REALITY, RELEVANCE AND REASON

REALITY, RELEVANCE AND REASON:
A DEFENCE OF SCIENTIFIC REALISM ABOUT MICRO-ENTITIES

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ABSTRACT

The thesis develops and argues for a version of realism about micro entities postulated by scientific theories. It begins with analyses of the concepts of reality, observation, interaction, causality and reasonableness, which are crucial in the modern realism debate. In this way, it is found that, within the debate, a move from the acceptance of the causal role of an entity to the acceptance of its reality is legitimated. Hence "good reasons" for accepting the reality of the entity are those that indicate that its causal relevance to some other objects has been conceptually and experimentally identified. The acceptance is reasonable in the sense that it both is the best choice and is relative, open and critical. Given this recognition, the constituents of "good reasons" for identifying a postulated entity's causal relevance are sought. Such logical attempts as hypothetico-deductivism, Glymour's bootstrapping theory and Sylvan's programme for relevance logics of cause are demonstrated to be incapable of identifying the causal relevance. Hence the "experimental approach" is preferred and developed. An account of good reasons based on a cause identification condition, which consists of a model constituent and an experiment constituent, is proposed, clarified and defended by case studies of models and experiments in elementary particle physics and molecular biology. These experiments are shown for the first time to have a special tree-structure, which reflects a synthesis of their self-checking and -correcting mechanisms and which makes them manipulations of the postulated entity. It is argued that through the tree structure scientists' belief in the reality of DNA molecules is rooted in the nature of the technology used to manipulate them. Finally, with a comparative case study of the neutrino and the ether, the challenge by Laudan (1981) for realism is met and the adequacy of the account of good reasons is further proved. The thesis has philosophically accounted for the reasonableness of scientific belief in the reality of the micro-entity .

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CONTENTS

Chapter 1 Introduction

Nearly a decade ago, Fine declared: "Realism is dead." According to him, the death had already been announced by the neopositivists who realized that they could declare the questions raised by the existence claims of realism as mere pseudoquestions without conflicting with any result of science. It was hastened by Bohr's victory over Einstein in the debates over the interpretation of quantum theory, and certified by the fact that the last two generations of physical scientists could do science successfully without realism. So any philosophical effort to "pump up the ghostly shell and to give it new life" will be eventually regarded "as the first stage in the process of mourning" (Fine, 1984, p. 83).

In spite of this, however, the realism debate, one of the currently central issues in the philosophy of science, shows no sign of stopping. The ghost appears to argue that its death announcement is premature. Indeed, if realism is a philosophical issue, history tells us that philosophical issues hardly die, even though their forms may vary. If it is an empirical or scientific issue, there has been no lack of announcements about its living in science, some of them are more specific and seemingly truer than Fine's (see, for example, Albright, 1982, p. 150). It has been generally realized that in such branches of science as elementary particle physics or molecular biology, the issue is not whether most of the scientists are realists, but why they are or whether this is philosophically "reasonable." If the realism debate is a "mourning process," the mourning might last as long as we human beings do. As Wartofsky explains, we all know that when the Greeks shoot arrows from bows, arrows can fly and are flying. What Zeno's paradox shows is just that "something must be wrong with our 'rational' account, with the language in which motion through space and time is described, if it yields a flightless arrow" (1991,

p. 39).

So it appears that it is still not too out of fashion to talk about realism today. In this thesis, I join the debate and discuss a certain form of realism about theoretical entities. Scientific realism of the kind held by Putnam, Boyd, and others, has two main principles, which divide realism into two subgroups:

Entity realism: The observational and theoretical terms within the theories of a mature science genuinely refer;

Truth realism: Scientific theories belonging to a mature science are typically approximately true.

Entity realism is logically independent of truth realism, as many people recognize (see, for example, Devitt, 1984, Hacking, 1983): we can be realists only about entities, and argue for entity realism without arguing for or depending on truth realism. I take this for granted in this thesis, and I argue solely for a form of entity realism.

The quarrel between scientific realists and anti-realists, up to now, has been in four main related fields: (1) the issue of reference of theoretical terms; (2) the function of realism in science; (3) historical and scientific assessment; and (4) the problems with distinguishing theory from observation.

The reference issue came especially from this fact: the meanings of a term, say "electron", in competing and successive theories are so different that we can hardly say that the term in different theories refers to the same thing. Among others, early Putnam and Kripke developed a theory of meaning to meet this challenge. This theory holds that what determines the reference of a word is a historical process of naming that establishes a connection between a natural kind of thing and its theory, which, in Putnam's opinion, need not be a correct one. Bohr's theory of the electron, for example, is not a strictly correct description of the properties of electrons, but it did successfully carry on a process of naming the particle. Theories of the electron changed a lot in their short history, but all of them, on Putnam's theory--for instance, those of Rutherford, Bohr and Schrodinger--were certainly talking about the same thing which is the stable extension of the term "electron" (Putnam, 1979, Kripke, 1972).

There was much discussion concerning the suitability of this theory, but now the issue of reference is less important in the realism debate. Many people, like Hacking, Rom Harre, W. Salmon, and van Fraassen, believe that philosophical studies of logic and language have very little place in the philosophy of science. Hacking, for instance, rejects the manner of the arguments in the semantic realism/anti-realism debates on ground that the debates are empty, and will typically lead to a confused or wrong conclusion since they pay so little attention to the details of a science (Hacking, 1983, p. 90).

The necessity of realism in science is very doubtful to anti-realists. They pose two questions: (a) Is the postulation of theoretical entities really necessary in science? (b) Is a realistic interpretation of theoretical entities really necessary? Some older anti-realists, like some idealists, firmly say no to both. For instrumentalists, theoretical terms are only useful intellectual fictions, or mathematical tools. Some people even deny that scientists have ever intended to explain phenomena. Positivists believe that, like other metaphysical nonsense in science, the statements involving theoretical terms could be replaced by logically equivalent statements with only observational terms, although no such logical or linguistic reductionism, like Ramsey's, has worked so far.

Boyd and McMullin argue that the realist way of thinking has a methodological role in science (McMullin, 1984, pp. 31-34, Boyd, 1990). But Fine argues to the contrary that realism has actually blocked the road of scientific progress. He claims that a nonrealist attitude has led to many important developments in relativistic physics and quantum theory, while the realist attitude has made little progress in science, as shown in the later Einstein (Fine, 1984, pp. 91-95).

Fine's argument is neither sound nor consistent with the intuition of most modern scientists. Since there are so many factors responsible for scientific progress, it is hardly reasonable to blame the realist attitude of a scientist for his failure to make a great success on this or that issue. Scientists' realistic attitude does not fundamentally lie in their specific concepts of what the world is like, such as Einstein's idea of determinism, but in the necessity and rationality of such tendencies and behaviours as postulating a theoretical entity to explain phenomena in the laboratory. At least in some branches of the physical sciences, scientists need to explain by postulating theoretical entities. Even though it might be reasonable to say that these scientists are philosophically naive, believing in realism about their entities, this does not lead to saying that they don't have the belief, nor can we say that the belief can be removed from science without any serious effects.

Realists think that historical and scientific practices offer support for realism. There is the so-called "no miracles argument": namely, it would be a great miracle if a theory had a good explanatory function and made many novel and correct empirical predictions without what the theory says about the fundamental structure of the world being correct. So realism is reasonable because it can best explain the success of science.

Yet antirealists don't accept the validity of this "inference to the best explanation," or Peircian abduction. Besides, the argument begs question for them. Laudan argues that in the history of science, successful theories had non-referring terms, like caloric, phlogiston and, specially, ether. Conversely, theories with referring terms, like atom, were not, originally, successful. A "pessimistic induction" follows from this kind of explanation of the history of science: given those historical cases, who can guarantee that the present existence claims about, say, the electron, will not be thrown away by some new scientific discoveries someday?

There have been arguments against Laudan. Dilworth holds that the theories Laudan refers to are today generally considered to be incorrect with regard to the ontologies they suggest, not because this (realist) way of thinking is itself mistaken, but on the contrary, because this way of thinking has led to revision of judgement. It is largely because technological advance has shown those particular ontologies not to exist (Dilworth, 1990, p. 456). The problem here is, of course, how to know what in science could reliably be an indicator of the existence of an entity. I will argue in this thesis that Laudan's explanation of the history is doubtful; many of his counter-examples can be proven not to work if we have a suitable concept of "empirical success", one which means that the causal relevance of the entity in question can be located and controlled.

The problem of how to find the causal connection between certain observable evidence and an unobservable entity is inevitably connected with the issue of distinguishing theory from observation. The doctrine of the underdeterminism of theories by their evidence tells us that theories with different ontologies can be empirically equivalent or evidentially indistinguishable. This is another argument for the age-old idea that our empirical knowledge cannot extend to "unobservables."

Shapere argues that the traditional theory-observation distinction is not suitable (1984 pp. 366-76). Indeed the problem of realism largely or eventually depends on how we conceive the line between "theory" and "observation." It is an open question whether the old, rigid, and unchangeable style of defining the concept of observation in terms of a human being's direct sensations does fit the practice of modern sciences. At least, I think, it is a fact that the development of modern physics makes it harder to define the concept of observability clearly, consistently and nonarbitrarily. If an anti-realist like van Fraassen takes the moons of Jupiter to be observable through a telescope, but takes genes or chromosomes to be unobservable with a microscope, he needs to offer a more fundamental and convincing reason than saying that that is because only in the former case could a human being (say an astronaut) ever be in a position, in principle, to see the objects in question directly (van Fraassen, 1980, p. 16). My question is: if one accepts that the causal connection between us and the objects *via* the optical instrument in the former case is real, why not in the latter case, if we can show a similar causality?

Hence, as many realists now realize, the problems of realism are eventually related to our concepts of causality, observation, experience and physical interaction (see, for example, Boyd, 1990). To show the existence of an object, micro or macro, the strongest evidence is its empirical interaction with other objects and us. To show the existence of the interaction, we need both to "know" it and to "do" it. Cartwright says: "One important thing we sometimes want to do is to lay out the causal processes which brings the phenomena about, and for this purpose it is best to use a model that treats the causally relevant factors as realistically as possible . . . But this may well preclude treating other factors realistically" (Cartwright, 1983, p. 152). Hacking holds that the strongest proof of the existence of electrons consists in the fact that electrons are tools used for creating phenomena in some other domain of nature (Hacking, 1983). The problem here is in what sense do we "manipulate" electrons? Dilworth argues: "But experimenters do not manipulate electrons--what they manipulate are pieces of experimental apparatus. They assume such manipulations to have an effect on a trans-empirical reality--so they are themselves realists: but nothing in Hacking's argument supports a stronger conclusion than this" (Dilworth, 1990). This is an issue whether there is really a physical relation between our manipulation of the apparatus and the entities. Eventually, therefore, the core issue is to show what can indicate the existence of physically causal processes across the border of observability, wherever that border might be said to be.

So my task is to address the issue of how to decide when a causal relevance between an entity and some other physical objects has been really found. Following Hacking's experimental approach (1983), I will try to find the conditions for the causal relevance along two lines, a descriptive model of the relevance, standing for "knowing," and an experiment of materializing relevance, standing for "doing." Such a model is needed because of the fact that in science an entity assumption will not be taken seriously, let alone be confirmed, unless it is a plausible (mature) description, namely, a model, of some causal property of the entity and of how the property plays a role in a causal relation resulting in certain observable effects. Also, such a model is needed because, I argue, taken together with experimental confirmation, it affords the strongest evidence for an entity.

I argue that the conditions thus found for deciding causal relevance constitute a good reason for, or the reasonableness of, entity realism. "Good reason" has a three-fold meaning: (1) it is strongest; (2) it is also relative, open, restricted, critical and moderate; and (3) it is essentially similar to what we use in deciding the existence of macro objects. So this is the form of realism I will support: *realism about a micro entity in question is reasonable if it meets the conditions for the existence of a desired causal relevance, since the reason it thus gains is relative and temporary on the one hand, but also, on the other hand, it is the strongest that a knowledge claim can possibly obtain even in cases of macro objects.*

I will begin by discussing the concepts of reality, observation, physical interaction, and causality in the case of micro entities - the part of theoretical entities I focus on. In this way I try to find a common place in the realism debate that both realists and antirealists stand on. Then from this shared position, I seek what approach to the study of the existence problem could get the greatest agreement among them. I shall argue that the model-experimental approach, developed from Hacking's approach, is the best choice. And then I shall draw a schema of what the conditions for deciding the existence of some required causally interactive process would be like. This is what I shall do in chapters 2 and 3.

In chapter 4, I argue further for my choice of a practical approach by showing some fatal weaknesses in formal or logical ways of dealing with the problem of evidential relevance.

In chapters 5, and 6, I shall turn to scientific practices to enrich, clarify and support the conditions given in chapter 3. In chapter 7, I shall discuss a special structure of experiments and the nature of technology to reveal the ultimate foundation for the conditions, for reasonableness and thus for entity realism.

And, finally, in chapter 8, by comparative case-studies of the neutrino and the ether, I argue that my criteria for a good reason (my notion of success for entity realism) not only fit modern scientific practice, but also meet no objection in history: Laudan's ether case could not defeat a realism which has met the criteria. Such a realism is a reasonable position in the three-fold sense mentioned.

Chapter 2 Scientific Reality

In this chapter I examine some main features of the concept "physical reality" which is at the core of the realism debate. By this some distinctive presuppositions of the debate will be shown, which, as a result, also become preconditions for the studies in this thesis.

2.1 Meanings of Reality

The concept "physical reality" is crucial to the realism debate. Most scientific realists agree that "physical reality" means "a real object existing independently of our mind" (see, for example, J. Leplin (ed.) 1984). Yet a clarification of this interpretation is needed: what are the meanings of "real" and "independent," for instance?

Russell analyzes the meaning of reality this way: "A thing is real if it persists at a time when it is not perceived; or again, a thing is real when it is correlated with other things in a way which experience has led us to expect" (Russell, 1963, p. 91). As for "independence," Russell relates it to causation: "*A* is independent of *B* when *B* is not an indispensable part of the *cause* of *A*" (p. 91). Of course, as a result of his empiricist position concerning causation, Russell argues that independence thus interpreted is still indefinite and ambiguous(p. 92).

Russell's interpretation is not completely adequate for characterizing "physical reality." The second part, specifically, of the interpretation of reality is ambiguous. What it refers to depends on what we mean by "correlated" and "other things." If the correlation between a real object and "other things" is not specified, it would not entail to what category the "real" object belongs: there can be correlation between mind and its productions as "other things," belonging to Popper's "third world." Mind, for example, could persist even when "it is not perceived," and it can have a correlation with other things "which experience has led us to expect." If Russell's correlation means physical interaction and if the "other things" refers to physical objects, then in the case of theoretical entities, it appears that Russell would have been talking about a kind of relation which he, as an empiricist, does not accept. For it would be a kind of physical relation among unobservables, which is a key reason for empiricists to reject the concept of causality. The paradox is that in the case of theoretical entities, if we define reality by its relation to our experience of it, as Russell attempts to do, and if we avoid taking the relation as a physical one, we might not be able to show what kind of object we are referring to; if, on the other hand, we take the relation as physically interactive, then Hume's scepticism concerning causation would be undermined.

It seems to many people that "physical reality" means, first of all, an independent existence. The meaning of independence can be explained either as Russell's non-human-causal-product or as his persisting existence. That is to say, an independent reality in this physical world can be explained as persisting existence or as non-human-product or both. But, "independence" thus interpreted is not enough for characterizing physical reality. For "something real" is not necessarily something persisting, as is shown by our own sensations; and something persisting is not always physical, like the contents of Popper's world 3. This indicates that the reality of physical objects should be understood both by their independence and by their physical nature which is represented by their interactions with other physical objects. A physical reality is one that exists independently and is capable of interacting with other physical objects.

"Physical interaction" between two things here stands for the processes in which both things could affect each other by the transfer or exchange of energy and information[1]. This is a doctrine on which many contemporary realists rely. Popper put it this way: "entities which we conjecture to be real should be able to exert a causal

[1] Clearly this stipulation is basically the same as Salmon's definition of causal interaction, which I adopt in this thesis. See the last section of this chapter.

effect upon the *prima* real things; that is, upon material things of an ordinary size: ...we can explain changes in the ordinary material world of things by the causal effects of entities conjectured to be real" (Popper and Eccles 1977 p. 9). (Hacking favourably quotes this passage in his (1983) p. 146.) Shapere stipulates that to say "A exists" means that, first of all, A "can interact with other things as well as us" (Shapere, 1984, p. 229).

Thus it seems to me that experience, as a kind of interaction, is not always necessary for explaining the "realness" of a physical object. The independence thesis entails that the existence of an object does not depend on our perception of it. What we experience now probably existed before, and, might exist after, our short-lived existence on the earth. It follows that in the world there probably is something that we have not experienced yet. In this sense, for stipulating the externality of physical objects we do not need experiencability.

There is a conflict, therefore, between the independence and the interaction. If accepting the existence of a physical object entails that it will interact with another physical object, and since all kinds of physical interactions that we have known so far can be transferred in one way or another to certain interactions of the forms sensible to us, we should allow for the possibility of experiencing it, instead of saying that there may be something we could never experience. It seems to me that the solution to this conflict is to take the further inference to unexperiencable existence as unnecessary for realism about unobservables. Most scientific realists today are only interested in arguing for the realness of *some* theoretical entities which they claim have been experienced - usually manipulated one way or another - instead of the realness of something we can not experience. That is, most of us would argue that interaction logically entails existence, but might be reluctant to assert the reverse. Hence, to be safe, we might say that physical interaction is a sufficient condition for being a physical reality, even though we don't claim that it is a necessary one. Now the two theses characterizing physical reality can be stated as follows:

(1) Independence: the reality of an entity means that it exists independently of our perception and mind; this independence implies that (1.a) it might persist when we do not perceive it and (1.b) though our sensation of it may be the result of our consciousness

or mind, they are not the cause of the existence of the entity; and

(2) Interaction: anything that is capable of physically interacting with some other physical entity including observable ones and human beings exists in the sense of (1).

The two doctrines entail some epistemic outcomes, such as the conjectural nature of theories (Popper, 1982, p. 102, and Shapere 1984 pp. 229-33).

Some of the distinct features of the above characterization of physical reality will emerge out of the realism debate about theoretical entities, compared with a traditionally empiricist understanding of reality and experience. In what follows, I shall begin by considering this empiricist understanding in this regard.

2.2 Three Ways to be Unobservable

Historically, people have been more concerned with problems about macro "observable" objects like rocks or tables or the whole world, rather than those of unobservables such as electrons or DNA molecules. Yet a common basis for people such as traditional empiricists, when deciding upon the existence of a table, was that we have to take it as an "unobservable," and only when we could accept its existence as an unobservable, could we accept its existence as observable.

For Hume, first of all, the assertion of the existence of a macro-object, like a table, must involve an assertion that it persists when we do not see it - an assertion about its existence when it is outside the focus of human perception. Thus the table between two perceptions of it is an unobservable. So the existential assertion contains an acceptance of the object both as an observable and as an unobservable - an acceptance of "the fiction of a continu'd existence" (Hume, p. 205, see Popper, 1983, p. 88). Hence such an existential claim is an inductive conclusion based on limited individual observations. As is shown by the problem of induction, although we might admit the "existence" of the table when seeing it, we have no objective reason to claim that it is impossible that the table not exist when we turn around and do not see it. Thus individual sense-perceptions, no matter how many they are, cannot establish the existence of an observable object.

To Berkeley, "independent" means not only persisting between perceptions but also perception-transcendent. For him, to assert the independent existence of a table is equal to asserting that there is an entity, "a table," existing behind and causing the related sensations or phenomena. Yet what an individual observation has access to is no more than its own products - sensations. All of our ideas about the things in this "external world" are constructions of our sensations and mind - thus "reality" has its origin within our mind. Without the activity of our mind there is no world of objects at all; so a reality which is beyond human sensation and consciousness is impossible.

Along similar lines, Mach's neutral monism claims that the only real things are our sensations; we have sensations of such properties as colour, smell, sound, etc.; and material bodies are no more than constructions - by these sensations - of properties. Another neutral monist, James, used the term "independent reality" occasionally in his pragmatist ("radical empiricist") philosophy. The term for James, however, just refers to sensations, part of experience before our minds act more intentionally on it. Sensations are, according to him, the first part of the developing chain of experience; so this so-called "independent reality" means just independent of another part of experience, i.e, mind or self. He claims: "when we talk of reality 'independent' of human thinking, then, it seems a thing hard to find. It reduces to the notion of what is just entering into experience and yet to be named, or else to some imagined /aboriginal presence in experience, before any belief about the presence had arisen" (James, 1967, p. 453).

In short, determining the independent existence of macro objects requires a decision about their existence as unobservables. For Hume, they are unobservables because we could not see them when we are not seeing them; for Berkeley or Mach, they are unobservables because all we can perceive when we are seeing are our own sensations, and we can never see a "reality" behind them. So the existence of matter or whatever - as external cause of the sensations - must be an unjustified and unnecessary construction.

Surely these empiricist or idealist positions apply to any kind of objects, macro or micro. However, for the existence of micro entities, like atoms, the empiricists could argue that besides the argument concerning macro objects as unobservables, micro entities have an additional problem: we have no sensation or experience of them at all - they are unobservable because we cannot have sensation related to them in any way.

In this century, the sceptical and idealist traditions are followed by some people, like some linguistic philosophers, some philosophers of science and even some scientists.

The realism debate in the philosophy of language is about both micro and macro objects. The Humean way of understanding physical reality prevails with some people, especially those in Dummett's camp, such as Luntley.

In his (1988), Luntley divides usage of the term "realism" into two kinds. According to him, sometimes people use the term in association with such claims as, say, there are electrons (or there really are electrons); there are pains; there are libidinal forces; etc. This is a usage of realism for "ontic commitment," which Luntley embraces heartily. Another reading of the term is the view that there are things, of whatever kind you like, which exist independently of the possibility of our knowledge of them. The core concept in the second kind of usage is the independence thesis, which Luntley does not accept.

Luntley argues that distinguishing between the two kinds of realism is crucial to his anti-realism which, he claims, can offer as good an account of the objectivity and activity of science as the realist's. He claims that he has developed a theory of perception capable of asserting the existence of direct observation of such unobservables as electrons or α-particles when we, as trained observers with necessary equipment, observe them. This theory of perception tells us that we indeed can obtain, by training, non-inferential perceptual capacities or skills by which we can directly observe theoretical entities. What Luntley refers to by these non-inferential perceptual capacities or skills is actually something like the trained skills by which we can identify a vapour trail as that of an α-particle with a certain angle of deflection and velocity from other trails or phenomena in a cloud chamber. This recognition of the trail is triggered by an activation of a perception with only "a lower level of informational content" (p. 237). This perceptual skill is acquired by learning. And the perception is often carried out quite simply and immediately, so the recognition of the trail is a direct (non-inferential) observation of α-

particles.

Hence, when we are making this kind of observation of an α-particle at time t_1, it exists in the "ontic" sense. But, at time t_2, when we are not making such a "direct observation" of the particle, either because we are not skilful scientists or because we do not have the required equipment, this particle become a "recognition-transcendent reality," which means that we are not permitted to say that there is α-particle at this time. Luntley claims that "I am not denying that terms like 'electron' refer. Rather..., I am questioning the intelligibility of assuming that they refer to a determinate independent reality"(P. 245). It is obvious that Luntley's interpretation of the independence thesis as the core of realism for both macro and micro entities is Humean - continued existence which involves an unobservable state as "interpolation" between two observable states. He rejects the thesis by arguments which are essentially Humean.

A similar interpretation can be found among philosophers of science. Let us take a look at Reichenbach's view. Although Reichenbach claims that his analysis of quantum physics is philosophically neutral, he is in fact a follower of Hume, as Popper, for example, has indicated (Popper, 1983, pp. 123-126). Reichenbach makes an important distinction between observed phenomena and interpolations (he calls it "interphenomena") between these phenomena. A tree is observable when I am looking as it; but the same tree is unobservable, or, interphenomenal, when I close my eyes and do not see it. If we assert the existence of the same tree when we are not looking at it, "we interpolate an unobserved object between observables; and we select the interpolated object in such a way that it allows us to carry through the ... *postulate of identical causality* for observed and unobserved objects..." (Reichenbach, 1948, p. 341, as quoted by Popper in his 1983, p. 124). He declares that the postulate as *normal system* is just for simplifying our language.

That is to say, when we say a tree exists, we actually invent an unobserved tree between two instances of seeing the tree to keep the tree as a continued existence. What the unobserved object is like is our selection and the selection, according to Reichenbach, is eventually free. We invent the unobservable tree as the same as the observed tree - instead of a tree with a different character or a rock, etc. - not because other selections are false, but because they are awkward.

In quantum physics, Reichenbach says, we could not maintain such a postulate of identical causality for observed and unobserved objects - it is "impossible to give a definition of interphenomena in such a way that the postulates of causality are satisfied" (Reichenbach, 1944, p. 32f). Hence in this case we have "the principle of causal anomaly."

My final example of modern traditional empiricists is Henry Margenau, a physicist-philosopher. In his (1977) and (1984), Margenau attempts to synthesize science, philosophy and religion on the ground of "a form of idealism" (1984, p. 1). He holds that since all our knowledge arises within consciousness - our own or someone else's - an inevitable conclusion is that the so called independent world is no more than a creation of human mind.

This is Margenau's interpretation of the process in which a person's mind proceeds from sensations as the most *primary* experience, named "P-plain" experience by him, to the conclusions about the features of the external world. First, I have sensations of colour, shape of a "table," for example. I also see repeatedly that the colour is associated with the shape. And in my mind these experiences repeat themselves on similar occasions. "My tendency therefore is to assume some sort of permanence that has fixed itself on my mind" (1984, p. 49). In a similar process I experience my body and other bodies and I find that they have the same mental experience as I.

"The miracles," says Margenau, "of repeatability and individual agreement between different minds induce me to ascribe my sensations to permanent external bodies: I have taken the first step into the external world. This passage, which starts 'at P' (that is, is initiated by a number of primary experiences), may be termed *reification*, making a thing out of a set of sensations. Obviously, it there were no mind, there could not be reification. In the strictest sense, therefore, mind creates the external world" (p. 50).

Based on this, Margenau sketches the method of empirical science. Science does not directly involve P-experience or P-facts. Rather, science converts each P-fact "into an idea that *corresponds* to it" (p. 52). As to the sense of heat in my fingertips, for example, the corresponding idea is a measured quantity on a thermometer. "The two - P-fact and measured quantity - are related by what I have called *rules of correspondence*" (p. 52). These rules of correspondence were called "epistemic correlations" by Northrop, or "operational definitions" by Bridgman. According to Margenau, the measured quantities are constructs or concepts, C's; they are connected to sensations by this operational definition: they denote properties or observables; and so they are close to P-experience. Some C's, like temperature and time, have very simple operational definitions, they lie close to P-experience. Some C's have more complicated definitions, which are more difficult to visualize and "may be said to lie at a greater distance from the P-plane" (p. 57). "In constructing the external world several such C's, such properties, are assigned to a body, or a particle, or some other more abstract or elaborate system, which is the carrier of C's. An atom or a light beam or three-dimensional space might be such a system" (p. 55). These systems, the carriers of observables, are also constructs in science, the constructs which are not connected by operational definitions (rules of correspondence) to P-facts, but are connected by logical relations or else to C's which can be defined operationally.

To be clear: for Margenau, external things are not experienceable; rather, they are constructs of sensations. A macro object, such as a desk, is a construct which is obtained by the process of reification, and so is connected to the P-experience by the rule of correspondence. In the case of the desk, each of my numerous sensory qualities, like a brown colour, a certain shape, and a solid substance, "is capable of an operational definition" (p. 58); but usually "I do not trouble to invoke these definitions" (p. 58). The repeatability of the qualities every time I look in the same direction leads me to pass, "for the most part unwittingly, from the recurring complex of sensations to a single construct, desk, which I endow with permanence, with presence, even when I am not looking at it..." (p. 58).

Margenau clearly thinks this reification is also the case with such theoretical entities as molecule, atom or electron. According to him, these entities are constructs of quantities. But the quantities themselves, nevertheless, are definable operationally. Like the quantities of all macro objects, they correspond to certain sensations; so we can reasonably say that their quantities are observable qualities. Let me quote him:

> Some constructs in every science are not operationally definable in a direct way .... To name a few, a body in general, an electron, a positron, or in fact any object as distinct from a property is a scientific entity that does not acquire its meaning from an operational definition alone. Its "properties," such as the charge of an electron or its mass, can be defined operationally, but in order to define the electron itself, it will ultimately be necessary for us to say: the electron is that "entity"(*on*) which possesses such and such operationally defined quantities. The carrier of the quantities - the entity specified in terms of its observable, operationally definable quantities - will be called a *system*. Thus, in general, systems have quantities, that is, observables define them constitutively; the quantities themselves, however, are always capable of operational definitions (p. 60).

This is his "review of the methodology of science, of what might be called the Human creation of the external world" (p. 62). Being a scientist, he asserts that "properties," such as the charge of an electron or its mass are observable; being a philosopher, however, he asserts that the electron itself is just a human-creature. No doubt he is a follower of Berkeley or Mach, with some Vienna Circle tint.

Before I discuss these new Humean, Berkeleyan or Machian views, I would like to talk about a remarkable change in the scientific realism debate over the last thirty years.

2.3 Problem-Shifts: A New Empiricism

Since the middle of this century, the realism debate has become a central topic in the philosophy of science. As I mentioned in chapter one, we have seen that the objects, arguments, standards and even language involved in this debate are quite distinctive. People in this debate are fighting in these four overlapping fields: the reference of theoretical terms; scientific, methodological and historical bases for realism; the roles of realism in science; and differences and similarities between unobservables and observables. We can hardly find in the debate any of the approaches, arguments and standards of traditional and modern idealists from Berkeley, Mach to James, or Dummett. This is partly because the debate contains or is based on two remarkable deviations from all other realism debates in history. One is associated with a change in our view of the nature of knowledge and reasonableness, and the other is with the change in the way we understand and discuss the problem of reality.

I shall talk about the first deviation before launching into a detailed discussion of the second.

In this century, scientific knowledge, in the form of theories, laws and observational statements, is no longer thought of as absolute, infallible or unchangeable truth. People realize that nothing in science is immune to revision or rejection in the light of new scientific discoveries. The ideal of knowledge with certainty is now widely regarded as impossible. As Popper urged, historical lessons ask us to take even the best theories we have now only as best conjectures.

Keeping this recognition in mind, however, contemporary realists are still struggling to hold that some beliefs, like that of an independent world, are "reasonable" in a more than purely psychological sense. For Boyd, Hacking, McMullin, Sellars, Shapere, Giere and many others, a "reasonable belief" no longer means a belief with certainty. Rather, any belief, including that of an independent world, is subject to empirical tests and could be abandoned if enough negative evidence occurs someday. The positive argument for belief is, the realists argue, that until now we do not have such negative evidence at all. This view is formed with the help of Popper and is highlighted by Shapere.

