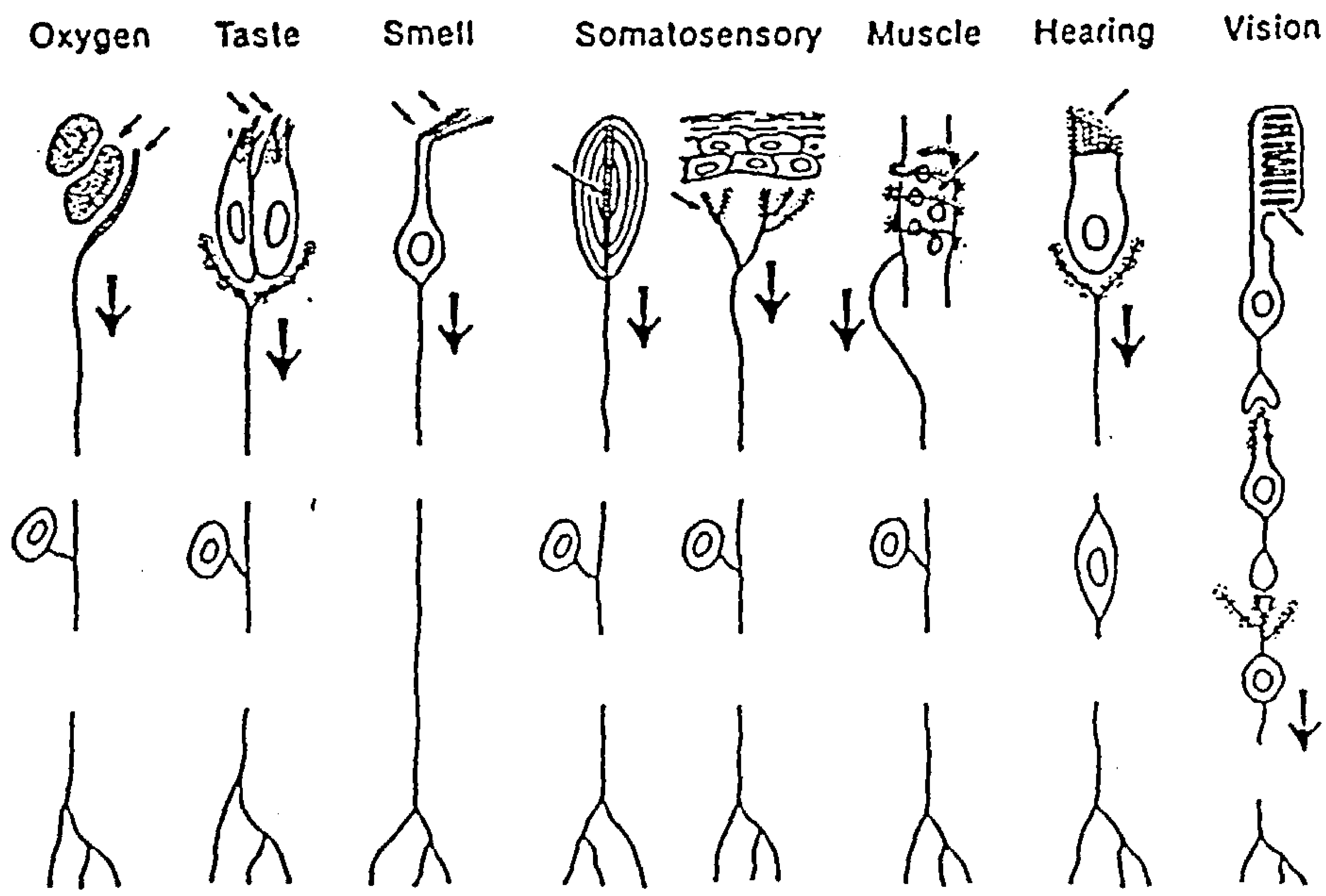
The belief that we possess five sensory modalities (sight, hearing, touch, taste, and smell) is deeply entrenched in the layperson's scientific knowledge. In school, children are taught they have five senses. The term "sixth sense" is well established, often used to indicate unfamiliar, or sometimes paranormal, sensory faculties which humans may possess. It seems that nothing could be more manifest to us than the objects of our own senses. We need only introspect our own sensory experiences to find evidence in their support. However, belief in these five familiar sensory modalities is systematically distorting our observation. The ordinary common sense or "folk" beliefs about the senses, which are represented in our everyday language, in our culture, and in what we learn are significantly false. As with many of our folk theories, its comprehension of the chemical senses has roots in Aristotelian science, and has not developed substantially since.

7.2.1 THE MANY LESS FAMILIAR SENSORY MODALITIES OF CONTEMPORARY SCIENCE

Contrary to everything that most of us (non-specialists) believe, humans have more than five senses. However, no claims are being made here about us having paranormal powers. Shepherd (1988) has identified many different types of sensory receptors, each responding to different external stimuli. As well as the familiar transductory mechanisms that are responsive to sound, light and volatile chemicals, others are sensitive to temperature, magnetic fields, oxygen, motion and deforming powers, such as pressure. (See Figure 7.1) Already our ordinary knowledge looks like a scanty version of the fuller picture presented to us by modern physical and biological sciences. It might be argued that the five familiar modalities represent the most important senses to us, *homo sapiens*. However, when we evaluate our knowledge of these five senses against scientific knowledge, those which are supposedly of the most use to us, not only does folk theory present an incomplete knowledge of human sensory capacity, it is also a very confused knowledge.

As has already been indicated, in the light of modern scientific knowledge, it is increasingly apparent that we are less than familiar with those senses that we claim to know. What folk theory might have characterised as a single sense resulting in an apparently simple sensation has been refuted. For example, Peter Smith (1989) cites the example of visual perception. At one time, he says, we might have been able to think of visual perception as "a relatively straightforward matter, involving the triggering of a play of uninterpreted images before the mind's eye." However studies of neurophysiology and clinical cases have shown that vision has a highly complex substrate, involving discrete neural computations, such as the perception of edges, of movement, of light and dark and vertices. Clinical studies of patients who have sustained localised neural damage have identified the different contributory computations that constitute normal vision. For example, the inability to perceive movement. Here, the patient was unable to pour herself a cup of tea, for she saw the fluid emanating from the spout as a solid, and was unable to detect the level in the cup rising. Another patient suffering from a different form of visual agnosia was unable to recognise individual's faces, despite having acute vision. He could recognise silhouettes, indicating knowledge of global forms, and could make good line by line drawings of faces. Through such clinical studies, it is known that visual perception is

Figure 7.1: Different types of sensory receptors in vertebrates. (Shepherd 1988)

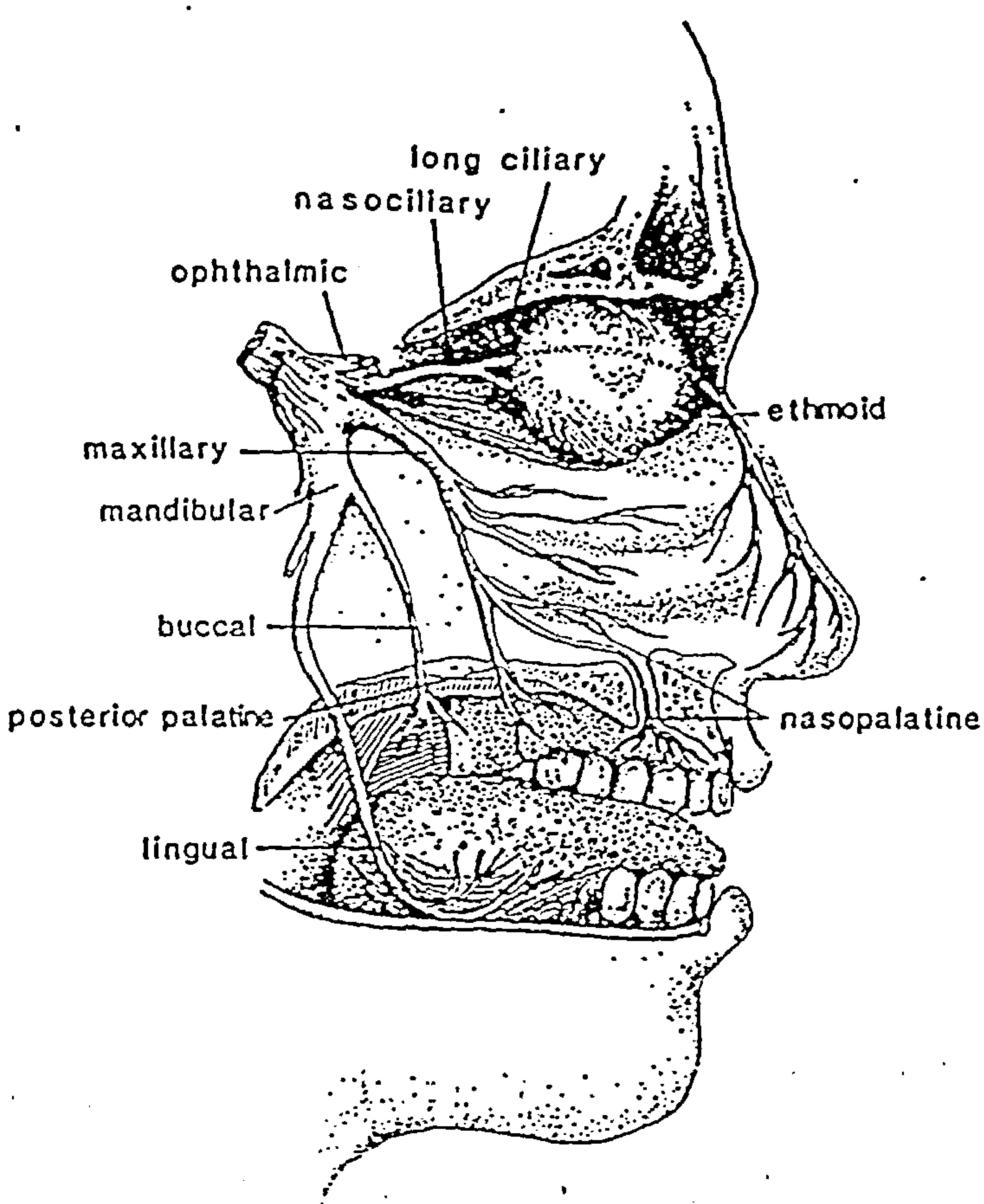


constituted by distinct sub-computations, some of which may be dysfunctional. However, none of these factors could become apparent within, and are highly counterintuitive to, our folk theory of vision.

7.2.2 "TASTE", GUSTATION AND FLAVOUR

The chemical senses have a highly complex structure that is not reflected in the vocabulary and use of ordinary language. Scientific knowledge of olfaction and gustation is now at odds with our common understanding of taste and smell, which is muddled and mistaken. For example, the term "taste," as it is commonly used, refers not to a single sensory modality but to several. When a person "tastes" a glass of wine, most of what she experiences is in fact contributed by the olfactory sense, that is, "smell." Taste is scientifically restricted to the single modality of gustation. Taste, in this sense, only contributes about 10% of variance to the overall flavour of the beverage or foodstuff. This may be readily demonstrated when olfaction is inhibited by a cold or 'flu and the nasal passages are obstructed. Consequently, we observe that food loses almost all of its flavour. What is generally called "taste" may be more accurately designated by the term "flavour." Overall flavour quality is constituted by several different senses: olfaction, gustation, the common chemical or trigeminal sense and what is somewhat erroneously described as "mouthfeel," as well as temperature and sensitivity to pH (acidity and alkalinity). (See Figures 7.2 and 7.3) What might have been perceived as a single, homogeneous sensory experience of "taste," can be broken down into at least four different channels of sensory information. It is the ability to discriminate and identify these distinct components that is used by the professional (wine or beer) taster. For example, the American Society of Brewing-Chemists (ASBC) Flavour Wheel, which is used for analysing beer, has forty-four descriptors for flavour notes. These are arranged into twelve general odour descriptors, and six or seven general gustatory descriptors, some of which overlap with those of odour. Within the gustatory classes, there is a mouthfeel subclass, a fullness of body subclass as well as descriptors that identify trigeminal qualities. (See Figure 7.4) The trained taster is then able to utilise forty-four different indicators for analysing and describing the flavour of beer. She is doing her perceiving within a far richer conceptual understanding of beer than other folk. Having been trained to discriminate these properties, they are in a much stronger position to exploit and appreciate the sensory information which is presented to them.