Popper, of course, rejects "essentialism" or foundationalism about the truth of any theory, however good it is. On the one hand, accepting, adopting, preferring, or believing in[1] a theory does not mean that the theory is justified to be true in the essentialist sense (see, for example, Popper, 1983, p. 79). On the other hand, Popper does believe that adopting a well-corroborated theory is reasonable. He clearly declared that "it is reasonable to act upon (and thus to believe in) a thoroughly discussed and well

[1] Popper used all these words interchangeably to refer to the same attitude toward the conjectural nature of theories. See his (1983), pp. 22-33, pp. 58-62, pp. 77-80, p. 125, p. 129, and p. 134.

tested scientific theory, provided we are ready to change our mind in the light of new arguments; of new empirical evidence, for example" (p. 79)[1]. When arguing for his "metaphysical realism," he said: "According to my approach, it is reasonable to accept the views of common sense as long as they stand up to criticism: science arises from criticism *and* common sense *and* imagination" (p. 129, italics original). He indicated what he meant by its being "reasonable" to believe in or prefer a theory:

> This persuasion, this belief, this preference, is reasonable because it is based upon the result of the present state of the critical discussion; and a preference for a theory may be called 'reasonable' if it is arguable, and if it withstands *searching critical argument* - ingenious attempts to show that it is not true, or not nearer to the truth than its competitors. Indeed, *this is the best sense of 'reasonable' known to me* (p. 59, italics original)

Also, he said, "In fact, I have suggested what distinguishes the attitude of rationality is simply openness to criticism" (p. 29)[2]. In this spirit, Popper gives three kinds of reasons for accepting realism. The first one is that realism follows naturally from his view of the conjectural nature of knowledge and his methodology of science (also Shapere, 1984, pp. 229-233). If, Popper declares, we recognize that knowledge is essentially hypothetical and the key to obtaining such knowledge is not how you get it, but how you test it and try to improve it by testing, then the conflict that bothers such great philosophers as Hume and Russell between the common sense that the world does exist and the idealist position as a result of the philosophical "inferences from experience to the world of physics" (Russell, 1944, p. 16, quoted by Popper, 1983, p. 87) will be avoided. In contrast, this recognition is "exactly what we should expect," says Popper, "if realism is true - if the world around us is, more or less, as common sense, refined by adjusting ourselves to our environment, then our knowledge can be only the trial-and-

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[1] It should be clear that Popper thinks, as put by Watkins, "accept" and just "work on" are different (Watkins, 1984 pp. 156-159).

[2] To stress this point, here Popper refer to his (1962), chapter 24, and (1963) pp. 25-30.

error affair which I have depicted" (p. 102).

The second kind of reason is that so far no negative evidence in science exists to refute realism. Popper attacks Hume, Berkeley, Mach, Russell and James for their idealist positions or subjective theory of knowledge. He argues that the problem with them has its roots in, not science, but this traditional empiricist desire or assumption: we shall and can derive or extract our knowledge of the world out of our sense experience by methods which can be seen to be reasonable (p. 100). This desire or assumption will be bound to lead people to the idealist view (as is shown by Margenau). Popper concludes: "no serious arguments can be found" in Humean subjective theory of knowledge "against the reasonableness of metaphysical realism" (p. 128).

The third kind of reason is that, Popper thinks, realism has indeed got some empirical support in relation, especially, with well tested physical laws. "The reality of physical bodies is implied in almost all the common sense statements we ever make; and this, in turn, entails the existence of laws of nature: so all the pronouncements of science imply realism" (p. 128). Popper stresses: "Since in fact we have a considerable number of thoroughly discussed and well tested laws of nature, there are indeed empirical reasons for the belief that there exists at least one true law of nature" (p. 79). Popper tends to think that realism, although being "metaphysical" in the sense of neither verifiable nor refutable, is reasonable because it has got the strongest (even though inconclusive) arguments which any knowledge could ever have so far (pp. 82-83). For Popper, this "obtaining the strongest argument" is part of reasonableness.

Differing from Popper's metaphysical realism, current scientific realists are more explicitly taking realism as an "empirical hypothesis" about the nature of science and thus believe that only "specific" negative evidence is eligible to refute it. Shapere, insisting that he himself is an empiricist, claims that our concept of an independent world consists of (1) the sum of our specific substantive beliefs, and (2) the recognition that doubt may always arise with regard to any of those beliefs. The concept itself, as a result, can also be altered if someday there are enough new specific reasons to change many parts in the sum (Shapere, 1984, pp. 230-2). Boyd holds that scientific realism, as a kind of naturalized epistemology, is, like a scientific hypothesis, subject to scientific confirmation or falsification (Boyd, 1984). Giere says that a realist judgment "can be questioned, of course, but only in the specific way that any empirical hypothesis might be" (Giere, 1988, p. 170). Van Fraassen also stresses that his constructive empiricism regarding science and his emphasis on empirical adequacy as the restraint to acceptance "delivers us from metaphysics" (van Fraassen, 1980, p. 69).

These realists of course are aware of the arguments by Hume, Berkeley, Mach, James and other traditional thinkers. But they just ignore them. This is both because they have reformed the concept of reasonableness and because they hold that scientific realism about the external world is a hypothesis which is properly subject to scientific tests. Shapere defines "good reasons" as reasons which are successful, relevant, and free of doubt from any specific evidence within science. In this way, he rules out purely philosophical arguments, like Hume's, as not pertinent to the debate of scientific realism. Like Popper (1983, p. 79), he points out that if there is no specific reason to doubt a very successful account of the world, then the pure possibility of doubt regarding the account is not itself a reason for doubting that this account is realistic (1984, p. 229). This relative and "naturalized" reasonableness is an aspect of modern realism wherein the scientific realists differ from those older empiricists or materialists, such as Locke, who mainly tried to obtain knowledge with certainty by philosophical argumentation.

This change results somewhat in the second deviation I want to talk about. This second one represents an even deeper change in modern philosophical thinking with regard to the problem of an independent world. This is another reason why many traditional metaphysics find no room in the debate. This can first be seen by the fact that the realism debate is only focusing on the issue of theoretical entities and unobservables.

Although the independence thesis is what primarily distinguishes the realists from the anti-realists, they all share an understanding of the thesis. In one sense, scientific realism is a doctrine of commonsense: people no longer debate the existence of macro objects. This is not just for the convenience of discussion. Most anti-realists involved in this debate, like van Fraassen, Fine and Laudan, appear to accept the existence of macro objects since they are "observable." What they argue against is the idea that such theoretical entities as electrons or the DNA molecule can be proved to exist - because they are unobservable.

Let me say a little more about this. Surely we can say that these are two claims at different levels: claim (a), "x exists (or there is x)", and claim (b), "the existence of x is independent (or external)." Claim (a) is a scientific statement about what exists in the world, like "electrons exist"; while claim (b) is a realist claim, which is actually about the ontological consequence of claim (a), about the way in which x exists; or, in other words, claim (b) implies that if claim (a) is supported in such such a way, then, using Wartofsky's terms, what "x" is about or refers to or asserts to exist does in fact exist independently of our knowledge of it[1]. In this way, we can say what realism tries to propose is just to add externality or independence to the existence of x (idealism, for instance, of course adds "internality" to that).

The point is whether in any case claim (a) can indeed reasonably lead to claim (b), or, whether the addition of externality is reasonable. For a traditional empiricist, the answer would be negative in any case. Yet for a modern antirealist, the answer could be positive in cases of observables, but negative in cases of unobservables. Fine's *natural ontological attitude* (*NOA*) tries to accept claim (a) for an entity, while, at the same time, reject claim (b) about it. According to *NOA*, we are counselled to accept the results of science as true. "NOA sanctions ordinary referential semantics and commits us, via truth, to the existence of the individuals, properties, relations, processes, and so forth referred to by the scientific statements that we accept as true" (Fine, 1984, p. 98). A "noaer," as a scientist, "within the context of the tradition in which he works," "will believe in the existence of those entities to which his theories refer" (p. 98). Nevertheless, the noaer will add neither externality nor internality to this acceptance. These additions have illegitimate and unrequired features outside what can be "contained in the presumed equal status of everyday truths with scientific ones," in "the customary epistemology, which

[1] See M. Wartofsky, 1991, pp. 34 - 35, where he takes claim (b), realistic claim (he calls it second-level ontological assertion, or meta-ontological assertion in contrast to claim (a) as first-level one) as asserting the ontological consequence of the truth of claim (a). This differs from my approach since I in this thesis avoid the issue of realism about truth.

grounds judgements of truth in perceptual judgments and various confirmation relations" (p. 101). Presumably this is especially referring to the cases of unobservables. For, besides arguing that arguments for realism are not sound and realism will prohibit the development of science, Fine's main objection is based on the difference between observables and unobservables (his example is electrons). Unlike "matching a blueprint to a house being built, or a map route to a country road," says Fine, we cannot take an external stance "to judge what the theory of electrons is *about*," for we ourselves are in scientific world: what we know about electrons is given by the theory that we want to check (p. 99). Fine claims that the case of the external world is like that of the justification of induction in which only an inductive justification will do, but cannot do the job: only ordinary scientific inference to the existence (of electrons) will do, "and yet none of them satisfies the demand for showing that the existence is really 'out there'" (p. 99). (We can see what Fine demands for a rational or reasonable belief includes a "foundationalist" style of justification. As I said above, this has been put aside as unnecessary by most philosophers of science, since their question is whether a *relatively reasonable* argument in favour of realism even could be made[1])

Van Fraassen is another antirealist who bases his answer to the question concerning the inference from claim (a) to claim (b) on the difference between observable and unobservable. According to his constructive empiricism as opposed to realism, science aims at only "saving the phenomena"; science is an activity of construction of models that must be adequate to the phenomena, and not discovery of truth concerning the unobservable. For him, what is observable is of importance or "eminently relevant" to what scientists reasonably believe. "Indeed, we may attempt an answer at this point: to accept a theory is (for us) to believe that it is empirically adequate - that what the theory says *about what is observable* (by us) is true" (van Fraassen, 1980, p. 18). In cases of unobservables, he argues that realism which asserts the truth of accepted theories

[1] Giere, for example, says: "A realist interpretation of science requires no such ultimate justification" (1988, p.170). Also see Laudan's argument against Bloor (1990, pp. 290 - 291).

cannot be supported by any empirical evidence: "when the theory has implications about what is not observable, the evidence does not warrant the conclusion that it is true" (p. 71). But in cases of observables, the truth-claim can be accepted, because "saving the phenomena" is just giving "a true account of *what is observable*" (p. 4); "When the hypothesis is solely about what is observable," accepting the hypothesis as empirically adequate and believing it to be true "amount to the same thing. For in that case, empirical adequacy coincides with truth" (p. 72). And he continues: "But clearly this procedure leads us to conclusion, about what the observable phenomena are like, which goes beyond the evidence available. Any such evidence relates to what has already happened, for example, whereas the claim of empirical adequacy relates to the future as well" (p. 72). Clearly, van Fraassen has thus distinguished himself from the Humean tradition which disallows individual observations to prove persisting existence.

Van Fraassen mentions a Mach-Margenau style of objection which claims that observable objects "are also postulated entities, believed in because they best explain and systematize the sense-experience or series of sense-data..." - so should he not be as unwilling to postulate tables and trees as forces, fields, and absolute Space...(p. 72)? Van Fraassen's answer is: "I mention this objection because I have heard it, but it astonishes me since philosophers spent the first five decades of this century refuting the presuppositions that lie behind it"(p. 72). He declares: "I wish merely to be agnostic about the existence of the unobservable aspects of the world described by science - but sense-data, I am sure, do not exist" (p. 72). Presumably here "sense-data" refers to Berkeley or Mach's ones distinguished from (observable) entities.

This antirealism, built by Fine, van Fraassen and Laudan, constitutes a great leap in the inference from our sensation to the independent existence of the world, a leap that no traditional philosopher would allow. Theoretical entities, like electrons, atoms, DNA molecules or Brownian particles under microscopes, are "unobservable," but in a different sense from the unobservability of a macro object as persisting (I call this unobservability "Up") or phenomenon-transcendent ("Ut"). The former are unobservable because they are too small to be seen even when we try to look at them ("Us"). For even if we suppose that we are able to see the tiny thing someday, for Hume, we still could not claim that it exists in the persisting sense by any number of individual observations; and for Berkeley or Mach, what we obtain by "seeing the electron" is no more than our sensations or sensory qualities; we can never see the "reality" behind the electron-sensations; and the entity "electron" is just a collection of, or, a construct from, these sensations.

This difference between a micro entity and a transcendent reality or among three aspects of unobservability, Up, Ut, and Us, has not, as far as I know, been clearly expressed or realized by either realists or anti-realists. (It seems to me that Berkeley and Mach have collapsed the three different senses of theoretical entities, taking the entities as traditional things-in-themselves when opposed to such "unobservable" entities as atoms. They appear to think that if atoms could be proven to exist, this would be equal to proving the existence of a sensation-transcendent reality.) Not everyone who has accepted the existence of macro objects has realized the jump they have made. Indeed, the unobservability of electrons, molecules or Brownian particles depends upon all three aspects. But, if we accept the existence of macro objects, then we have ignored the Humean and Berkeleyan aspects of unobservability, Up and Ut. For Up and Ut apply to macro objects as well. We thereby no longer think their arguments are reasonable or legitimate. In this way Kant's phenomenalism which applies to micro objects too is also avoided. Undoubtedly it is incorrect to take a micro entity as transcendent reality, like the reality of a table behind its phenomenon, just because it is too tiny to be seen. What a microscope could reach will never be a "transcendent reality." Up or Ut cannot be reduced to Us. The relations between such an entity, like a photon, and its effect on a receptor or on our sense organs, should not be taken as relations between a transcendent reality and its phenomena in the traditional sense. The existence of a micro-entity is the existence of something we need to prove only by overcoming the obstacle of Us. This is the problem-situation of the realism debate within which I argue. If one complains by saying something like: but macro objects are also unobservable entities after all, how can you use them as your standard of objectivity? what I can reply is that this is an irrelevant question to the debate.

In other words, this dismissing of Up and Ut, and invalidating objections based on them, forms a distinctive presupposition of modern realism debate, followed by the main stream of most scientists and philosophers of science, both realists and antirealists, involved in the debate. Scientists take entities observed individually as existing. They jump right from individual observations of an entity - by measuring its properties, for instance - to claiming its independent existence. The problem of interpolating unobservables does not matter. The so-called "principle of causal anomaly" for the descriptions of "interphenomena" between the "observed phenomena" of electrons (if the observed phenomena are indeed from electrons), say, will no longer bother them as far as their existence is concerned. The situation of electrons before any possible observation, could be a target of high level theoretical speculation, but it is irrelevant to the problem of determining their existence. The jump is also the one from the observation of properties - the sensations - to the independent existence of entities bearing the properties - from the properties to the existence of their carriers. Once scientists detect some distinct properties of a micro entity that they postulate, they conclude that the entity is found. The entity is not a construct of mind, nor is it a collection of sensations. The philosophical gap between sensations and their "constructs," or "phenomena" and "things-in-themselves" just does not make sense for the scientists. As we will see, this presupposition clears the way for people to depend only on the interaction thesis to get to entity realism.

The invalidation of the "rational" doubts based on the two traditional aspects of unobservability, as an indicator of a movement away from "old metaphysics," marks a distinct line between traditional empiricism and new empiricism, a line that separates contemporary realists and anti-realists from others in the history of philosophy. This is a threefold problem-shift: idealism, subjectivism and phenomenalism have all been dropped from the debate[1]. This indicates a significant turn in the philosophy of science of this century, under the influence of the science of this century, as a by-product of the

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[1] Of course it could be argued that for at least some people, dropping idealism or subjectivism from the debate is not because they believe in macro objects as real, but because it is convenient for discussion. Yet it still means that the traditional ways of thinking have been rendered irrelevant to the current problem-situation.

death of the positivist anti-metaphysics campaign. Of course, on the other hand, we must be aware that this means that any resolution there might be to the realism debate is restricted - valid only within the range the presuppositions of the debate can cover - it will not be a resolution to the traditional problems with our ideals of rationality and foundation.

2.4 Observation: Causal Interaction

We can now say that both the realists and the antirealists are empiricists since experience is even more important to them than it is to those old philosophers. For those antirealists in the debate, *accepting the independent existence of a macro object is reasonable simply because it is observable.* So it seems that what people need to indicate the existence of a micro entity is also observation of it - individual observations which can be shown to be essentially the same as, or as good as, the observations of macro objects. Individual observations could lead to belief in independent existence.

The key problem is, then, can we indeed have such individual observations of any suitable property of a micro entity? If so, how? If not, why not?

From the preceding discussion we can see that Reichenbach, Luntley, and Margenau should be classified as traditional anti-realists. But they nevertheless have one thing in common with the realists. That is, they agree, in one way or another, that we could have (directly) individual observations of micro entities or the properties of them. What prohibits them from accepting the existence of these entities are the arguments based on Up and Ut. Deprived of their philosophical framework, they might have no conflict with realists.

On the other hand, such anti-realists as van Fraassen, Fine, and Laudan are some distance from traditional empiricism. In accepting the existence of macro-entities, they accept the independence thesis in cases of observables; and in this way, they have assumed, at least implicitly, claim (a) can imply claim (b) as long as claim (a) is supported by individual observations. The success of proving that a theoretical entity exists in the same way that a table does would be considered by them equivalent to the success of proving their existing independently of human beings. Observations in the case of macro-objects can become a standard for "good reason." And surely what they refuse to believe is just that any such observation is available in the cases of theoretical entities. They argue that what we see in a typical observation are the phenomena on our films or computers or other receptors, while what I want to call the physical relations behind and leading to the phenomena are invisible, and thus just as inferential or hypothetical as the entity. By refusing this, they, as empiricists, have certainly rejected the existence of any condition by which claim (b) can reasonably be taken (or believed) as a consequence of claim (a) in the case of unobservables.

To illustrate this point, let us look at van Fraassen's views on observation again. For him, there is such a thing as a mouse, "For the mouse *is* an observable thing" (1980, p. 21), but electrons are different. Just contrary to Reichenbach, Margenau and Luntley, who accept the observation of some properties of a postulated entity but deny the existence of it, van Fraassen denies the observation of any such properties. He claims:

> The term 'observable' classifies putative entities (entities which may or may not exist). A flying horse is observable - that is why we are so sure that there aren't any - and the number seventeen is not. There is supposed to be a correlate classification of human acts: an unaided act of perception, for instance, is an observation. A calculation of the mass of a particle from the deflection of its trajectory in a known force field, is not an observation of the mass (p. 15).

What is the difference, then? Van Fraassen rejects Maxwell's claim that there is a continuum from looking through a vacuum, a windowpane, binoculars, a low-power microscope to a high-power microscope. He claims that there is a difference between seeing through a window - which he takes as an observation, and seeing through a microscope - which he does not take as an observation. For if something can be seen through a window or a telescope, it can also be seen with the window raised or without a telescope if we are close enough (p. 16). Yet "seeing" through a microscope is not an observation since we could not make our senses small enough to see a micro entity. That is, again, observability is still that of unaided perception[1].

But, first of all, why should that unaided observation be such important for believing? Van Fraassen's answer to this does not sound satisfactory to me. He admits that what counts as observable can change and "is a function of what the epistemic community is (that *observable* is *observable-to-us*)" (1980, p. 19). "But," insists he, "the point stands: even if observability has nothing to do with existence (is, indeed, too anthropocentric for that), it may still have much to do with the proper epistemic attitude to science" (p. 19). But unfortunately he does not explicitly explain why this is the case, since the anthropocentric nature of the concept "observability" makes it have nothing to do with existence.

It seems to me that one way out of the puzzle is to have a non-anthropocentric view of the concept, or, in other words, to look at the non-anthropocentric nature of observation, in order to understand (1) why observability can change and (2) why it matters so much to the people like van Fraassen. This will bring us back to the second thesis characterizing an item of physical reality, the interaction thesis. As I argued earlier, it is safe to say that physical interaction is a sufficient condition for being physically real. We know that any observation must be based on this or that kind of physical interactions. A kind of experience is a kind of complex of interactions. Hence the reason that an experience or observation matters ontologically and epistemologically is just its nature of physical interaction. This role of the non-anthropocentric aspect of observation can been seen by this fact: an interactive process could be called "observation" without human perceptions (which ends up with recording and calculating signals by computers and printing out the results on paper). Shapere has reported that observation processes in science could be carried out without the presence of human beings (Shapere, 1984). So, to be more accurate, the dominating factor here is the

[1] He gives an example to show the difference between a "purported observation of micro-particles in a cloud chamber" and an observation of the vapour trail of a jet - in the latter case we can confirm the trail as the causal outcome of jet by seeing the jet itself a bit ahead of the trail (1980, pp. 16-17).

interactive character of observation. Observations play a decisive role just because they are causally interactive processes. Although human perceptions are often involved in the end of the process, they are not necessary for observations to play their philosophical roles. This is where interaction, rather than observation, becomes crucial to the question of existence. In the following, therefore, when I talk about an observation, I always mean actually a causal interaction. Now this is our starting point to consider whether in any case we can have an "observation" of any micro entity.

Keeping this recognition in mind, one of the central issues of the scientific realism debate is, therefore, how the realist can convincingly argue that some micro entity is as physical as macro objects are. That is, how to prove that we do causally interact with such a tiny entity in a way we do in the case of macro objects.

Indeed, as the antirealist would argue, Luntley's theory of perception insofar as it claims that we do have direct observation of electrons has completely missed the point. The point in the observation is not to recognize or read out skilfully required clues from observable pictures or whatever on films or computers; but it is how to show that there exists a physical connection between the clues on the picture and the postulated entity at issue. As van Fraassen asserts: "[T]he observation of the phenomena did not point unambiguously to the supposed causal connections behind them" (van Fraassen, 1980, p. 2). The connections will not show up on the picture, they are micro processes. The observable effects on the picture are a certain transformed macro-form of the micro processes, the form which is observable to our perceptions.

Yet there are two aspects in which Luntley and Margenau are right for accepting that there exist observations of the mass of electron: first, there is a basic similarity between that observation and observations in the case of macro objects: they could be based on similar kinds of physical interactions. Hence, we can also say that we could not experience the interaction in the process of my seeing a table with the unaided eye (photons' interaction with the particles in the air between me and the table, for example). We assert that our seeing is a real physical process and the table is real also on the ground of repetition, regularity, comparison, and synthesis (by what Margenau calls "reification") - especially by (1) the consistence of perceptional results about a table from different minds and (2) the agreement of action from the perceptions of the table with all other actions which assume the existence of the table. In short, if we found the rationale for determining micro interactions essentially the same in kind as that for accepting that our seeing a table is a real experience process, this would constitute a reason for accepting entities, a reason as good as that in the case of undisputed observables.

Secondly, the human ability of observation, or more accurately, of experimentation, is "evolving." This evolution of human ability does not refer to our sense-perception, but to the evolution of human physical influence, or the extension of physical means and processes controlled by human beings. Actually this is displayed by the increase of the number, length and range of human activities being determined as physical. As a result human beings are able to reach physically deeper and deeper levels of the world. This explains the fact that what counts as observable changes due to the progress of technology. When technology improves, our net of experimentally testable causal generalizations or claims grows; and our reachable levels of objects increase.

Determining that seeing processes through spectacles or telescopy or optic microscopy are as real as seeing with the naked eye might just indicate a small increase of the length of human activity being determined as real, but it is of great importance since it indicates that an extension from our senses has epistemologically been taken as eligible. Actually, after making this step I see no reason why we should stop this extension at any place or level where the same principles or means of determining apply. The gradual increase is made together with discoveries of more and more types of physical interactions. At first we found that transfer within one form of physical interaction, say, visible light, was a "realness"- or "physicalness"-keeping transfer. Later, by means of "reification," we realized that it is also the case with the interactions within a wider range of optical interactions, including invisible light. Meanwhile we found that there are other forms of interactions, say, electricity, and we discovered that the interactions between electricity and light, which can be shown by an optical spectrum, are also realness-keeping processes. So the physical structure of the phenomena or information of different forms could be transformed without serious loss or distortion and the energy or information aspects of this transformation are kept throughout. To this stage the length of our physical antennae or feelers further extends. That means, some kind of causal connection between certain optical effects on film or microscopy and certain unobservable electrical action is physical or real. Human beings have been able to "touch" or experience the reality at this micro level. So far, scientists believe, this kind of extension has reached the level of, at least, the atom or even atomic nucleons.

Hacking has a case-study of the grid for the continuum in observations (Hacking, 1983, pp. 203-205). The visible shapes and the labels of a metal grid visible to the naked eye could be reduced photographically to be so tiny that the naked eye could no longer see. Then using almost any kind of microscape, we can see exactly the same shapes and labels as were originally drawn and seen on a large scale. This has convincingly proved that the rationales based on which observations of macro objects are established can directly be used to prove that our observations of micro objects are real and our observability limitation can thus be broken. There is absolutely no reason to argue that the rationale of the microscope which proved to be reliable in the case of grid will not work when focusing it on a "real" micro thing that we might not be able to compare with what we see by eye.

For example, we do not need a particularly good low power microscope to see the Brownian movement of particles, the size of which varies from one four-thousandth to about five-thousandth of an inch in length (much bigger than molecules). Since we can easily prove that the microscope is not a fiction-maker by focusing it on something that is tiny but that we could just see, the very same rationale can lead scientists to assert the reality of something we just could not see and that our statements about it are empirical ones. And futhermore, as Einstein states, applying some macro level laws to putative micro level entities, scientists at the beginning of this century could infer:

The Brownian particles visible through a microscope are bombarded by the smaller ones... The Brownian movement exists if the bombarded particles are sufficiently small. It exists because this bombardment is not uniform from all sides and cannot be averaged out, owing to its irregular and haphazard character. The observed motion is thus the result of the unobservable one. The behavior of the big particles reflects in some way that of the molecules... The irregular and haphazard character of the path of the Brownian particles reflects a similar irregularity in the path of the smaller particles which constitute matter. ... It is apparent that the visible Brownian motion depends on the size of the invisible bombarding molecules. There would be no Brownian motion at all if the bombarding molecules did not possess a certain amount of energy or, in other words, if they did not have mass and velocity. That the study of Brownian motion can lead to a determination of the mass of a molecule is therefore not astonishing. (Einstein and Infeld, pp. 60-61)

Certainly in this observation of Brownian particles, it can be argued that in this case we still could not see the micro causal interaction (e.g., electromagnetic/optical transmission) in the microscope: we can only decide that there is an interaction at the level of electricity or magnetism, for instance, or a corresponding relationship between certain colour patches and the structure of cells or DNA fragments, by their observable results, especially by the regular correspondence between our manipulation of experimental devices and the output of detectors. Yet again, this is exactly the way we decide the realness of our macro-observations. We prove that what we see is real by Margenau's *reification* process (repeatability and individual agreement between different minds). That is, *not only are the rationales used at the micro-level interactions essentially similar to those used at macro-observations, but also the ways of proving the working of the rationales are similar to those in macro-observations.* Furthermore, there is a progressive process here. After we find that this regular correspondence can be repeated in different kinds of tests, we take it as a fact that an interaction does exist at the micro level between the input and output. And this recognition of physicalness of the correspondence in turn becomes a means or base to decide further correspondence relations which contain it as a component. So still, the basis of determining the realness is not that we can see the action at micro levels or the real properties of electrons like spin, but that we have this gradual increase of the observability of correspondence relations; we use the methods like comparison of their detected results through different means and so on. As a result, we now can see that van Fraassen's observability distinction is indeed artificial and arbitrary. Old empiricists would not accept it with regard to its implication concerning existence, and would urge him to reject beliefs in the existence of his observables, since he rejects beliefs in unobservables. Going the other way, modern realists also dismiss the distinction and urge him to accept the existence of some of his unobservables, since he accepts that of observables. The people from both camps have good reason for their requests: his unobservables can be proved to have the same characters as his observables.

It is not surprising to find that the basic difference between realists and antirealists has its root in their attitudes towards the status of physical causal laws. Reichenbach interprets the postulate of identical causality this way: it is a simpler convention to assume that the unobserved tree - the tree when we do not look at it - remains one rather than splits into two trees. Popper holds that "the splitting of the tree if we look away, and its re-assembling if we look back, would clearly violate the laws of physics. For this would involve (according to physics) very considerable forces, accelerations, energy expenditure, etc. But this is the same as saying that *physics informs us unambiguously that the tree does not split when we look away* (1983, p. 125, italics original). Realistic belief in causal physical laws leads Popper to claim that realism is implied by science. Popper concludes that Reichenbach's instrumentalist view of science is a result of his subjectivist theory of science, rather than a result of science itself (p. 125).

Now let me summarize the argument so far.

I argue that a postulated relation between a micro entity and certain registrations on a receptor would not be between "reality" and "phenomena," in the old-fashioned sense, but between different ranges of physical realities or properties. In most cases, the job for a scientist trying to decide the existence of a micro entity is just to determine the existence of a physical relation between the properties on receptors and the properties of some other objects put in by the experimental equipment, a physical relation in which the postulated entity is causally required as a necessary link. In this way, the scientist's acceptance of an entity is a scientific decision about claim (a) about the entity in the causal process.

I also argue that Up and Ut, and their roles, are beside the point for both modern realists and modern antirealists, so that individual observations could legitimately imply independent existence. I explain that observations are important because they are causal interactions, and I argue that the scientific acceptance of claim (a) under that condition has already opened a way to claim (b), the externality addition. That is, if we know that a micro causal process is successfully confirmed in a way similar to the confirmation of macro objects, then the entity's unobservability, Us, would no longer be an excuse for saying that we could not experience it; and since this is the basis for accepting claim (a), then claim (b) about the entity's independent existence is implied.

Before we move to the next chapter to discuss how we know such a causal interaction exists in the case of experiment with a micro entity, or what is a good reason for knowing when such a causal requirement has been met in order to move from claim (a) to (b), I shall say something about the concept "causal interactions."

Of course this term means causation that is produced by interactions, causality with the character of an interaction. This thesis focuses on those interactions that have causal features. There might be causality that is not from any kind of interaction or there might be interactions that do not form a causal process. I do not deny them, but I do not need them to argue for the existence of theoretical entities.

This concept inevitably brings us back to the world of metaphysics. My arguments in this thesis depend on a view of the causal structure of the world. But this is a well supported metaphysics. Popper points out that the world is a world of various causal interactions. It is formed through them and it can be explained by them. Structural properties of an entity are due to interactions between its parts; interactions keep the parts together and the entity in shape. Popper concludes: "the structural laws of co-existence of animals, or molecules, or even of atoms may in principle be reduced to 'causal' laws - those causal laws in accordance with which these structures are produced and keep for a time (relatively stable)" (1983, p. 151). Matter, field, information and we ourselves are part of causal interactions - this is the realist ontology and the objective ground for us to hold the realness of what we experience[1].

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[1] Popper's view of causal interaction does not imply a position on the question what the most fundamental entities, physical bodies, physical process or ideas, are. As Settle points out, Popper does not want to answer this question, nor does he think that physical interaction is the whole story. He thinks, on the contrary, that the world is causally open, as is indicated by his "conjecturing the active reality of three different kind of things, the third being ideas..." (Settle, 1989, p. 400).

But what is a causal process or an interaction after all? I accept Salmon's definition of causality because it is in terms of physical interaction, namely, a causal process is, in effect, a process which has energy and/or information (signals) transmission in spacetime (Salmon, 1984, chapter 5; and Kitcher and Salmon 1989, pp. 108 - 110). This is a good definition for some reasons. First, it is well supported by our commonsense and by scientific practices and theories like the special theory of relativity. Secondly, it defends the above causal view of the world against some agelong objections. Among these objections, the most important or basic one is still Humean. As Salmon argues, this definition can answer Hume's challenge about the connection between intermediate causes and their effects in order for a causal process to be contiguous, with the help of the special theory of relativity and Russell's *at-at theory of motion* (Salmon, 1989, pp. 107 - 111). Cartwright (1983) has convincingly offered a practical argument to meet the Humean challenge about the necessity of a causal connection that is more than just constant conjunction. And third, indeed, this definition has some trouble to get support from quantum mechanics. We know the problem is basically that from Reichenbach's "the principle of causal anomaly." That is, we could not maintain the postulate of identical causality for observed and unobserved states of a quantum object. But as I said, given the dismissal of Up and Ut, this trouble does not hurt us now. The evidence for existence begins and ends where observation begins and ends, nothing beyond that is needed. Failures of applying classical laws to interpolating unobservables in quantum world is irrelevant to our purpose. What matters is whether there is any individual "observation" in which, once it forms, a causal connection between a group of particles and some observed phenomena really forms and can be traced somewhat - even though we might not know the contribution of each individual particle, it can surely be known where the phenomena are from as a whole.

The above "energy/signal transfer" characterization of causal process might be partial. Some might argue that there might be some such transferring processes which are not causal in any sense of the word. But clearly the burden of this argument is too heavy to bear. Is there, on the other hand, any causal process without energy or signal transferring? Settle raises a question about energy transfer in terms of the issue of "downward causation". Indeed we know that wholes could be larger than the sum of their parts, "the activities and properties of wholes will be *under*determined by the activities and properties - and energy transactions - of their parts" (Settle, 1989, p. 398). And at least in biology, we could say that processes at lower levels of a hierarchy are restrained by and act in conformity to higher level processes. So the conjecture of downward causation could mean that "some states or processes of wholes control, in some measure, the fates of their parts, or more generally, some processes at a higher level influence outcomes for elements at a lower level" (p. 398), without, possibly, energy transfer. One example seems to be non-physically causal actions from our minds or ideas to our bodies.

Whether downward causation is accompanied by energy transfer is a scientific question. Some people argue that it is possible to have a signal transfer process without energy being involved in it[1]. It is still arguable whether psychological influences to physiological actions are carried out without a physical aspect[2]. Yet, anyway, the incompleteness of the definition of physical causal processes does not undermine what is being put forward here. What concerns me is whether we can move from identifying a cause, with its ability of transferring energy or a signal, to declaring its physical reality. For this, as we do not claim that causal interaction is necessary for reality, we do not need to deny the (actually very likely) existence of physical causation without energy or even signal transfer.

In what follows let me list some intuitions about the character of physical causality resulting from interactions. I shall call them "causality characters." In chapter

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[1] Generally they accompany each other - sometimes only the signal aspect carries causal action or information but the signal might be formed by a particular structure of distribution of energy.

[2] It is correct to say that processes at lower levels of a hierarchy are *restrained by* and act *in conformity to* higher level processes - the lower ones surely could not go beyond the limit represented by the higher ones. This kind of "acting in conformity to" higher ones does not mean an *active* downward causal influence from the higher ones to the lower ones, hence no energy will be needed. But when talking about an active and dynamic causal influence from higher ones to make some adjustments in lower ones, it would be a separate question whether either energy or a signal is needed.

4, I will use them to discuss the effort of constructing a (relevant) logical system of causality in order to describe syntactically evidential relevance.

F1. Generally, physical causal processes are those of transmission of signals and/or energy.

F2. The characteristics of a causal action depend on how and in what order its causal components interact with each other. The different order or time that a component joins a causal interaction could produce a different effect. Combined with a causal component in different ways, causal components can lead to different results. That also means that interaction among the components will bring about different effects which are often not identical with any of these causal components alone.

F3. Causal relations are of many kinds. When saying A causes B, denoted by A $\blacktriangleright\!\!\rightarrow$ B, sometimes we mean A is a direct or immediate or actual cause of B; but sometimes A is an indirect or non-immediate or potential cause of B (sometimes A causes B, while sometimes A is only causally relevant, one way or another, to B); and the difference is not clear. This brings about difficulties in precisely formalizing causal action, i.e., in interpreting the meaning of and stipulating the usage of the formula A $\blacktriangleright\!\!\rightarrow$ B as A causes B in various situations.

F4. Also as a source of ambiguity of '$\blacktriangleright\!\!\rightarrow$', when we say that A causes B we sometimes mean that A is a sufficient condition for B ($A \blacktriangleright\!\!\rightarrow B \vdash A \rightarrow B$), sometimes that A is a necessary condition for B ($\neg A \rightarrow \neg B$), sometimes that A is both necessary and sufficient for B ($A \equiv B$), and sometimes none of these things (Skyrms, 1986, p. 89). Also, people sometimes call a factor a cause simply because it is, using Mackie's term, an *insufficient* but *non-redundant* part of an *unnecessary* but *sufficient* condition for an effect, i.e., it is an *inus* factor (event or condition) for the effect (Mackie, 1974, pp. 61-62, p. 304)[1].

F5. A thing is not the cause of itself.

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[1] The *inus* condition means: if there are two conjuctions of factors: (1) ABC, and (2) DGH; either of the conjunctions can be followed by an effect P (i.e., "ALL (ABC or DGH) are followed by P"); then any conjunct (factor) of the two conjunctions, say, A, is an *inus* factor for P.

F6. At a give time t, if A is a cause of B, then B is not a cause of A.

F7 In contrast to F4, very often, a "single cause," A, can be analysed as a compound of more than one causal component, each of them a partial cause for the effect of A.

F8. We know that in many cases causal relations are a matter of probability, or, to put it more strictly, can only be expressed statistically.

F9. Causal relations are context-dependent. Individual causal actions depend upon the existence of special and concrete circumstances. As a result, we know that a causal chain can be non-transitive (especially when A is just an *inus* factor). There is no guarantee that when A ►→ B, B ►→ C and C ►→ D, then A ►→ D - certainly the ambiguity in " ►→" contributes to it. For the procedures from B to C and from C to D may only occur in circumstances different from those of A, while A and its circumstances can only produce B but not D.

F10. There might be more than one model to represent one causal process. Yet unlike mathematical or instrumental, there should not be any inconsistency between or inside causal models which represent something really existing in the physical world.

F11. It is possible that the same logical formula might represent different causal situations in different times, or two truth-functionally equivalent formulas might have different (even inconsistent) references[1]. A formula such as A ►→ B, representing a causal process between specific A and B, might not hold between other things, or in other situations. Also it is not guaranteed that any occurrence of A and B can be replaced by any other formally (or logically) equivalent variable or formula without losing the

[1] One example is from Maxwell's theory (1862) of electromagnetism. His concept of ether was constructed through analogies with both fluids and solids. Thus the concept of the medium contains internal inconsistencies, and in different situations it plays different roles. This is a reason why Maxwell himself did not believe that such an ether exists as the real cause of electromagnetism. Also, Maxwell used the same one mathematical symbol to denote different situations. In Chapters 4 and 8, detailed discussions about this will be given.

formula's original validity[1].

[1] For example, if $A \rightarrow B$ refers to a fact that A causes B, substituting it with its logical equivalence $\neg B \rightarrow \neg A$ will surely lose this reference.

Chapter 3 Scientific Reason

In this chapter, I shall discuss the concept of a good reason for adopting claim (a) and thus moving to claim (b). I shall show what a 'good reason' means in science and what could possibly constitute a good reason.

3.1 From Relativism to Realism

Giere, a former physicist, asserts that "few students of the scientific life would deny that most scientists are realists regarding many subjects of scientific inquiry. The question is whether scientists are *correct* in so believing, and whether we, as students of the scientific enterprise, should adopt their accounts as an even roughly correct description of what they are doing" (1988, p. 124).

In the discussion in last chapter I have laid out a beginning and some clues for answering this question. Surely scientists often speak in a language of the form of claim (a), like "DNA molecules exist." It can also be argued that this language does not really necessarily imply any realist tint, that is, claim (a) does not always imply claim (b). Within the current realism debate, this depends on the subjects involved in claim (a). In the case of observables, (a) can imply (b), but in the case of unobservables, it might not. And these different answers have formed some distinctive presuppositions of the debate, with regard to the form of reasonableness and the validity of methods, standards and arguments. Given this recognition, I argue, on the basis of an agreement between both antirealists and realists about the role of observation, that if adoption of claim (a) about a micro entity is on the ground of a finding of causal physical interaction involving the entity, and if the way of proving the existence of the interaction is similar in principle to that for macro objects, then the movement from claim (a) to claim (b) is "reasonable" in the comparative, relative, concrete, critical and restricted sense of the word.

Before I go along this line further to discuss what kind of reasons (conditions) can indicate that a causal interaction in question is really found, namely, what the conditions for the movement from claim (a) to claim (b) are, I need to say more about the key concept "reasonable." For the way of displaying my arguments in this thesis does not actually begin by showing any case from science, but by (1) looking for a philosophical ground, a concept of reasonableness that is agreed on by both antirealist and realists. (2) Then I move to infer from this concept what conditions we need for claim (a) to meet in order to move to claim (b). And (3), finally, I come to science, try to find if the conditions fit its practice and if in it there is any claim (a) which has met these conditions. If we find one, then we can say that scientists' acceptance of claim (b) about the entity in question is legitimately implied in their acceptance of claim (a): their realistic belief is, within the range of the realism debate, philosophically "reasonable."

I have said in the last chapter that what I want to call a "reformed reasonableness" - "RR," for short - is shaped by the influence of science and the views of such people as Popper, Hacking, Shapere, Laudan, and van Fraassen. One possible formula covering part of the content of RR could be like this:

> Accepting an hypothesis is reasonable when (1) the hypothesis is well-corroborated in the sense that so far, unlike all other rival hypotheses, every severe test designed especially for falsifying it over a long time has failed to offer any specifically negative evidence against it; and (2) the acceptance is open to further test and once there is specifically negative evidence against it, the acceptance shall be given up.

Considering what I said in the last chapter, we can see that this reformed reasonableness of an acceptance has temporary, open, critical, empirical, concrete, flexible, comparative, relative, synthetic, restricted and moderate features. That means, "acceptance" here does not have any foundationalist, essentialist or justifactionalist commitment. We do not need any "sound," in the sense of logical inference, proof for the acceptance, nor is the acceptance final. Again, the evidence leading to the acceptance or rejection should be concrete and specific. Evidence for an hypothesis is largely due to failure to find evidence against the hypothesis and the fact that all its rivals fail some tests. That means that acceptance of an hypothesis is because it is the best on the ground of comparison: it gets the strongest proof in the sense that its proof is stronger than the proofs of any others, or only it has got the strongest possible proof so far[1].

Now it is easy to see that RR will consider the challenge from the doubt that our scientific enterprise might be just an error in this way: if this means something like a platonic style of arguments, it is just ruled out. Words like "independence" (claim (b)) just make sense in our problem-situation, nothing beyond that could be reached by methods accepted in this debate. If this refers to a possible result from science itself, well, accepting a theory like Einstein's is still reasonable "even if we assume that we shall find tomorrow that the laws of mechanics (or what we held to be the laws of mechanics) have suddenly changed" (Popper, 1983, p. 57)

Let me spend more time on the relativeness of RR. This suggests that scientific realism could be made compatible to a great extent with (Kuhnian) sociologist and relativist views of science. Giere holds this position (Giere, 1988, p. 109). And anarchist Feyerabend is an example (Munevar, 1991, pp. xiv - xv).

Indeed, a realist can surely live with the facts that science is always changing, that the contents of presuppositions, concepts, methods, distinctions or criteria could differ in different fields and times, that social factors play an important role in science. Given the concrete feature of RR, which considers "specific evidence" as the only base for a reasonable doubt, relativists' and sociologists' views do not undermine the foundation of realism about an entity. On the contrary, those views have some consequences that realism about entities requires. Let me list three of them here.

(C1) Change in the observable/unobservable distinction. From a relativist view, we could infer that the line between them is changeable as well. The distinction in modern physics could be different from that in the physics of the last century or that in

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[1]Laudan allows a weaker statement than this. He says: a rational retainment of a theory needs that the reasons for "holding that theory... as true (or empirically adequate)" be "(preferably stronger than but) at least as strong as the reasons" for any of its known rivals (1990, p. 277).

our ordinary view. Van Fraassen is right when he says that what is observable is a matter for science (1980).

(C2) Change in the levels of items in a theory. Since the structure of a theory is changeable, the division of the levels of items of a theory, theoretical ones and observational ones, or higher level and lower level, is also changeable and relative. An item viewed as being at a high theoretical level of a theory at one time may be taken as belonging to a lower, observational level of the same theory at another time; and one and the same item may be placed at different levels in different theories.

(C3) A non-holist way of treating different items in a theory. That is, different items will be treated differently. This indicates that some requirements or arguments applying to some issues could be irrelevant to the cases of theoretical entities, and vice versa, since the cases have their own features. It follows that the establishment of the foundation for realism about a certain entity does not need to use every concept, datum or hypothesis from the theory postulating the entity.

There is one important problem in making RR compatible with the relativist position as a whole. This is the relativist denial of progress in science and of the theory-transcendent status of any scientific standard or conclusion. This is the hardest nut for realists to swallow. Yet, is it not really possible for RR to stand without a commitment to progressiveness? Or is there not any kind of progress anywhere in science? I give the both questions positive answers. That is, I think that we could reasonably accept an existential claim without believing that it has a theory-transendent status; and that there is a "technological progressiveness" which is strong enough for supporting entity realism. From these answers, a division of reasonableness into two levels, one stronger than the other, results.

The two levels of reasonableness differ depending on how much they take the doctrine of relativism. The first level is a moderate one, I call it "internal reasonableness." The reasonableness of a judgment, namely, will be decided only in terms of the criteria of the framework that the judgment belongs to. This level of reasonableness is that of a judgement in its own system of standards and methods; the issue of progressiveness is not involved in it. If a judgment has met the standards of its own paradigm, then we shall say that it is reasonable in this sense to accept the judgment. Now the question is: can realism stand on the ground of this kind of reasonableness? And if it can, then in what sense?

Of course scientists have been justifying their judgements in terms of their own framework. It is a widely accepted view, especially among philosophers, that justification in terms of a given framework will not naturally obtain a framework-transcendent status. However, even though we might not be able to argue positively that a justification relative to a given framework could have a life longer than the framework as a whole, can we really conclude that no judgments from any kind of framework can possibly enjoy such a longer life? I am quite certain that no such general negative conclusion has been drawn, partly because it would be a proof concerning all possible kinds of frameworks.

But can not a reasonable doubt be raised about a possible inter-framework status of any existential claim? Given the concrete feature of RR, any reasonable and serious doubt should be based on cases from the practice and history of science. What can be used seems to be only an inductive inference starting from some instances in history, where the judgment about a theoretical entity had met its framework's standard of good reason but later the entity turned out to be a fictitious tool. The most serious instances for the induction, as far as I know, are raised by Laudan in his (1981)[1]. The most powerful instance of Laudan's is that theories of ether were successful but now the ether is believed not to exist. From this instance an induction stands up: terms in a successful theory now or in future might not refer.

This argument is a weak form of "pessimistic induction" from history. The strong form of the induction proceeds from the premise that "all scientific theories so far

[1] Laudan raises this kind of counter instance to reject the alleged realist claim, construed by him as a premise of a deductive argument, "terms in a successful theory will (always or usually) refer." Yet in order to beat realism, he needs such an induction: using the conclusion of this rebuttal: "in the past some successful theories' terms did not refer," to imply that "so now and in future some successful theories' terms will not refer either." Later, my discussing the way Laudan defines "successful" shows that both his rebuttal itself and the induction from it would be undermined.

proposed have been false" to the conclusion that "all scientific theories are false.[1]" I think that at least the premise is untenable. Some people, like Musgrave and Watkins, have rejected that premise (see Watkins, 1991, p. 357).

If the weak form of the induction changes the premise to "some (splendid) scientific theories have turned out to be false," and if the conclusion claimed is no stronger than "some current and future theories, however great, might turn out to be false" we have an even weaker form of the induction which appears safer, but obviously loses its teeth for attacking realism. Yet, even if this kind of induction based on some instances could still form a reasonable doubt concerning realism about an entity, we can rebut it by either challenging the induction itself or destroying its base. The latter way is viable at least in principle: if we can prove that there has been no such evidence so far in the case of any theoretical entity, no induction could start from it. Namely, if we can argue that no such instance can be found in the case of the entity even judged inside any given framework, we can say that this form of induction has no base. In this way the reasonable doubt will vanish. Thus realism based on this reasonableness is alive.

In Chapter 8, I shall show, as a result of the character of "good reason" developed based on the second level of reasonableness in this thesis, that Laudan's counterexample of ether is not eligible even inside its own framework, and thus the pessimistic induction is undermined in the case of entities.

The second level of reasonableness is a stronger one and is expected to serve as the foundation for the realism I am defending. I call it "external reasonableness." It requires, unlike the internal variant, a concept of the progress of science. Namely, we need to admit that science is progressive at least at some level. From this point, reasonableness means that things will change, but previous work is a fruitful base for the change - a necessary step toward further achievement.

It is easy to see that the second level of reasonableness can explain the trans-

[1] I use the concept of truth to display the induction. Of course this is just for convenience, in this thesis this concept and related issues (realism about truth) are avoided.

framework status of an existential claim made by some kind of scientific activity, assuming the activity be regarded as a progressive one. Yet in contemporary philosophy of science, under the influence of scientific revolutions, the concept of trans-historical reasonableness has generally been thought of as hopeless since it seems incompatible with the fact of scientific changes. But some ideas are worth being cleaned up here. One is, has any argument ever been developed to reject the possibility of scientific progress in any sense, at any part or any level of science? Not really. Again, such a general argument is impossible. Another one is: will a scientific revolution shake up or overthrow every part of the deepest foundation of previous theory in the sense that everything thought to be real will turn out to be fictitious after a revolution? Not really. On the contrary, I found that most experiences taken as real remained real, no matter how profound a theoretical revolution was. As a matter of fact, the ontological status of an object already decided experimentally is the most difficult part in the empirical base of science to overthrow. DNA has finally been identified as existing by the causal processes which connect it to many biological phenomena and by its double-helix structure. Now, after many technological revolutions in molecular biology, the ideas about these processes have been great enriched; some ideas, such as those about the subtle form and stability of the structure, have even been proven not very accurate; yet the ontological status of the biological molecules has never been in doubt ever since.

Our experimental ability to interfere with the world is obviously progressive and this kind of progress is sufficient for realism about entities. No matter what has been said with regard to the instability of scientific theories in history, little negative evidence has been found to deny the progressive character of experiment and technology today. Our experiences are accumulating due to experimental and technological progress which has, more and more, become part of modern science. I shall scrutinise this as the foundation for realism in chapter 7.

The "external reasonableness" which this level of progress can underwrite is stronger than the internal one. According to internal reasonableness, realism about entities stands when no specifically negative evidence exists if the pessimistic induction can be rejected; while with external reasonableness, a foundation for a positive argument might be developed for the trans-framework status of reasons about theoretical entities.

Given this recognition of the features and status of RR, now we turn to consider what a good reason would be like for accepting both claim (a) and claim (b), or, what will constitute a good reason for that purpose.

3.2 What Constitutes a Good Reason?

Many philosophers of science in the realist camp have long attempted to find what a good reason for theoretical entities in science is composed of. I have mentioned that Shapere thinks that in science a good reason for realistically believing a theory must be: (a) successful; (b) free of specific doubt and (c) relevant to the subject-matter at hand.

Here, however, a clear and adequate notion of success is of central significance. Laudan starts his attack on the realist idea about relationships between the reference of theoretical terms of a theory and the success of the theory by fixing a general and vague notion of success for the realist. He declares: what a realist wants to explain, after all, is "why science in general has worked so well." In his opinion, the realist believes that a theory is 'successful' "so long as it has worked well, i.e., so long as it has functioned in a variety of explanatory contexts, has led to confirmed predictions and has been of broad explanatory scope." (Laudan, 1981, p. 22). Stipulating "success" for realism this way, Laudan finds realism remarkably easy to defeat since there have been plenty of examples in the history of science where "successful" theories had non-referring theoretical terms, such as ether theories, which had even, he claims, enjoyed "empirical successes."

Some people, like Worrall, have given up trying to find a positive relation between success and realism (Worrall, 1989). Yet it seems that any kind of reason for realism presupposes a concept of success in some sense. Popper, for example, actually uses one. Although Hacking avoids characterizing his "experimental argument for realism" as inferring the reality of theoretical entities from success, it is clearly based on a certain experimental notion of success (Hacking, 1983, p. 265).

The question is, then, whether Laudan's notion of success the only possible one for a realist to accept.

I have mentioned by (C3), a non-holistic way of treating different items in a theory: there is no standard or method which can universally apply. For different purposes, fields, levels, times, objects and requirements of scientific investigations, there have been many different kinds of notions or criteria of success. The significance of success of robotics in modelling human behaviours certainly differs from that of physiological theories' success in accounting for the behaviours. The criteria of success, as methodological standards, evolve with their interaction with practical scientific achievements over time.

So it is not plausible that the realist will use, and has to use, Laudan's concept - "working well" - as evidence for her claim about a particular entity. The realist will not, for example, claim that empirical successes of Newton's theory are the reason for the existence of absolute time and space. Nor would the realist want to say that the fact that a computer's modelling of some functions of the human brain "works well" can be taken as a reason for proving how the human brain really works. Laudan's generous notion of success is inadequate, it is both too wide and too narrow - it covers irrelevancies and neglects necessities.

But how are we to find a notion of success which is required by realism about entities? It appears to me that the correct way is to restrict the notion more, to obtain a special one for the special case. Worrall thinks that if we restrict the term "mature science" to refer to only those theories which enjoy some novel predictive success, Laudan's list of difficult cases for the realist can be pared down considerably (e.g., the caloric theory of heat did not get any such success) (Worrall, 1989). Of course this notion should not exclude some well-known standards, which Kuhn listed, such as empirical adequacy, consistency, coherence, and so on. They constitute a preliminary condition for any theory to be successful. So a good reason, generally speaking, should have satisfied such criteria, which will usually indicate the degree to which a theory receives conceptual and empirical support from its background knowledge. I shall call this preliminary condition which contains Worrall's requirement a "maturity condition" on a hypothesis or a model.

Now the point is, what is the distinct feature of the notion of good reason, or success, as required for the purpose of realism about entities?

As one might expect, our reformed "reasonableness," RR, has suggested some clues: ideally, a good reason for a hypothesis should be the best support it could possibly obtain. But what kinds of support could it possibly have from evidence? Laudan lists four major kinds: a hypothesis can (1) be logically compatible with the evidence; (2) logically entail the evidence; (3) explain the evidence; and (4) be empirically supported by the evidence. He correctly holds that (4) is the most important and difficult one (Laudan, 1990, pp. 275-277). But the key question is, how do we know that an empirically evidential support has been established? Laudan does have a suggestion about this. He holds that this will not come from (2): "Equally, theories may entail evidence statements, yet not be empirically supported by them (e.g., if the theory was generated by the algorithmic manipulation of the 'evidence' in question)"(p. 275). This suggests that the empirical support for a theory should come from physical considerations, rather than logical or mathematical inferences, about natural processes in the light of which the theory is invented to account for the cause of the evidence. In this way Laudan is close to the realists like Boyd, Hacking and many others, with regard to the ultimate role of physical (causal) laws in forming (good) reasons for any belief about the world. As is shown in the last chapter, this consideration is an inevitable consequence of the problem situation in the realism debate shaped by the reforms and shifts in the views of observation, knowledge, reasonableness, and the world. So, finally, guided by the recognition of the contents of RR, we come to the conclusion again that the very foundation for our empirical beliefs in micro entities is empirical consideration of causal relations. That is, if based on this foundation, acceptance of claim (a) might reasonably lead to acceptance of claim (b).

However, the above key question still remains in this form: How do we know that a certain required causal relation has been found? Or in other words, what constitutes a good reason for us to know that this foundation is laid out?

Since Hacking's work in his (1983), many people have recently paid more and more attention to the role of experiments in providing arguments for realism[1]. It is in fashion in the philosophy of science now to argue for realism by studying experimental practices of science in detail. This approach is called the experimental argument for realism or simply experimental realism. Of course, asking for experimental support as a constituent of good reason is not a new idea. The distinct nature of this experimental approach is to judge the foundation for realism about an entity by looking closely at whether, inside an experimental system, an understood physical relationship connecting the entity and some other physical objects is determined and the way it is determined.

According to this approach, an experiment detecting an entity could be successful only if the causal relation desired has been established or sought successfully in the experimental system. Scientists often talk about observation of micro-entities as evidence for their existence. Shapere reports that scientific usage of the term "observation" or "see" can be characterized as follows:

> x is directly observed if (1) information is received by an appropriate receptor and (2) that information is transmitted directly, i.e. without interference, to the receptor from the entity x (which is the source of the information) (1984).

This is to say that observation is an experimental process of finding, preserving, and transferring the causal reaction of an entity. It can be inferred that "direct observation" is a function of the reliability of the experiment in finding the causal process.

Surly Hacking is a representative of this approach. He holds that the directness of observation in the case of theoretical entities is eventually a matter of the reliability of scientific ways of distinguishing outside causal effects from artifacts of the observation systems themselves. Hacking's example is microscopes. His idea is, first, that what makes a scientist suppose that the images produced by his microscopy are "true" is not his theoretical beliefs about the entity, but his belief that he knows how to put aside

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[1] Among them are, to name a few, Armstrong, 1983; Cartwright, 1983; Franklin, 1986, 1990; Giere 1988; Galison, 1987; and Worrall, 1989a.

distortions - that he knows how to tell if the images are simply artifacts of his microscope (Hacking 1983, p. 200). Hacking holds that the crucial point for treating something as observing rather than inferring is that the theory of the causal mechanism of the entity is not related to the knowledge of how the experiments are working.

In a similar spirit, Hacking gives two warrants to assure us that what we see through a microscope is a "real thing" - some element in a cell, for instance - rather than an artifact of the microscope itself. The first is that if we could observe this element, which is unobservable by our senses unaided, in two or more totally different kinds of microscopes such as a low powered electron microscope and fluorescent microscope, then we could assert that it has been seen and is proven to exist. For electron transmission and fluorescent re-emission processes " are essentially unrelated chunks of physics. It would be a preposterous coincidence if, time and again, two completely different physical processes produced identical visual configurations which were, however, artifacts of the physical processes rather than real structure in the cell" (p. 201).

The second point is similar to G. Maxwell's argument - called the "naturalist argument" - about the continuum of vision between looking through a window pane and looking through a high power microscope. Hacking, using what he calls "the argument of the grid," tries to show that this continuum exists. Again, he believes that such a continuum is eventually shown to be real by the fact that we can check the result with any kind of microscope, using any of a dozen unrelated physical processes to produce an image. "Can we," he asks, "entertain the possibility that all the same, this is some gigantic coincidence?" Certainly not, he says. " To be an anti-realist about that grid you would have to invoke a malign Cartesian demon of the microscope" (Hacking, 1983, p. 203).

Observation, however, is just one and not the most important element in the whole argument for realism, says Hacking. Now here comes Hacking's manipulability criterion: the most important element is "manipulating," rather than "seeing" - manipulating a theoretical entity to produce a new phenomenon is the firmest proof of the correctness of the understanding of its causal power. He says: "The best kinds of evidence for the reality of a postulated or inferred entity is that we can begin to measure it or otherwise understand its causal power. The best evidence, in turn, that we have this kind of understanding is that we set out, from scratch, to build machines that will work fairly reliably, taking advantage of this or that causal nexus...Hence engineering, not theorizing, is the best proof of scientific realism about entities" (p. 274). "The 'direct' proof of electrons and the like is our ability to manipulate them using well-understood low-level causal properties" (p. 274). In his (1988), Giere says that he reached the same idea independently at about the same time as Hacking did.

3.3 The Experimental Argument for Realism

I do think that such an experimental approach has to a large extent reflected the practice of science. It is in the right direction where we may find an answer to the key question of what constitutes a good reason for realism. One major merit of the approach is, of course, its emphasis on causal interaction and activities - doing - as the ultimate basis for understanding why many scientists really are realists and whether this is reasonable. That is, the basis we should use for deciding wheher scientists are realists is not what they say, but what they do. Hacking has stressed the importance of this in his recommendation that philosophers shift their focus from theories to experiments or from language to activities (Hacking, 1983). Giere also spells out the importance of this approach. He says that some empiricists like van Fraassen would admit that scientists have to use a realist style of language to talk about their theories or models or whatever. But, such empiricists might declare, "we should understand scientists as operating under a 'supposition' that this is what the world is like" (Giere, 1988, p. 130). Scientists speak in this way like actors in a play: "It is true *in the play*, that the butler did it and false that the wife did it. But this is all only within the supposition of the play"(p. 130). "This reply," points out Giere, "suffers from the philosophers' preoccupation with language to the exclusion of causal interaction with the world" (p. 130). And if it can be extended to activities as well, says Giere, "this response is totally ad hoc, and in the end vacuous. Except for the desire to save an empiricist account of science, there is no reason in the world to think that activities in the laboratory are part of a grand supposition. If that were true, then, indeed, all the world is a stage. All our lives are nothing but complex sets of suppositions. This is a strange conclusion from some who would 'deliver us from metaphysics' (van Fraassen, 1980, p. 69)" (p. 130). That is, such an empiricist excuse in cases of theoretical entities will lead to a conflict with van Fraassen's own non-metaphysical view of philosophy of science (1980, p. 82) and the presuppositions of the realism debate accepted by him through his position on observable objects.

On the other hand, however, although Hacking emphasizes the decisiveness of "engineering," he also points out the importance of understanding, which is on the ground of "low-level generalizations of causal properties" of the entity, instead of theoretical concepts. That suggests that understanding or knowing, should be part of good reasoning, too. I will defend this approach in this thesis both by arguing against some criticisms to the approach in this section and by trying to improve it, since it is not, I think, perfect yet. In this way a wider and more detailed comprehension of the practice of science is developed and embedded into the notion of a good reason. Let me question Hacking's criteria first[1].

Hacking implies that direct observation alone can sometimes be enough for supporting realism about an entity at issue, while manipulability is always decisive. This shows that he tends to take manipulability as a sufficient condition but not a necessary one. In his (1989), Hacking says that his experimental argument is the most compelling one, but not the only one, for realism about an entity.