Figure 7.2: Primary branches of the trigeminal nerve that innervate the nasal and oral cavities. (Silver 1987)



our wheel (MBAA 1976)

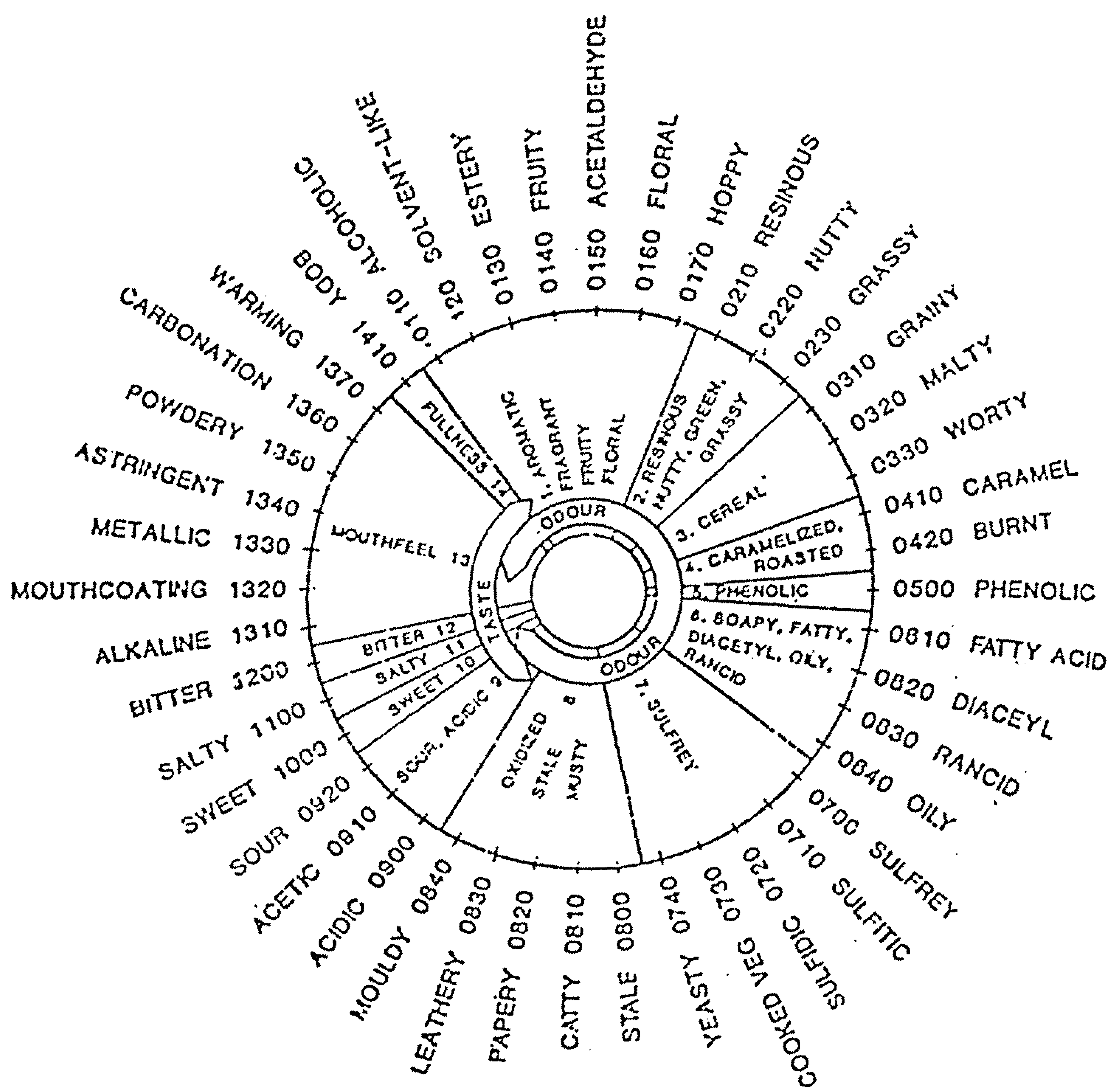
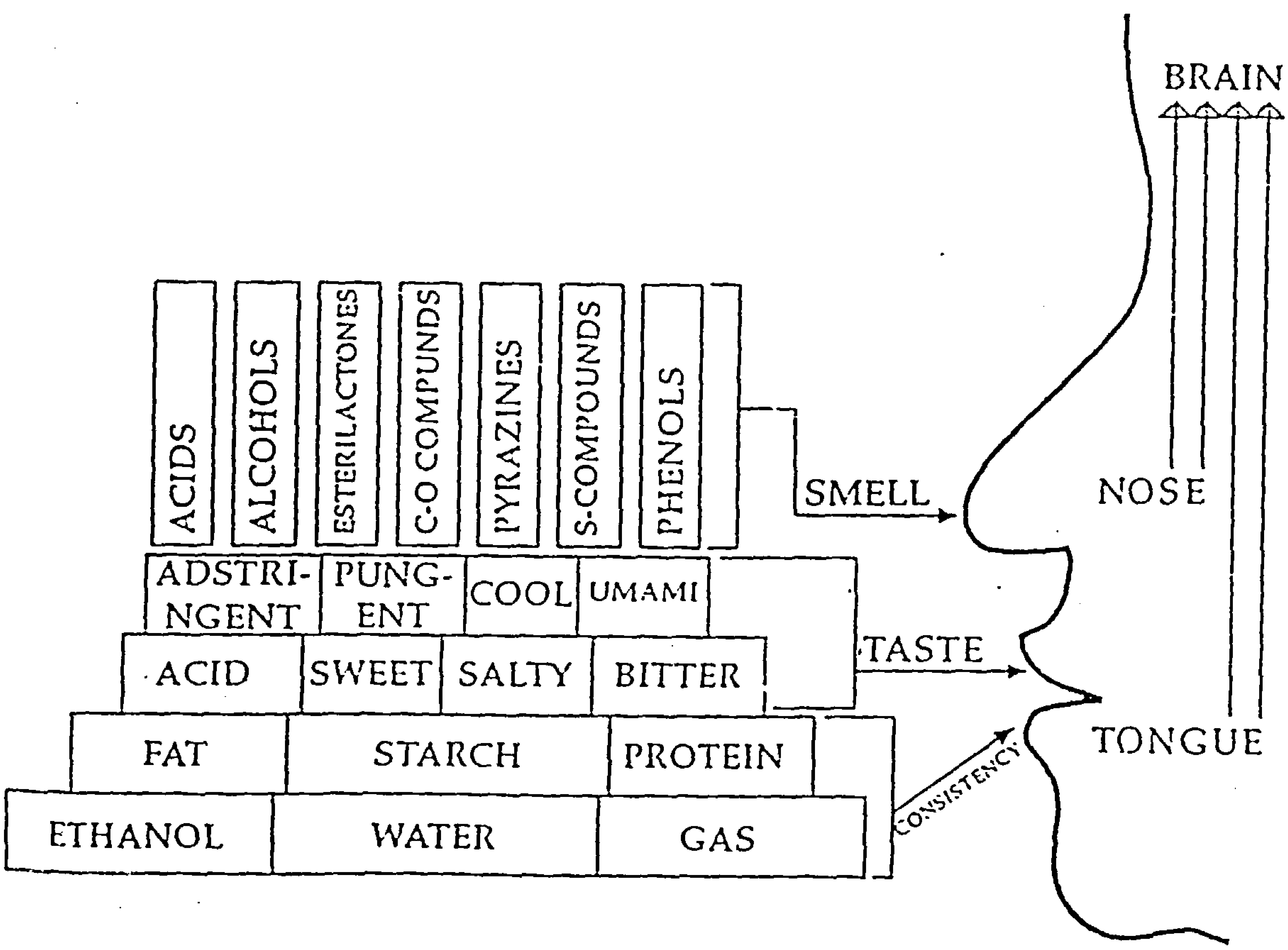


Figure 7.3: Role of the chemical senses in flavour assessment (Ney 1990)



The theory-laden nature of observation statements permeates every item of knowledge, down to the "simplest" of sense perceptions. It is not only grand theories of science, such as flat earth theories, Ptolmeic astronomy and caloric theories of heat, that are vulnerable to elimination. The example of the training of the professional taster shows the plasticity by which we can adapt to changes within our perceptual categories. It also illustrates how, through comparison with more sophisticated theories of perception, these folk theories qua theories can be made manifest. Their theoretical nature obscured by their familiarity. The example of the folk theory of "taste" compared with contemporary scientific understanding of olfaction and gustation has exposed the theoretical framework within which our perceptual judgements about what are variously called sense-datum, qualia or rawfeels are always made. It directly challenges those philosophers, particularly those in the phenomenological school, who would have us believe that there can be such a thing as a pure object of conscious experience, that is noemata untainted by theory. Though the framework may not be explicit, our observational statements are already determined by impromptu theories. The meaning/sense of our observations derives from this framework, and is not, as some would argue, intrinsic to the qualitative nature of the sensory information itself.

It is within this common theoretical structure that we speak of the five senses, and believe that we know what we are talking about when we taste a pint of beer. However, as has already been shown, our folk theories are often very wrong about those things that seem most manifest to us. In the case of the chemical senses they have produced a superficial awareness and thus understanding of the senses. What our folk theories describe as taste is scarcely "taste" at all. What is perceived as a discrete modality is a complex combination of several different senses.

7.3 CULTURAL INFLUENCES ON GUSTATORY RESEARCH

The example of "taste" will demonstrate how folk theories are influenced by environment and culture. Subsequently, these influences have been inherited by scientists working within the same culture and have had an identifiable effect on investigation and findings. For example, Western theories of gustation suggested that there were four basic categories of taste, corresponding to specialised receptor cells in the oral cavity, and more specifically areas of the tongue. This is evident in Henning's taste tetrahedron, each corner responding to the four categories of tastes: sweet, sour, bitter and salty. The literature on gustation on the whole still cleaves to these four prototypical categories, however recent studies, most notably the work of Japanese researchers, Kawamura and Kare (1987) have called into question their usefulness. Kawamura and Kare argue the case for the adoption of a fifth primary taste - umami - or protein taste. Umami's most common form is the food additive monosodium-l-glutamate (MSG), but its gustatory quality can be also noted in fish, meat, mushrooms and cheese, as well as other foodstuffs. As well as MSG, guanosine 5'-monophosphate and inosine 5'-monophosphate are also examples of umami stimuli. (Rolls, 1994) In the West, we have recently become familiar with MSG as a cited flavour enhancer in the ingredient's list of many processed foods. However the use of MSG in Eastern cooking is far more commonplace. In the domestic Japanese kitchen, MSG is as basic an ingredient as sucrose ("sugar") or sodium chloride ("salt"). (Downer, 1986, 10) So it seems that our theories about taste have been influenced and obscured by the learned sensitivities of the Western palate, and historically scientific theory has reflected our cultural mores.

Work by Bailiss and Rolls (1991) examined the viability of this fifth taste or gustatory stimulant. They found single neurons in the primary taste cortex and orbitofrontal cortex taste areas that had optimal responses to the following chemicals: MSG (umami), glucose (sweet), sodium chloride (salty), hydrogen chloride (sour) and quinine hydrogen chloride (bitter.) They concluded that glutamate, which produces umami taste in humans, is approximately as well represented in the primary taste cortical areas as the four familiar categories. Similar conclusions were reached by Plata-Salaman et al. (1992.)

The position of the four primary gustatory categories have been further undermined recently by the proposal of a sixth gustatory primary - starchiness - by Sclafani (1987.) He claims that glucose containing oligosaccharide (Polycose) can be identified as producing a distinct carbohydrate taste. The proposal of new gustatory primaries suggests is the debate concerning the existence of primary tastes is far from over. Indeed Scott and Giza conclude,

> [I]t is clear that the number of primary tastes is not settled. Other amino acids, proteins, nucleotides, lipids, vitamins, or minerals may vie for primacy in selected species or across phyla. The tools - biochemical, electrophysiological, anatomical, behavioural, and psychophysical - for evaluating the criteria for primacy are becoming available, though the discipline has yet to agree on what those criteria should be. (Scott and Giza 1994, 619)

Related to the issues introduced by the positing of umami and starchiness as additional primary gustatory qualities, is the gradual demise of the theory which postulated specialist receptors cells for each of the primary qualities. (See Figure 7.5) Nineteenth century theories of gustation claimed there were four discrete and mutually independent receptor cells. (Oehrwall, 1891) The evidence that there were primary tastes initially came from introspection, and received support from classical psychophysics, electrophysiological measurements and modern psychophysics. Boring's "labelled line" theory of gustation (1942) claimed that each primary quality had its own distinct receptor mechanism that would transduce non-volatile chemical information to the CNS via its own dedicated channel. (Scott and Giza, 1994) There were disagreements about the number of primary tastes, ranging from two (Valentin, 1853) to eleven (von Haller, 1786), but from Aristotle to the present time general consensus clustered around the four familiar categories.

Scientific research, specifically the search for primary receptors, has been determined by an Aristotelian theory that categorised tastes into distinct classes. Evidence suggests that it was Aristotle's obsession with taxonomy as a means of systematic enquiry that laid the foundations for the theory of primary tastes.