Presumably, since different roles have been given by Hacking to observation and manipulation, it appears that observation should be viewed as a less convincing reason for realism. But what is the ground for such differentiating? I have pointed out that like an experiment which uses or manipulates an entity, an observation could constitute a support just because it is a process based on a certain causal interaction between the entity and some other objects. In short, an observation could be a support for realism for

[1] Hones (1991) also holds that although Hacking's view of realism fits the practice of science, such as high-energy physics, it lacks more details to be satisfactory.

just the same reason as manipulation. In this way they are on par. Hacking suggests that a manipulating experiment is one using the causal power of the entity to create other entities or study other parts of nature, while most observations or experiments designed for detecting entities may just use other entities to touch or influence them. This is a difference between an experiment "with" an entity and that "on" an entity, as some people call it. It is not clear why this difference matters in deciding their roles in constituting a good reason. Obviously in the case of micro entities, observations or detecting experiments also need to control or interfere with the entity at issue - we can observe some entities just because we can interact with them. And more often than not, in an observation, the procedure of identifying an entity as a necessary part in a causal process or a "link" between two parts of a causal process or chain by forming such a causal interaction often cover a step of "manipulating" in Hacking's sense. In this procedure, we might start not by exercising the very entity, Y, in question, but by manoeuvring another one, X, to produce Y; Y is the effect of X. Then in turn Y will produce its own effect, Z; Y is the cause of Z. In this X-Y-Z chain, Y is a link as both an effect and a cause. We might only be able to make X and Z visible on photographs, but all physical laws and knowledge assure us that we have manipulated not only X, but also Y, to produce Z. To be clear, such a "causal link" identification is required by many observations, and it involves Hacking's manipulation step. Hence simply taking this role of an entity in a causal interaction as the foundation for believing in the reality of it should be better.

Moreover, some important concepts in Hacking's argument need clarification. Here I try to make two of them more transparent: one is "manipulating" and the other is "low-level causal generalization."

When one says: "I am manipulating a DNA fragment, x," one thing I want to know in order to be sure that is the case is, of course, if indeed it is that fragment, x, instead of any other thing. Yet how do we know that that *is* x, not y? Clearly just telling its name, x, is of little use because we may not know to what that name refers. A way which appears plausible to me for us to identify a thing or to know that we are talking about the same entity is, again, by its distinctive causal character. Actually a name, like

"DNA fragment x" or "electron" is used to stand for its different properties and distinctive causal effects, if it is interacted with directly: different length (number of bases), particular kind of structure (sequence), special genetic function, and so on. In other words, what a name stands for is "something" corresponding to a set of special quantities, which no other entity possesses, and which will have some unique causal effect if we interact with it in an appropriate way. In short, therefore, saying "x" exists is to refer to the existence of a kind of a particular situation involving a causal "potentiality." So when I assure others that I am really manipulating x, what I have to show is that I have used *its* distinctive causal properties to bring about some outcome which I could not have brought about if I had used any other entity. So, if I produce something that y could produce, and if I have no way to tell which is responsible in this case, I can not say I have manipulated x. If I have made some change in certain cellular phenomena, for example, which could possibly be made by cellular method even without assuming any distinctive property of a component of the cells or which does not show any distinctiveness due to using any postulated causal property of the component, then even if we could assume that the cells have this component like DNA molecules, or even atoms, we can not say that we have used the component's causal power, nor that we have manipulated it. This is commonsense: throwing a chair to break a window does not mean manipulating some atoms, even though we think the chair is made of atoms.

This also tells us something about what "manipulating" may mean. A minimal requirement for us to say that we can manipulate x includes: taking a physical causal chain to interact with a distinctive property of x and then, by this, to act on another thing so as to produce a distinctive outcome. This is enough for our purpose here.

Now as for "low-level causal generalizations," they are also called "home truths" by Hacking, suggesting that they are both close to experience and uncontroversial. But, considering how theory-laden even observation reports can be, more needs to be said about how to decide that a certain low-level causal generalization is uncontroversial; about form in which they exist in a theory; and about the way they can be distinguished from other elements as the only relevant factors for realism.

In Watkins's (1984), a five-level structure of scientific knowledge is suggested, as follows:

level-0: perceptual reports of a first-person, here-and-now type;
level-1: singular statements about observable things or events:
level-2: empirical generalisations about regularities displayed by observable things and events;
level-3: exact experimental laws concerning measurable physical magnitudes;
level-4: scientific theories that are not only universal and exact but postulate unobservable entities[1].

One thing needs revising here. As far as one can tell, Watkins could mean to include as a scientific "theory," as Popper has done, simply a single universal statement. I would rather take it to mean something more comprehensive, and I prefer the "model view" of the structure of theories (van Fraassen, Cartwright, Giere, etc.): a theory consists of theoretical (or Cartwright's fundamental) presuppositions or laws and one or more descriptive models which in turn are made of phenomenological laws and statements like those from level-2 and level-3 (but not from level-0 statement[2], as Margenau said). Level-1 statements are often experimental reports, and not part of theories.

Now where are those low-level causal generalizations placed? Presumably at levels 2 and 3, for I found that the most adequate unit in a theory for displaying an entity's causal power or property is its causal model. (I will discuss this fully in chapter

[1] This kind of division is not adequate in many ways. For example, it does not display the differences among generalizations from various kinds of perceptions (private, ordinarily public and machine-assisted public perceptions). Nor does it indicate that descriptions or statements involving a theoretical entity could be at different levels within a theory. Also, it could too easily remind people of the inductivist view of science, with scientific discovery an upward movement from levels 0 to 4, and it does not suggest the historical downward movement of the observable /unobservable distinction. But this is another issue. I use Watkins's table here just as an illustration.

[2] Cartwright correctly points out that "We have a very large number of phenomenological laws in all areas of applied physics and engineering that give highly accurate, detailed descriptions of what happens in realistic situations" (1983, p. 127).

5 by talking about Rutherford's model of the atom and the Watson-Crick model of DNA structure.) Such a causal model often consists of a core assumption about the existence of an entity. Since the entity is "unobservable," the assumption is at level-4; it is a "theoretical statement." Yet besides this assumption many laws and hypotheses are needed in the model to describe a kind of causal property of the entity. Believe it or not, all these are from levels lower than level-4. Some are experimental laws, and some generalizations about observable things and events. This is where the "postulate of identical causality" works - people use laws applying to observable things to describe some causal property of an unobservable entity. This is exactly the case of Rutherford's model of atom: the laws used in the model are those from classical mechanics concerning the force between two moving solid balls. These lower level causal laws play a decisive role in describing the situation in which an entity exists - the situation involving the causal tendency of the entity and thus implying the reality of it. And in turn, the causal model is a form in which low level causal generalizations exist in a theory.

Hacking's "understanding" or "knowing" also includes the laws used in experimentally manipulating the entity. These laws play the role of modelling the causal power of the entity in an experimental system - modelling in terms of the laws of the ways experimental instruments function. It is these laws or "theories" of the apparatus that will connect the causal properties described by level-3 and level-2 laws with observed effects reported by level-1 statements (readings on a scale). Hence it is easy to see that these laws or theories of apparatus will not be higher than level-3. So the two kinds of causal laws, one is for the descriptive model, another for the experimental model, are both more likely at a lower level than that of theoretical assumption. Thus my indicator of what is a low level law is (1) being a causal law; (2) being used in either of these two kinds of model; and (3) have been long confirmed experimentally in different ways. In short, those causal laws which are used in the two kinds of descriptive models and which have been confirmed independently can be distinguished as low level ones. This is a viable indicator, since models are units of theories, and they are intuitively, at least, recognizable[1].

Let me consider the issue in another way. It could be said that any generalization talking about a micro entity should be regarded as a higher level statement in a second sense[2]: as a universal claim, it goes beyond individual experience; and it postulates the existence of a causally effective entity which is not directly observable to our unaided senses. (In this way I say that a causal model does contain some high level assumptions.) The first sense does not bother me since our RR allows for the move from individual observation to independent existence. The second sense actually means that the generalization is high level one just because the entity is an unobservable entity, as Watkins suggested. So, the division of high/low levels is a matter of observability by unaided senses: all statements about observables in this way are low level and all about unobservables high level. This kind of division by unaided eye is simple, but too naive to fit scientific practices. As I argued, no scientific reason is available to make the division clear-cut. From small pieces of biological tissue to cell, cell membrane or nucleus, ... and macromolecules, I do not see where to draw a high/low level line and, especially, what philosophical significance concerning the issue of reality such a line (between cell and cell nucleus or between Brownian particle and DNA macromolecule?) could have.

Some might say, on the other hand, that theoretical presuppositions using a term denoting a mirco entity are high level one because they could be regarded as high level by some independent standard, whatever. But first, as we know, not every term involved in a theoretical statement must be theoretical. Second, such involvment of the term in a theoretical statements does not mean that the term itself will make every statement containing it theoretical (high level). Hence, still, a statement about a micro entity could

[1] Of course, the distinction is relative. And this just covers some models since we need to argue only that some entities have got good (the best) reason for being believed in.

[2] There might be other ways that a law is called theoretical or "fundamental." One example is idealization.

be at low level.

The third point brings us back to our experimental approach. Even if we say that any theory about a micro entity is always a high level one, the existence of a causal model displaying its causal role in a physical process and some convincing experiments implementing the process and identifying this causal role could make us take the item out of the theory as part of our existing causal net - namely, as real. That is, if we could display conceptually and practically its causal role, the generalization about it can stay at a high level but it itself can be selectively discolsed as real. After all, it does not matter very much whether the causal laws of experiments which implement a physical process in which the entity in question is found to be a necessary link are high level or low level ones. They are causal laws; they are those that are also used to show the reality of even macro entities. Given this recognition, insisting that they are high level for this or that reason is just a verbal matter, as long as "high level" does not mean "unrealistical." This is all I need to clarify the idea of low level generalization.

In chapter 7 I will avoid one more weakness in Hacking's argument. Although he reveals the importance of the use of theoretically unrelated techniques in determining the reality of theoretical entities, he does not indicate the role of some techniques which have the same rationales as those in the theory of the entity at issue. Both techniques with different rationales from the theory of the entity and those with similar ones to the theory can be the components of the framework of an experiment, and both can constitute ways inside an experiment to check the process and to interact with the entity. Once considering the inner mechanism of an experiment itself in checking the process, it will be evident.

While the experimental or causal approach to realism has some problems, it is worth defending and can be made better. I shall show this by discussing some recent criticisms aimed at Hacking's criterion. So far I found two major criticisms aimed at the experimental argument from McKinney and Morrison. I consider McKinney first.

In his (1991), McKinney claims that as revealed by his case study of the polywater episode in the history of chemistry, the distinction between experimenting on some entity and manipulating that entity is best seen as a distinction between experimenting on some entity and experimenting with that entity; and Hacking's manipulability criterion is not a necessary condition for realism: scientists can, and do justifiably change their minds as to the reality of theoretical entities.

At first this claim sounds trivial, but let us go on. I have already said that the point is not whether we can experiment "with" or experiment "on" an entity, but is the success of exercising a well-understood causal interaction experimentally. Yet McKinney means something differently when he argues that Hacking's criterion is not a necessary condition. This is shown by his use of the case of polywater.

According to McKinney, the case of polywater shows that "Scientists were justified in believing, for a short time, in the reality of polywater without ever using its causal properties to experiment on other more hypothetical parts of nature.... there was never a doubt that the production of anomalous water and the measurement of its physical properties were valid experimental results... However, the interpretation of these results was greatly mistaken. The material was not polywater, it is now considered to be an experimental artifact" (pp. 298-299).

First of all, two things need to be noted. What does "the measurement of its physical properties" really refer to, namely, the strange effects of the water or the causal properties of a novel micro-structure of the water? And to what purpose is the experimental result valid, i.e., is the result valid for establishing the existence of new kind of water of a novel micro-structure? A serious confusion of these aspects occurs in McKinney's use of the case.

Let us analyze the case in terms of McKinney's description. In 1962, in the Soviet Union, it was reported that an anomalous form of water was discovered. One of the astounding properties of the water is that it had a viscosity approximately fifteen times that of normal water. Another is that it exhibited a density of 1.1 - 1.5 g/cm^3 which is significantly greater than that of water. Believing that their sample of water was pure enough, the scientists were sure that the anomalous phenomena of the water were not caused by contamination, but because it is a new kind of water, called then polywater, which should have a new structure of hydrogen and oxygen atoms. So the polymeric hypothesis of this new structure of the atoms was proposed.

Then was there any eligible experiment on or with the micro-structure? Mckinney just mentions one, infrared spectroscopic evidence which "seemed to confirm that the new substance contained just hydrogen and oxygen"(pp. 301-302). Note that this was actually the same kind of work as those done before by the Soviet scientists to confirm the purity of the water sample. By Hacking's criterion, this result was not valid for confirming the existence of any specific structure of the atoms. Actually, as McKinney noted, it was later found not even to be valid for establishing the purity of the sample (p. 302). From the very beginning, this evidence was by no means that which confirmed the proposed novel structure which was thought to be the cause of the anomalous properties of the water. All these experiments, therefore, were "valid" for showing that the water sample, not the molecular structure, indeed had such chemical features, but not valid for establishing the polymeric hypothesis. It is not the case that there was a valid experiment for the hypothesis but interpretation failed. So it is not correct for McKinney to assert that the most common method, infrared spectroscopy, "presented scientists with the bulk of their evidence for a new stable structure of two hydrogen atoms and one oxygen atom"(p. 304). Only the experiment which could interact with, and display the proposed character of the structure is valid for this assertion. Indeed we can assert that "scientists were able to experiment on anomalous water" (p. 304), but it differs from asserting that they can experiment on its postulated micro-structure. An experiment on a table is not equal to an experiment on its atomic components.

Again, according to McKinney's description of the development of the case, presumably the experiment producing the infrared spectroscopic evidence was not strong enough for Hacking. Only about 8 years later, the polymeric hypothesis was abandoned; the proposed structure was regarded untrue, and the anomalous properties of the sample were found to be caused by contamination. This was just the result of repeating the test of infrared spectroscopy and some different experimental tests with unrelated techniques and methods, including electron microprobe and mass spectrometer. This indicates that the original methods for checking the purity of the sample were not reliable according to Hacking's idea that different methods shall be used for ruling out the possibility of artifact. The experiments based on these methods were not as good as those experiment systems which have a mechanism of self-checking, self-comparing and self-confirming by combining a diversity of methods and techniques into them.

McKinney asserts that it is reasonable that the scientists did believe, for eight years, in the reality of the polywater because the model (the polymeric hypothesis) accounted for some experimental data, although the explanation was later found wrong. He then asserts: "there certainly seemed to be good reason to believe in polywater" (p. 306). The "good reason" here obviously refers to the success of the model in explanation. In this way he concludes that "Reason for belief in the reality of theoretical entities are not iron-clad guarantees of ontological truth" (p. 306). I bet no realist will think that is a "good reason." Hacking's experimental argument for realism remains unchallenged.

One final remark about McKinney's argument. He thinks that the term artifact refers to two different results: one is, like aberration, an extraneous *instrumental* effect, so this artifact is an invalid experimental result. But since the term basically refers to "the result of some human intervention"(p. 296), another type of the referent of the term can be something like the contamination of a sample or the introduction of a cellular structure that does not exist. In this context, the tests of the anomalous water were valid in the sense that the results had no instrumental effect, and they found finally that "the cause of its anomalous properties is contamination, not an altered structure of normal water," so this contamination is a part of the valid experiment. Calling anomalous water an "artifact" is, then, a decision made at the level of interpretation. So McKinney says: "Hacking's experimental realism does not adequately take into account the conceptual frameworks within which scientists judge whether theoretical entities are real or mere experimental artifact" (p. 296).

McKinney's argument has some problems. I shall just mention two. First, let me clarify the meaning of "artifact" in the issue of realism. Artifact denotes a certain kind of experimental result which we have found could be created by the experimental system itself, even without the existence of *a given* postulated entity. This stipulation implies three consequences: (1) "artifact" does not generally refer to contributions of our human intervention or effects of instruments - at micro-level, what observation can be done without human intervention in the objects or instrumental effects? - but refers to the result that is created by an experimental system alone. (2) an artifact is relative to a given entity which is the goal of an experiment. So changing the goal of the experiment would lead to a change in what will be regarded as artifact in it. And (3) if the system alone can create the effect, it shall be asserted that no other cause exists for the result. By this stipulation, things like aberration or photographs of the tracks of electrons in cloud chambers are not artifacts, although there are so many instrumental effects in them. On the other hand, since the anomalous properties are not the effects of the postulated structure - no such structure exists - but caused by the contaminants which exist in the water sample as part of the experimental system, to the postulated structure, the properties are artifacts. The experiment taking the artifacts as the structure is invalid.

Second, again, finding that the anomalous properties are artifacts -the cause of the anomalous properties is the contaminants rather than the postulated structure - is the work of experiments, not the result of interpretation. McKinney does not say anything about why the finding is not an experimental result but a result of interpretation and what conceptual framework was involved in the interpretation.

Now let me turn to Morrison's challenge to the experimental realism in her (1990), since it appears to be more challenging.

Morrison first questions Hacking's view of "understanding" which consists of low-level generalizations (and home truths) about their causal properties. She claims: (1) the low-level characterizations are not sufficient to describe the way in which entities like electrons interact in concrete situations, much more complex theories are needed to describe such an interaction. (2) if more elements, which are theoretical, are needed for the purpose, "then due to their instability we face the problems that beset theory realism" (p. 16). And (3) these "home truths" are connected one way or another with high-level theoretical elements, so "What allows us to distinguish between" them? (p. 6).

My objection to this argument is about (1) and (2). As for (3), I have indicated a way, a causal model way, of telling which are low level "home truths," and I will fully display that in the following chapters.

Morrison's example is the interactions of particles in cloud chamber. In order to know that the causal process happens in it, we need many theories. Electromagnetic theory, for example, is needed to supply "the conditions under which electrons can ionize atoms." Mechanics is needed to provide "information about the electron's collision with the gas atom. This together with other information from heat theory and atomic theory allows us to make claims about the behaviour of electrons in specific circumstances" (p. 7). Hence "knowing how and why the electron behaves as it does requires more theoretical presupposition than Hacking is willing to acknowledge" (p. 8). Morrison asserts: "If we insist on being anti-realists about the theories used to operate detectors and accelerators while maintaining a realism about electrons or other subatomic particles, it is difficult to articulate what this form of realism would commit us to,..." (p. 7).

One problem is that Morrison has never cleared up all of the following points: what she means by theory, or theoretical presupposition; what and in what way theoretical presuppositions must be involved in the description of the interactions in the chamber; what elements in a theory shall be taken as high-level one or low-level ones and what part in a theory for operating a detector shall be understood unrealistically. This makes her argument lead to some vague and implausible consequences.

For instance, since she does not stipulate what is necessary for Hacking's "knowing," she can always ask for more as a necessary part of it. In the end, she in effect suggests that unless we know everything about an entity, we could not know or describe any causal interaction of it with any other object; and, therefore, no experiment is possible in exploring (manipulating) even one causal property of it without the involvement of "unrealistically understood theories of the objects and apparatus."

This is evidently incorrect. First, it is obvious that for the purpose of knowing the existence of an entity, we do not need to know everything about it. "Knowing" for this purpose is not a full-fledged theory or description of everything about the entity. Nor is it required to know all causal properties of it in order to decide its existence. Even before the property of spin of electrons was found, most scientists had accepted it. What is required by realism for this kind of knowing is, to say the least, a description of one kind of causal interaction of the entity with some other physical objects, namely, as I said earlier, a realistically intended model of a causal process in which the entity is required as a link between some parts of the process. Such a model neither needs to describe every causal property of it, nor will it inevitably involve theoretical interpretation. There is a big difference between what we need to know whether there is a causal reaction or how an entity exercizes its role in it - this is basically an experimental question - and what we need to account for why there is this reaction - this might, sometimes but not very often, lead to a question of how to put it into a high-level framework. Morrison's confusion of these two aspects shows also in her grasp of the case the ruby laser (p. 16).

Another problem is that it seems to me that she just takes anything with a name of "theory" as something definitely opposed to what Hacking calls low-level knowledge. She takes in effect anything from different fields as high-level theoretical - equating "different" with "theoretical" and even "unrealistic." She even claims the we can insist on being anti-realists about the theories used to operate detectors and accelerators. Yet the fact that more elements from other fields are needed for describing a causal process - this is often the case, so she is right here - does not demonstrate that they are high level ones or high-level theories are necessary, nor will it hinder a claim to the reality of the causal process. In her case of cloud chamber, the laws from mechanics, for instance, about the collision of electrons and atoms are the laws of elastic bodies, which are well-founded laws of experiments, as opposed to parts like the concepts of absolute space and time. It can be argued that theories or laws of apparatus, of detectors accelerators are not higher than level-3, since they are mainly used for modelling certain causal processes in order to transfer causal properties given by level-3 and level-2 descriptions to phenomena reported by level-1 statements. It is just incorrect to call them high-level theories and even claim an unrealistic interpretation of them.

Again, according to (C2), in science the distinction of high/low levels in a theory changes with the development of our ability to interact with things at deeper levels of the world. As van Fraassen admits, observability is a matter of science. When Cartwright's "highly confirmed phenomenological laws," which constitute causal descriptions, can be about something like movement of molecules in air (Cartwright, 1983), she has assumed a dynamics of the distinction. So has Feyerabend when he claims that this is a factual statement that the Brownian particle (which is of course unobservable according to van

Fraassen's distinction) is a perpetual motion machine of the second kind and its existence refutes the phenomenological second law (Feyerabend, 1975, p. 39). Hacking and Shapere have explicitly stated this thesis (Hacking, 1983, p. 168). In today's particle physics, experimental laws such as that about the electromagnetic interaction have been taken as home truths - without them, a large part of contemporary physics is impossible. Contrary to Morrison's claim that "the characterizations of micro-processes rarely enjoy the kind of stability exemplified at the macro level" (pp. 15-16), the laws about the causal process in cloud chambers, have already become so basic for science that removing them as unrealistic will be very harmfully influential.

Let us take Morrison's ions as an example. When the ions were used in cloud chambers in 1912, they could be created and detected more than one way. Different causal processes of ionization were used by electroscope in studying radioactivity, in scintillator as used in Rutherford's experiment in 1910, in Geiger-Muller tubes (1913 and 1928), in cloud chambers in 1912 (the use of cloud chambers as particle detectors began in 1912 or so), and now in bubble chambers in 1952, and in spark chambers since 1959. These were based on the ions' different properties, like their ability to increase electrical conductivity (the electroscopes and Geiger tubes) and to combine with electrons and in doing so emit light (scintillations). So, contrary to Morrison (p. 9), if some nature of ions as condensation nuclei was in doubt, their existence was not, because of the exercising of these causal interactions. There interactions have long been regarded as the firmest bases of a variety of experiments. They have never been doubted as factitious ever since. The causal laws are stable and "home truths" in all sense of the words, although theoretical characterizations of these properties may be complex and changeable. Consider the electromagnetic interaction found by Faraday in 1831, the causal relation between the motion of a magnet and the current in a near coil has never changed whatever the interpretation had been. In order to decide the existence of causal interaction and the cause of the current in the coil, for example, we do not need to know something like Maxwell's equations, let alone the concept of electric aether which was ignored by Faraday but greatly used by Maxwell in developing his theory (Maxwell, 1862). In his (1832), Faraday made a conclusion about the reality of induced electromagnetic force, a theoretical entity in his time, only by stating the causal connection between electric current and magnetism after phenomenologically describing the apparatus and processes of his experiments (p. 127).

Thus the fact that an experimental investigation was motivated or interpreted by a certain framework does not imply that the experiment must have it as "the fundamental presupposition" (p. 9) in order to describe a causal process of the entity in question and to know the existence of it.

So the stability of low-level causal generalizations at the micro-level is a fact. That is why they can relatively be isolated from theoretical interpretations or assumptions. This is due to the progressive character of experiments and technology. Of course in principle we can have axiomization of a theory in a way that any theoretical assumptions can be made necessary, together with observational laws, for deducing a certain prediction. But it is evident that confirmation of a prediction does not extend its evidential support to every item connected by a logical way to the process of making the prediction. Logical relation between two items does not necessarily transfer evidential relevance. Some parts, usually the low level parts, are much easier to be confirmed than other part by the same evidence.

It is important to know that different theories may be at different levels. Modern molecular biology theory, for instance, is much more experimental than some physical theories. The highest level theoretical assumptions in biology are at the level of molecules (groups of atoms), which have long been taken for granted as phenomenological descriptions by elementary physics. In the experiments of molecular biology, therefore, it is safe to say that what we need for understanding the experiments could not involve more than low-level generalizations. Causal models in this field are purely phenomenological. And it is very hard for scientists to accept that "theories" used to operate gel-electrophoresis or autoradiographs shall be understood unrealistically.

The most serious challenge by Morrison is that Hacking's manipulability criterion is not a sufficient condition for the realism. She claims in effect that even if we can manipulate an entity, we can still reasonably deny its reality (p. 18).

Morrison has an example for this claim: charmed quarks. The example shows, according to her, that in investigating the nature of charm, quarks had been manipulated but people still did not believe in them. Let us take a look at what she takes to be the indicative of manipulation of quarks. She says:

Attempts to experimentally isolate these particles got under way in 1975 and met with great success. This would seem to confirm Hacking's manipulability thesis, except that at the time there was no consensus within the physics community about the nature or even the existence of quarks. Experiments involving quarks and the search for charm made use of photon, neutrino, and hadron beams. At the time (early to mid-seventies) low energy resonance physicists thought quarks to be the constituent of hadrons and so in that sense the quark *was* being manipulated in order to investigate the nature and the existence of charm" (pp. 12-3, her italics).

For one thing, the fact that there was no consensus about both quarks and significance of the process of manipulating hadron beams shows that the case does not meet Hacking's manipulation thesis which claims that a basis of manipulation of an entity is those uncontroversial ("home truths") low level generalizations. Also, it is not clear what she means by saying "experimentally isolating" quarks since quarks are regarded not to be isolable in the common sense of the word. Yet the most amazing thing here is that she takes manipulating hadron beams as manipulating quarks because the latter is thought of as a constituent of the former! Considering what I said earlier, i.e., manipulation of a certain entity happens only when its own unique, distinct causal properties are used by various means to produce some special action on other objects, the example by no means fits her purpose of challenging the manipulability criterion[1].

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[1] Seeking the existence of charmed quarks was carried out by finding the charm-carrying particles: if this particle had some properties predicted by the charmed quark model (electric charge 2/3, strangeness 0, and a mass greater than any of the three other quarks) then this particle might be composed of quarks including charmed ones. The first successful experiments was done in 1974 by Richter and Ting. Their finding was a kind of hadron with desired properties, called J/ψ, an electrically neutral particle, spin 1 and negative parity, which could be formed both in annihilation of an electron and positron and in interaction between protons and nucleons, and could decay either into an electron-positron pair or into hadrons. These properties can be explained well by charm model so it is inferred that the particle is a meson containing two charmed quarks ($\bar{c}c$). The experiments about the charmed quarks after 1974 were also done in similar ways by

3.4 Character and Base of Good Reason

By checking the criticisms of the experimental argument for realism, I found that realism has so far not met any serious and tenable challenge. The road is worth travelling.

I have pointed out some weaknesses of the argument for the realism and suggested the ways to avoid them and to develop the argument into a more comprehensive form. Together with the above philosophical discussion of reasonableness, I am in a position to draw a frame for the constituents of it. The full description and demonstration for it will be given in the following chapters.

A good reason consists of two main components: one is understanding or knowing; and another is doing. Understanding is based on low level causal generalizations or laws. In many cases, we recognize them by their involvement in a causal model to describe a process of particular causal interaction of an entity. Such a model is the form of their existence in a theory. So let me say that the generalizations or laws involved in such a phenomenological model are low-level ones; they are stable; and only they are necessary to the existential claim about an entity (i.e., a causal mechanism).

So the success required by realism is the success of such a causal model. For that, it shall first of all meet some general standards, which I call *maturity condition*. That is:

> The maturity of such a model is the degree of its being embedded into the relevant body of background empirical knowledge rather than a theoretical framework. The higher the degree, the more mature. This degree is measured by the model's consistency with other empirical laws, its internal coherence and its progress in getting greater support for the model's realistic status from a variety of fields.

Note: according to the relativist principle, the standards of consistency, coherence

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interaction at the level of electrons, positrons and hadrons, there had never been such a thing as "manipulating quarks" to find the existence of charm.

and progress might be understood differently in different paradigms and times. Hence, what is asked for by the maturity condition is that the model should satisfy this type of standard interpreted in its own framework. This relativist interpretation could apply to all following descriptions of good reason. As I said, the stability of realistic belief in a particular entity built by a suitable "good reason" consists largely in the lack of even inductive argument against it. However, I also support understanding the constitution of good reason by the strong notion of reasonableness. In Chapter 7, I will show the progressive character of experiment and technology and thus a positive support for realism.

The maturity condition is a preliminary requirement to be satisfied by any kind of success. We do need to apply this condition to realism about entities. For, for example, by it we can disallow logical inconsistency inside a causal model, which is different from instrumental models measuring or calculating an object for different purposes and which thus could be inter-inconsistent (also see Giere, 1989, p. 169, Cartwright, 1983). Another reason for the condition is related to the issue of the so called underdetermination, one doctrine of which says that if two models enjoy the same "empirical support," no rational way exists to choose them unequally. The maturity condition indicates the otherwise. If, for example, the two models have different degrees of making use of existing causal laws, something like one's having to postulate more new (auxiliary) hypotheses or more devices in it having been regarded "awkward" in the sense of "unnatural" in terms of existing laws and knowledge, they can be treated unequally. Hence the maturity condition implies and rationalizes roles of such evaluation as "this model is awkward so is less acceptable."

By the discussion in the last three sections, we have had some ideas concerning the requirement for the model as a distinct device in our notion of success. Now I briefly state them as *Model Constituent*:

(MC): There is a realistically intended phenomenological model showing some distinct causal properties of an entity in question by describing its internal structure or its necessity as a component in a causal structure of the world; the model should satisfy the maturity condition; it should display how the properties or structure make the entity an initiating cause of some other parts of the causal structure or why the entity is a required link in the causal structure.

It is the model that offers what we need for knowing the existence of causal process. If such a causal model occurred before experimental studies of an entity, the model would have offered a blueprint for the studies; in many cases, the studies are to find technologically viable way to implement the causal relationship described by the model. It becomes a reason for us to be confident in the experimental results. It is not necessary, however, for the model to occur as a whole before experimental work on the entity. A stable causal relationship between a substance and some phenomena could be found before we have a detailed model to describe the internal micro-structure of the substance. The model and the experiment will develop together. The final decision about the reality of the entity is always a result of their interaction. In chapter 6, I will show this by the case of the discovery of DNA.

As for "doing," it refers, of course, to experiments in exploring, singling out, preserving and transferring the causal process described by the causal model. I have stressed that "doing" refers not only to manipulating, but also observing. Once we are assured by the character of an experiment that in it we indeed interact with the entity, or what we get is not an artifact of the experiment system, then the entity becomes part of our experience, its existence is not a theoretical decision. I state the experiment requirement for us to assure this as *Experiment Constituent*:

(EC) There are experiments in which we can observe or manipulate an entity on the ground that the experiments convince us that a directly causal interaction with the entity has been reached. For this such experiments must contain a self-checking mechanism inside their systems which is composed of a variety of differently and independently alternative ways to interact with the same entity.

I call (MC) and (EC) together "Cause Identification Condition" (CIC). I believe they constitute a required notion of success or good reason for identifying an entity's causal role in a physical process and thus, given RR, for realistically believing in the entity. CIC is a sufficient condition; once a belief satisfies it, it is a reasonable one. So my argument is this: *if a belief in the reality of an entity meets the CIC, then the belief is reasonable (in the sense stated in RR); in other words, if claim (a) about the entity is supported by CIC, then claim (b) about it is reasonably implied.* As stipulated by RR, to rationally argue against it is to find in the practice of science that there is a belief which satisfies the condition but which turns out to be wrong. Are they both necessary for realism? I am not sure. But I can not imagine what else could constitute a good reason for the reality of something except for knowing and experiencing it. What I want to argue is that (MC) plus (EC) constitutes the strongest or most convincing, using Hacking's words, reason that any belief could possibly get. And I think that the beliefs in the reality of *some* unobservables do get the reason - what I have in mind as a good candidate for that is DNA molecule. I will show that finally.

The constituents of good reason, the premises of my argument, have been just very briefly stated here. In the following chapters, by studying science, I shall make them more intelligible and argue that they do fit scientific practices. I shall also explore that the ultimate foundation for such constitution of good reason, for the second level reasonableness and eventually for realism about entities lies in the natures of experiments and technology today: the tree-structure of experiments (the sign of the existence of the self-checking mechanism, as indicated in EC), the holistic relation between technology and experiments, and the accumulativeness of the development of technology.

Before I go on into science, there is one question with regard to our approach to be discussed. That is: why shall we study the problem of realism in a "metaphysical" way, that is, in a way depending on a view of the causal structure of the world, rather than in a purely logical or methodological approach? To be clearer, why shall we try to consider the reasonableness of a belief in an entity by directly looking at whether a physical relevance between the entity and some other entities has been identified in an experimental practice rather than by just looking at the logical or syntactic relation between the hypothesis of it and some observational statements? Watkins has asked this question in discussing another problem. He says although he is sympathetic to the causal approach, he would rather follow a methodological (i.e.,logical) way in solving problems

(Watkins, 1991).

People who are loyal to the logical approach have attempted to show that logical methods are able to express the establishment of the truth status of some part by certain evidence. If any one of their efforts is successful, my approach, the "model-experimental approach," to the realism problem would have lost much of its appeal since it does not aim at offering an normal pattern for evaluating our beliefs in question - it, rather, asks for a case-by-case decision by looking into the practice of science.

In the next chapter, therefore, I shall further explain why I prefer a non-logical way of identifying and describing the causal relevance between theoretical entities and their effects.

Chapter 4 Relevance and Causality

In this chapter I discuss the appropriateness of some logical approaches to determining whether a certain experimental result can be attributed to a micro-entity. I will show that these formal approaches are of little use for this purpose.

4.1 Relevance and Natural Axiomatization

As I pointed out, within the logical positivist tradition it is natural to take the relation between an entity and certain experimental results as a confirmational one between the hypothesis that the entity exists and a set of observational (experimental) statements.

The problem is, in many cases, how to find out if an observational statement is indeed relevant to a particular hypothesis in a complex theory, a precondition of knowing whether the statement helps to confirm or falsify the hypothesis. Many philosophers believe that the attempt to find this kind of relevance is bound to be fruitless. One reason is that all statements or hypotheses in the theory are connected with each other in a "web." For this reason, advanced by Duhem (1906) and Quine (1960), we cannot tell in any logical way whether a hypothesis in a theory has got support from the theory's predictive successes or not. This position was developed into a sort of radical "holism" in epistemology: whatever the illusions of practice may be, evidence can only be significant for the theory as a whole, or even for science as a whole, and so cannot be parcelled out here and there in the theory.

Yet many people forget this point: even though terms, beliefs and hypotheses in a complex theory which entails some empirical evidence are "linked" with one another, this kind of "holism" does not follow - the possibility of distributing the bearing of evidence within the theory is not ruled out. As far as I know, it has not been stated by those holists why a linkage between two items in theory must also be a linkage of evidential relevance between them. There exist various kinds of linkage among beliefs, such as heuristic, conceptual or logical ones, but they will not always deliver evidential confirmation or falsification from one to another. Scientists are by no means totally blind about the significance of a particular evidence to parts of a certain theory. Scientific reasons are often available to lead them to judge, given the evidence, which part has most probably got support or been undermined or which part should be adjusted first. Lee and Yang realized that the weak interactions might not be invariant under space inversion, charge conjugation, and time reversal because, first of all, "experimental proof has so far only extended to cover the strong interactions" (Lee and Yang, 1957, p. 1671). Glymour is surely correct in pointing out, "Much 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, p. 3).

But can we account, philosophically, for the relevance of some bit of evidence to some bit of theory? For the logical positivist, this is to ask for a logical account. Yet we know the above "holism" was a result of the failure of the positivist attempts. In particular, the positivist programs, from Carnap's to Ramsey's, of establishing a correspondence between languages of theory and observation in terms of some analytic sentences or "meaning criteria" in order to eliminate theoretical terms has never worked as they required (see Hacking, 1983, Watkins, 1984).

Another type of attempt to account for evidential relevance is made by hypothetico-deductivism (H-D, hereafter). H-D says that a hypothesis h, in a complex theory T, consisting of more than one hypothesis (including auxiliary hypotheses), can get confirmation or falsification from a piece of evidence e if h entails e (given T) but T alone doesn't. h is confirmed simply because h is necessary for the deduction of e. So h is confirmed by e with respect to T iff

i. e is true and h & T is consistent

ii. h & T entails e (hereafter, $h \& T \vdash e$)
iii. T does not entail e ($T \neg \vdash e$)

H-D, however, runs into some problems immediately. Problem (1): it allows this situation to happen: any true evidential consequence of a consistent theory T could confirm almost any hypothesis in T with respect to some subtheory. For if $T \vdash e$, $T \vdash h$, and $(h \rightarrow T) \neg \vdash e$, then e confirms h with respect to subtheory $(h \rightarrow T)$, which is a consequence of T[1].

Problem (2): if e is true and h is any consistent hypothesis such that $h \neg \vdash \neg e$ and $\neg e \neg \vdash h$, then h is confirmed by e with respect to a true theory T if T is $(h \rightarrow e)$. That means, any true piece of evidence confirms almost any hypothesis with respect to some true theory[2].

Problem (3) is called that of "irrelevant conjunction." Namely, if h entails e, so will h & g, where g is any sentence whatsoever. That means H-D will allow an intuitively irrelevant g to be confirmed. For if T, h, and e satisfy condition i-iii, then T, $(h \& g)$, and e also satisfy these conditions for any g that is consistent with $(h \& T)$. While "& g" is not used to derive e, yet it is possible to make an irrelevant g logically necessary for the deduction of e.

Glymour has invented an example of a theory consisting of Copernican doctrine C and Kepler's laws K1, K2, K3 to show this problem. In this case, although by our intuition the observation O' of several positions of a single planet, say Mars, won't confirm K3 which has something to do with any two planets, H-D can not tell the difference between K1 and K2 on the one hand, and K3, on the other hand. Glymour argues, H-D can not tell that O' just confirms K1, or K2, but not K3 by saying that because only C & K1 & K2 necessarily entails O' but C & K3 does not, for there are

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[1] Since $(h \rightarrow T) \neg \vdash e$, condition iii is satisfied; since $h \& (h \rightarrow T) \vdash e$, given $T \vdash e$, so is condition ii.

[2] Namely, let $T = h \rightarrow e$, condition iii is satisfied: $(h \rightarrow e) \neg \vdash e$; since $h \& (h \rightarrow e) \vdash e$, so is condition ii.

subtheories of T containing K3, but not the first two of Kepler's laws, and entailing O' (Glymour, 1980, pp. 36-39). That is, in axiom set T, only K1 and K2 are necessary for O':

$$T = \{C, K1, K2, K3\}$$

But we can find some alternative axiom sets which may seem unnatural but are logically equivalent to T and which make K3 necessary to deduce O', but K1 and K2 not. Such an alternative is, as suggested by Christensen (Christensen, 1983, p. 447):

$$T' = \{(K3 \rightarrow C), (K3 \rightarrow K1), (K3 \rightarrow K2), K3\}$$

According to Glymour, the problem of irrelevant conjunction constitutes the most serious challenge to H-D. One way to rule out the problem is by taking T' and the like as "unnatural axiomatizations." Glymour admits that a satisfactory explanation might be given if one could say that the hypotheses tested are those necessary for the deduction of the evidence statement from certain "natural" axiom systems. "But", says he, "the positivists had no account of what, if anything, makes one system of axioms more 'natural' than another, in any sense imaginably relevant to confirmation, and today we are no better off in this regard." (Glymour, 1980, p. 39). Glymour concludes that H-D is hopelessly mistaken; and any effort of putting additional constraints so as to remove the second and third as well as related problems will fail.

But Glymour does not think that this failure indicates that nothing syntactic works. He believes that a formal way to account for evidential relevance and even natural axiomatizations can be obtained. In his (1980) and (1983), such an account, called the theory of bootstrap testing, was developed and revised. Since this is the latest and most significant effort in this approach, let us see if it works for the purpose.

4.2 Bootstrap Testing

Glymour's theory is based on a notion of instance derived from Hempel's notion and his own "bootstrap condition". The condition is designed to allow the derivation of an instance of the tested hypothesis from the evidence, by using parts of a general theory which typically includes the tested hypothesis. (That is why it is called "bootstrap", B-S, hereafter, yet later, with the new condition *R*, the tested hypothesis is disallowed in calculations.) The correspondence between a hypothesis to be tested and a bit of evidence is given by other hypotheses or the hypothesis itself in the theory which is viewed by Glymour as consistent, deductively closed collection of sentences of first order.

The central notion used in B-S is that of a computation of a value of a quantity. Glymour stipulates that a "quantity," *Qi*, is simply an open atomic formula; a "value" of a quantity is any sentence obtained by substituting constants or definite descriptions for the variables in the quantity. Two quantities are identified if and only if they have the same set of values. A "computation" of *Qi* from *e* with respect to theory *T* is a sentence *Ti* among the consequences of *T* that determines a value of *Qi* from *e* (Glymour, 1983). A computation for a quantity will be represented by a graph, such as:

$$\begin{array}{ccc} & \underline{P(x)} & \\ \nearrow & & \nwarrow \\ R(x) & & S(x) \end{array} \quad \longleftrightarrow \; Ti\text{: } (x)\ [(R(x)\ \&\ S(x)) \rightarrow P(x)]$$

Given certain values for R(x) and S(x), *Ti*, the sentence displayed to the right side (the hypothesis "used" in this computation) will yield a value for P(x). The hypothesis is confirmed if and only if all quantities in a hypothesis can be computed in such a way.

Glymour defines his bootstrap conditions for an evidence statement *e* to confirm a hypothesis *h* with respect to a theory *T* as follows:

(1) *e*, *h* and *T* are consistent;
(2) For each quantity *Qi* occurring essentially in *h*, *T* entails a sentence *Ti* such that *e* and *Ti* jointly entail a value for *Qi*, and these vaules of the *Qi* confirm *h* according to an extended version of Hempel's satisfaction criterion *S*;
(3) There is a sentence *e*' in the language of *e* such that *e*' and the *Ti* are consistent and

jointly entail values for the *Qi* that disconfirm *h* according to the satisfaction criterion *S*[1].

Glymour claims that this account gives opposite results to each of the problems of H-D(1980, pp. 168-9). As to the problem of irrelevant conjunction, for example, the conditions do not allow *e* to confirm *g* & *h*, since *g* contains a quantity not occurring in the theory *T* with respect to which evidence *e* confirms *h*, not occurring in *h* either, and also not occurring in *e*; thus from *e* there will be no way to determine a value for that quantity (1980, p. 135).

As a proof, Glymour argues that in that Copernicus-Kepler example, by the conditions above, it can be shown that only K1 and K2, but not K3, can be tested by that given evidence (pp. 134-5).

Glymour suggests that his theory can be used to check if a quantity, such as ether or atom or the like is "unobservable" or "redundant" by showing if it has got any bootstrap confirmation: because actually those "unobservables" were just "incalculable" quantities. He says:

> ...theories that are said to be objectionable because they contain "unobservable" or "redundant" quantities are simply theories containing central assumptions that cannot be tested from recognized kinds of evidence precisely because these theories contain no means for computing, from such evidence, values for certain theoretical quantities (pp. 142-3).

I shall argue later that this interpretation is in no way suitable.

Some people argue that the theory has some serious problems. And these problems are in nature very similar to those of H-D which he tries to avoid. Among those critics, Christensen, in his (1983), gives some convincing arguments showing that the bootstrap strategy of evidential relevance is problematic, including in the case of Copernicus-Kepler. The following is one of his counterexamples.

Suppose there is a theory *T*, consisting of the conjunction of two hypotheses, one

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[1] These bootstrap conditions are first presented in Glymour's (1980), but the form of expression of them here is in Earman and Glymour's (1988) (p. 261).

the famous "Raven hypothesis" (all ravens are black):

$$h_1\text{: } (x)[R(x) \rightarrow B(x)]$$

and the another a pantheistic hypothesis:

$$h_2\text{: } (x)G(x)$$

We naturally want the evidence, a black raven, i.e.,

$$e\text{: } R(a) \ \& \ B(a)$$

to confirm h_1 but not h_2. But on Glymour's account, e also confirms h_2 by the following computation:

$$\begin{array}{c} \underline{G(x)} \\ \nearrow \quad \nwarrow \\ R(x) \quad B(x) \end{array} \longleftrightarrow (x)[(R(x) \rightarrow B(x)) \equiv G(x)] \qquad (Ti)$$

e will yield G(a) from this computation, which is an instance of h_2. The hypothesis used in the computation is a consequence of h_1 & h_2, hence is in T. Finally,

$$e'\text{: } R(a) \ \& \ \neg B(a)$$

is consistent with the used hypothesis, and would yield $\neg$G(a), which is an instance of $\neg h_2$.

Christensen points out that the problem with the B-S's account of evidential relevance indicates, again, that the crucial point is still how one is to choose which parts of a theory may be used to confirm which other part. If no restrictions are imposed, almost any bit of evidence entailed by the theory will be counted as relevant to almost any hypothesis in the theory. As shown by the above example, like H-D, B-S fails to disallow one to pick out any special class of sentence in T (any logical consequences of T, no matter how "unnatural") to confirm any other part of T. It has no way to rule out those "unnatural" axioms which do not express any intuitive regularities of nature that would occur to us as explaining the data.

The similar failure in both H-D and B-S may come from the feature common to them: each account attempts to define the confirmation relations within a theory only in terms of the theory's syntactic structure. Christensen argues that this approach, called by him the "classical approach", of determining relevant confirmation by only looking at the formal structure of *T* as a deductively closed set of sentences is hopeless.

To meet such counterexamples, Glymour, remaining loyal to his classical approach, adds a new condition, *R*, in his B-S:

> *R*: For all *i*, *h* does not entail that *Ti* is equivalent to a sentence whose essential vocabulary is a proper subset of the essential vocabulary of *Ti* (see Earman and Glymour, 1988, p. 262).

This new restriction on the bootstrapping account can rule out Christensen's counterexamples. The above example can be shown to have violated *R*: h_2 entails that *Ti* is equivalent to h_1, and the essential vocabulary of h_1 is a proper subset of that of *Ti*:

$$(x)G(x) \vdash (x)[(R(x) \rightarrow B(x)) \equiv G(x)] \equiv (x)[R(x) \rightarrow B(x)]$$

But Christensen claims that this revised version of the account is still both too tight (they rule out as absurd many confirmations which scientists commonly count as legitimate) and too loose (confirmations which meet it could still be absurd). For this, he has given some interesting examples (Christensen, 1990). Let me quote just one here.

Suppose a theory consists of the following two hypotheses:

h_1: (x)(Rx $\rightarrow$ Bx) - Raven Hypothesis

h_2: (x)(Fx $\rightarrow$ Gx) - only Gods can fly

The original bootstrap account permitted us, absurdly, to confirm h_2 by spotting a flying black raven, Ra & Ba & Fa, since the auxiliary hypothesis *Ti* is included in the theory because of h_2:

Ti: (x)((Rx $\rightarrow$ Bx) $\equiv$ (Fx $\rightarrow$ Gx))

Now condition R prevents us from using Ti to confirm h_2, because h_2 entails the equivalence of Ti with h_1, which uses only a proper part of the vocabulary in Ti.

Consider, however, a slight variation on this example. Suppose that the theory is supplemented by another hypothesis, say, that all winged things can fly:

h_3: (x) (Wx $\rightarrow$ Fx)

The modified bootstrap account now allows us to confirm the hypothesis that only Gods can fly, simply by finding a black raven with wings. Computation goes as follows (the possible counterevidence: a non-black, winged raven):

$$h_3 \longleftrightarrow \begin{array}{c} \text{Fx} \\ \uparrow \\ \text{Wx} \end{array} \qquad \begin{array}{ccc} & \text{Gx} & \\ \nearrow & \uparrow & \nwarrow \\ \text{Rx} & \text{Bx} & \text{Wx} \end{array} \longleftrightarrow (x)((\text{Rx} \rightarrow \text{Bx}) \equiv (\text{Wx} \rightarrow \text{Gx}))$$

The only difference between this and the previous example is that the direct measurement of F has been replaced by indirect measurement through h_3. Intuitively, the quantity G is still "measured" by assuming the hypothesis being tested. But while the auxiliary hypothesis used in the right hand computation is intuitively dependent on h_2, this dependence is not disclosed by the syntactic test R; thus the computation passes muster in the revised B-S account.

Christensen also shows that a theory in which some kind of "confirmation" is absurd can have the same logical structure as a theory in which a similar confirmation is reasonable. Thus any further modification which resulted in disallowing confirmations of the above form would (given his basic approach to theories) necessitate rejecting clearly acceptable confirmations along with them. Christensen, therefore, concludes that no account that takes into account only the logical structure of the set of first-order consequences of a theory will be able to discriminate between relevant and irrelevant confirmations (Christensen, 1990).

So the problem of how to pick certain axiomatizations as "natural" - as suitable expressions corresponding to, e.g., accepted causal laws - or privileged is at the centre of the issue of relevance confirmation, as realized by both Christensen and even Glymour. The current discussion seems to suggest that, without considering the content, epistemic status and historical background of hypotheses in a complex theory, logical analysis of the syntactic structure of hypotheses is of little use.

In the following section I shall discuss the problem of B-S in terms of a real-life case, with a little adjustment for the purpose. Since currently all of the cases used in the discussion might be objected to as imaginary, oversimple and non-realistic ones[1], my argument will have some unique merits. Besides, my example will show one more problem with Glymour's account. And I also hope that it can serve as a base for my discussion on the matter of relevance logic.

4.3 Bootstrapping and Electromagnetism

What I use here is Maxwell's molecular vortices theory of electromagnetic phenomena, published in his (1861-2). It was based on a model of a mechanical medium in space, constructed with the help of analogies with the mechanics of fluids and solids. He intended to determine what tension in, or motions of, such a medium are capable of producing the electromagnetic phenomena observed.

According to the model, the mechanical medium was conceived as a cellular medium, each cell containing a molecular vortex, and surrounded by a cell wall consisting of a monolayer of small particles, idle-wheel particles. When deformed by the action of the idle-wheel particles, the vortices exert elastic reaction force on the particles and thus produce a displacement flux of the particles. By hypothesizing an identity of the items, properties and relations in the model with those in the context of electromagnetic phenomena, Maxwell was able to use the model to account for all known electromagnetic facts and develop some new concepts about them, such as displacement current.

That is, in this theory, there were two models or complex hypotheses, consisting

[1] Christensen admits this, see his (1983), p. 48 and (1990), p. 652.

of a number of subhypotheses respectively: one was about the mechanism of the electromagnetic phenomena, let me call it "*Me*", the other about the characteristics of the mechanical medium, "*Mm*". The two models were parallel, connected by a set of correspondence hypotheses about the relations between the items, properties, laws, etc. in the two models. Most importantly, the logical or mathematical forms of the two models were the same; they were syntactically indistinguishable in most respects. They shared the same axioms and structure of inferences; in *Mm*, these might indicate the mechanical properties, forces or motion of the fluid or solid vortices and particles, but in *Me*, they were Maxwell's equations of electromagnetism. In fact, the majority of his inferences were made only in *Mm* with respect to the mechanism of the medium; only by the correspondence between the two models, or by giving the inferences electromagnetic interpretations, did they then turn into the laws of *Me*.

For example, in *Mm*, in terms of the mechanics of the fluid, a relation was obtained between the alternations of the vortices' angular velocities α, β, and γ (let me denote them by ***S***, in modern vector form) and the inertial force P, Q, R (***L***, in vector form) of the vortices on the particles:[1]

$$\frac{dQ}{dz} - \frac{dR}{dy} = \mu\frac{d\alpha}{dt}; \qquad \frac{dR}{dx} - \frac{dP}{dz} = \mu\frac{d\beta}{dt}; \qquad \frac{dP}{dy} - \frac{dQ}{dx} = \mu\frac{d\gamma}{dt} \qquad (1)$$

By interpreting α, β, and γ also as magnetic field (intensity) ***H*** and P, Q, R as electromotive force, i.e., electric field (intensity) ***E***, (1) turns in *Me* into the relation between changes in the state of magnetic field (intensity) ***H*** and electric field (intensity) ***E***. Here μ was explained, in *Mm*, as the fluid (mass) density, while in *Me* it is the magnetic permeability. So (1) also represents the relation between changes in ***H*** and resultant ***E***. The corresponding hypotheses were that ***H*** corresponds to ***S***, and ***E*** to ***L***. The equation becomes, in modern vector form:

[1] See, Maxwell, (1861-2), p. 475, equation (54). Maxwell here used triples of scalars, instead of vector quantities, as we do now. And also he used the total derivative notion d/dt, instead of ∂/∂t to refer the partial derivative. I shall translate Maxwell's equations correspondingly into the modern terms and forms hereafter.

$$\text{in } Mm: \qquad -\rho_m \frac{\partial \boldsymbol{S}}{\partial t} = \nabla \times \boldsymbol{L} \qquad (2)$$

$$\text{in } Me: \qquad -\mu \frac{\partial \boldsymbol{H}}{\partial t} = \nabla \times \boldsymbol{E} \qquad (3)$$

(3) is Faraday's law of electromagnetic induction in differential form. We can see $\boldsymbol{H}$ is logically equivalent to $\boldsymbol{S}$, $\boldsymbol{E}$ to $\boldsymbol{L}$, and (2) to (3). They can be interchanged both in the process of inference to observational facts and, in turn, in the process of the computation of quantities in the two models from observational facts. Thus the correspondence hypotheses can be written as $\boldsymbol{H} \equiv \boldsymbol{S}$ and $\boldsymbol{E} \equiv \boldsymbol{L}$. These hypotheses play crucial roles in the development of Maxwell's theory.

Also, the relation between the motion of particles, denoted by the density of the flux of the particles, $\boldsymbol{W}$, and the angular velocity of vortices, $\boldsymbol{S}$, in *Mm*, and correspondingly, the relation between electric current $\boldsymbol{J}$ and magnetic field $\boldsymbol{H}$, in *Me*, are the same in their mathematical form:[1]

$$\text{In } Mm: \qquad \nabla \times \boldsymbol{S} = \boldsymbol{W} \qquad (4)$$

$$\text{In } Me: \qquad \nabla \times \boldsymbol{H} = \boldsymbol{J} \qquad (5)$$

Hence, $\boldsymbol{W} \equiv \boldsymbol{J}$. The relations between the cells' elastic reaction force on the particles, $\boldsymbol{L}^*$, and the displacement flux of idle-wheel particles, $\boldsymbol{W}^*$ in *Mm* was also formally the same as that between the "displacement electromotive force," $\boldsymbol{E}^*$ and the new kind of electric current, displacement current, $\boldsymbol{J}^*$ in *Me* (Note: $\boldsymbol{L}^*$, $\boldsymbol{E}^*$, $\boldsymbol{L}$ and $\boldsymbol{E}$, were all denoted by the same set of quantities in Maxwell, i.e., P, Q, and R):[2]

$$\text{In } Mm: \quad \boldsymbol{W}^* = -\frac{1}{4\pi^2 m}\,\frac{\partial \boldsymbol{L}^*}{\partial t} \qquad (6)$$

$$\text{In } Me: \quad \boldsymbol{J}^* = -\,\epsilon\,\frac{\partial \boldsymbol{E}^*}{\partial t} = \epsilon\frac{\partial \mathrm{E}}{\partial t} = \frac{\partial \boldsymbol{D}}{\partial t} \qquad (7)$$

Hence $\boldsymbol{W}^* \equiv \boldsymbol{J}^*$; and $\boldsymbol{L}^* \equiv \boldsymbol{E}^* \equiv \boldsymbol{L} \equiv \boldsymbol{E}$. Here m is the shear modulus of the medium,

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[1] For (4) and (5), see Maxwell, (1861-2), p. 462, equation (9). Both $\boldsymbol{W}$ and $\boldsymbol{J}$ correspond to Maxwell's p, q, and r in (9).

[2] See ibid., p. 496, equation (111), $\boldsymbol{W}^*$ and $\boldsymbol{J}^*$ are f, g, and h in it.

$1/\pi m$ proportionally corresponds to the electromagnetic constant ϵ. $\boldsymbol{D}$ is electric displacement or electric flux density. Thus the relation between $\boldsymbol{D}$ and $\boldsymbol{E}$ is:

$$\boldsymbol{D} = \epsilon \boldsymbol{E} \qquad (8)$$

Based on the hypothesis of displacement current, $\boldsymbol{J}^*$, which was also regarded as the changes of electric displacement, $\partial \boldsymbol{D}/\partial t$, Maxwell developed to a crucial relation that shows that not only electric current in conductors, but also electric field everywhere, including in a dielectric, can produce a magnetic field. Still, the relation was formed in *Mm*: It was originally regarded as the relation between the angular velocity of the vortices, the intensity of the flux of the particles and the intensity of displacement flux of the particles. So again the following two equations were the same:[1]

$$\text{In } Mm\text{:} \quad \nabla \times \boldsymbol{S} = \boldsymbol{W} + \frac{1}{4\pi k^2}\frac{\partial \boldsymbol{L}^*}{\partial t} \qquad (9)$$

$$\text{In } Me\text{:} \quad \nabla \times \boldsymbol{H} = \boldsymbol{J} + \frac{1}{4\pi k^2}\frac{\partial \boldsymbol{E}^*}{\partial t} \ ; \text{ or } \quad \nabla \times \boldsymbol{H} = \boldsymbol{J} + \frac{\partial \boldsymbol{D}}{\partial t} \qquad (10)$$

k is a coefficient depending on the nature of the dielectric[2]. The excess density of idle-wheel particles ρ_ρ corresponds to the density of electric charges ρ (Maxwell used e to refer to both quantities). And the following equation of continuity of the flux of the idle-wheel particles and that of the flux of electric charges were identified in Maxwell:

$$\text{In } Mm\text{:} \quad \nabla \cdot \boldsymbol{W} = -\frac{\partial \rho_\rho}{\partial t} \qquad (11)$$

$$\text{In } Me\text{:} \quad \nabla \cdot \boldsymbol{J} = -\frac{\partial \rho}{\partial t} \qquad (12)$$

and also these were the same equation originally:

$$\text{In } Mm\text{:} \quad \nabla \cdot \boldsymbol{L}^* = \rho_\rho \qquad (13)$$

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[1] See, ibid., p .496, equation (112).

[2] k here is Maxwell's E. For the multiplex meanings of E, see Maxwell, ibid, pp. 491-5, and pp. 498-9.

In *Me*: $\nabla \cdot \boldsymbol{D} = \rho$ (14)[3]

So $\boldsymbol{L}^* \equiv \boldsymbol{D}$; and $\rho_\rho \equiv \rho$. So the main hypotheses of *Mm* are (2), (4), (6), (9), (11), and (13), etc., while *Me* consists of paralleling (3), (5), (7), (10), (12), and (14). The equations in *Me*, except (3) and

$$\boldsymbol{B} = \mu\, \boldsymbol{H} \text{ and} \tag{15}$$

$$\boldsymbol{J} = \sigma \boldsymbol{E} \tag{16}$$

($\boldsymbol{B}$: magnetic flux density or magnetic induction, σ: dielectric constant or specific inductive capacitance) are the "Maxwell equations" we need here.

Now we see there was one set of logical axiom systems shared, in terms of a set of corresponding hypotheses, by two different models, one about factual phenomena and empirical laws of electromagnetism, and the other about some theoretical entities (molecular vortices and idle-wheel particles). Yet by the logical structure we in most cases have no way to tell any of these differences. According to Glymour's B-S, therefore, we will be forced to accept, in terms of the evidence for *Me*, that almost every hypothesis in *Mm* can be confirmed in the sense that all of the quantities in it can be computed from the evidence; and in this way all of those unobservables shall have been "observed". If we keep in mind that actually Maxwell himself, who generally believed in ether, didn't believe at all such hypotheses in *Mm* as those about the existence of those tiny vortices and idle-wheel particles, etc., in each molecule of the medium as real things in physical world[4], we can realize how many points B-S has missed in accounting for scientific practice and how limited the usefulness of B-S is for the problem of unobservables. Now let me show these problems in detail.

On the one hand, B-S will not allow for some real and reasonable confirmations. Let us take a piece of evidence similar to the result of Faraday's experiments in 1831,

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[3] (11) and (13) correspond to Maxwell's (113); (12) and (14) to his (115). See, ibid., pp. 496-7.