> [T]he types of taste, just as in the case of colours, in their simplest forms are opposites, sweet and bitter, but connected with these are oily and saline; in between come acid, pungent, astringent and sharp. Those seem to be all the differences in taste. (Aristotle, De anima, 422a)

The hypothesis that there are a small number of identifiable primary tastes has structured much gustatory research until the present time. It is doubtful whether the concept of taste categorisation in the sense perceived by Aristotle and his heirs is plausible. The theory that there are primary tastes initiated the search to identify taste receptors that are uniquely tuned to the four putative primaries. However research has not corroborated this hypothesis. Pfaffman (1941), using electronic amplifiers and filters to record the discharges of single neurons, discovered specific axons responded to more than one basic gustatory quality. That is, rather than possessing specialised receptors, taste cells in mammals were "broadly tuned," responding, in varying degrees, to several different taste stimuli. Pfaffman's conclusion has found support in more recent research done by Kimura and Beidler (1961) and Sato (1986.)

Figure 7.5: Diagram of tongue showing areas of sensitivity to four traditional primary tastes. (Buss 1973)

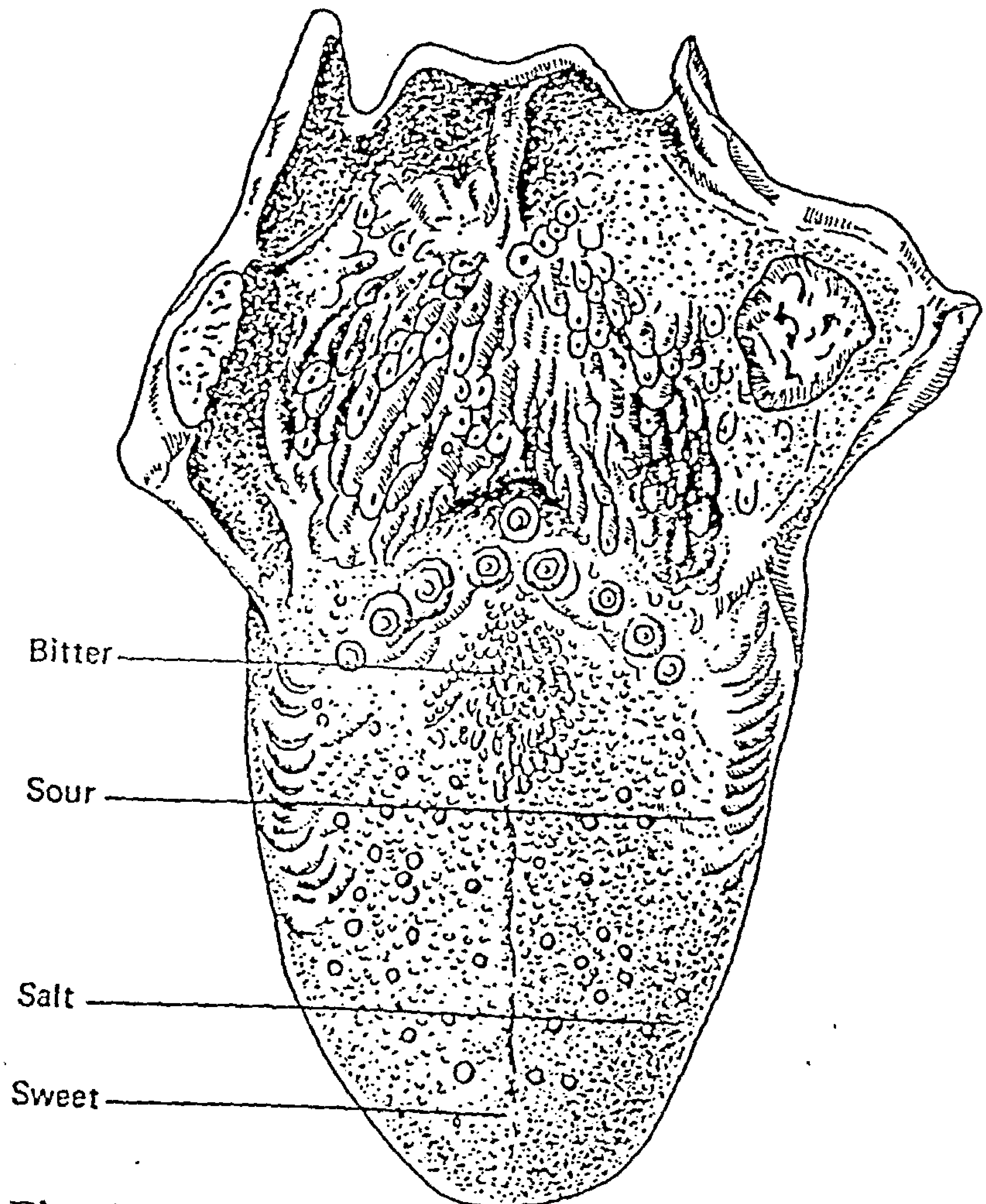


Fig. 9.5a *Distribution of taste sensitivity along the tongue. (From Psychology: Man in Perspective, by A. Buss, Wiley, New York, 1973,' p. 179. Reprinted by permission of the publisher.)*

The research programme, which sought to posit a small number of primary gustatory qualities, is being compromised by the discovery of additional classes. As has been shown the initial schema proposed by Henning positing four primary tastes has been subject to ad hoc modification. Furthermore, as Scott and Giza have stated, it looks as if further additions can not be ruled out. Clearly, wider investigation is driving theory and not the other way around.

7.4 THEORIES OF OLFACTORY CODING

The study of olfaction has followed a similar path to that of gustation. Research has again been characterised by, firstly, attempts to identify a small number of primary odours, and secondly, the identification of odour type specific receptors in the olfactory epithelium. However the failure of this project is more pronounced for olfaction. Whereas in gustation, work on identifying primaries is still seen as viable, in olfaction, interest in traditional odour classificatory systems has been all but abandoned. In the face of theoretical arguments proposed by Lancet (1986) and more recent experimental evidence from Buck and Axel (1991) suggesting that there are between 300 to one thousand primary receptor types in the epithelium, and not the hoped for handful. Even more conservative estimates (Ben-Arie et al. 1994) conjecture that there are approximately 130 olfactory receptor genes in humans.

Yet again, research in olfaction has been influenced by Aristotelian enthusiasm for classification. It will be recalled that Aristotle divided odours into five primary groups: pungent, sweet, harsh, astringent and rich. His influence is in evidence in subsequent olfactory research programmes that sought to identify primary odours which were correlated with type specific olfactory receptors. Up until the 1960's, work by Amoore was still postulating seven primary odours. It is an oddity that his influence over research in this field should be so tenacious. His other attempts at classification have been long dispatched to the textbooks for the history of science, such as his theory of the primary elements: earth, fire, wind and water.

A successful theory of olfaction would explain the qualitative aspects of odour perception, that is the relationship between physico-chemical properties and the perceived odour. It was Lucretius who first suggested that molecular shape underlay the quality of flavour.

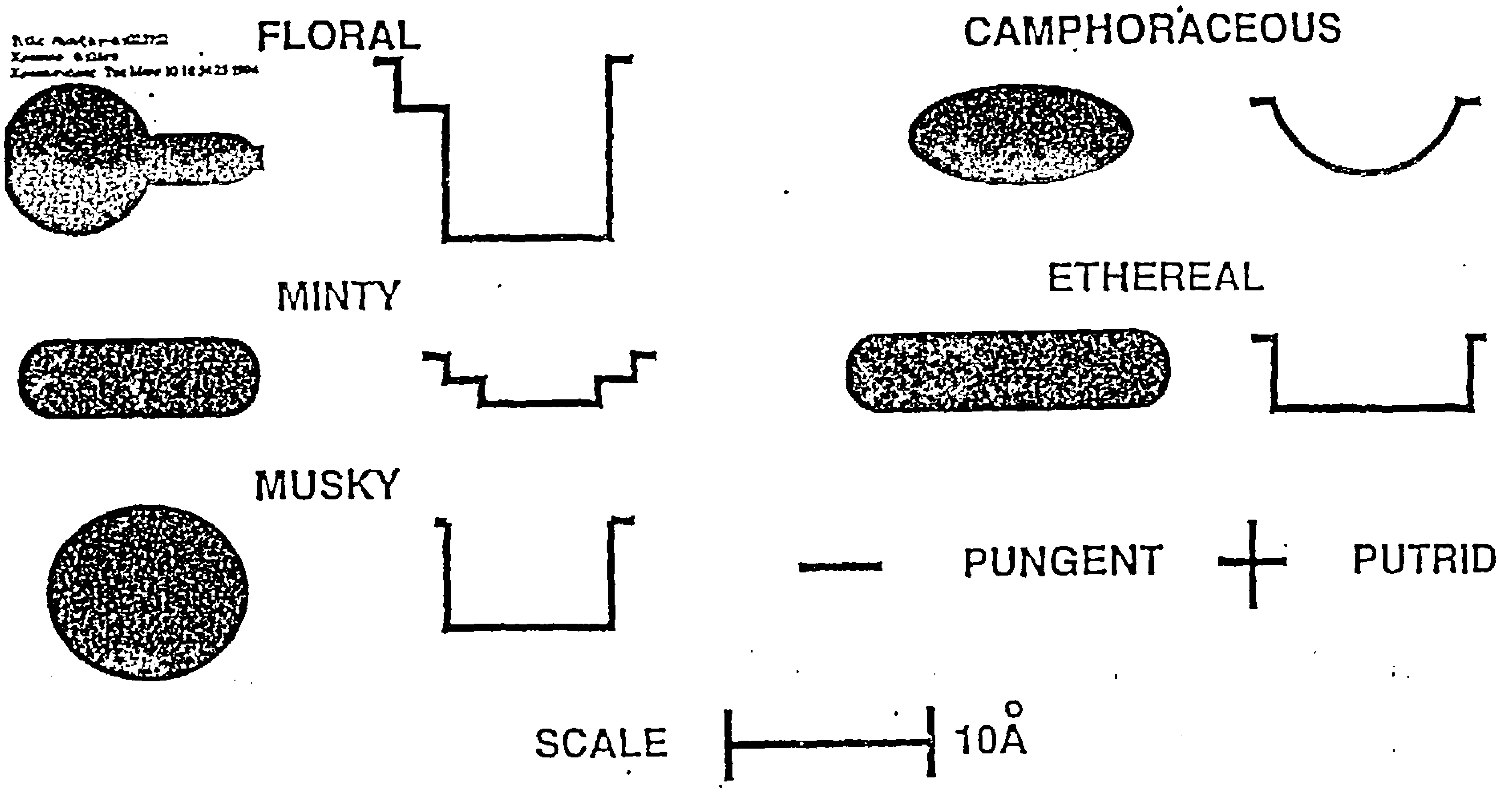
> [T]he liquids of honey and of milk have a pleasant taste as they are moved about in the mouth; but contrariwise the loathsome nature of wormwood and of harsh centaury twists up the mouth with a noisome flavour; so that you may readily recognise that these bodies which can touch our senses pleasantly are made of smooth and round atoms, but contrariwise all that seems to be bitter and rough are held in connection by atoms more hooked, and are therefore accustomed to tear open their way into our senses and to break the texture by their intrusion. (Lucretius, De rerum natura II, 398-407)

Currently, our knowledge of the neural basis for odour perception remains sketchy. But it is conjectured that these percepts may be mediated by the congruence between the odour molecule and its olfactory receptor. (Laing, 1994) There have been other theories - vibrational and teleactivation models, but these programmes degenerated approximately twenty-five years ago.

During the sixties and seventies Amoore's stereochemical classification system (steric theory) was the dominant explanation of odour coding. Amoore attempted to explain how perceived differences in odour quality are represented at the receptor level of the nervous system. A common approach to the odour coding problem was the hypothesis that odours which are judged to be similar, will have similar molecular shapes and sizes. (Moncrieff, 1966) Hence it was believed that correct odour classification could provide a vital clue to cracking the olfactory code. Amoore (1952 & 1962) postulated that there are seven primary odours: ethereal, floral, pepperminty, camphoraceous, musky, putrid and pungent. He argued that the first five primary odours were related to the shape of the molecules concerned. Accordingly, Amoore proposed that odour molecules have distinctive shapes and that the putative receptors for these molecules will have a complementary accommodating shape, sometimes referred to as a lock and key theory. Putrid and pungent odours, it was postulated, were linked to the negative and positive charger properties of the molecules respectively. (See Figure 7.6)

The plausibility of such an approach depends on the development of successful theories of odour classification, which would identify a set of primary odours, corresponding to the set of hypothesised receptor sites. Little agreement on such odour categories exists, and there is doubt as to whether primary odours exist at all, in the manner Amoore conceived. As with gustatory receptors, it seems more likely that olfactory receptor cells are not mono-osmic, that is, responsive to a unique odour type. Rather each

Figure 7.6: Amoore's seven primary odours, illustrating the "lock and key" approach (Amoore 1952 and 1962)



cell has a optimal response to one particular type of odour molecule and graded response to other molecules. Theories that predicted that alike molecular structures would have similar odours have been largely unsuccessful. There are many contrary examples of significant qualitative change where there has only been a small, discrete change in the stimulus. Engen (1982) cites the case of the stereoisomers d-carvone, which smells like caraway, and l-carvone, which smells like spearmint. Conversely, other chemicals with very different formulae have similar odours, for example, exaltone and musk ambrette.