[4] See, e.g., Maxwell, (1861-2), p. 486; and (1864-5), pp. 563-4.

which led Faraday to a generalized conclusion about the induction between current and magnetic field, namely, (3). However, the confirmation of (3) by this evidence could be formalized as one which is not allowed by Glymour's revised B-S. Let us have two hypotheses: h_1: a varying magnetic field $\boldsymbol{H}$ will induce an electric field $\boldsymbol{E}$; and h_2: the change in the magnetic field will induce a current $\boldsymbol{J}$ just in case an induced electromotive force $\boldsymbol{E}$ in the conductor exists:

h_1: $(x)(\boldsymbol{H}(x) \rightarrow \boldsymbol{E}(x))$ (from (3))[1]

h_2: $(x)(\boldsymbol{H}(x) \rightarrow (\boldsymbol{J}(x) = \boldsymbol{E}(x))$ (from (3) and (16) which is known as Ohm's Law)

Suppose we have a cylindrical condenser, consisting of an inner conductor and an outer conductor. Suppose we move a coil with a current *I*1, or move a magnet, near to the condenser. Now we can expect there is a resulting electric current *I*2 in the wire connected with the condenser and measure the values of its density as J2. Since we can also measure the magnetic field intensity around the moving coil or magnet, H1, we get an evidence

e: H1 & J2 (the possible counterevidence is H1 & $\neg$J2)

It seems to us that *e* would confirm h_1 with the help of h_2, because by Ohm's law which shows the relation between $\boldsymbol{E}$ and $\boldsymbol{J}$, and by the evidence showing varying $\boldsymbol{H}$ and resulting $\boldsymbol{J}$, the value of $\boldsymbol{E}(x)$ can be computed. Indeed, scientists would justifiably take the evidence as a relevant one for h_1, as Faraday did. But confirmation of this form is

[1] Hereafter, $\boldsymbol{H}(x)$, $\boldsymbol{E}(x)$, $\boldsymbol{J}(x)$, $\boldsymbol{D}(x)$, etc. stand for functions of $\boldsymbol{H}$, $\boldsymbol{E}$, $\boldsymbol{J}$, $\boldsymbol{D}$, etc., not necessarily identical in form with any items of them in Maxwell's equations. This is to simplify the presentations of mathematical inferences among them. So in h_1: $\boldsymbol{H}(x) \rightarrow \boldsymbol{E}(x)$ just means a function of $\boldsymbol{H}$ can mathematically imply a function of $\boldsymbol{E}$ and a value of $\boldsymbol{E}$. If, for example, we take $\boldsymbol{H}(x)$ as $-\mu\partial\boldsymbol{H}/\partial t$, but $\boldsymbol{E}(x)$ simply as $\boldsymbol{E}$ (so that $\boldsymbol{J}(x) = \boldsymbol{E}(x)$ in h_2 can conveniently be taken as $(1/\sigma)\boldsymbol{J} = \boldsymbol{E}$ or (16)), then h_1 says in effect that $\boldsymbol{H}(x)$ implies $\boldsymbol{E}$, i.e., based on (3), through $\nabla x\boldsymbol{E}$ we can get a value of $\boldsymbol{E}$ from $-\mu\partial\boldsymbol{H}/\partial t$. No doubt, this can be done mathematically.

not permitted by Glymour's revised B-S, because it violates the new condition *R*. For *Ti* here shall be:

$$Ti\text{:}\quad (x)(H(x) \rightarrow E(x)) \equiv (x)(H(x) \rightarrow (J(x) \equiv E(x))$$

h_1 entails that *Ti* is equivalent to h_2. So Faraday's finding that a varying magnetic field produces a current in a conductor is irrelevant to his conclusion, the electromagnetic induction law! Of course it is possible to find other computations of h_1 which do not violate *R*, but the point is that this "computation" was what actually happened, or this formalization is "natural" one; and thus B-S itself could or should give no reason syntactically for disallowing this one.

On the other hand, B-S will permit evidence to confirm what it should not be able to confirm. In the present case, the evidence, H1&J2, which should be a manifestation of the causal relation between magnetic field and current in the conductors, would be allowed by B-S to "confirm" the existence of an induced electric field or even "displacement current", in the dielectric between the two conductors of the condenser. As we know, $\partial D/\partial t$ (or J^*) is regarded as the effect of *H*1 and *J*2 in a dielectric (and in turn as the cause of a new magnetic field, *H*2). In other words, $\partial D/\partial t$ is the result of an extension of the concept of electromotive force in conductor circles to dielectrics. We understand that the existence of $\partial D/\partial t$ could not be proved by simply presenting its supposed cause H1&J2 without actually discovering it (it was not confirmed until Hertz's experiment in 1887). Yet by B-S, the unreasonable confirmation is allowed. Suppose, as part of hypothesis (10), that a current *J* in the conductors produces a varying electric field $\partial D/\partial t$ or displacement current J^*, ($J^* = \partial D/\partial t$), in the dielectric (which will contribute to inducing a new magnetic field nearby):

$$h_3\text{: } (x)[J(x) \rightarrow D(x)]$$

and suppose another three hypotheses:

h_4: $(x)[H(x) \rightarrow J(x)]$ (3)

h_5: $(x)[\rho(x) \rightarrow J(x)]$ (from (12) and (14))

h_6: $(x)(D(x) \equiv J^*(x))]$ (7)

ρ is the density of charge in the conductor which can be calculated from the value of current in the conductor. So we can have an experimental report in this case

e: H1&J2&ρ2

and we can surely prove h_3 by calculating the value of D(x) and so J^*(x). The structure of the confirmation is:

$$
\begin{array}{llccl}
 & J(x) & & J^*(x) & \\
 & & & \uparrow & \longleftrightarrow h_6\text{: } (x)[D(x) \equiv J^*(x)] \\
h_5 \longleftrightarrow & \uparrow & & D(x) & \\
 & & \nearrow & \uparrow \quad \nwarrow & \longleftrightarrow Ti\text{: } (x)[(H(x) \rightarrow J(x)) \equiv (\rho(x) \rightarrow D(x))] \\
 & \rho(x) & H1 & J2 \quad \rho2 &
\end{array}
$$

The computation above has avoided any violation of B-S conditions, including R. (h_3 does not entail the equavilence of Ti with h_4 or h_5.) But we are unlikely to call such a computation of $\boldsymbol{D}$ just by Faraday's work a confirmation of the existence of $\partial \boldsymbol{D}/\partial$t, predicted by Maxwell. Moreover, this is not a confirmation of J^*, which is a really "fictitious" concept, even though it is not redundant in the sense of being a useful tool. We can accept $\boldsymbol{D}$, denoting the change of electric field strength in dielectric, as a real thing but not J^*. Yet if we only take into account the form of the sentence $J^*(x) \equiv D(x)$, any evidence which is relevant to the confirmation of the real existence of $\boldsymbol{D}$ shall be relevant in the same way to that of J^*. J^* in h_3 is calculable and h_3 gets an instance. Hence, according to Glymour, h_3 has been confirmed by e, and J^* shall ontologically obtain the same status as that $\boldsymbol{D}$ does. It seems to me, therefore, that B-S can not tell us what is "observable" or not and its connection between being computable and being observable is unsuitable. A formal calculation can hardly be equal to a confirmation when there is an ontological decision involved in it.

This leads us to a new problem with B-S in the case of Maxwell's theory: it could not distinguish instrumental but non-referring concepts like those in *Mm* and referring terms since both could be calculated in a B-S way. According to B-S, evidence for items in *Me* can confirm their counterparts in *Mm* as well. Assume, for example, we have the following hypotheses:

h_7 $\quad -\frac{\partial \boldsymbol{B}}{\partial t} = \nabla \mathrm{x} \boldsymbol{E}$ $\qquad$ (from (3) and (15))

h_8 $\quad (\boldsymbol{J} = \sigma \boldsymbol{E}) \rightarrow \nabla \mathrm{x} \boldsymbol{E}$ $\qquad$ (from (16))

h_9 $\quad \mu \frac{\partial \boldsymbol{H}}{\partial t} = \frac{\partial \boldsymbol{B}}{\partial t}$ $\qquad$ (from (16))

No doubt by h_8 and h_9, h_7 can be confirmed when *e* is H1&J2. If, however, we replace $\boldsymbol{E}$ by $\boldsymbol{L}$ according to $\boldsymbol{E} \equiv \boldsymbol{L}$, which was, like all other such hypotheses, extremely useful for Maxwell, h_7 will be:

h_7* $\quad -\frac{\partial \boldsymbol{B}}{\partial t} = \nabla \mathrm{x} \boldsymbol{L}$

So the same *e* will confirm the hypothesis about the existence of a mechanical force of molecular vortices on idle-wheel particles. In fact, almost every hypothesis about the vortices and particles can be confirmed this way. The problem comes not only from the syntactic form of theoretical sentences, but also from the syntactic form of observational sentences. The form of the evidence, *e*: H1&J2, shows nothing about what kind of physical or causal relation obtains between H1 and J2. Neither does it tell us that in *Me*, there is a realistically proposed causal process from H1 to J2, rather than *vice versa,* so that it confirms only this process; nor does it show that in *Mm* the relation between $\boldsymbol{S}$ (= $\boldsymbol{H}$) and $\boldsymbol{W}$ (= $\boldsymbol{J}$) is not realistically proposed and taken so that any computation of the value of $\boldsymbol{S}$ (from H1) means nothing ontologically. If Maxwell were a Glymourian methodologist, due to the known fact obtained by Faraday, he would not have declared that the hypotheses about the vortices and particles were "artificial", "temporary", and "unwarrantable"; and he would not have been so eager to get rid of them[1]. His success in avoiding them just two years later by a "dynamical theory" of electromagnetism[2] invites us to think about evidential relevance in a different way.

4.4 Can Relevance Logic Work?

It turns out that H-D and B-S share the same weaknesses. They both are unable to pick out "natural axiomatizations" or, in other words, appropriate auxiliary hypotheses, and thus fail to account for the practices of evidential relevance. As we have seen, the so called "natural" axioms mean that they are actually those postulates put forward by scientists about certain regularities in nature; they are those with credibility, in Edidin's terms (Edidin, 1988), in the sense that they have already got, maybe for a long time, reliable evidential supports which, in most cases, are independent of the evidence involved in the confirmation in which they are used as auxiliaries. In particular, in the case of theoretical entities, as shown by the example in the last section, these postulates are typically *causal* ones. In order to find out which hypothesis is corroborated, scientists must work with these natural or causal postulates, not with whatever they can find in any deductively closed set of sentences of their theories. In short, the syntax of a theory gives no clue which sub-set of the sentences is a natural axiomatization of the theory that can give us a handle on the evidential relevance relations within the theory.

Some people, however, still tend to think that the failures of both H-D and B-S may just come from the special logical system, i.e., classical logic, used in the two models, not from the fundamental character of the formal approach. Indeed, many defects of

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[1] See, Maxwell, 1861-2, in Niven 1890 and 1965, p. 486; Maxwell to Hockin, 7 September 1864, in Campbell and Garnett 1884, p. 255; Maxwell to Stokes, October 1864, in Larmor (ed) 1907, P. 26; and Maxwell, 1864-5, in Niven, 1890 and 1965, pp. 563-4.

[2] Maxwell, 1864-5.

classical logic have long been recognized. As far as the problems of H-D or B-S are concerned, the most serious one is that classical logic is an "irrelevant logic"; the relation between antecedent P and consequent Q of the conditional P $\rightarrow$ Q is just truth-functional. This defect is typically shown by the "paradox of material implication": a falsehood materially implies any proposition whatsoever; and a truth is materially implied by any proposition whatsoever. As a result, classical logic obviously allows for some absurd inferences as valid; and it certainly can not, one thinks, serve to distinguish natural axiomatizations from unnatural ones. According to classical logic, for instance, $h \rightarrow e$ is true once e is true, no matter whether h is true and whether there is any factual relation between h and e; so there is no way in it to show if e is relevant or not to h. That is where the problems with H-D and B-S come from.

A possible way to solve this problem within the syntactic approach might be, therefore, to find a suitable logical system which can rule out these irrelevant inferences and unnatural axiomatizations. Many people, including Glymour (1983a) and Christensen (1990), tend to accept that relevance logic, mainly developed from Anderson and Belnap's work (1975), may be the right candidate for the task. Waters, in particular, has seriously argued for this choice (1987). Mainly based on Dunn's description of relevance logic, Waters claims that by relevance logic (mainly Anderson and Belnap's system **R**) the three problems for H-D (so also for B-S) are resolved. As to problem (1), **R** can help to choose an appropriate subtheory, because in **R**, $h \rightarrow T$ is not an appropriate subtheory, for $(h \rightarrow T)$ & h is not logically equivalent to T (even though h is a consequence of T), since in **R**, $h \rightarrow T$ does not follow from T even though T is true. As to problem (2), $h \rightarrow e$ is not necessarily a true theory because, again, in **R**, $h \rightarrow e$ does not follow from e. As to problem (3), the point is how to decide which part of a theory is necessary to deduce e. Glymour uses the Copernicus-Kepler example to show that there are logically equivalent axiomatizations of the same theory which, contrary to our intuition, can make K3 necessary to deduce O'. But in relevance logic, T and T' in the example are not the same theory. T' is not logically equivalent to T. For (K3 $\rightarrow$ C) can not be derived from T. So K3 was not necessary to deduce O', if using relevance logic to choose axiom sets.

Hence Waters asserts: "In effect, relevance logic serves to select out the problematical alternative axiomatizations"(Waters, 1987, p. 62).

If this is so, at last we obtain a logical method to pick out natural axiomatizations (causal laws) from all possible axiomatizations and decide evidential relevance without considering the epistemic content or status of the hypotheses and evidences involved. But can relevance logic really find out all and only relevant confirmations by a given piece of evidence? Especially, in the case of micro entities, as I have shown in the last chapter and by the example in the last section, confirmation of the existence of an entity is a matter of finding its effect and figuring out that a given evidence is really causally related to the entity. To confirm $\partial \boldsymbol{D}/\partial t$ in a dielectric is to find its effect, like Hertz's finding that an electric spark in one place transmits through space only with a dielectric and induces another spark in another place, a finite time later. Can relevance logic, as it stands, serve to account for this kind of evidential relevance, i.e., causal relevance?

In order to compare some characteristics of relevance logic and those of causal relations, let me first talk very briefly about relevance logic. What is of relevance to us here, first of all, is how relevance logic explains and defines the very concept of relevance. To my surprise, relevance logicians give no separate account of relevance. That means, unfortunately, I think, that neither is the logical system a formalization based on a fundamental aspect of practical relation that has been well revealed, nor does it have any independent criterion of relevance for choosing the axioms, constraints and inference rules for the logical system in a non-arbitrary way. This makes relevance logic, notably Anderson and Belnap's system **R**, vulnerable to the accusation that such a system is a false formal analogy to some original intuition and some of those axioms or constraints should be rejected. Indeed, relevance logicians have tried to copy some intuitive aspects of relevance by making different formal constraints. One main constraint for deciding relevant inference is from this intuition: there is a meaning-connection between A and B if A is relevant to B in the entailment A $\vdash$ B. So a necessary but not in itself sufficient condition for a relevant entailment to hold between A and B is that there be a common variable p (sentential in sentential relevant logic, predicate in relevant first order predicate logic) that occurs in both A and B to show the meaning-connection:

variable-sharing.

This position is problematic. First, it can be doubted if there is any precisely formalizable sense of "meaning" in the phrase "meaning-connection"[1]. Second, variable-sharing even as a necessary constraint is problematic: a causal interaction may result in something that is not like anything involved in the interaction; a formulation of such a causal process in sentential relevant logic could be "irrelevant" in the sense there is no sentential variable-sharing in it.

Constraint on the relevance of the antecedent A of a conditional to its consequent B is called by some people (S. Read, 1988) derivational utility, namely, that there is a derivation of B from A in which A is actually used. This constraint is taken to be both necessary and sufficient for entailment; and it is given a formal analogue in the natural deduction formulation of **R**.

Several problems occur. For example, "used" is very vague here. Sometimes it is hard to decide if an assumption has been used or not. A well known example is the "Lewis Argument," which shows that in the inference from P & ¬P to any consequence Q, both P and ¬P seems to have been used for the derivation of Q. All different solutions for this have been tried, like rejecting this or that inference rule in it[2]. Yet the question is on what ground these principles of inference are denied. Unfortunately, as Diaz declares, because of the lack of an independent and satisfactory theory of relevance, none of these rejections has been convincing and so they are all subject to being accused of being arbitrary and *ad hoc* (M. R. Diaz, 1981). Thus **R** as a whole is simply an attempt to choose suitable axioms and principles of inference so as to exclude as provable those obvious counter-intuitive formulae; and no procedure independent of the method

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[1] See, for example, G. I. Iseminger, 1980.

[2] Two solutions are possible: either (1) we could take entailment as not transitive: P entails "P v Q"; "P v Q" entails "¬P → Q"; but P does not entail "¬P → Q"; or (2) one of the steps of the inference is ambiguous or unacceptable. Here we may say "P v Q" is ambiguous, the location of the ambiguity is in the word "v", rejecting the step from P to P v Q; also we can say that to "&", rejecting the step from P & ¬P to P (or ¬P).

of proof is given for determining which proofs from hypotheses satisfy conditions of relevance. This eventually makes any decision concerning "natural" axiom sets by **R** exposed to the accusation of being arbitrary and *ad hoc*.

Even if we ignore this problem of foundation, is **R**, with its chosen axioms and principles of inference, good enough to exclude all non-relevant implications and include all relevant ones? That is, can it show that all provable implications are relevant and all unprovable ones are non-relevant?[1]

Let us just consider the case of entities. For this, if relevance logic is useful, it must be able to express causal relevance. That is, all axioms and principles of inference in **R** must be compatible, at least, with basic characteristics of causal relevance. In Chapter 2, I listed some characteristics of causality. Keeping the list in mind, let me have a look at ***R***, a well-developed system of relevance logic. The implicational fragment of it, $R_{\rightarrow}$, consists of the rule *modus ponens* (A, A → B ⊢ B) and the following axioms:

(1)	A → A	Self-Implication
(2)	(A → B) → [(C → A) → (C → B)]	Prefixing
(3)	[A → (A → B)] → (A → B)	Contraction
(4)	[A → (B → C)] → [B → (A → C)]	Permutation

If we interpret the relevance, " → ," in the axioms as causal relevance, ►→, at once we find these axioms do not hold. As to (1), if it comes to A ►→ A, it violates F3 - it does not make sense to say A causes A. As we can see from F7 and F9, (2) fails, when it becomes this one: (A ►→ B) → [(C ►→ A) → (C ►→ B)], because C may be a cause of A, and A may be a cause of B, but C might not be a cause of B (something else in A is causally relevant to B, since an effect may involve more components than its cause). (3) has its own problem: when B is substituted by A, (3) becomes

[1] Some have argued that there are certain provable formulae in ***R*** which are not relevant implications on the intuitive level. The antecedent of such formulae as (A → B) → ((B → A) → (B → A)) and (B → A) → ((A → B) → (B → A)) is intuitively irrelevant to the consequent, but is "used" in the subscripting sense to prove the consequent in ***R***. See, e.g., T.J. Smiley, pp. 239-240.

$$[A \rightarrow (A \rightarrow A)] \rightarrow (A \rightarrow A)$$

and it faces the same problem as (1) does. And (4), if explained as a causal account, is a violation of F6. Maybe if A occurs first, and B second, an effect C will follow, but it does not imply that if B occurs first and A second, C will also follow. Sometimes the occurrence of a result strictly depends on the order of the occurrence of its components.

When we extend our check on ***R***→ to ***R***, a system containing conjunction or disjunction, we are involved in further difficulties in giving ***R*** causal interpretation. The following formulae, for instance, can by no means be interpreted as causal accounts:

(5). $A \rightarrow (A \vee B)$, or $B \rightarrow (A \vee B)$

because they violate F5; and it does not make sense at all to say that something causes itself or anything else. Nor can

(6). $(A \,\&\, B) \rightarrow A$, or $(A \,\&\, B) \rightarrow B$

be interpreted causally because they violate F2. For one thing, if B is not causally relevant to A so that there is no interaction between A and B, the first one will be equivalent to saying that A causes A. And for another thing, if the interaction between A and B occurs, so that the effect of the interaction could differ from any of them, it again is not right. "&" can not be unambiguously interpreted to cover both cases[1]. For similar reasons these theorems do not hold as causal laws either:

(7). $(A \rightarrow B) \rightarrow [(A \,\&\, C) \rightarrow B]$

(8). $(A \rightarrow B) \rightarrow [A \rightarrow (B \vee C)]$

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[1] In ***R***, a new operator $\circ$, called the intensional conjunction operator to denote "relevant conjunction," is introduced. ***R*** defines $A \circ B = \neg(A \rightarrow \neg B)$; and $(A \circ B) \circ C \equiv A \circ (B \circ C)$; $A \circ B \equiv B \circ A$; $(A \rightarrow B) \rightarrow ((A \circ C) \rightarrow (B \circ C))$; $A \rightarrow (B \rightarrow A \circ B)$; and if $\vdash A$ and $\vdash B$, then $\vdash A \circ B$. But it fails to have the property $A \circ B \rightarrow A$. Yet it is easy to see that none of these formulae using $\circ$ has escaped the problems mentioned above.

(9). $(A \rightarrow B) \rightarrow [\,(A \,\&\, C) \rightarrow (B \,\&\, C)]$

A good example is given by Cartwright to show the failure of such an inference involving & : $(A \rightarrow C)$, $(B \rightarrow C)$, $\vdash (A \,\&\, B) \rightarrow C$. The example is: ingesting an acid poison may cause death $(A \rightarrow C)$; so too may the ingestion of an alkali poison $(B \rightarrow C)$; but ingesting both may have no effect at all on survival $\neg[(A \,\&\, B) \rightarrow C]$ (Cartwright, 1983, p. 31)[1].

More problems for ***R*** to be compatible with causal relevance come from the distinctive features of this sort of relevance as given by, say, F1, F3, F4, F6, F7, F8 And F10. They give rise to serious doubts whether it is possible to construct a logic of cause within the framework of ***R***. Such an attempt has been made by Sylvan (Sylvan, 1989). Trying to construct a basic framework for "logics of cause", Sylvan has considered many problems arising from the distinctive features of causal relevance, the problems mainly from F3, F5, F6, and in some ways, F2 and F10. His attempt, however, indicates he does not realize some other problems and does not fully recognize the significance of the problems he has tried to avoid, like those from F6 and F11. He acknowledges that since logics of cause must involve such theses of the form $C \rightarrow D$ where C and D share no variables, the logics are irrelevant. But he declares: "Amusingly we could have avoided this problem, and other hassles by basing the theory of causal implication on an irrelevant strict implication or associated irrelevant conditional". In the note for this, he simply says that "Such an irrelevant approach to deeper relevant logics themselves is not entirely excluded". This just leaves too much for us to guess in the dark what such an approach is like. And this also brings the problem of foundation back for the people who are willing to try whatever they can for their logical constructions: why use this irrelevant approach for a relevance system of cause? Is that not arbitrary and *ad hoc*?

Actually since so many axioms, theses and rules used by ***R*** are incompatible with

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[1] In chapter 6, we can find another example which led to a critical step toward the discovery of the causal role of the DNA molecule.

causal relevance, it is also hard to imagine what a logic of cause is like as a purely formal system of axioms. From F11 we know, for instance, that the rule of substitution fails in it. But without this rule what kind of "logic" is it and what use does it have after all?

Sylvan claims that causal conditions satisfy some rules including Transitivity (p. 32)[1]. By F9 we know that at least in some sense Transitivity fails for causal relevance.

Now considering Contraposition in the context of causal relevance ($A \blacktriangleright\!\!\rightarrow B \vdash \neg B \blacktriangleright\!\!\rightarrow \neg A$), Sylvan accepts that this seems wrong for no lung cancer doesn't cause no heavy smoking, and direction of cause also appears wrong for such Contraposition. But he insists that at least no lung cancer implies no heavy smoking. So "Cause induces an implication connection that <u>extends</u> it, $\vdash\!\ni$ say. Then in place of Contraposition the following appears to hold:

$$A \blacktriangleright\!\!\rightarrow B \vdash A \vdash\!\ni B \qquad\qquad A \blacktriangleright\!\!\rightarrow B \vdash \neg B \vdash\!\ni \neg A\text{"}$$

(p. 33). Nothing has been said about this "implicational extension" - whether it is more like $\blacktriangleright\!\!\rightarrow$ or more like classical $\rightarrow$. Since he thinks the following hold as well in logics of cause:

$$A \vdash\!\ni B \vdash A \,\&\, C \vdash\!\ni B \qquad\qquad A \vdash\!\ni B \vdash A \vdash\!\ni B \vee C$$

(pp. 33-34), it appears that this implication extension is nothing more than $\rightarrow$ in ***R*** which takes into account no interaction at all. So still none of these theses is allowed in any formalization of causal relevance.

Sylvan does give a definition of causal implication as an "organising process account":

$$A \blacktriangleright\!\!\rightarrow B \quad \text{iff} \quad (A \rightarrow B) \quad \& \quad (A < B)$$

Here $(A \rightarrow B)$ represents sufficiency conditional, and $(A < B)$ stands for direction,

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[1] My $\blacktriangleright\!\!\rightarrow$ is his $\ni$. See Sylvan, 1989, p. 32.

time order or "initiation," which just denotes that the time of A precedes that of B (pp. 36-7). That is, A causes B iff B is constantly conjoined with A which is prior (p. 37).

Clearly it would hardly take much power to find a great number of counter-examples for this account. This is a definition taking into account no role of causal interaction at all. This weakness is fatal. Although Sylvan says, "We may want to restrict situations also to those that conform to various constraints, for instance (at some risk of circularity) to natural laws" (p. 37), I don't see how it can be done within a formal framework; if any constraint is introduced to restrict the rules in logics of cause to natural laws, then such a constraint will make the logics more than purely formal systems.

Hence it seems to me that for a "logic" of cause, if it is made to be like a purely formal system with any interesting axioms, principles of inference and generality or usefulness, it would likely be both too wide and too narrow in accounting for causal relevance. And if such a system can just cover causal relevance, it would hardly be a purely formal system. So relevance logic will not bring hope to the resolution of evidential relevance at least in the case of theoretical entities and causal hypotheses.

Even worse, such a relevance logic might distort or conceal real relationships between data as effects and theoretical entities as causes, since it accounts for neither all nor only features of causal relevance. This can be shown in terms of "Simpson's paradox". Put in Cartwright's way, the paradox says that any association - $Prob(A/B)=Prob(A)$; $Prob(A/B) > Prob(A)$; $Prob(A/B) < Prob(A)$ - between variables which holds in a given population can be reversed in the sub-populations by finding a third variable which is correlated with both (Cartwright, 1983, p. 24). If relevance logic or a logic of cause comes to deal with this kind of causal relevance in any way, a parallel situation will happen in them, because the third variable which is correlated with both would almost for sure be allowed as relevant by them. More likely, relevance logic may encourage us to seek a causal relationship between A and B by using a "relevant" third factor which may just lead us to an opposite conclusion. If B really increases the probability of A so that we might say that B causes A (it does not matter which association we choose as representing causal relevance), we might end up concluding, in light of a certain third variable, that there is no causal relation between them (the variable may make B have no effect at all on A's probability). Indeed relevance logic as it stands would guide us to find causal relevance between A and B in terms of what it allows as relevant; so this may lead to a result worse than if we don't use it. It is clear now that relevance logic could not save H-D, B-S or even perhaps the whole syntactic approach in the case of evidential relevance. Why and how something is a cause or what causal role it has is usually hidden in both syntactical and semantic analyses of theories. There seems nothing in those analyses that could distinguish generalizations in general from causal hypotheses. For this reason, these analyses are of little use for the issue of entity realism. That is why the "experimental approach" is preferred.

Chapter 5 Model, Relevance and Directness

Now we have good reasons to come back to the "experimental approach" to the issue of realism about entities. In this chapter, I will illuminate by examples, especially that of the discovery of Ω^-, the importance of a "model constituent"(MC) for acceptance of an entity as real. Meanwhile, some key concepts, like "low-level" and "direct", will get further clarification.

5.1 Causal Model: Structure and Process

Usually, when a scientist causally explains some phenomena by postulating a hypothetical entity, the postulation is accompanied by postulation of a model describing some of the entity's properties, composites, or causal functions responsible for the phenomena. Obviously this model becomes a basis for asserting the reality of the cause. I will call it an "existential model", indicating its specific and limited composition and usefulness. Sometimes, postulation of an entity is preceded by postulation of a model. A model, called "the eightfold way" model proposed in 1961 by M. Gell-Mann and Y. Ne'eman, was aimed at classifying the known hadrons into families and at finding connections between the various members of each family by using the mathematical notion of groups. Later it was realized that the model implied a new particle, called by Gell-Mann Ω^-, omega minus, to fit the stipulation that a family has 27 member baryons. (In the model each particle in the semi-stable baryons octet was made up of three fundamental components, each possessing a baryon number of 1/3 (Ne'eman, p. 203).). The particle Ω^- was identified in 1964, and the eightfold way model was later regarded as part of a comprehensive theory or model of elementary particles, quark theory. But at the beginning the model did not attract much attention. According to Ne'eman's recollection, one reason was that the concept "eightfold way" was not widely recognized then. Another reason was that the model "did not go far enough. The authors had not yet decided whether to regard the fundamental components as proper particles or as abstract fields that did not materialize as particles" (Ne'eman, p. 203).

This indicates the importance of maturity of a model as describing a real entity: its degree of getting into the body of existing empirical knowledge and facts. The basic idea in the eightfold way model, that the hadrons were composed of a small number of fundamental building blocks, was considered a discarded notion. What was popular then was Shaw's bootstrap theory that some of the hadrons were the building blocks of all the rest - a notion contrary to that of the eightfold way.

Also, a model should be clearly realistically intended in order for it to be accepted as having plausibly described the physical components of an entity. In other words the model should not involve as a physical cause of a given physical result something which should be treated non-realistically. Hence, the underlying ground of an existential model should be physically causal laws which are treated realistically.

As an indicator of a model's maturity, physical laws in a model should be synthesized in such a way that a description of a causal interaction between the entity and some other objects is specifically and plausibly given. As a result, a particular set of properties of the entity could be given. This is the time a model would be focused on. In the example of the omega minus, before the test, the possible place where the particle would occur was inferred: the place was actually a certain energy interval - at this energy some other particles could interact with it and result in some decay products. Also, very importantly, some unique properties of the entity were specified for scientists to single out and identify it: it would be a simple particle; located in the S-I_3 plane in connection with the property of a strangeness of -3; and alone at the edge of the triangle in connection with zero isospin. Its mass would increase in roughly equal intervals of about 150 MeV with each unit of strangeness. It is in a group of "resonances" and has the largest mass in this group. Yet unlike other members of the group, it would be a semi-stable particle (longer lifetime) because its high strangeness would prevent it from decaying by the strong interaction. All this gives a clear indication of the particle's character: there are no particles whose sum of strangeness is -3 and sum of mass less than the expected mass of the omega minus[1]. In practice, therefore, the demonstration of a particular set of quantum numbers of an entity will lead to both identifying it and accepting the reality of it.

Hence often a causal model of a particular entity is intended to imply the reality of it and help to identify it.

An observation is an information transfer process, as Shapere says. The process can be regarded as consisting of three parts: source of information - cause; path of information transferring - chain of causal interaction; and reception of information - effect of the interaction chain on receptor. So, correspondingly, there could be, in principle, three kinds of causal models about an entity: (1) models about the cause; (2) models about the process; and (3) models about the reception. The first kind of model could be either about internal structure of an entity or about the structural relation between the entity and other objects. The second kind could be about a process or a chain of interactions between the entity and other objects. The third kind would describe the process of interaction happening on receptors or the output-end of the experimental system. It is worth mentioning that the classification is relative. Usually a model would mainly focus on one part, but it could also describe the other two parts. A structure model about a spatial relation between the entity and other objects could be taken as describing a time process between them as well. Here I shall discuss the first two kinds of models to show how they fulfil their purposes.