As with gustation, the alternative to these labelled line theories of olfactory coding are "pattern" theories. Responding to evidence that indicates that receptors are not dedicated to a single odour type, pattern theories assume that there are only generalist receptors, providing a xenobiotic response. (Gesteland et al., 1965) Here, each generalist receptor is broadly tuned to respond to a wide range of stimuli, however it has its own individual response spectrum determining the specificity to which it senses particular odour types. The perception of different odour qualities would be then mediated by the pattern of activation across a population of such generalist receptors. Each particular quality would generate its own response matrix across the receptor population, termed a cross-fibre pattern. (Erickson, 1968)

The shift away from a labelled line theory, in which one receptor family type codes one odour, has brought with it a decrease in interest in odour classification systems. As we have seen, the success of the labelled line theory proposed by Boring relies upon two factors. Firstly, the successful categorisation of olfactory stimuli into odour primaries. Secondly the categorisation of receptor neurons into discrete family or class types corresponding to these primary odour categories. As has been shown in this discussion, neither of these has been satisfactorily achieved. Significantly, both with gustation and olfaction, the ancient theoretical framework based in folk theory that underlay much scientific research has floundered. Below, the programme of odour classification is analysed, and several symptoms of a degenerating research programme will be highlighted.

7.5 THE PROBLEM OF ODOUR CLASSIFICATION

The inability to establish a set of primary odours has stinted the explanation of the relationship between odour quality as perceived and its physico-chemical structure. To our knowledge, odour is exclusively determined by molecular structure. Hence it is believed that there is a systematic relationship between perceived odour and chemical constitution, and therefore a potential reduction of odour qualia. Beets (1957) wrote that the relation between structure and odour is complete and unambiguous. The details of that relationship were to be defined by empirical investigation. However this has proved a harder task than initially imagined. For example, Meilgaard (1991) writes that chemists have identified hundreds of volatile and non-volatile compounds present in beer, but could not account for this information in terms of flavour type and the relative flavour strength of each.

The development of a feasible, if provisional, classification system has been regarded as an essential intermediate step towards the elucidation of the underlying mechanisms. Harper *et al.* (1968) argue that it is the lack of a definitive odour classification system has hampered the development of a systematic relationship between what is perceived and its physico-chemical constitution. For though it is a reasonable project to explain odour quality in terms of chemical structures, the qualities themselves first need to be identified.

The failure to develop a successful empirically based odour classification system reflects the lack of understanding of the different contributions made by the chemical senses to flavour within traditional theory. Many of the early studies attempting comprehensive odour classification confused olfaction and gustation. This hindered scientific enquiry into the chemosensory modalities and their effect on flavour perception. Boring (1942) suggested that odour classification was still at a "pre-Newtonian" stage. He likened its stage of development to the pre-Newtonian understanding of colour where, for example, red was defined as the colour of blood and green that of grass. Boring noted that smell was often defined in terms of an object. For example, referred to as being like the smell of fish or the perfume of a flower.

Attempts to classify odours into a number of primary groups or categories have not progressed significantly since Aristotle. Several factors have contributed to this, including: (1) incomplete knowledge of the odour code, defining the complex relationship between molecular physico-chemical properties and corresponding odour perception, similar to that available for identifying colours; (2) the multidimensionality of the stimulus and its discrete nature hindering the isolation of primary odours; (3) the lack of proper names for odours; (4) our own lack of accuracy at recognising smells; and (5) the introduction of hedonics and affectivity into odour classification. These factors will be discussed in more detail below.

It has already been noted that there has been some confusion between what might be described as pure odours and trigeminal impact on flavour. This confusion is evident in classification systems from Aristotle (circa 350 b.c.) to Amoore (1952.) For example, Aristotle identifies seven main categories of odour: pungent, sweet, harsh, astringent, rich, bitter, and foetid. Pungency, harshness and astringency are qualities associated with the trigeminal nerve. Sweetness and bitterness are categories more usually identified with gustation. Amoore's influential work and that of Wright and Mitchell (1964) still sustain the pungency or trigeminal category. Some authors have regarded this confusion as a hindrance to the development of a successful odour classification system. Both Engen (1982) and Harper et al. (1968) have argued that it is necessary to clearly distinguish trigeminal qualities from odour qualities before it is possible to successfully identify odour quality with its physico-chemical properties. It was hypothesised that if pure odours could be successfully arranged into primary groups or classes, then this would facilitate the identification of particular physico-chemical similarities within each class, or common transduction mechanisms underlying each corresponding receptor class. Similar odour qualities might have a similar chemical instantiation. In turn these might provide clues to the transduction mechanisms underlying odour.

Since the seminal work of Linnaeus (1756), there have been several attempts to categorise odours. Figure 7.7 shows some of the most renowned attempts. However, overall attempts at odour classification have been largely unsuccessful, and interest has waned in the project. Sagarin (1955) commented that the only consistency is the lack of consistency between odour categories. No consensus has yet emerged as to which classification system to use, or even which set of descriptors should be used to describe the categories. Engen comments that when reviewing odour classification systems, writers feel obliged to include all the methods used so far as there is so little confidence concerning what a successful system would look like. There is not enough evidence to reject any theory which sounds plausible. Attention has shifted and is now mainly concentrated on neurobiological studies, following on from the work of Buck and Axel (1991), which have successfully identified specific genes thought to be responsible for odour reception.

In the absence of a universally accepted system of odour classification, professionals requiring standardised odour and flavour systems, such as perfumers, wine, beer and beverage makers, and botanists have continued to develop their own idiosyncratic heuristic and application specific systems. They employ their own distinctive vocabularies to facilitate effective communication between specialists within the same field.

Figure 7.7: Historical table showing renowned odour classification systems

Author	Linnaeus	Zwaardemaaker	Henning	Crocker Henderson	Amoore
Year	1752	1895	1915	1927	1952
No. of primary odours	7	9	6	4	(6)7
Descriptors	aromatic fragrant ambrosial alliaceous hircine repulsive nauseous	ethereal aromatic fragrant ambrosial alliaceous empyreumatic hircine nauseating foul	spicy fruity flowery burnt resinous foul	fragrant burnt caprylic acid	ethereal camphor minty floral musky putrid pungency

Odour classification systems

7.5.1 OBJECTIVE CLASSIFICATION

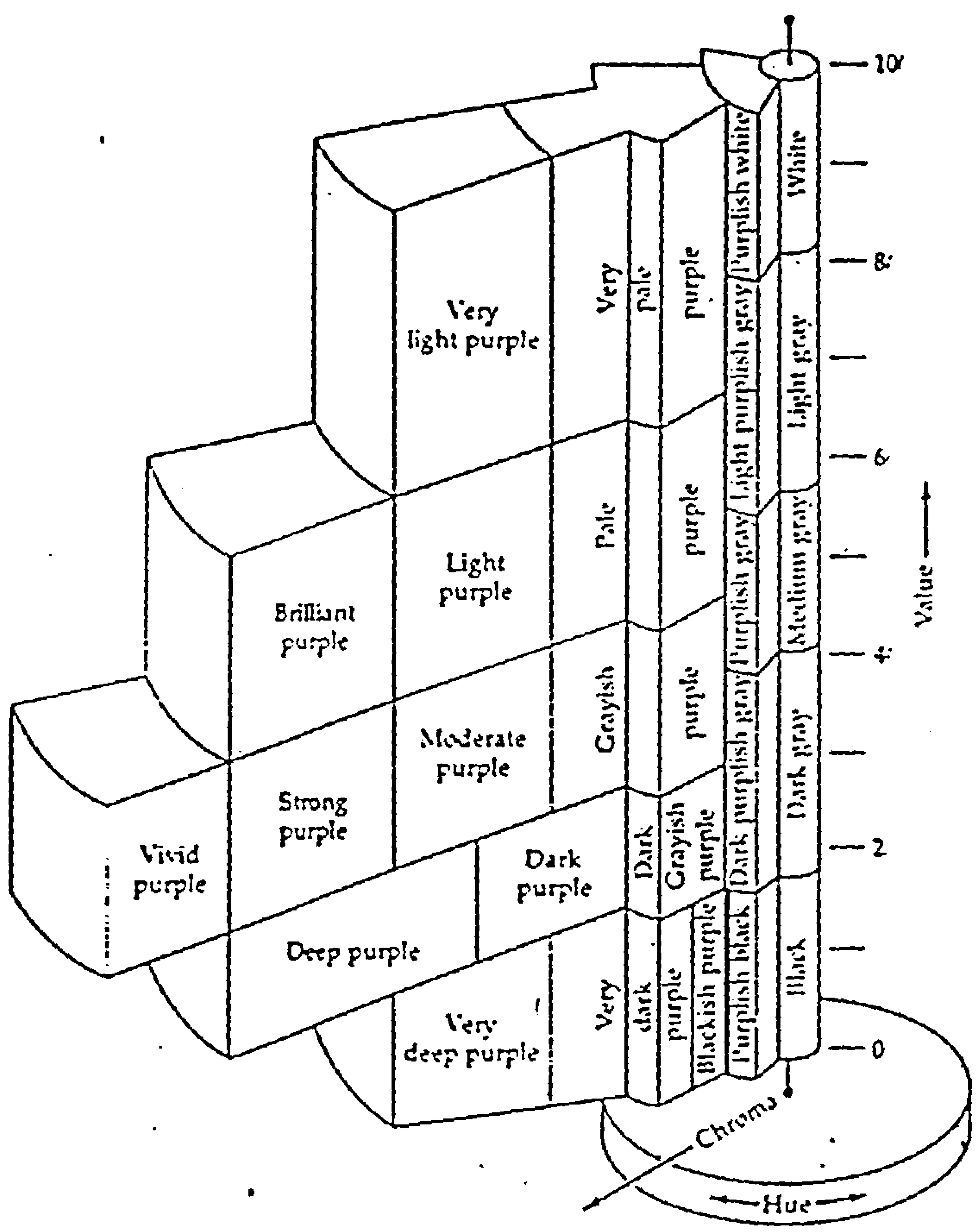
As detailed above, several reasons have hampered the development of a universal odour classification system. Firstly there is no single physico-chemical continuum upon which all odour quality categories could be ordered. Other sensory modalities have systematised in this manner, such as hue with colour (the Munsell System.) (See Figure 7.8) Colour is regarded as the ideal psychophysical model for the other sensory modalities where the full range of perceived colours depends on the underlying physical parameter(s) of wavelength. (However the parameter of wavelength alone is not sufficient to determine the colour perceived.) The primary hues are defined in terms of physical correlates. Each hue has its corresponding wavelength: Green is 530 nm. However, as Engen (1982) points out, it is the ability to provide a single physical parameter, that allows mixing the wavelengths, so that any colour in the spectrum can be produced. Understanding colour as a wavelength makes it possible to predict and manipulate colours. However, there is the problem of whether the potential for similarly devised primary odours exist, as no such correlates have so far been identified.

7.5.2 LANGUAGE

As has already been noted, another indication of the immaturity of the science of olfaction is the lack of proper names for odours. This was noted as early as the fourteenth century when Contarini observed that olfaction often uses the language of gustation, and that there was often confusion between the two. Gustation has at least its own putative vocabulary: salty, bitter, sweet, sour. However, for odours there has been no standardisation around a particular taxonomy, of which there are three main types:

1. descriptive, e.g. fruity, resinous, spicy.
2. names of specific materials, e.g. almonds, violet, lemon, lilac.
3. names of odourous chemicals, e.g. menthol, coumarin, vanillin.

The development of a language for odour has also been complicated by the development of specialist terminologies by professionals in the food industry. Their interest lies in developing a language that encompasses all the qualities present in, for example, a pint of beer. So taste, odour, irritant impact and mouthfeel are all included in the classification gure 7.8: Munsell system for colour notation (Hurvich 1981)



system. Due to the difficulties of standardizing a language of smell, perfumers often still communicate by ostensive reference to an odour, that is, by using cards impregnated with a particular smell or by odour bottles.

It has been argued that if there were a standardisation of language then much of the difficulties with describing odours would disappear. Not only professionally, but for the layperson too. Successful colour recognition, like odour recognition, seems to depend on people's vocabulary. It has been noted in the study of colour recognition that people have difficulty in recognising colours if there is not a well-known and accepted name for them. (Engen. 1982) That is, (and this is almost a platitude) our language limits what we are able to describe. However our lack of verbal agility does not prevent the human olfactory sense from being able to detect over ten thousand different simple (pure) odours. Our ability to recognise odours is superior to our ability to name them. (Mair and Engen, 1976) However when asked to identify a smell by naming it rather than simply discriminate, the subject is restricted to using the olfactory terminology with which she is familiar. If the vocabulary available is inadequate, incorrect or unfamiliar, this will have an adverse effect on her capacity to make a verbal identification. (Gregson, 1963) However a well-known categorisation system does not guarantee that accurate responses will always be elicited. For example, the four putative qualities of gustation are often confused. There is a high level of misrecognition. Often the primary tastes are misidentified. In tests one out of seven students reported citric acid as being "bitter" (prototypical sour) and one in twelve said that quinine hydrochloride as being "sour" (prototypical bitter) (Wenzel, 1954.) This has been blamed on unsystematic learning of the categories, whether through incorrect association or lack of accurate or constructive feedback. (Meilgaard, 1991) Much time and effort in the infant school classroom is dedicated to teaching children visual recognition skills, such as colour and shape recognition. However, gustatory and olfactory recognition are not deemed to be so important and are left to the individual to supervise their own learning.