5.2 Model(1): Initiatives and Rationales

The purpose of the first kind of model is mainly to illuminate why an entity could be a cause of some other objects. In other words, the model describes the entity's

[1] To sum up: mass, 1680 MeV; strangeness, -3; charge, -1; spin, 3/2; isospin, zero; parity, positive; lifetime, about 10^{-10} sec. etc.

internal or external structure to show that it contains a particular kind of causal initiation mechanism, a mechanism which makes the entity able to exert a certain kind of action on some other entities. In this way the model has provided a "causal initiative" for the entity to start a causal process, or stipulated a rationale by which a unique causal process is produced. Let me present two examples here.

One is the Watson-Crick model of DNA.

Studying the structure of DNA started in the last century. Over the years, experimental evidence about its components gradually accumulated. In the 1930s, when P.A.T. Levene proposed the first notable comprehensive model of the structure of DNA, it had already been known that DNA was a "macromolecule" composed of sugar, phosphorus and base components with the inter-nucleotidic linkage (chemical bonds), and so on. The structure of the sugar and phosphorus were studied by Levene and others. The phosphate group was found to have two ends: one joins the third carbon atom of one ribose ring to the fifth carbon of the next (later the two ends were labeled 5' end and 3' end respectively). Also, four bases were identified: guanine (G), cytosine (C), thymine (T) and adenine (A) (C and T called pyrimidines, G and A purines). In 1935, Levene and Tipson proposed the model of "Tetranucleotide structure" for DNA. The model was based on Levene's tetranucleotide hypothesis from his studies of yeast nucleic acid that four bases were present in equal proportions in the nucleic acids. This model was proven inadequate not only because it was inaccurate, but also because, more importantly, it suggested little relationship between DNA and the rest of the cellular substance[1]. Of course many other reasons, including a lack of suitable techniques to study carefully DNA rather than its degradation products, accounted for the failure of the model.

The idea that DNA might be the genetic substance started receiving remarkable attention only in the late 1940s, after O. T. Avery's work following the discovery of genetic transformation by F. Griffith. Nevertheless, even around 1950, many scientists

[1] We would not expect it at that time to account for hereditary phenomena, because most biologists believed that the genetic substance must be protein. The point is that the model also showed little about DNA's relationship with other components in cells.

were still sceptical of the idea. Yet the birth of Watson-Crick's model of B-form DNA became a crucial turning point in the recognition of DNA's hereditary significance. For this model convincingly showed that DNA has just such a causal mechanism. This is a good case showing that the discovery and acceptance of an entity result from a combination of understanding it and manipulating it. For now I will describe the model; the whole story will be finished in the next chapter. As we know, the model is called the "model of the double-stranded helix structure". See Figure 5.1 (G. Felsenfeld, "DNA", p. 60).

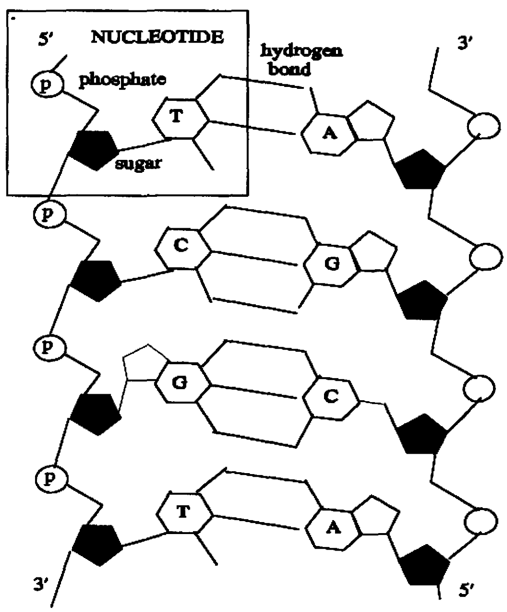

Figure 5.1 The structure of DNA

According to the model, a DNA molecule consists of two strands; each has a backbone made up of a bonded sugar and a phosphate group; and each sugar is attached to one of four bases G, C, T, or A. One phosphate group connects the 5' carbon atom of one sugar to the 3' carbon atom of the next. One sugar, one phosphate group and one base constitute a nucleotide. One strand thus consists of a huge number of nucleotides. The two strands are connected by the hydrogen bond between the bases along the two strands. The nature of the hydrogen bonding of the bases is such that T aways pairs with A and

C always pairs with G. So the two DNA strands twine about one another, and run in opposite directions, linked by hydrogen bonds between complementary base pairs. Like a spiral staircase, the sugar-phosphate backbone of each strand represents a bannister and the joined bases are the steps themselves..

The most important step to the invention of the model was the occurrence of the ideas of double strands and base-pairing. This was the key difference between this model and any others. From the late 1930s to the early 1950s, some other biologists had also developed DNA models, some of which were very close to the Waston-Crick one in many details[1]. These models, however, differed from Watson-Crick's either in the number of strands - some have one strand and some (e.g.,Pauling's) three; or in the way of fitting bases (Franklin). And most of all they all could not show the genetic significance of DNA.

Watson and Crick also had many failures before their success. They once made a model with three chains, with the phosphates in the center, and used a wrong way of fitting the bases of two strands - conceiving a "like to like" hydrogen bond.

But Watson and Crick had a strong desire to find the structure of DNA because, unlike many other biologists, they clearly realized its significance. Waston said that he felt that "Avery's experiments strongly suggested that future experiments would show that all genes would be composed of DNA". He agreed with Luria that "the real answer would only come after the chemical structure of a virus (gene) had been cracked open" (p. 249). Keeping this in mind, on Feburary 21, 1953, Watson came across the critical idea of their model: after trying other pairing possibilities among A, T, C, and G, the

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[1] For example, in 1938, Astbury and Bell gave a diagrammatic representation for the DNA structure they proposed, based on their X-ray diffraction photograph of a stretched dried film of DNA. In 1949, Furberg, following the approach of Bernal and Pauling, tried to elucidate the structure of DNA by doing detailed work on the crystalline components, and proposed two models of DNA (see Portugal and Cohen, p. 235). And most remarkably, through their own X-ray diffraction photographs which clearly showed the helix pattern of DNA's structure, Pauling, Wilkins and Franklin had actually developed some models which are very similar in many ways to the Watson-Crick's one. Wilkins and Franklin had found that the bases were in the center and the backbone outside. Franklin had even considered the presence of two chains, namely a double helix.

idea suddenly occurred to him that the A-T pair were like the C-G pair in that the hydrogen bonds between them were the same and thus all the bonds seemed to form naturally. Watson at once realized the implication of this idea. If this was right, Watson recalled, two irregular sequences of bases could be regularly packed in the center of a helix if a purine always hydrogen-bonded to a pyrimidine; and furthermore, this kind of bonding meant that A would always pair with T and C with G. If so, not only many known pieces of evidence then suddenly stood out as consequences of this double-stranded helix structure; but also, "Even more exciting, this type of double helix suggested a replication scheme much more satisfactory than my briefly considered like-with-like pairing. Always pairing adenine with thymine and guanine with cytosine meant that the base sequences of the two interwined chains were complementary to each other. Given the base sequence of one chain, that of its partner was automatically determined. Conceptually, it was thus very easy to visualize how a single chain could be the template for the synthesis of a chain with the complementary sequence" (Watson, 1968, p. 186).

This was one of the most important advantages of the model over others. Watson and Crick knew this. In their article that appeared in the April 25 issue *Nature*, they noted, "It has not escaped our notice that the specific pairing we have postulated immediately suggests a possible copying mechanism for the genetic material" (1953a). Very soon, worried that the importance might be understated, they published a second note, "Genetical Implications of the Structure of Deoxyribonucleic Acid," in *Nature* especially aimed at emphasizing it. In it they clearly explicated how genetic continuity is assured by the chemical structure of DNA proposed by the model. Biologists soon recognized this. M. Delbruck wrote to Watson, "I have a feeling that if your structure is true, and if its suggestions concerning the nature of replication have any validity at all, then all hell will break loose, and theoretical biology will enter a most tumultuous phase"[1]. This has been proved true. The model provided a great guide for the development of genetic biology. Pauling asserted that the model and its results constitute the greatest advance in biological science and our understanding of life that has taken

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[1] Delbruck to Watson, April 14, 1953, quoted in Portugal and Cohen, p. 361.

place in the last hundred years (p. 271). Why does the model have so great influence? G. Felsenfeld says it is:

> ...because the structure contained within itself indications of how DNA might perform its function of storing and transmitting genetic information. Much of the explosive advance in molecular biology set in motion by that discovery has been aimed at understanding how DNA interacts with the other components of the living cell to express the information it encodes (Felsenfeld, p. 58).

My second example is also a well known case, Rutherford's model of the atom. What I want to show by it is that, by manifesting a special structure of an entity, a good model can entail a distinctively physical way in which the entity interacts with other objects, or a rationale to describe a new causal chain.

As we know, according to Thomson's model of the atom, an atom was a positively charged sphere about 1 Å in diameter in which electrons were embedded like plums in a pudding: namely the density of matter in the sphere shall be more or less uniform throughout its volume. If this model is true, then in Geiger's experiment using fast-moving alpha particles to strike a very thin gold foil, the angles of the scattering of the particles must be small; the probability of large angles of the scattering would be extremely low. In particular, the probability of a 90° angle of scattering would be, by calculation in terms of Thomson's model, about 10^{-3500} - for practical purposes impossible; but the experimental result was 1/8000!

Based on this result, Rutherford's model of the atom was proposed (Rutherford, 1911). According to it, the structure of an atom is like a small solar system. There is a minute nucleus (the sun) in the atom; the nucleus has a positive charge; although it has a diameter only 1/100,000 of that of the atom as a whole, it has most of the mass of the atom. The negative electrons, like planets, apparently circle around the nucleus. Based on this structure, the model entails a new causal story about the scattering result in a very simple and natural way: on the one hand, by known physical laws and facts, the positive, minute but heavy nucleus would exert a strong electrically repulsive force. Hence, when some alpha particles passed near enough to the minute nucleus, the strong repulsive force would deflect or even push them back. On the other hand, since the nucleus occupies only a very small space in the atom, most alpha particles would pass far enough from the nucleus, and their path would hardly be influenced by the light electrons. That accounted for the fact that some, but not many, alpha particles experience a large angle scattering. See Figure 2 (Yang Fujia, 1985, p. 16)

The core assumption of the model is that there is a new structure of the atom - a minute, heavy nucleus, a "tiny ball", of radius 10^{-12} to 10^{-13} centimetres surrounded by light electrons. This entails a new way of interaction between alpha particles and atoms, a way which Thomson's model could not offer. In this process, alpha particles interact with a new entity in their motions by electrically repulsive forces.

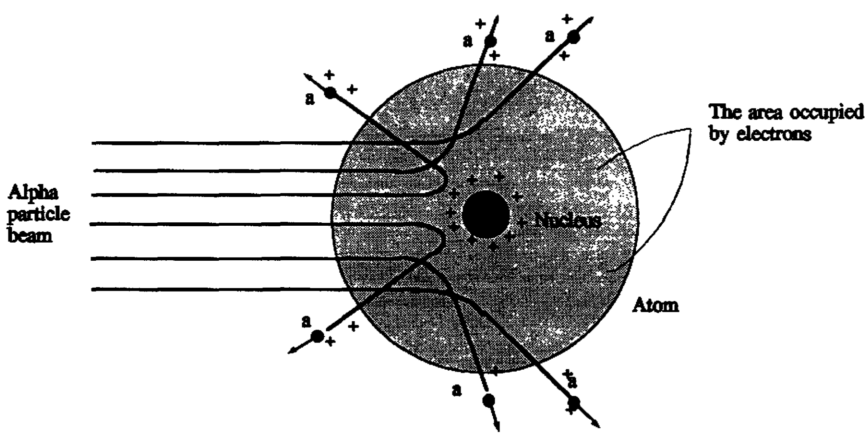

Figure 5.2 Rutherford's model of atom

So only in terms of the two kinds of particles' masses, charges, and electrically repulsive forces; some classical physical laws (mainly, F = ma, and Coulomb's law); and the laws of conservation of angle momentum and energy, the whole causal process from the interaction to the scattering results was fully described.

In terms of this modelling, a mathematical relationship between the speed, energy, charge (+2e) and number of the alpha particles, the number and charge (+Ze) of the scattering nucleus, and the scattering of the particles was established. This equation shows that N(θ), the number of alpha particles scattered through an given angle θ, will be:

$$N(\theta) = N_0 \, ns \, \frac{Z^2 e^4}{(4\pi\epsilon_0)^2 4T_\alpha^{\,2}} \, \frac{1}{\sin^4 \frac{1}{2}\theta} \, d\Omega \qquad (5.1).$$

This is Rutherford's equation. Here N_0, denotes the initial number of the particles in the incident beam; n, the number of atoms per unit volume of the target; s, the thickness of the target; $d\Omega = 2\pi \sin\theta$; and $T_\alpha = \frac{1}{2} M_\alpha v^2$, the kinetic energy of the alpha particles (Jackson, 1989, p. 70)[1]. It was soon proved by Geiger and Marsden that the calculations by the equation were in a very good agreement with their experimetal outcomes.

It is known that today, after the revolution of quantum mechanics, the basic principles of the Rutherford model still hold, and the decision about the reality of the nucleus remains unchallenged. Also, it made it possible to determine the value of Z, the number of electrons of the scattering atom; with another independent way of determining the value of Z (from the properties of the X-rays emitted by the element), it was found to be the same as what once was denoted as "atomic number," and the connection between chemical properties and the number of electrons in the atom underlying the periodic table of elements was implied. This was another great discovery then. Moreover, Rutherford's success sets up the physical reality of the process of scattering of a particle in seeking the possibility of sub-components. As we know, what convinces most physicists of the reality of quarks is the scattering experiment involving them.

5.3 Model (2): Indispensable Link

Now let us turn to the second kind of model. In this kind of model, an entity is postulated to be a component, a link, of a causal chain. Building up, therefore, a

[1] In addition to the above mentioned causal principles and conditions, the equation was first inferred on the ground of some assumptions, for instance, that one can ignore the movement of the target nucleus and the interaction between the alpha particles and electrons. In this way a simpler Coulomb's equation was derived. By realistically considering the assumptions and many practical details, Rutherford's equation, which was more accurate, was derived.

plausible causal connection, in which the particle is required by known physical laws for the existence of other already realistically accepted objects, becomes a major task of the model and crucial for the entity's reality. Namely, the model implies, in terms of all available knowledge and experimental facts, that there must be such a link between two parts of the chain, in which almost every other stage has been accepted as existing, otherwise the chain is realistically broken and the causal continuity is violated - an unacceptable situation for natural science.

The discovery of Ω^- is a good example for this kind of model. As mentioned earlier, the model involving the need for the particle had stipulated its distinct characteristics . This designated a circumstance in which if some kind of reaction happened, it would be very unlikely that something other than the particle would be involved in the reaction. This circumstance was a certain degree of energy in the accelerator.

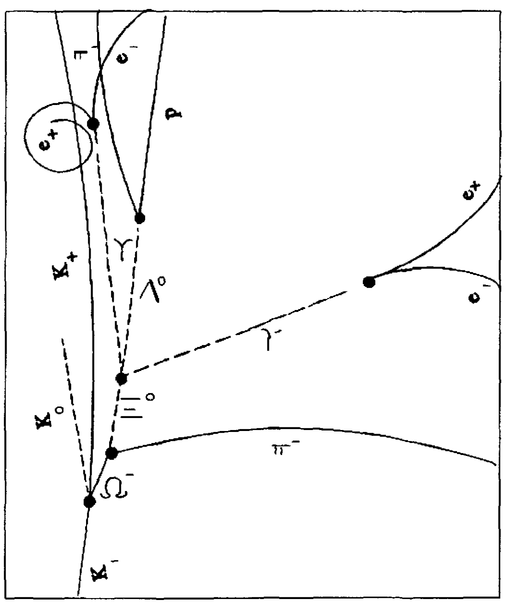

Figure 5.3 A reconstruction of the tracks in connection with the formation and decay of the Ω^-. The dotted lines indicate the paths of the neutral particles.

In 1960, a new synchrotron at the Brookhaven National Laboratory reached such a degree of energy. In 1963, by the synchrotron, protons were accelerated to 33 GeV and hit a target and produced 5-GeV kaons. Then the protons and kaons were shot into a 3-meter-diameter bubble chamber filled with liquid hydrogen. There the collisions of kaons with protons in the chamber produced some other particles. The paths of all these particles were then photographed by stereoscopic cameras. See Figure 5.3 (Ne'eman, p. 205). Of about 100,000 photographs taken during the experiment, one indeed showed the stamp of the particle. In the following years, numerous Ω^- events were recorded. With the progress of accelerators, by 1984, beams of Ω^- particles could be made and 100,000 photographs of the particle could be taken in any one experiment, compared with only one out of 100,000 at the beginning. The properties of the particle were found to fit the prediction by Ne'eman and Gell-Mann (see V.E. Barnes et. al., 1964, p. 204).

The causal process in the whole experiment started at (1) the interaction between the protons accelerated to 33 GeV and the target, resulting in 5 GeV kaons, neutral strange particles with strangeness S = -1. Then they both entered into the bubble chamber. (2) Now the interaction between the protons(S = 0) and the kaons happened, which produced Ω^- (S = -3), the particle being sought, and another two strange particles K^-(S = +1) and K^0 (S = +1). (3) Next, by the decay of the Ω^-, a xi hyperon Ξ^0 (S = -2) and a π^- meson (S = 0) were created. (4) In turn Ξ^0 resulted in another strange particle Λ^0 (S = -1) and a π^0 meson (S = 0). (5) And finally, from the π^0 meson two γ (both with S = 0) were created. Hence the process in the bubble chamber, i.e., from stages (2) to (5), was like this:

2. $K^- + P \rightarrow \Omega^- + K^- + K^0$

3. $\quad\quad \llcorner\!\!\longrightarrow \Xi^0 + \pi^-$

4. $\quad\quad\quad\quad \llcorner\!\!\longrightarrow \Lambda^0 + \pi^0$

5. $\quad\quad\quad\quad\quad\quad \llcorner\!\!\longrightarrow \gamma + \gamma$ (5.2)

So the sub-process from stages 2 to 3 was:

$$\Omega^- \rightarrow \Xi^0 + \pi^- \qquad (5.3)$$

from 3 to 4:

$$\Xi^0 \rightarrow \Lambda^0 + \pi^0 \qquad (5.4)$$

and from 4 to 5:

$$\pi^0 \rightarrow \gamma + \gamma \qquad (5.5)$$

Actually some sub-processes have not been shown here. Λ^0 in stage 4, for example, would decay into a proton p and a meson π^-:

$$\Lambda^0 \rightarrow p + \pi^-. \qquad (5.6)$$

And two γ in stage 5 would become two positron-electron pairs:

$$2\gamma \rightarrow 2e^- + 2e^+ \qquad (5.7)$$

We can see that the causal sub-process before Ω^- was a two-stage process, while the sub-process after the particle was a three-stage one. The existence of Ω^- was necessary to link the two sub-processes for the following reasons.

First, it was known that only at this degree of energy could a proton hitting the target produce kaons which were needed for creating Ω^-. This made the circumstance for it rare and unique.

Second, the whole process was impossible unless there was another particle, in addition to $K^- + K^0$, produced by the interaction $K^- + P$ at stage 2; otherwise some known physical laws, such as conservation laws of energy, charge, spin, and especially, strangeness, would be violated. The strangenesses of $K^- + P$, for example, must be equal to that of the right side of 2, $\Omega^- + K^- + K^0$ ($-1 + 0 = -3 + 1 + 1$). (The strange particles would decay by weak interactions, so the conservation law of strangeness in strong interaction would not apply to stages 3, 4, and 5.)

Third, without the particle Ω^- in stage 2, the decay processes from 2 to 3, and from 3 to 4, would be impossible. Most kaons and hyperons can decay only by weak interaction, hence the law of conservation of strangeness must be violated. Therefore their decay mode must be as follows: a strange particle with $S = n$ or $-n$ ($n = 1, 2,$ or

3) must decay firstly into a particle with S = (n - 1) or (-n + 1), and then into a particle with S = (n - 2) or (-n + 2), until it decays into a particle with S = 0 which would be a non-strange particle, often a nucleon or meson. Hence a particle with S = -3 must have a three-stage decay process until it produces a non-strange particle, and a particle with S = 2 must have a two-stage decay process, etc. Also, all the processes (5.3), (5.4) and (5.6) show that by the conservation laws of charge, energy, and baryon number, the particles could not decay into particles of the same strangeness. Hence the decay processes must be necessarily a weak interaction as shown by the equations from (5.2) to (5.7).

In turn, by this rule, when the decay mode in (5.6) $\Lambda^0 \rightarrow p + \pi^-$ was found, it was realized that since Λ^0 has S = -1, it must be a decay product of a particle with S = -2; but there is more than one strange particle with S = -2 : Ξ^0 and Ξ^-. Which one is the right one could be decided by some other conservation laws and by the analysis of photographs. Since the charge of both Λ^0 and π^0 in (5.4) was 0, and the particle producing them showed no track in the photograph, it must be Ξ^0, a neutral xi hyperon. By the same kind of reasoning, since Ξ^0 in (5.4) had S=-2, it must be a decay product of a particle with S = -3, but the only particle with S = -3 was Ω^-. Therefore by both the analyses of the process leading to Ω^- and the analyses of that resulting from it, it can be determined with no doubt that there must be a particle there as a link and that the particle must be the one we sought - it is necessary, both as a result of the sub-process before and as a cause of the sub-process after; and it is unique: only it could play the role for keeping the whole process going.

The fact that a particle is required as a link between two sub-processes of a causal chain strengthens its ontological status, because the entity is required and understood by known laws, especially various conservation laws, along two sub-processes of different characters and directions; and by the principle of causal connection. Even if the particle is not visible in any sense, or it "travels" in a way we could not describe individually and classically, if it is required in this way, its invisibility will by no means be an obstacle to its existence (it does not matter, for instance, that we could not predict, deterministically, when a certain singular micro-event will take place; what concerns us is the determination of the existence of such an event after it happens). There must exist an entity, visible or not, connecting some visible tracks in order to keep the causal continuity of the process. The process before it needed it as a result; and the process after needed it as a cause.

The short lifetime of the particle π^0 is another reason why the particle could not show any visible track in the chamber and on the photographs. (Actually those short-lived particles have even shorter lifetimes than the neutral particle.) What we can see on the photographs are, again, the causal process leading to it and the process (decay products) resulting from it. But Ne'eman says:

...if a particle C is created through the reaction A + B → C, and decays within 10^{-22} - 10^{-23} seconds by the process: C → F + G, the overall reaction observed will be: A + B → F + G, but by probing this reaction under various conditions, the existence of particle C can be deduced, and its properties revealed (p. 179).

One method for detecting short-lived particles shows how scientists typically make their decisions as to the reality of a particle by checking each sub-process of the chain involving it.

Suppose that one sends a beam of π^+ of a certain energy into a cloud chamber and obtains many photographs of the reaction in the chamber. By the visible tracks on the photographs, one would find the paths of p, π^+ and an electron-positron pair; so the reaction would seem to be: $\pi^+ + p \rightarrow p + \pi^+ + e^- + e^+$. However, by one's knowledge, a process exactly like what is shown would be unlikely to happen: the electron-positron pair should not be created directly by p and π^+. There must be something connecting, one way or another, the visible tracks - something invisible to make the process plausible. What is it, then? One may postulate a model of another two-stage process here: (1), the electron-positron pair should be the decay product of protons, while the protons might form in the decay of a neutral pion π^0. And (2) in turn, π^0, together with π^+, could be the product of an unknown short-lived particle ρ^+, the track of which is invisible in the photographs. So the real process would be as follows:

1. $\pi^+ + p \rightarrow p + \rho^+$
2. $\pi^+ + \pi^0$
3. $\gamma + \gamma$
4. $2e^- + 2e^+$ (5.8)

Now how to check the plausibility of the model?

First, the sub-process 1 could be established by various laws of conservation which play the most important roles in causal interactions: the products in it shall involve something other than p, in order to keep the conservation of charge, energy, momentum and others. Something with characteristics like those of ρ^+ is needed.

Second, checking the sub-processes from 2 to 4 step by step, we found no doubt about them. For instance, it has been already decided by some independent evidence that, although the track of π^0 is not visible, its existence is real.

Third, since we propose that the two pions are the decay products of the unknown particle ρ^+, its total mass should equal the sum of the masses of the pions and a known part of their kinetic energies. If the distribution of the masses of the pions and other products is exactly as desired, their sum just equals the desired mass of the unknown particle, then the particle should be there. This is confirmed by analyzing the photograph.

The final procedure is to make sure that this process is not an accident. This can be done by carrying out similar calculations of the distributions of masses for many such photographs of this type of experiment. In checking a similar reaction, 800 photographs were examined and the values obtained in all the cases were similar. The scientists decided then that the similarity is not a matter of coincidence. The evidence, therefore, has been obtained that the presumed short-lived particle, named ρ^+, does exist. (see Ne'eman, p. 182). As at the macro-level, a non-random character of an energy distribution implies, to scientists, an underlying causal process as well.

5.4 Alternative Independence and Directness

In terms of the discussion in the above two sections, we can further clarify two important concepts: "low-level" generalizations or laws and "directness" of an observation process. My suggestion is that low-level laws are those which can be used in a causal model, used to describe a causal process. But most basically they are the laws which can be confirmed in more than one way. Accordingly, if in a modelled causal process, the underlying physical laws for each sub-process can be multiply confirmed, then each sub-process is a real process and the whole process is real, too. Hence, the observation based on this process will be direct.

Let me use the first kind of model to illuminate the concept "low-level". In the Watson-Crick DNA model, one of the most basic ideas is, of course, that of a double-stranded helical structure. The physical laws initiating and supporting this idea were about the relation between the pattern on X-ray diffraction photographs of a substance and the corresponding crystalline structure of the substance. It had been recognized that a certain pattern of X-ray diffraction indicates a helical structure. The X-ray diffraction pattern of DNA fibers clearly exhibited helical characteristics. The spots in the X-ray photograph indicated the reflections of some physical particles in the fibers; the precise location and intensity of the spots reflected the physical parameters of the helix, its pitch or repeat distance, and its diameter. Before 1953 actually, Wilkins, Franklin and Gosling, another researcher in King's College in London, had realized this and Wilkins had estimated the pitch angle and the diameter of the molecule. By the end of 1952, Franklin got her experimental results on the B form, including the strongest meridional reflection indicating the stacking of the bases with a separation of 3.4Å, a helix diameter of 20 Å, and the hydration results indicating external phosphates and two or three strands. This "really was an important clue as it suggested the existence of two-fold symmetry axes running normal to the fiber axis, requiring the two chains of a double helical model to run in opposite direction," said Crick (M.F. Perutz, 1969). These were taken as known facts before Watson and Crick's model was born[1].

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[1] For the list of the known facts, see Portugal and Cohen, pp. 263-4; for the figure showing the relation between the pattern on X-ray diffraction photographs and the crystalline structure of the substance, see p. 241.

The first X-ray diffraction photograph of a stretched dried film of DNA was obtained in 1938 by Astbury and Bell. Around 1950, more and clearer photographs were obtained or analyzed by Wilkins, Franklin, Riley, Oster, Randall, Gosling, etc. before Watson and Crick. From the very beginning, no disagreement occurred about the significance of the pattern at all. They all realized the helical characteristics of the structure of DNA. The reason is very simple: the correspondence between patterns of an X-ray diffraction photograph and the structure of the substance had long been an experimental law in physics. Due to its wide application in many different fields, in particular its physical use in studying the movement of electrons in crystals, X-ray diffraction had been used as one of the most basic experimental tools in particle physics for years. There was no doubt about its detailed mechanism and the correspondence between the X-ray picture and the structure of its object.

Moreover, from the point of view of contemporary physics, I can see no reason to take the DNA model as a "high-level theory". This is a model of the structure of macromolecules. The most theoretical presuppositions, background knowledge and laws, needed in the model are concerned with the relations between groups of atoms (phosphate group, sugar group, for example). By that time, they had been accepted as experimental laws in physics and chemistry (at level 3 in Watkins' list). Compared with theories like quantum mechanics, the DNA model is a low-level descriptive model.

I have said that in Rutherford's model of the atom, some classical laws and facts were synthesized by the hypothesis of the nucleus to draw a picture distinct from Thomson's. The model treated the nucleus and electrons as tiny bodies, the state of the whole system being decided by their number, energy, momentum, charge, speed and direction of movement, and coulomb force (electrically repulsive force between alpha particle and nucleus, since both are positively charged). These properties are also all that are needed in describing the process from the interaction of alpha particles, nucleus and electrons to the resultant distribution of scattering particles, as is shown by Rutherford's equation (5.1). They were "home truths" in classical physics because they had long ago got independent experimental confirmation. They were "operationally definable quantities," to use Margenau's term.

Of course one can still argue that this model has this or that relationship with some high-level theoretical elements, otherwise it is incomplete or problematic. This model had some implications which can not be explained classically, for instance, the stability and identity of atoms. And this is where quantum theory is needed.

Indeed, any "model" might be incomplete in this sense and thus might be related to some theoretical elements. Yet this fact just indicates that a model's purpose is often limited and that when we consider its value, this kind of weakness can be thought of as irrelevant. The point is whether a model can satisfy its own goal. For a causally descriptive model, this could be to see whether it has adequately displayed some aspect of the reality of a causal process. No doubt, Rutherford's model of the atom has achieved this goal. It has correctly shown that because of the existence of such a nucleus and of such a relationship between a nucleus and electrons in an atom, there will be such an interactive process between alpha particles and the nucleus, leading to such a result. For this purpose, the model is complete and does not really need quantum theory or whatever. That is why Rutherford's equation, derived from classical physics, remains unchanged in quantum physics. A good low-level phenomenological description will hardly be abandoned as merely fictitious, however theories change.

So what makes a quantity or generalization "low-level" or "phenomenological" consists both in its being causal and in its being experimentally corroborated in various independent ways (or, in Margenau's term, being defined operationally on the basis of various experiments). Both aspects are needed here in order to rule out, for example, the case that the "independent testability" might be taken to refer to any (theoretical) element which could not be used in a causal model.

This is also true of quantities and laws in causal models of causal chains. That is, what makes something a sub-process of a causal chain is both its being part of the chain and its being independently corroborated as real. As in my interpretation of "low-level," there is an inter-dependent relation between sub-process and the whole chain: (1) a sub-process is real because it can be taken as a plausible and necessary part of a real causal chain (according to known laws); and (2) the whole chain is real because every part of it can be eventually corroborated independently. If someone were to object that this involves an unbearable circularity, I would like to take (2) as more fundamental. The reality of a hypothetical entity as a link in a chain will be established by its necessity for the chain, by the independent testability of every other sub-process of the chain, and also by the independent testability of the whole chain. This "alternative independence" constitutes the core of the "directness" of a causal chain, of the test based on the chain and of the evidence from the test. This conforms to what I said about the process of establishing the physicalness of human action at deeper and deeper levels of the world in chapter 2.

So the directness of an observational process depends on whether the reality of each sub-process in it can be established in this way. Let us take a look at the case of Ω^- again. I shall now show the independent testability of the most difficult sub-process, the stage from 4 to 5 involving the creation of π^0.