Thus the main difficulties in olfactory (and gustatory) perception do not stem from the subjectiveness of our response but from the lack of an adequate language for olfaction with which we are competent or agree on. At the moment, given the impoverished state of olfactory taxonomy, many descriptions rely on and are contaminated by personal association, experience and memory. It is argued that this problem would disappear if a workable vocabulary were developed. In memory tests where people were asked to identify colours and odours, people were more likely to draw upon an analogy to describe a particular odour, than to give it a particular odour name. For example they would report that an odour was similar to that of a dusty library book. Engen concluded from this that our description of odour is idiosyncratic and impoverished.

Finally, odour classification has arguably been hampered by the introduction of hedonics into the classifying categories. This was introduced by Linnaeus in his *Odores Medicamentum*. He postulated seven odour groups or classes, which were divided into three broader categories. (See Figure 7.9) These categories reflected the affect that particular smells were said to induce in the subject. Some odours, Linnaeus concluded, are felt "to be kindly and desirable to our nerves," whilst others are "repellent to life." (Harper et al. 1968, 20).(The alliaceous class was not placed under any of these headings.) The intermediate class was described as "aliis grati aliis ingrati", that is, pleasant to some and not to others. This tendency to divide the odour classes into affective categories was copied up by Haller (1763), Zwaardemaker (1895) and Boring (1928.) The affective judgement is also represented in the labels from Linnaeus' odour classes. Where some odour classes are linked to certain odorous substances, like musk, garlic and goats, the two unpleasant classes are described in terms of the affect they would induce, that is repulsion (tetros) and nausea (nauseosos.) As can be seen from Figure 7.7, this tendency is replicated in the odour categories of Zwaardemaker, Henning (1916), and Wright and Mitchells (1964), where basic odour classes are described as "repulsive" or "unpleasant." The basis for these affective categories may have stemmed from old folk theories about the purpose of smell. It was believed that if something tasted or smelt "good," then it was literally good for you. Bad tastes and odours were thought indicative of poisonous substances and the bad smell was there to warn us. However this approach is flawed. Firstly, dilution can effect whether a smell is judged pleasant or unpleasant. Secondly, there is evidence that pleasantness and unpleasantness are learnt categories related to cultural mores. Moncrieff (1966) examined the response of babies to a range of smells, which showed that they were more tolerant of odours than adults. They did not appear to express a dislike, for example, of faecal odours. The affective categorisation of odours seems merely to reflect and reinforce our Western cultural values, hence a floral odour is judged as nice and excrement odour judged unpleasant. However there are no natural categories of pleasant or unpleasant categories. In Senegal, the Serer Ndut tribe uses essence of onion as a perfume, a practice that is reported as common throughout Africa. (Classen et al. 1994, 104). In the last decade, aromatherapists have made an industry out of our belief that certain odours can alter our mood. Whether the reputed effects these aromas have work by faith (self-deception), by association or possibly contain chemicals which stimulate an autonomic/physiological

Figure 7.9: Linnaean categories including hedonics

Hedonic Category		Linnaean Classificatory Term	Approximate Translation
Pleasant *suaveolentes*	I	Aromaticos	aromatic 
	II	Fragrantes	fragrant
Relatively pleasing *aliis grati aliis ingrati*	III	Ambrosiasos	ambrosial
	IV	Alliacos	oniony, alliaceous 
	V	Hircinos	goaty
Unpleasant *foetidi*	VI	Tetros	foul, repulsive 
	VII	Nauseosos	nauseating

Linnaean system including hedonic categories (1756) response, independent of the actual odour, or combine aspects of all these both, is uncertain. However, it reflects lack of knowledge about olfaction that these hedonic categories were included. By analogy, imagine the colour classification system being divided into,

GOOD	Yellow
	Green
INTERMEDIATE	Blue
	Indigo
	Violet
BAD	Red
	Orange

(Though there is a theory which claims colour can effect mood.) The introduction of emotive description such as "unpleasant" into the categories moves the process away from trying to correlate it with general odour terms, to something more normative and less specifiable.

Scientific interest in developing a classification system for odour on the same lines as colour has waned. Amoore's steric theory and its subsequent refutation were the last serious attempt to identify a small number of primary odours that corresponded to specific types of receptor sites on the olfactory epithelium. As indicated earlier, work by Buck and Axel has undermined the basic theoretical postulates of the project, by positing between 300 to one thousand primary receptor types in the epithelium.

The failures of the odour classification programmes of Aristotle, Linnaeus *et al* to find theoretical corroboration that would integrate it with other disciplines adds support for the revisionist or eliminitivist argument. As we saw with the five familiar sensory modalities, it is not the case that folk theories are a diluted version of the full scientific picture. Sometimes folk theory is fundamentally wrong, as with the case of "taste" and flavour. In such cases, reduction between the old and new theory can not be justified. As we have seen, the science of flavour has been (and to a certain extent still is) obscured by confusions between the gustatory, olfactory and trigeminal senses that have prospered on the back of an inadequate conceptual framework. In these cases, rather than trying to explain and accommodate the old theory with the new, the unsuccessful theory should be eliminated. Its deep cultural embeddedness should not require that any new theory should have to encompass its idiosyncrasies. Similarly, theories positing primary odours have survived for two millennia, but without much success. Research programmes seeking chemical and biological corroboration for them have been theoretical dead-ends. The old theories underestimated the multifactorial character of olfaction, and current evidence suggests that the programme may be misconceived. In Lakatos' terms (1978), all the evidence points to a degenerating research programme. Furthermore, a rival research programme has superseded it and is finding empirical corroboration for its predictions.

Olfactory theory based on Aristotelian introspective methods provided a structure (or heuristic) for scientific investigation. That project now looks misconceived, and its historical longevity should not grant it protected epistemological status. New scientific programmes should not be saddled with explaining historical failures.

7.6 CONCLUSION

Our perceptual judgements are structured by an impromptu theorising which we have shown to be prone to error. Hence, what is commonly described as taste, is observed as a single sensory modality. However our observations are distorted by the inaccuracies of that theoretical framework. For example, it has been shown that our perception of flavour has a complex structure, comprising of sensory information from several different senses. If we adopt the new theory of flavour, this alters the framework in which our observations are made, and hence the nature of what is perceived.

Much can be learnt from the olfactory case for more general arguments concerning the ontological primacy of qualia. It has been argued that there is a property of our subjective experience that does not lend itself to scientific reduction, that is our sensations and their properties are not explicable in terms of a combination of neurological, biological or electrochemical properties. Insofar as the arguments that qualia are not reducible to neurophysical phenomena, the examination of the olfactory case suggests that qualia, in the philosophical sense, must go.

What has been shown adds further credence to the eliminitivist claim that our Folk theories (in this case, of sense perception) are so radically false that they require such substantial modification that total elimination in favour of a new explanation is indicated. In this case, traditional theories of odour perception which endeavour to classify odours in a small number of primary categories, have been eliminated in favour of a theory of odour perception based on odour receptors. The example of odour classification illustrates the degeneration of a research programme that had its foundations in knowledge based upon introspective judgements. Originating in the Aristotelian predilection for descriptive classificatory systems, for nigh on two millennia, have sought to group odours into a small number of primary classes. However, the putative primary categories of smell have not been corroborated by scientific evidence. Instead of a small number of primary odours, current theories suggest that there exists up to 1,000 primary olfactory receptors. A paradigm shift has been witnessed.

Interest in the traditional odour classification programme has waned in the face of conflicting neurobiological evidence, which suggests that the basic concept of identifying a small number of primary odours is fundamentally flawed. We have shown how the old theory has systematically structured empirical investigation into olfaction. Amoore's steric theory tried to accommodate the conjecture that there were a small number of identifiable primary odours. His work on anosmia was destined to fail as the underlying theories that motivated his work were false. Similarly attempts to correlate the putative primary odours with chemical structure have been unsuccessful for the same reason - there simply are not primary odours as originally conceived.

In conclusion, this study of the chemical senses has shown that even our simplest folk beliefs about ourselves, as manifested in perceptual judgements, are seriously flawed. Research programmes based on folk olfactory beliefs are now facing their long overdue demise in the face of empirical evidence from the developing neurosciences. Although investigation into higher forms of cognitive activity is still in their early stages, already new competing theories are emerging challenging the old. The disintegration of the Folk Olfactory programme does not bode well for other folk theories.

REFERENCES AND BIBLIOGRAPHY

† *The year indicates the date of first publication of the text, which is not necessarily the date of publication of the edition or translation referenced. This appears in the* [square brackets] *at the end of the reference.*

Adam, B (1990). *Time and Social Theory.* Cambridge: Polity.

Allan, L.G. (1992). News on the internal clock. *International Journal of Psychology.* 27:21.

Amoore, J.E. (1952). The stereochemical specificities of human olfactory receptors. *Perfum. Essent. Oil Rec.* 43: 321-323, 330.

Amoore, J.E. (1962). The stereochemical theory of olfaction: 1. Identification of seven primary odours. Special Supplement to no. 37, *Proceedings of Scientific Section Toilet Goods Association* 37:1-12.

Anliker, J. (1963). Variations in alpha voltage of the electroencephalogram and time perception. *Science* 140: 1307.

Aristotle (1929). The Physics. Translated by P.H. Wickstead and F.M. Cornford. Cambridge, Mass.; Loeb.

Aristotle (1924) De Anima. *The Works of Aristotle Translated into English.* Translated by J.A. Smith. Oxford: Clarendon.

Armstrong, D.M. (1968). *A Materialist Theory of Mind.* London: Routledge.

†Augustine, St. (397). *The Confessions.* Translated by R.S. Pine-Coffin. Harmondsworth: Penguin. [1961]

Bachelard, G. (1934). *Le Nouvel Esprit Scientifique.* Paris: Alcan.

Bachelard, G. (1953). *Le Materialisme rationnel.* Paris: PUF.

Bacon, J. (1986). Supervenience, Necessary Coextension and Reducibility. *Philosophical Studies* 49: 163-76.

Barrow, J.D. (1988). *The World within the World.* Oxford: Oxford University Press.

Barrow, J.D. and F.J. Tipler (1986). *The Anthropic Cosmological Principle.* Oxford: Oxford University Press.

Baylis, L.L. and E.T. Rolls (1991). Responses of neurons in the primate taste cortex to glutamate. *Physiol. Behav.* 49: 973-979.

Beets, M.G.J. (1957). *Structure and odour. Molecular Structure and Organoleptic Quality.* London: Society of Chemical Industry.

Ben-Arie, N., D. Lancet, C. Taylor, M. Khen, N. Walker, D.H. Ledbetter, R. Carrozzo, K. Patel, D. Sheer, H. Lehrach and M.A. North (1994). Olfactory receptor gene cluster on human chromosome 17: possible duplication of an ancestral receptor repertoire. *Human Molecular Genetics* 3(2):229-235.

†Bergson, H. (1896). *Matter and Memory.* Translated by N.M. Paul and W.S. Palmer. New York: Zone Books.[1988]

Block, N. and J.A. Fodor (1972). What psychological states are not. *Philosophical Review* 81:159-81.

Block, N. (1980a). What is functionalism?, in Block, ed. (1980b) *op. cit.*

Block, N., ed. (1980b). *Readings in the Philosophy of Psychology, Volume 1.* Cambridge, Mass.: Harvard University Press.

Block, N., ed. (1981). *Readings in the Philosophy of Psychology, Volume 2.* Cambridge, Mass.: Harvard University Press.

Boden, M.A. ed. (1990). *The Philosophy of Artificial Intelligence.* Oxford: Oxford University Press.

†Boltzmann, L. (1896-98). *Lectures on Gas Theory.* Translated by S.G. Brush. Berkeley: University of California Press. [1964]

†Boltzmann, L. (1896b). Reply to Zermelo's remarks on the theory of heat, in Brush (1966) *op. cit.*

Bonevac, D. (1988). Supervenience and Ontology. *American Philosophical Quarterly* 25:37-46.

Boring, E.G. (1928). A new system for the classification of odours. *American Journal of Psychology* 40: 345-349.

Boring, E.G. (1942). *Sensation and Perception in the History of Experimental Psychology.* New York: Appleton-Century-Crofts.

Borst, C.V., ed. (1970). *The Mind/Brain Identity Theory.* Toronto: Macmillan.

Boucheraine, P. (1978). Le mètre, la seconde et la vitesse de la lumière. *La Recherche* 91: 64-643.

Brush, S.G. ed. (1966). *Kinetic Theory. Volume 2. Irreversible Processes.* Oxford: Pergamon.

Brush, S.G. ed. (1965). *Kinetic Theory. Volume 1. The Nature of Gases and Heat.* Oxford: Pergamon.

Buck, L. and R. Axel (1991). A novel multigene family may encode odour receptors: a molecular basis for odour recognition. *Cell* 65: 175-187.

Buss, A. (1973). *Psychology: Man in Perspective.* New York: Wiley.

Carnap, R. (1936 & 1937). Testability and Meaning. *Philosophy of Science* 3: 419-471 and 4: 1-40.

†Carnot, S. (1824). Reflections on the motive power and on the appropriate machines to develop this power, in R.H. Thurston, ed. *Reflections on the Motive Power of Fire, etc.* New York: Dover. [1961]

Cartwright, N. (1983) How the Laws of Physics Lie. Oxford: Clarendon

Causey, R.L. (1977). *Unity of Science.* Dordrecht: Reidel.

Chalmers, A.F. (1976). *What is this Thing called Science?* Milton Keynes: Open University Press.

Charalambous, G. ed. (1990). *Flavours and Off-flavours.* Amsterdam: Elsevier.

Churchland, P.M. (1979). *Scientific Realism and the Plasticity of Mind.* Cambridge, Mass.: MIT Press.

Churchland, P.M. (1984). *Matter and Consciousness.* Cambridge, Mass.: MIT Press.

Churchland, P.M. (1992). *A Neurocomputational Perspective. The Nature of Mind and the Structure of Science.* Cambridge, Mass.: MIT Press.

Churchland, P.M. (1994). Betty Crocker's Theory, in *London Review of Books.* 12th May 1994: 13.

Churchland, P.M. and P.S. Churchland (1990). Intertheoretic Reduction: A Neuroscientist's Field Guide. *The Neurosciences* 2: 249-56. Reprinted in and referenced from Warner and Szuba, eds. (1994) *op. cit.*

Churchland, P.S. (1986). *Neurophilosophy. Toward a Unified Science of the Mind/Brain.* Cambridge, Mass.: MIT Press.

Churchland, P.S. and T.J. Sejnowsky (1992). *The Computational Brain.* Cambridge, Mass: MIT Press.

Classen, C., D. Howes and A. Synnott (1994). *Aroma: The Cultural History of Smell.* London: Routledge.

Collie, C.H. (1982). *Kinetic Theory and Entropy.* London: Longman.

Coveney, P. and R. Highfield (1990). *The Arrow of Time. The Quest to Solve Science's Greatest Mysteries.* London: Flamingo.

Currie, G. (1990). Supervenience, Essentialism and Aesthetic Properties. *Philosophical Studies* 58: 243-257.

Darden, L. and Maull L. (1977). Interfield theories. *Philosophy of Science* 44: 43-64.

Davidson, D. (1970). Mental Events, in L. Foster and J.W. Swanson, eds. *Experience and Theory.* Amherst, Mass.: University of Massachusetts Press. Reprinted in and referenced from Davidson (1980) *op. cit.*

Davidson, D. (1980). *Essays on Action and Events.* Oxford: Clarendon Press.

Davies, J.T. (1965). A theory of the quality of odours. *J. Theoret. Biol* 8:1-7.

Davies, P.C.W. (1974). *The Physics of Time-Asymmetry.* London: Surrey University Press.

Davies, P.C.W. (1982). *The Accidental Universe.* Cambridge: Cambridge University Press.

Davies, P.C.W. (1988). *The Cosmic Blueprint.* New York: Touchstone.

Davies, P.C.W. (1995). *About Time. Einstein's Unfinished Revolution.* London: Viking.

†Deleuze, G. (1968). *Difference and Repetition.* Translated by P. Patton. London: Athlone. [1994]

†Deleuze, G. (1969). *The Logic of Sense.* Translated by M. Lester with C. Stivale. London: Athlone. [1990]

Denbigh, K.G. (1981). *Three Concepts of Time.* Berlin: Springer-Verlag.

Dennett, D.C. (1987). *The Intentional Stance.* London: MIT Press.

Dennett, D.C. (1991). *Consciousness Explained.* Boston: Little Brown.

Dennett, D.C (1995). *Darwin's Dangerous Idea. Evolution and the Meanings of Life.* London: Allen Lane.

Derrida, J. (1967). *La voix et le phénomène.* Paris: Presses Universitaires de France.

Descartes, R. (1984). *The Philosophical Writings.* Volume II. Translated by J. Cottingham, R. Stoothoff and D. Murdoch. Cambridge: Cambridge University Press.

Dixon, J.E. (1993). *10^{29} AD: The earth becomes inaccurate.* (Unpublished MA dissertation, University of Warwick.)

Doty, R.L., ed. (1994). *Handbook of Olfaction and Gustation.* New York: Marcel Dekker.

Downer, L. (1986). *Japanese Vegetarian Cookery.* London: Jonathan Cape.

Dretske, F. (1981). *Knowledge and the Flow of Information.* Oxford: Blackwell.

Dretske, F. (1989). Reasons and causes. *Philosophical Perspectives* 3: 1-15.

Dreyfus, H.L and S.E. Dreyfus (1988). Making a mind versus modelling the brain: artificial intelligence at a branch-point. In Boden (1990) *op. cit.*

Dreyfus, H.L. (1982b). Introduction, in Dreyfus (1982a), *op. cit.*

Dreyfus, H.L. ed. (1982a). *Husserl, Intentionality and Cognitive Science.* Cambridge, Mass.: MIT Press.

Duff, M.J. and C.J. Isham. eds. (1982). *Quantum Structure of Space and Time.* Cambridge: Cambridge University Press.

Dupré, J. (1993). *The Disorder of Things. Metaphysical Foundations of the Unity of Science.* Cambridge, Mass.: Harvard University Press.

Dureman, I. and R. Edstrom (1964). EEG and time perception. *Technical Report no. 22.* Uppsala, Sweden: University of Uppsala.

†Durkheim, E. (1895). *The Rules of Sociological Method.* London: Allen and Unwin. [1964]

†Durkheim, E. (1897). *Suicide, a study in sociology.* London: Allen and Unwin. [1953]

†Durkheim, E. (1900). *The Elementary Forms of Religious Life.* London: Allen & Unwin. [1959]

Earman, J. (1974). An Attempt to Add a Little Direction to 'The Problem of the Direction of Time'. *Philosophy of Science* 41:15-47.

Earman, J. (1989). *World Enough and Space-time: Absolute versus Relational Theories of Time.* Cambridge, Mass.: MIT Press.

Earman, J. and C. Glymour (1988). What revisions does bootstrapping need? A reply. *Philosophy of Science* 55: 260-264.

Eddington, A.S. (1920). *Space, Time and Gravitation.* Cambridge: Cambridge University Press.

Eddington, A.S. (1929). *The Nature of the Physical World.* Cambridge: Cambridge University Press.

Edelmann, G.S. (1989). *The Remembered Present: A Biological Theory of Consciousness.* New York: Basic Books.

Edelmann, G.S. (1992). *Bright Air, Brilliant Fire. On the Matter of the Mind.* Harmondsworth, Mddx.: Penguin.

† Ehrenfest, P. & T. Ehrenfest (1907). *The Conceptual Framework of the Statistical Approach to Mechanics.* Translated by M.J. Moravcsik. New York: Ithaca, Cornell University Press. [1959]

Einstein, A. (1956). *The Meaning of Relativity.* 4th edition. London: Methuen.

Ellis, B. and P. Bowman (1967). Conventionality in distance simultaneity. *Philosophy of Science* 34.

Enc, B. (1983). In defense of the identity theory. *Journal of Philosophy* 80: 279-298.

Engen, T. (1982). *The Perception of Odours.* New York: Academic Press.

Erickson, R.P. (1968). Stimulus coding in topographic and non-topographic modalities: on the significance of the activity of individual sensory neurons. *Psychological Review* 75: 447-465.

Evans-Pritchard E. (1940). *The Nuer.* Oxford: Oxford University Press.

Feigl, H. (1960). Mind-body Problem, not a pseudo-problem, in Rosenthal, ed. (1971) *op.cit.*

Feigl, H. and G. Maxwell, eds. (1962). *Scientific Explanation, Space and Time. Minnesota Studies in the Philosophy of Science. Volume 3.* Minnesota: University of Minnesota Press.

Feigl, H., M. Scriven, and G. Maxwell, eds. (1958). *Minnesota Studies in the Philosophy of Science. Volume 2. Concepts, Theories and the Mind-Body Problem.* Minneapolis: University of Minnesota Press.

Feyerabend, P.K. (1958). An attempt at a realistic interpretation of experience. *Proceedings of the Aristotelian Society* 58: 160-2.

Feyerabend, P.K. (1962). Reduction, explanation and empiricism, in H. Feigl and G. Maxwell, eds. (1962), *op.cit.*

Feyerabend, P.K. (1963). How to be a good empiricist: a plea for tolerance in matters epistemological, in *Philosophy of Science, the Delaware seminar*, vol. 2, ed. B. Baumrin, pp 3-39. New York: Interscience.

Finger, T.E. and W.L. Silver, eds. (1987). *Neurobiology of Taste and Smell.* New York: Wiley.

Fodor, J.A. (1971). Materialism, in Rosenthal (1971) *op. cit.*

Fodor, J.A. (1975a). *The Language of Thought*. Cambridge, Mass.: Harvard University Press.

Fodor, J.A. (1975b). Fodor's guide to mental representation: the intelligent auntie's vade-mecum. *Mind* 94: 76-100. Reprinted in and referenced from Stich and Warfield, eds. (1994) *op. cit.*

Fodor, J.A. (1981). The mind-body problem. *Scientific American* 244: 114-23. Reprinted in and referenced from Warner and Szuba eds. (1994) *op. cit*

Foucault, M. (1966). *Les mots et les choses. Un archéologie des sciences humaines*. Paris: Gallimard.

Friedman, M. (1981). Theoretical Explanation, in Healey, ed. (1981) *op. cit.*

Friedman, W.J. (1990). *About Time. Inventing the Fourth Dimension*. Cambridge, Mass.: MIT Press.

Frosch, R.A. (1997). Reductionism and unity of science. *American Scientist* 85: 2.

Gadamer, H.-G. (1965). *Truth and Method.* Second edition. London: Sheed and Ward.

Galison, P. (1987). How Experiments End. Chicago: University of Chicago Press.

Gesteland, R.C., J.Y. Lettvin, and H.W. Pitts (1965). Chemical transmission in the nose of the frog. *Journal of Physiology* 181: 525-559.

Getchell, T.V., R.L. Doty, L.M. Bartoshuk, and J.B. Snow, eds. (1991). *Smell and Taste in Health and Disease*. New York: Raven Press.

Gibbs, J. (1960). *Elementary Principles in Statistical Mechanics*. New York: Dover.

Glandorff, P. and I. Prigogine (1971). *Thermodynamic Theory of Structure, Stability and Fluctuations*. London: John Wiley.

Glymour, C. (1980). *Theory and Evidence*. Princeton: Princeton University Press.

Green, B.G. and H.T. Lawless (1991). The psychophysics of somatosensory chemoreception in the nose and mouth, in Getchell *et al.*, eds. (1991) *op.cit.*

Greenwood, J.D. (1991a). Folk psychology and scientific psychology, in Greenwood, (1991b) *op. cit.*

Greenwood, J.D. ed. (1991b). *The Future of Folk Psychology*. Cambridge: Cambridge University Press.

Gregson, R.A.M. (1963). The effect of psychological conditions on preference for taste mixtures. *Food Technology* 17(3): 44.

Gregson, R.A.M. and M.J. Mitchell (1974). Odour quality similarity scaling and odour-word profile matching. *Chemical Senses and Flavour* 195-101.

Grünbaum, A. (1973). *Philosophical Problems of Space and Time. 2nd Edition.* Edited by R.S. Cohen and M.W. Wartofsky. Dordecht: Reidel

Guyau, J.M. (1890). *La Genèse de l'Idée de Temps*. Paris: Alcan.

Hacking, I. (1983). *Representing and Intervening*. Cambridge: Cambridge University Press.

Haller, L. von (1763). *Elementa Physiologiae, Vol. 5*. Reprinted as *First Lines of Physiology*. New York: Johnson Reprint Corp. [1966]

Hallowell, I. (1937). Temporal orientation in western civilization and in a pre-literate society. *American Anthropologist*, 39: 647-670.

Hancock, P.A. (1993). Body temperature influence on time perception. *Journal of General Psychology* 120(3):197-215.

Harper, R., E.C. Bate-Smith, and D.G. Land (1968). *Odour Description and Odour Classification. A Multidisciplinary Examination.* London: Churchill.

Harré, R. (1970). *The Principles of Scientific Thinking.* London: Macmillan.

Harris, E.E. (1988). *The Reality of Time.* New York: State University of New York Press.

Hassard, J. ed. (1990). *The Sociology of Time.* London: Macmillan.

Haugeland, J. (1982). Weak supervenience. *American Philosophical Quarterly* 19: 93-103.

Haugeland, J. (1984). Ontological supervenience. *Southern Journal of Philosophy* 22: 1-12.

Hawking, S.W. (1982). The cosmological constant and the weak anthropic principle, in Duff and Isham (1982) *op. cit.*

Hawking, S.W. (1988). *A Brief History of Time. From the Big Bang to Black Holes.* London: Bantam.

Healey, R., ed. (1981). Reduction, Time and Reality. *Studies in the Philosophy of the Natural Sciences.* Cambridge: Cambridge University Press.

†Hegel, G.W.F. (1807). *Phenomenology of Spirit*. Translated by A. V. Miller. Oxford: Oxford University Press. [1977]

†Heidegger, M. (1927). *Being and Time.* Translated by J. Macquarrie and E. Robinson. Oxford: Blackwell. [1967]

†Heidegger, M. (1924a). *The Basic Problems of Phenomenology.* Translated by A. Hofstadter. Bloomington: Indiana University Press. [1988]

†Heidegger, M. (1924b). *The Concept of Time.* Translated by W. McNeill. Oxford: Blackwell. [1992]

Hellman, G. (1985). Determination and logical truth. *Journal of Philosophy* 82: 607-16.

Hellman, G. and F. Thomson (1975). Physicalism ontology, determination and reduction. *Journal of Philosophy* 72: 551-564.

Hellman, G. and F. Thomson (1977). Physicalist materialism. *Nous* 11: 309-345.

Hempel, C.G. (1965). *Aspects of Scientific Explanation.* New York: Free Press.

Hempel, C.G. (1966). *Philosophy of Natural Science.* Eaglewood Cliffs, N.J.:Prentice-Hall.

Henning, H. (1916). *Der Geruch.* Leipzig: Barth.

Hesse, M. (1966). *Models and Analogies in Science.* Notre Dame: Notre DameUniversity Press.

Hoagland, H. (1933). The physiological control of judgements of duration: evidence of a chemical clock. *Journal of General Psychology* 9: 267-287.

Hofstadter, D.R. and D.C. Dennett (1981). *The Mind's Eye. Fantasies and Reflections on Self and Soul.* Harmondsworth, Mddx.: Penguin.

Honderich, T. (1982). The argument for anomalous monism. *Analysis* 24(1): 59-64.

Honderich, T. (1984). Smith and the champion of mauve. *Analysis* 44(2): 86-89.

Hooker, C.A. (1975). Systematic philosophy and meta-philosophy of science: Empiricism, Popperianism, and realism. *Synthese* 32:177-231.

Hooker, C.A. (1981) Towards a general theory of reduction. Part I: Historical and scientific setting. Part II: Identity in reduction. Part III: Cross-categorial reduction. *Dialogue* 20: 38-59, 201-236, 496-529.

Horgan, T. (1978). Supervenient bridge laws. *Philosophy of Science* 45: 227-249.

Horgan, T. (1981). Token physicalism, supervenience and the generality of physics. *Synthese* 49: 395-413.

Horgan, T. (1982). Supervenience and microphysics. *Pacific Philosophical Quarterly* 63: 29-43.

Horgan, T. (1983). Supervenience and cosmic hermeneutics. *Journal of Philosophy* 22: 19-33.

Horgan, T. (1994). Non-materialist reduction, in Warner and Szuba, eds. (1994), *op. cit.*

Horwich, P. (1987). *Asymmetries in Time: Problems in the Philosophy of Science.* Cambridge, Mass.: MIT Press.

Hurvich, L.M. (1981). *Color Vision.* Sunderland, MA: Sinauer.

†Husserl, E. (1893-1917). *Zur Phänomenologie des Inneren Zeitbewußtseins. (1893-1917). Husserliana X.* Den Haag: Martinus Nijhoff. [1966]

†Husserl, E. (1894). *Philosophie der of Arithmetik, mit ergänzenden Texten. Husserliana XII.* Den Haag: Martinus Nijhoff. [1970]

†Husserl, E. (1900). *Logical Investigations. 2 Volumes.* Translated by J. N. Findlay. London: Routledge. [1970]

†Husserl, E. (1905-10). *The Phenomenology of Internal Time-Consciousness.* Edited by M. Heidegger. Translated by J.S. Churchill. The Hague: Martinus Nijhoff. [1964]

†Husserl, E. (1910-11). Philosophy as rigorous science, in Husserl (1965), *op. cit.*

†Husserl, E. (1913). *Ideas. General Introduction to Pure Phenomenology.* Translated by W. R. Boyce Gibson. London: Routledge. [1931]

†Husserl, E. (1921). *Cartesian Meditations.* Translated by Dorion Cairns. The Hague: Martinus Nijhoff. [1960]

†Husserl, E. (1928-9) *Formal and Transcendental Logic.* Translated by Dorion Cairns. The Hague: Martinus Nijhoff. [1969]

†Husserl, E. (1936a). Philosophy and the crisis of European man, in Husserl (1965), *op. cit.*

†Husserl, E. (1936b). *The Crisis of European Sciences and Transcendental Phenomenology.* Translated by D. Carr. Evanston: Northwestern University Press. [1970]

†Husserl, E. (1965). *Phenomenology and the Crisis of Philosophy.* New York: Harper and Row Publishers Inc.

Jackson, F. (1982). Epiphenomenal qualia. *Philosophical Quarterly* 32, 127:127-136.

Jackson, F. (1986). What Mary didn't know. *Journal of Philosophy* 83, 5:291-295.

James, W. (1890). *The Principles of Psychology.* New York: Holt.

Johnson-Laird, P. (1988). *The Computer and the Mind. An Introduction to Cognitive Science. Second edition.* London: Fontana Press.

†Kant, I. (1769-80). *Critique of Pure Reason.* Translated by N. Kemp Smith. London: Macmillan. [1929]

Kauffman, S. (1995). *At Home in the Universe. The Search for Laws of Complexity*. London: Viking.

Kawamura, Y. and M.R. Kare, eds. (1987). *Umami: A Basic Taste*. New York: Marcel Dekker.

Kim, J. (1993). *Supervenience and Mind. Selected Philosophical Essays*. Cambridge: Cambridge University Press.

Kimura, K. and M.Beidler (1961). Microelectrode study of taste receptors of rat and hamster. *J. Cell. Comp. Physiol.* 58: 131-140.

Kitchener, R.F. (1985). Genetic epistemology, history of science and genetic psychology. *Synthese* 65: 3-31.

Koehler, W. (1957). *The Mentality of Apes*. Translated by E. Winter. Harmondsworth: Penguin.

Kuhn, T.S. (1970). *The Structure of Scientific Revolutions. Second edition*. Chicago: Chicago University Press.

Laing, D.G. (1994). Perception of odour mixtures, in Doty, ed (1994) *op. cit.*

Lakatos, I. (1978). *The Methodology of Scientific Research Programmes. Philosophical Papers volume 1.* Edited by J. Worrall and G. Currie. Cambridge: Cambridge University Press.

Lancet, D. (1986). Vertebrate olfactory reception. *Annual Review of Neuroscience* 9: 329-55.

Landes D.S. (1983). *Revolutions in Time: Clocks in the Making of the Modern World.* Cambridge, Mass.: Belknap.

Lash, S and J. Urry (1994). *Economies of Signs and Space*. London: Sage.

Laudan, L. (1977). *Progress and its Problems*. Berkeley: University of California Press.

Lauer, Q. (1965). Introduction, in Husserl (1965), *op. cit.*

Lefebvre, H. (1991). *The Production of Space.* Oxford: Blackwell.

Leibniz, G. (1705-16). *The Leibniz-Clarke Correspondence, together with extracts from Newton's Correspondence.* Manchester: Manchester University Press. [1956]

Le Poidevin, R. and MacBeath, M. eds.(1993). *The Philosophy of Time.* Oxford: Oxford University Press.

LePore, E. and B. Loewer (1987). Mind matters. *Journal of Philosophy,* 84: 630-42. Reprinted in and referenced from Warner and Szuba, eds. (1994) *op. cit*

Lewis, D. (1966). An argument for the identity theory. *Journal of Philosophy* 63:17-25.

Lewis, D. (1983a). Extrinsic properties. *Philosophical Studies* 44: 197-200.

Lewis, D. (1983b). New work for a theory of universals. *Australasian Journal of Philosophy* 61: 343-377.

†Linaeus, K. (C. von Linne) (1749-1769). Odores medicamentorum, in *Amoenitaes Academicae,* Vol. 3, p. 1183 ff. Stockholm: Lars Salvius. [1956]

†Lucretius (99-55 B.C.). *De Rerum Natura. Second edition.* Translated by W.H.D. Rouse. Cambridge, Mass.: Harvard University Press. [1924]

Mackie, J.L. (1974). *The Cement of the Universe.* Oxford: Oxford University Press.

MacDonald, C. (1979). *Mind-Body Identity Theories.* London: Routledge.

†Mach, E. (1883). *The Science of mechanics.* Translated by T.J. MacCormack. LaSalle: Open Court. [1960]

Mair, R.G. and T. Engen (1976). Some effects of aphasic lesions on odour perception. *Sensory Processes* 1:33-39.

McTaggart, J.M.E. (1908). The unreality of time. *Mind* 17: 457-74.

Meilgaard, M.C. (1991). The flavor of beer. *MBAA Quarterly* 28:131-141.

Mellor, D.H. (1981). *Real Time.* Cambridge: Cambridge University Press.

†Marcus Aurelius (169 A.D.). *Meditations.* Translated by M. Staniforth. Harmondworth, Mddx.: Penguin. [1964]

†Marx, K. (1867). *Capital. Volume 1.* London: Penguin. [1976]

Merleau-Ponty, M. (1962). *The Phenomenology of Perception.* Translated by C. Smith. London: Routledge.

Meyerson, J.E. (1930). *Identity and Reality.* Translated by K. Leowenberg. London: Allen and Unwin.

Miller, I. (1982). Husserl's account of our temporal awareness, in Dreyfus ed. (1982a), *op. cit.*

Mohanty, J.N. (1982). *Husserl and Frege.* Bloomington: Indiana University Press.

Moncrieff, R.W. (1966). *Odour Preferences.* New York: Wiley.

Moncrieff, R.W. (1967). *The Chemical Senses.* London: Leonard Hill.

Morley, I.E. and G.M.K. Hunt (1996). *Philosophy of Psychology.* Coventry: University of Warwick.

Moskowitz, H.R. and C.B. Warren (1981). Odour Quality and Chemical Structure: Based on a symposium of the 178th meeting of the American Chemistry Society, Washington D.C., September 13, 1979. Washington: American Chemical Society.

Nagel, E. (1961). *The Structure of Science*. New York: Harcourt.

Nagel, T. (1974). What is it like to be a bat? *Philosophical Review* 83, 4:435-450. Reprinted in and referenced from Hofstadter and Dennett, eds. (1981) *op. cit*

Nagel, T. (1994). Consciousness and objective reality, in Warner and Szuba, eds. (1994) *op. cit.*

Nef, P. (1995). Early events in olfaction: diversity and spatial patterns of odorant receptors. Receptors and Channels 1:259-266.

Neurath, O. *et al.*, eds. (1938). *International Encyclopedia of Unified Science*. Chicago: University of Chicago Press.

Newton-Smith, W.H. (1980). *The Structure of Time*. London: Routledge.

Newton-Smith, W.H. (1981). *The Rationality of Science*. London: Routledge.

Ney, K.H. (1990). Aromagrams: a new approach to flavour, in Charalambous (1990) *op. cit.*

Nguyen, H. (1992). The spatialization of metric time. *Time and Society* 1:29-50.

Nichols, H. (1890). The psychology of time. *American Journal of Psychology* 3:453-529.

Nilsson, N. (1984). *Shakey the Computer*. SRI Report, SRI International. California: SRI.

Nozick, R. (1981). *Philosophical Explanations*. Oxford: Clarenden.

Oddie, G. and P. Tichy (1990). Resplicing properties in the supervenient base. *Philosophical Studies* 58: 259-269.

Oehrwall, H. (1891). Untersuchungen über der Geschsmacksinn. *Arch.Physiol. Scand.* 2: 1-69.

Ohnuki-Tierney, E. (1973). Sakhalin Ainu time reckoning. *Man* 8: 285-299.

Oppenheim, P. and H. Putnam (1958). Unity of science as a working hypothesis, in Feigl, H. et al., eds. (1958) *op. cit.*

Pearce, T.C. (1997). Computational parallels between the biological olfactory pathways and its analogue "The electronic nose." Part I. Biological Olfaction. Part II. Sensor-based machine olfaction. *BioSystems* 41: 43-90.

Penrose, R. (1979). Singularities and time-asymmetry, in S Hawking and W Israel eds. *General Relativity: An Einstein Centenary Survey.* Cambridge: Cambridge University Press.

Penrose, R. (1989). *The Emperor's New Mind.* Oxford: Oxford University Press.

Penrose, R. (1994). *Shadows of the Mind. A Search for the Missing Science of Consciousness.* Oxford: Oxford University Press.

Persaud, K. and G. Dodd. (1982). Analysis of discrimination mechanisms in the mammalian olfactory system using a model nose. *Nature* 299: 352-55.

Persaud, K. and G. Dodd. (1982). Analysis of discrimination mechanisms in the mammalian olfactory system using a model nose. *Nature* 299: 352-55.

Petrie, B. (1987). Global supervenience and reduction. *Philosophy and Phenomenological Research* 48: 259-269.

Pfaffman, C. (1941). Gustatory afferent impulses. *J. Cell. Comp. Physiol.* 17: 243-358.

Piaget, J. (1972). *Psychology and Epistemology: Towards A Theory of Knowledge.* Harmondworth, Mddx.: Penguin.

Pieron, H. (1923). Les problemes psychophysiologiques de la perception du temps. *Année Psychologie* 24: 1-25.

Place, U.T. (1956). Is Consciousness a Brain Process? *British Journal of Psychology* 47: 44-50. Reprinted in and referenced from Rosenthal, ed. (1971) *op. cit.*

Plata-Salaman, C.R., T.R. Scott and V.L. Smith-Swintovsky (1992). Gustatory neural encoding in the monkey cortex: L-amino acids. *J. Neurophysiol.* 67: 1552-1561.

Poincaré H. (1890). On the three-body problem and the equations of dynamics, in Brush, ed. (1966) *op. cit.*

Poincaré, H. (1893). Mechanism and experience, in Brush, ed. (1966) *op. cit.*

Popper, K and J. Eccles (1977). *The Self and its Brain.* New York: Springer.

Popper, K. (1982). *The Open Universe.* London: Hutchinson.

Price, H. (1996). *Time's Arrow and Archimedes' Point. New Directions for the Physics of Time.* New York: Oxford University Press.

Prigogine, I (1980). *From Being to Becoming. Time and Complexity in the Physical Sciences.* San Francisco: Freeman.

Prigogine, I. (1984). *Order Out of Chaos.* New York: Bantam.

Putnam, H. (1967). The Nature of Mental Events, in Putnam (1975) *op. cit.*

Putnam, H. (1975). *Philosophical Papers. Volume 2. Mind, Language and Reality.* Cambridge: Cambridge University Press.

Putnam, H. (1992). *Renewing Philosophy.* Cambridge, Mass.: Harvard University Press.

Pylyshyn, Z. (1980). Computation and cognition: issues in the foundation of cognitive science. *Behavioural and Brain Sciences* 3, 1:111-134.

Quine, W.V.O. (1953). *From a Logical Point of View.* Cambridge, Mass.: Harvard University Press.

Quine, W.V.O. (1970). *Philosophy of Logic*. Englewood Hills: Prentice-Hall.

Rammsayer, T.H. (1993). On dopaminergic modulation of temporal information processing. *Biological Psychology* 36: 209-222.

Rammsayer, T.H. (1994). A cognitive-neuroscience approach for elucidation of mechanisms underlying temporal information processing. *International Journal of Neuroscience.* 77: 61-77.

Raphael, B. (1976). *The Thinking Computer. Mind Inside Matter.* San Fransisco: Freeman.

Ray, C. (1991). *Time, Space and Philosophy*. London: Routledge.

Reichenbach, H. (1956). *The Direction of Time.* Edited by M. Reichenbach. Berkeley: University of California Press.

Reichenbach, H. (1958). *The Philosophy of Space and Time.* Translated by M. Reichenbach and J. Freund. New York: Dover.

Richardson, R.C. (1979). Functionalism and Reductionism. *Philosophy of Science* 46: 533-58.

Ricoeur, P. (1984). *Time and Narrative. Volume 1.* Translated by K. Blamey and D. Pellauer. Chicago: University of Chicago Press.

Ricoeur, P. (1988). *Time and Narrative. Volume 3.* Translated by K. Blamey and D. Pellauer. Chicago: University of Chicago Press.

Rose, S. (1997). *When making things simple does not give the right explanation.* Times Higher Education Supplement 1296 (5th September 1997): 16-17.

Rolls, E.T. (1994). Central taste anatomy and neurophysiology, in Doty, ed.(1994) *op. cit.*

Rorty, R. (1965). In defense of eliminative materialism. *Review of Metaphysics* XIX. Reprinted in and referenced from Rosenthal, ed. (1971) *op. cit.*

Rosen, E. (1959). *Three Copernican Treatises.* New York: Dover.

Rosenthal, D.M., ed. (1971). *Materialism and the Mind-Body Problem.* Englewood Cliffs, New Jersey: Prentise-Hall.

Rychlak, J. (1981). *Introduction to Personality and Psychotherapy* (Second edition). London: Houghton Mifflin.

Sagarin, E. (1955). On the inherent invalidity of all current systems of odour classification. *Journal of the Society of Cosmetic Chemists* 2: 25-35.

Salmon, W. (1971). *Statistical explanation and statistical relevance.* Pittsburgh: University of Pittsburgh Press.

Salmon, W. (1977). The philosophical significance of the one-way speed of light. *Nous* 11: 253-92.

Sato, T. (1986). Receptor potential in rat taste. *Prog. Sensory. Physiol.* 6: 1-37.

Schaefer, V.G. and A.R. Gilliland (1938). The relation of time estimation to certain psychological changes. *Journal of Experimental Psychology* 23: 545-552.

Scheffler, I. (1963). *The Anatomy of Inquiry.* New York: Knopf.

Schiffman, S.S.(1981). Characterization of odor quality utilizing multidimensional scaling techniques, in Moskowitz and Warren, eds. (1981) *op.cit.*

Schurz, G. and G.J.W. Dorn eds. (1991). *Advances in Scientific Theory. Essays in Honour of Paul Weingartner on the Occasion of the 60th Anniversary of his Birthday.* Amsterdam: Rodopi.

Sclafani, A. (1987). Carbohydrate taste, appetite and obesity: an overview. *Neurosci. Biobehav. Rev.* 11:131-153.

Scott, T.R. and B.K. Giza (1994). Theories of gustatory neural coding, in Doty, ed. (1994) *op. cit.*

Scriven, M. (1962). Explanation, predictions and laws, in Feigl and Maxwell (1962) *op.cit.*

Shepherd, G. M. (1988). *Neurobiology. Second edition.* Oxford: Oxford University Press.

Shoemaker, S. (1969). Time without change. *Journal of Philosophy* 66:363-81.

Shoemaker, S. (1994). The mind-body problem, in Warner and Szuba, eds. (1994) *op. cit.*

Silver, W.L. (1987). The common chemical sense, in Finger and Silver, eds. (1987) *op. cit.*

Sklar, L. (1974). *Space, Time and Spacetime.* Berkeley: University of California Press.

Sklar, L. (1985). *Philosophy and Spacetime Physics.* Berkeley: University of California Press.

Sklar, L. (1992). *Philosophy of Physics.* Oxford: Oxford University Press.

Smart, J.J.C. (1959). Sensations and brain processes. *Philosophical Review* LXVIII. Reprinted in and referenced from Rosenthal, ed. (1971) *op. cit.*

Smart, J.J.C. (1980). Time and becoming, in van Inwagen ed. *op. cit.* pp 3-15.

Smart, J.J.C. (1994). Mind and brain, in Warner and Szuba eds. *op.cit.* pp19-23.

Smith, P. (1989). Consciousness: a functionalist approach. *Cogito* Spring: 20-25.

Sokolowski, R. (1964). *The Formation of Husserl's Concept of Constitution.* The Hague: Martinus Nijhoff.

Sosa, E. (1984). Mind-body interaction and supervenient causation. *Midwest Journal of Philosophy* 9: 271-281.

Speziali, P. ed. (1972). *Albert Einstein and Michele Besso, Correspondence 1903-1955.* Paris: Hermann.

Spinoza, B. de (1969-75). *Ethics, Treatise on the Emendation of the Intellect and Selected Letters.* Translated by Samuel Shirley. Indianapolis: Hackett.

Sterelny, K. (1990). *The Representational Theory of Mind. An Introduction.* Oxford: Blackwell.

Stevens, S.S. ed. (1951). *Handbook of Experimental Psychology.* New York: Wiley.

Stewart, I. (1993). A new order. *New Scientist* 137: s2(2).

Stich, S.P. (1978). Autonomous psychology and the belief-desire thesis. *The Monist* 61: 573-591.

Stich, S.P. (1983). *From Folk Psychology to Cognitive Science. The Case against Belief.* Cambridge, Mass.: MIT Press.

Stich, S.P. (1989). *The Fragmentation of Reason.* Cambridge, Mass.: MIT Press.

Stich, S.P. and T.A. Warfield, eds. (1994). *Mental Representation. A Reader.* Oxford: Blackwell.

Stöckler, M. (1991). Reductionism and the new theories of self-organization, in Schwurz and Dorn eds. (1991) *op. cit.*

Strawson, G. (1994). The experiential and the non-experiential, in Warner and Szubka eds. (1994) *op. cit.*

Suppe, F. (1974). *The Structure of Scientific Theories*. Chicago: University of Illinios Press.

Taylor, C. (1970). Mind-body identity: a side issue? in Borst, ed. (1970) *op. cit.*

Teller, P. (1983). A poor man's guide to supervenience. *Southern Journal of Philosophy*, supplement 22: 137-62.

Teller, P. (1986).Relational holism and quantum mechanics. *British Journal for the Philosophy of Science* 37: 71-81.

Thompson, E. (1967). Time, work-discipline and industrial capitalism. *Past and Present* 36: 57-97.

Thompson, W. (1870). The size of atoms. *Nature* 1: 551.

Thrift, N. (1990). The making of a capitalist time-consciousness, in Hassard (1990) *op. cit.*

Toulmin, S. (1961). *Foresight and Understanding*. New York: Harper.

Treisman, M., A. Faulkner, P.L. Naish and D. Brogan (1990). The internal clock: evidence for a temporal oscillator underlying time perception with some estimates of its characteristic frequency. *Perception* 19:705-743.

Turton, D. and C. Ruggles (1978). Agreeing to disagree: the measurement of duration in a southwestern Ethiopian community. *Current Anthropology* 19: 585-600.

Valentin, G. (1853). *A Textbook of Physiology*. London: Harry Renshaw.

van Cleve, J. (1990). Supervenience and closure. *Philosophical Studies* 58: 225-238.

Van Fraassen, B.C. (1980). *The Scientific Image*. Oxford: Oxford University Press.

Van Fraasen, B.C. (1970). An Introduction to the Philosophy of Time and Space. New York: Random House.

van Inwagen, P. ed. (1980). *Time and Cause.* Dordrecht: Reidel.

†Virilio, P. (1984). *Lost Dimension.* Translated by D. Moschenberg. New York: Semiotext(e). [1991]

von Neumann, J. (1955). *Mathematical Foundations of Quantum Mechanics.* Oxford: Oxford University Press.

Walker, S. (1983). *Animal Thought.* London: Routledge.

Warner, R. (1994). Introduction: the mind-body debate, in Warner and Szuba, eds. (1994) *op. cit.*

Warner, R. and T. Szuba, eds. (1994) *The Mind-Body Problem. A Guide to the Current Debate.* Oxford: Blackwell.

Weinberg, S. (1992). *Dreams of a Final Theory.* New York: Pantheon.

Wenzel, B.M. (1954). Problem in odour research from the viewpoint of a psychologist. *Ann. N.Y. Acad. Sci.* 58:58-61.

Whitrow, G.J. (1972). *What is Time?* London: Thames and Hudson.

Whitrow, G.J. (1980). *The Natural Philosophy of Time. Second Edition.* Oxford: Oxford University Press.

Whitrow, G.J. (1988). *Time in History. Views of Time from Prehistory to the Present Day.* Oxford: Oxford University Press.

Wilkes, K. (1978). *Physicalism.* London: Routledge.

Wimsatt, W. (1976). Reductive explanation: A functional account, in *PSA 1974*. Edited by R.S. Cohen, C.A. Hooker, A.C. Michalos, and J.W. van Evra.

Wood, D.C. (1989). *The Deconstruction of Time*. Atlantic Highlands, NJ.: Humanities Press International, Inc.

Wright, R.H. (1976). Odour and molecular vibration: a possible membrane interaction mechanism. *Chemical Senses and Flavour* 2:203-206.

Wright, R.H. (1981). Odour and molécular vibration: redundancy in the olfactory code, in Moskowitz and Warren, eds. (1981) *op. cit.*

Wright, R.H., and K.W. Mitchels (1964). Evaluation of far infra-red relations to odour by a standards similiarity method. Ann. N.Y. Acad. Sci. 116:535-51.

Zahar, E. (1989). *Einstein's Revolution. A Study in Heuristic.* La Salle, Illinois: Open Court.

Zermelo, E. (1896a). On a theorem of dynamics and the mechanical theory of heat, in Brush, ed. (1966) *op. cit.*

Zermelo, E. (1896b). On the mechanical explanation of irreversible processes, in Brush, ed. (1966) *op. cit.*

Zwaardemaker, H. (1895). *Die Physiologie des Geruchs*. Leipzig: Engelmann.

Zwart, P.J. (1976). *About Time. A Philosophical Enquiry into the Origins and Nature of Time.* New York: Elsevier.

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