There has been more than one method to detect neutral particles and more than one method to detect short-lived particles. The troublesome thing here is that the neutral pion π^0 is a combination of two difficulties: it is both neutral and short-lived. So the length of its path on photographs from the point of its formation to the point of its decay was shown as only a dot between the paths of Ξ^0, Λ^0 and two γ in the reconstruction. But there are already two methods for detecting it.

The first direct proofs for its existence were found in 1950 in the cyclotron of the University of California at Berkeley. In the cyclotron, the photons, produced by a proton beam striking a target, struck a tantalum plate, producing the electron-positron pairs. By comparison, people found the energy of the electron-positrons did not match the energy of the photons. This could not be explained by any known process unless it was assumed that the photons were formed in the decay of a neutral meson with a mass of about 150 MeV - the expected value for π^0 .

An "even more direct proof" (Ne'eman, p. 106) was found in the same year at the University of Bristol, UK. When a rapidly moving proton from cosmic rays struck a photographic plate elevated to a height of 21 kilometres by balloons, a great number of paths were seen to emanate from a single point, like a "star", indicating many particles were produced by the collision at this point. Some paths were identified as tracks of π^+ and π^-. In their vicinity tracks of an electron-positron were found, with no visible tracks connecting them to the tracks of π^+ and π^-, or to the single point - the centre of the star. It was known that the electron-positron could only be produced by the decay of photons. The energy and directions of the photons were determined. It was found that the photons did not emanate from the centre of the star (Ne'eman, p. 106).

This discovery indicated that the photons were the decay products of π^0, which was formed together with the charged pions. By analysis of the energy of the photons formed when π^0 was decaying, the mass of π^0 was estimated to be 150 $\pm$10 MeV. The process was

$$\begin{array}{l} P \rightarrow \pi^+ + \pi^- + \pi^0 \\ \qquad\qquad\qquad \lfloor\!\longrightarrow \gamma + \gamma \\ \qquad\qquad\qquad\qquad\quad \lfloor\!\longrightarrow e^- + e^+ \end{array} \qquad (5.9)$$

Moreover, it was found later that there was another process in which a neutral pion was produced:

$$\pi^- + p \rightarrow \pi^0 + n \qquad (5.10)$$

By the interaction, the energy of the neutral pion was also estimated. It was concluded that the difference between the mass of π^- and that of π^0 is (5.4$\pm$) MeV. This conformed to the results from the above two processes. A neutral pion of such a character was needed as the link. Its character was revealed. Therefore the particle was found.

Hence the stage from 4 to 5 in the discovery of Ω^- could be determined in many independent ways. The connection between Ω^- and its final product in stage 5, two γ, was direct since they were connected by such a chain of interactions.

Of course scientists use the terms "direct" and "indirect" test in more than one way. Sometimes an indirect test could be getting the quantum numbers of a particle by measuring the numbers of its decay products or by calculating the probability distribution of the particles. Yet the direct/indirect distinction can change. A method might be taken as an indirect one when it first occurs because we are not sure if its result really makes sense. But once the result is confirmed by some other method as reliable, then the next time when the same method is applied to another kind of particle, the test will be regarded as "direct", since it has in this way become an alternative independent method. At first it was agreed that neutrons were to be studied only indirectly through the use of chambers, using neutrons to strike a substance and studying the protons ejected from the collision to know what the properties of the neutrons were. Yet after proving the method's successes by some alternative techniques and by using the particle to create the phenomenon of artificial radioactivity, scientists accepted the same method of finding out neutrons as a direct method of studying other similar particles. In the case of π^0 and many other particles, calculations of the energy and other quantum numbers of the decay products have become direct for identifying the properties of their cause.

It follows that whether evidence is direct or not does not depend on the "length" of the causal chain - the numbers of sub-processes in the chain. Once each sub-process has been accepted as real for independent reasons, the whole process, however long, will be taken as producing direct evidence. Directness depends, therefore, on how well each of the connections involved was already corroborated.

As well, the meaning of directness has very little to do with that of being "observable", whatever that means. Scientists would with no hesitation take tracks in chamber photographs as "observations" of their particles' paths, so they are direct evidence. Yet what about the particles the paths of which are invisible in the photographs? Still, the scientist take some tests of the invisibles as being "direct" as observational ones. Why? For the same reason: those observation processes are direct just because the reality of each of their stage has been taken for granted owing to its independent testability, and this is exactly the case with some invisibles.

My account of directness reflects the practice of science. Examples include the finding of charm-carrying particles, J/ψ, independently by Ting's group and Richter's group in completely different types of experiments in 1974. Again, discoveries of charm-carrying particles were always made both by measuring properties in various ways and by identifying them as links between two sub-processes of a causal process[1].

The most convincing evidence for quarks was provided by the scattering experiments in SLAC (Stanford) and DESY (Hamburg) since the late 1960s. As I mentioned, confirmation of Rutherford's model in more than one way had established the reality and directness of the corresponding scattering process. When some electrons with energies of 20 GeV or more hit a proton head on, some large-angle deviations of the electrons passing through the target were found, significantly larger than what would have been expected if the matter and charge in the hadrons were uniform. The hard parts inside the hadrons were thought of as very small compared with protons, with spin ½. Owing to this experiment, "it became generally accepted that quarks are real, and actually do exist in the hadrons. Despite the fact that no one has yet detected the tracks of released quarks experimentally, it is today difficult to deny their objective existence, or consider them a mere mathematical exercise" (p. 218).

5.5 Model's Relevance and Viable Path

Let me summarize what we know about model Constituents now.

The existence of an entity is the existence of its causal function. Its existential claim is a causal hypothesis. Acceptance of the entity is unlikely to happen if no causal connection has been grasped between it and any other object. For this goal, both an existential or causal model and experiments are needed.

Existential models can be of different kinds. One kind is the structure model. A model of this kind describes either the entity's inner mechanism or the entity's structural

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[1] Two neutral charmed mesons, $D^0\bar{D}^0$, for example, are the products of a two-stage process: (1) an e^+e^- annihilation decays into ψ (another charm-carrying particle), and then (2) ψ decays into $D^0\bar{D}^0$. Soon, however, they become the cause of another two-stage decay process: (3) they decay into a neutral kaon, a negatively charged kaon and some pions and (4) the neutral kaon decays into two other pions. Their masses are thus estimated. They are regarded as further confirmation of charm quarks (D^0 is proposed to be composed of the quarks $c\bar{u}$, for example, $\bar{u}$ being an anti-charm quark). (see Goldhaber, et. al., 1976, p. 255; and Peruzzi, et. al., 1976, p. 569)

relation with other parts. As a result, such a model shows either the entity's causal initiative or a distinctive way in which it would interact with other objects.

Another kind of model, the process model, mainly describes a process, a causal chain, in which a particular entity is a unique and necessary link between two other parts of it. These two kinds of models can be interlaced. A model of the first kind could be embedded as a sub-model in a model of the second kind or the former could "entail" the latter, as shown by Rutherford's case. In both cases, the entity is viewed as a component of both a structure and a process. A comprehensive hypothesis about it is formed. Both kinds of models should be mature in the sense that the causal mechanisms and chains are natural and plausible, according to the knowledge and facts available. They should also be realistically intended so that no important element, which is taken as part of the cause of certain phenomena, is treated only instrumentally.

Intuitively, a causal model is usually at a low level of a theory, since it is composed of physical laws and items which are at a relatively low level in the theory. But the dominant factor in deciding an element as a low-level one is the availability of alternative independent techniques to confirm it. We may say that if most basic causal laws used in a model could be tested independently in more than one way, the model is a low level one. Correspondingly, if, in a causal chain, each sub-process can be confirmed in this way, its reality will result. So will the reality of the whole process. And this chain is "direct".

Being part of a theoretical framework, such a model could be related to this or that "fundamental" presupposition about the world. But as far the issue of the reality of entity is concerned, the relatedness is neither necessary nor decisive. Like the DNA model, a model can be regarded as phenomenological or low-level simply because the whole science involving it is taken as low-level or experimental compared with other sciences.

Of course, what such a causal model tries to describe is a causal relevance between a micro-entity and some other object. If the model has the features described above, it should be regarded as having nicely described the relevance.

Although such a model offers us a blueprint for experiments to test the modelled relevance or chain, the success of an experimental system exmplifying such a chain is not guaranteed. Sometimes we need to find some new interactive relationship between the entity and the experimentally available substances to materialize the chain. The new interactive relations are made of causal laws of experimental devices, which are different from the laws underlying the model but are used to practice the modelled causal chain. I call these different experimental ways "causal paths," each of which may consist of one or more experimental steps. Sometimes a chain can not find a path to materialize it. Thus the hypothesis of the entity fails to obtain the experimental component of the required good reasons. And sometimes a chain can be represented by more than one causal path, or more than one kind of relation among more than one kind of object can show the existence of the chain. In this case, the experiment has a tree-structure; and the hypothesis at issue obtains the strongest support that a thing could possibly obtain. I will discuss this structure in Chapter 7.

The relation between constructing a model describing causal relevance and experimentally exemplifying the relevance is complex. Either can happen first. Sometimes experimental investigation of an entity can precede the maturity of its model. In any case, however, the maturity of an entity's model and its experimental investigation are interlaced, both are needed to advance and finalize the acceptance of the entity, as shown by the history of DNA discovery. Discussing the two aspects separately is mainly for convenience.

Chapter 6 The Story of DNA: How a Good Reason Obtains

This chapter is about the Experiment Constituent, EC. I shall discuss, through the case of the discovery of DNA as the genetic substance, how the EC is obtained combined with the MC, and how they compose a good reason to accept the reality of an entity.

6.1 Initial evidence for DNA

The discovery of the substance now called DNA (deoxyribonucleic acid) was an example that an entity, which is unobservable in van Fraassen's sense, could be an empirical one. From the very beginning, scientists never doubted the existence of the substance as "something there." But the discovery of DNA as gene - the cause of heredity - was delayed until the middle of this century, about 80 years later, after the causal connection between it and hereditary phenomena was decided experimentally and its mechanism to initiate the hereditary process was displayed by the Watson-Crick model of its structure. This is a typical and integrated case of the acceptance of the existence of a microentity.

The substance DNA was actually found by Friedrich Miescher in 1869 in two different but parallel experimental studies. First, in trying to identify and characterize the proteins[1] in the white blood cells that constitute the pus, Miescher observed under a microscope the behaviour of pus cells and the nucleus which had been a readily separable and identifiable cellular component. In different salt solutions, Miescher found, the cells

[1] Protein, from the Greek proteios, meaning "of the first importance", was found about thirty years earlier by Gerardus Johannes Mulder, and was considered the most significant material in cells at that time.

and nucleus produced marked differences in their behaviour, with swelling, dissolving, or shrinking. In the experiment with weakly alkaline fluids he obtained precipitates and found that, unlike a known particular protein, myosin, contained in the pus cells, the precipitates were not soluble in water, acetic acid, very dilute hydrochloric acid, or sodium chloride solution. So he decided that this substance could not belong among any of the protein substances known hitherto, but was a different one. Furthermore, from the observation of the cells under the microscope, Miescher saw that weakly alkaline solutions caused the nucleus to swell and eventually break open; hence he inferred that the substance could be in the nuclei.

Secondly, Miescher tried to confirm his inference in a different way. For this he had to develop some techniques to separate pure nuclei from the reminder of the cell. But this was of course difficult. For quite a while, his attempts led to no progress in purification of his preparation. Finally, he hypothesized that a certain fluid which contains pepsin, a protein-digesting enzyme, might be able to break up cells, and might allow a separation of the protoplasm from the nucleus. Guided by this hypothesis, he first washed the cells with warm alcohol to remove the fatty materials that would interfere with the subsequent analysis. Then he prepared filtered extracts of swine stomach - a source of pepsin. He put the cells into this solution for several hours so that gradually a pulverized, greyish sediment separated from this clear yellow solution. And under the microscope the sediment was identified to be pure nuclei. And finally, when these isolated nuclei were put, as was the original pus cell, into a weakly alkaline solution and then followed by acidification of the extract, the same precipitate was detected as first observed from the whole cell. This showed that the precipitated material had indeed come from the nuclear fraction of the cell.

In this way Miescher found the same substance in yeast, kidney, liver testicular, and nucleated red blood cells. He then concluded that it was not like any kind of protein and so termed it *nuclein*. In order to distinguish nuclein chemically from all other known cell substances, i.e., to further decide its distinct existence, Miescher at once tried to determine its elementary composition - the relative proportions of hydrogen, carbon, oxygen, and nitrogen present in it. The techniques available then enabled him to make chemical analyses which indicated that it was acidic, rich in phosphorus as well as in the four normal elements in cells, and with a unique ratio of phosphorus to nitrogen.

Soon after, the essential chemistry of the compound was thoroughly worked out by his student Richard Altmann, who called the substance "nucleic acid," and some other researchers. For the rest of his life, Miescher continued studying the composition of nuclein in the cells of different animals, particularly in salmon sperm. In 1878, he reported the elementary composition of salmon sperm nuclein to be $C_{29}H_{49}N_9P_3O_{22}$. Largely because of A. Kossel's work on the chemical constituents of nucleic acid at the beginning of this century, its basic components were known, but the structural relationships among them were not clear. There is a sugar - a ribose - that contains five carbon atoms in a ring, and a phosphorus atom surrounded by four group oxygen atoms to make a phosphate group which makes nucleic acid acidic. The phosphate group also links the sugars together in an unending alternation of sugar and phosphate. Along this chain there is a third type of compound called a base attached to each sugar. So the triptych of sugar, base and linking phosphate group is called a nucleotide. The bases have five varieties: guanine (G), adenine (A), cytosine (C), thymine (T) and uracil (U). By the 1920s, the nucleic acids had been distinguished into two different kinds: one is DNA, primarily inside the cell nucleus, consisting of bases C, G, A, T; the other is RNA, more common in the cytoplasm around the nucleus, consisting of bases C, G, A, and U, and a very slightly different sugar: ribose instead of DNA's deoxyribose, which lacks a single atom of oxygen. Except for those, all other aspects of DNA and RNA are the same.

Clearly, in this history the progress of studying nucleic acid was directly tied to the development of techniques. Around the time of Miescher's finding, people were faced with some conceptual problems. For example, descriptions of the nuclear chromosomes were unclear and indefinite; this led to difficulties in deciding what stage of cell division was seen or even whether chromosomes had been seen at all. But as time passed, in the second half of the last century cells, chromosomes and nucleus were all taken to be recognizable and identifiable. Some of their chemical behaviours were already also thought of as observed. This was due to the enormous improvement in the quality of instruments, such as microscopes, for visualization of the nucleus. For example, the problems of optical distortions due to the spherical shape of the lens and the differences in the deflection of the coloured rays composing the spectrum had been minimized by the invention of the compound microscope. The problem of spherical aberrations was solved by J. J. Lister in 1830. Meanwhile, many associated techniques, like those of staining different areas of the cell, were developed to make the nucleus more visible (see Portugal and Cohen, p. 32-36). We can understand this from the fact that the substance "chromosome" was named after the result of making it visible by staining. The discoveries of the mechanisms of staining reactions were also great achievements which led to the belief that the nucleus could really be seen under such circumstances.

The belief in the techniques was behind the fact that biologists in the last century were overwhelmingly realists about what they studied. We know now the dimension of DNA: the entire molecule is about 1 millimetre long, but about 20 Å (1 angstrom = one ten-millionth of a millimetre = 1/100,000,000 cm) across; the steps of the staircase of the helix, the purine-pyrimidine pairs, are 3.4 Å apart, and the helix goes through one complete turn every ten rungs, every 34 Å. Surely DNA, bases, etc. of this kind of dimension are unobservable in van Fraassen's sense. What scientists can see with unaided eyes in a test tube is something like, for example, a white precipitate - yet it is a collection of a million molecules. But it was believed that the molecules and the chemical components of the molecules are as real as the precipitate. From the very beginning of the history of the discovery of DNA, the realistic attitude has prevailed.

There were many reasons for this. Here I just pick out another one: there was absolutely no scientific reason for the scientists to make an existence/non-existence line anywhere in the inference or investigation from fragments of organisms to cells, nucleus, chromosomes, nucleic acid, nucleotide and bases. The search process along this line was a realism-preserving process: once you accept the reality of cells, you accept that of their components, and so on. The underlying rationale for deciding the reality of the cells was basically the same as that for the nucleus. If the cells are observable under the microscope, so are the nucleus, and so on. So even before they recognized the significance of DNA, they certainly regarded this substance as "something there". What is of interest here is, however, the fact that the scientists did not challenge the eligibility for observation of the cells or nucleus and thus made a realistic conclusion about them. This indicates that for the scientists, a physical process could be taken to be an "observation" without the necessity of comparison with the unaided senses, contrary to van Fraassen's criterion. Also we can see that in practice at least some scientists could accept an entity with a very limited conception of it. Scientists could make the decision before they thought they had fully understood it. They trusted fully the techniques for experimenting with the entity. As far as theoretical elements are concerned, what would always be involved in such an acceptance might be background knowledge of relevant technology, which is more likely to be at a low level than anything else.

Yet according to the opinion held in this thesis, a good reason, the strongest support for the reality of DNA as the gene substance, had yet to be established at this point: neither its causal model nor any causal connection from it to the phenomena of heredity had been found. We have declared that the existence of an entity is a function of the existence of a causal interaction involving it. Once scientists can put an entity in a causal process, they will accept the entity. In this way they have already put the entity into an existing empirical knowledge network to understand it. As a result, the acceptance becomes an almost unchallengeable part of the core empirical basis for any further scientific building. Of course we allow for the possibility that some people might accept an entity without understanding it or putting it into any causal connection. Yet strictly speaking, the case of Miescher's finding DNA did not belong to this possibility: for one thing, when isolating, staining and observing it, some causal interactions between it and some other parts of experiments had been made. For another thing, Miescher and many others had understood at least where the substance was from and what it was made up of. The point is, the good reason for the existence of the DNA molecules, as the *gene substance*, has to come from the combination of finding by experiments and understanding by a causal model the very causal relation to other parts of cells or heredity. I dare to say that obtaining such a combination is equal to obtaining some true empirical knowledge about an entity.

Like all of his contemporaries, Miescher could not realize the significance of his discovery, the central importance of DNA to heredity. He thought nuclein to be nothing more than a storehouse of phosphorus for the cells. For the scientists of that time, every important and interesting thing for the organism must be in protein, while nucleic acid was thought of as irrelevant (see, J. Cherfas, 1982, pp. 5-6).

6.2 Finding a Causal Path to DNA

When Mendel proposed his theory of heredity, the postulated heredity unit, called "gene" later, was surely a hypothetical entity. Nobody was sure where it was, even though they tended to think that protein must be the place to find it. It was also unknown what the entity would be like, how it would be constituted. Yet, like many other realistically proposed theoretical entities, one thing was clear: it must be the one that causes the phenomena of heredity. Although Mendel's work made it possible to predict the transfer of characteristics from parents to offspring, the long search for the substance did not stop there. Rather, Mendel's discovery made the investigation into the transferring mechanism more intensive in the early part of the twentieth century. People were more encouraged to find the substance corresponding to the "gene", which they thought would result in the transmission of specific characteristics. As W. S. Sutton declared: "We want to know the whole truth of the matter: we want to know the physical basis, the inward and essential nature, the causes as they are sometimes called, of heredity. We want also to know the laws which the outward and visible phenomena obey." (W. Sutton, 1903, p. 116) This effort to find the causal process of hereditary phenomena led eventually to the discovery of DNA as the gene.

Since Mendel, therefore, most studies of the physical and chemical nature of the gene were attempts to solve this crucial question, obviously a causal question: "What were the steps or mechanisms by which the genetic material of the cell produced each character? That is, how did the cell transfer the genetic information, whatever its molecular form, into specific phenotypes, such as eye colour or hair bristle shape?" (Portugal and Cohen, pp. 135-136)

For a long time, however, people were still trying to find it in protein. Within a few years after Mendel's work was verified widely by the end of last century, the relationship between the chromosomes (but not DNA) and Mendelian genetics was broadly accepted, with the help of the work of Sutton who had vaguely conceived that chromosomes might be divisible into smaller entities and they might be the basis of characteristics in organisms. (Sutton, pp. 116-7). But the relationship of gene to the chromosomes or any other physical entity was unknown; no experiment can show the relationship - the steps of the causal path from it to chromosomes - consequently it was not possible at that time to determine its substance and physical dimension.

However, in the 1910s and 1920s, the studies of chromosomes, the analysis of the factors in chromosomes were undertaken. Many theories or models for chromosomes were developed. Examples included: based on the studies on *Drosophila*, Morgan (T. Morgan and C. Bridges, 1916) proposed the possibility of thinking of chromosomes as made of a chain of chemical particles whose change would be reflected in the end-product of the activity of cells; R. Goldschmidt (1917) proposed a non-linear gene linkage theory for the chromosome; and Castle (1919) produced his "rat trap" model for the *Drosophila* chromosome. In 1922, Morgan's group had analyzed two thousand factors on the four *Drosophila* chromosomes (Morgan, 1922). They had tried to calculate the size of a gene by this number: the total length of the four chromosomes was 7.5 microns while the width was 0.2 microns; when the volume calculated from these figures was divided by 2000, the diameter of a single gene would be about 60/1,000 of a micron (later was 20/1,000). The tentative estimate of the size of a gene, however, was taken as another reason to think of protein as the genetic substance. For according to the then-popular "tetranucleotide hypothesis" that four bases were present in equal proportions in the nucleic acid, nucleic acid could not be large enough to contain so many genes, so it was too small to be a genetic substance.

But through studies of the mechanism of chromosomes with different techniques, including X-ray treatment of *Drosophila* and ultraviolet light's effect on its mutation, people found that DNA could have a molecular weight of 500,000 to 1 million. As a result the tetranucleotide hypothesis became less and less acceptable; more and more importance was given to nucleic acid. Nevertheless, until the end of the 1930s, most scientists still had some reasons to insist on the primacy of proteins as the genetic material.

The real starting point to find the connection between hereditary phenomena and DNA was the experimental discovery of genetic transformation of bacteria in 1928 by Frederick Griffith in London, followed by Oswald T. Avery's studies of it.

Several different types of pneumococci could be isolated from the sputum of individual patients suffering from pneumonia. It had been observed that over several years the incidence of one type of pneumococcus had markedly increased while another had decreased for the patients. In the search for chemical component or components making up the bacterium and responsible for the immunological specificity caused by bacterial infection, Griffith injected mice with two different types of pneumococcus, one the virulent "S" form, the other the nonvirulent "R" form. The R form did not, alone, have any harmful effects; the S form contained the usual virulent and lethal form of pneumococcus but the disease-causing bacteria had been killed by heating the culture to 60°C, so this one too had no ill effects when injected alone. The mice, however, that received the double injection of two harmless preparations died, and from their blood Griffith collected and cultured live and virulent pneumococci indistinguishable from those of the S form that had been dead when injected. This was a striking discovery: a transformation to the virulent S form had happened (F. Griffith, 1928). A formal expression of it is:

$$[(S' \rightarrow \neg H) \ \& \ (R \rightarrow \neg H)] \ \& \ [(S' \ \& \ R) \rightarrow H]$$

here S' is the S-form but with dead bacteria; R is the R form, $\neg$H harmless, H harmful. This case, as I indicated in chapter 4, might resist any formalization as a true proposition in a consistent system of logic. The causal interaction behind it plays a real role: with the presence of heat-killed and so nonharmful S-form pneumococci, the nonvirulent R-form pneumococci were transformed into virulent and thus harmful S-form pneumococci and greatly replicated (forming its colonies) and thus led to the death of the mice. This was contrary to what was known of genetics and inheritance. The problem was what were the factors within the R-form and S-form bacteria that carried out the causal reaction?

Griffith himself did not recognize what the factor was behind the transformation. He thought that the causal factor, called by him S substance, was still a kind of protein which enabled the R form to build up the specific protein structure of the S form. Later the transformation was demonstrated by Dawson and R. Sia using several different approaches. Meanwhile they made it possible to study the effects of various agents on the transformation process under well defined and adequately controlled laboratory conditions. Now naturally the critical work would be to isolate and identify the factor responsible for the transformation phenomenon. Late in 1931, L. Alloway, like Dawson and Sia, a member of Avery's laboratory, made a step in the process of identifying the substance. By some new methods of filtering, extracting and purifying, he obtained a much more efficient extraction of the active substance which could transform R cells into the S type of pneumococcus. That is, the cause of transformation would be in the material in this extract. When Alloway added alcohol to the filtered extract, he observed that "a thick stringy precipitate formed, which slowly settled out on standing." This character reminded him clearly of Miescher's "nuclein." Although he was inclined to think the active material in these extracts was protein (a nucleoprotein), he, like Dawson and Sia, concluded that "the exact nature of the active material in these extracts still remains to be determined ..." (see Alloway, 1933)

As far as I know, the first definite judgement about the role of the nuclein (DNA) in the transformation was made by Avery and his team. When Avery started to be involved in pursuing the transforming factor in the 1930s, he also believed it was protein. But after years of painstaking effort, the evidence was overwhelming. In 1944, he and his team published their findings (Avery, Macleod and McCarty, 1944).

At the beginning of the publication, they indicated that they clearly recognized that what they tried to find was the real cause of hereditary characteristics. They pointed out that the most striking example for studying this was the transformation of specific types of pneumococcus, because it could be experimentally induced and was reproducible under well defined and adequately controlled conditions. Next they stated the main steps their studies consisted of: "the major interest has centred on attempts to isolate the active principle from crude bacterial extracts and to identify if possible its chemical nature or at least to characterize it sufficiently to place it in a general group of known chemical substances" (p. 137).

So the first major step was to extract as purely as possible the genetic substance. For this they stated in detail what methods and techniques they used and how they worked. Their own methods for measuring transforming activity and the preparation of active extracts included, for example, the heat killing of the cells at 65°C before extraction to inactivate the enzymes that could destroy the transforming factor (for a long time they did not recognize the presence of the enzyme capable of degrading DNA that remained during the isolation procedure and so were frustrated by failing to know the cause of erratic results that damaged their transformation experiment); the use of chloroform to precipitate and remove protein from the material; the addition of a purified specific enzyme to destroy the carbohydrate responsible for the immunological reaction; and so on. After careful treatment by these methods, they supposed that the resultant substance was extremely pure and should be real genetic material.

Following that, the next step was to identify the chemical nature of the material. As Portugal and Cohen asserted, this approach "was essentially a process of elimination" (p. 147). In our terms, this was a process to identify a causal path from the transformation phenomena to their real cause. It consisted of procedures to determine that:

(1) it was not carbohydrate - both because it should have been destroyed by the specific enzyme, and because it was shown not to be soluble in alcohol and ether;

(2) it was not protein - again this is supposed both by the methods in the experiment, the deproteinization procedure, and by the negative outcomes of the use of sensitive chemicals to detect protein. Also, tests on the purified substance to determine its immunological specificity with antibodies for both the carbohydrate and protein components of pneumococcus were negative, showing the absence of contaminants in it.

(3) it was not pure enzymes used to destroy protein - the enzymes showed no effect on the transforming activity.

(4) it was not RNA - although "a weak positive response was found with a test

(orcinol) for RNA, this may have been an *artifact*, for the application of a pure crystalline enzyme that rapidly cleaved RNA had no effect on the transforming properties." (Portugal and Cohen, p. 150, my italics)

At this stage of determination, only nucleic acid was left as a potential major kind of known chemical substance.

Furthermore, the establishment of the causal path from this substance to the phenomena of transformation was also a process to identify the substance by its characteristics as that of DNA:

(5) The test of it for DNA had given a strong positive response; and the chemical analysis of the pure substance gave an elemental composition, the amount of phosphorus in different preparations, very close to that expected for DNA (8.5 to 9.1 percent).

(6) Its comparison with known preparations of DNA (including a sample from A. Mirsky) showed its chemical and physical properties were in fact of those of DNA. By studies in ultracentrifuge its molecular weight was estimated as 500,000. Its ultraviolet absorption properties were characteristic of nucleic acid.

And finally, the causal path was indicated by this fact:

(7) The application of a known DNA-degrading enzyme, deoxyribonuclease, completely destroyed its transforming activity.

Now for Avery and his co-workers, it could not be clearer that the causal path between DNA and the transforming activity had been established. They claimed: "The data obtained by chemical, enzymatic and serological analyses together with the results of performing studies by electrophoresis, ultracentrifugation and ultraviolet spectroscopy indicate that, *within the limits of the methods*, the active fraction contains no demonstrable protein, unbound lipid or serologically reactive polysaccharide and consists principally, if not solely, of a highly polymerized, viscous form of deoxyribonucleic acid" (p. 158, my italics). The conclusion would be obvious: DNA is the fundamental unit of the transforming principle of Pneumococcus type III.

Still, there were some people questioning the conclusion. As usual, the question focused on the reliability of the methods and techniques and the range in which they can apply. Mirsky questioned whether the techniques in the inactivation experiment guaranteed the purity of the extract as deoxyribose nucleic acid: even the best preparation of nucleic acid may still possibly contain a small amount of protein. Indeed he had some specific reason for this doubt: it was possible that as much as 1 or 2 percent of protein could be present in a preparation of "pure, protein-free" nucleic acid; but in one of the most sensitive direct tests for protein, the Millon reaction, as much as 5 percent of protein would give a negative result. Therefore Mirsky argued that "No experiment has yet been done which permits one to decide whether this protein is actually present in the purified transforming agent, and if so, whether it is essential for its activity, in other words, it is not yet known which the transforming agent is - a nucleic acid or a nucleoprotein. To claim more, would be going beyond the experimental evidence" (Mirsky and Pollister, 1946, quoted in Portugal and Cohen, p. 151). It is worth noting that Mirsky's doubt was based on his own finding that a chromosome fraction would lose its chromosome appearance, when the fraction was treated with protein-degrading proteolytic enzymes[1].

A series of papers published by Avery's team in 1946 were to convince the questioners of the reliability of their techniques, and provided an even more efficient procedure for isolating the transforming substance from several types of pneumococcus. McCarty, a member of the team, concluded "that the accumulated evidence has established *beyond a reasonable doubt* that the active substance responsible for transformation is a nucleic acid of the deoxyribose type" (McCarty, 1946, my italics).

But for some people, the reason for doubting the conclusion still existed. Besides the traditional tendency to prefer protein in this respect, this reflected the fact that the discovery was not yet completely embedded into the existing body of empirical knowledge. But this was largely due to the situation of the body, rather than to the discovery itself. The problems were mainly due to technical difficulties, which made people unable to demonstrate the causal path in a different and independent way. Until

[1] It is known now that protein components play a significant role in the degree of gene expression, but at that time this function was confused with the fundamental role of DNA. Again this shows the necessity of knowing the details of every major part of a causal path for deciding the whole causation and its reality.

1951, similar transformation experiments were very hard to repeat in other bacterial systems, with the exception of A. Bovin's one using the *E. coli*. Many repetition experiments gave erratic results or were associated mainly with antigenic traits. Some of them were from the unclarity of the detailed mechanism of interaction between DNA, protein and RNA - this led to the possibility of a variety of interpretations of Avery's experiment. Some who studied the ultraviolet absorbency of nucleic acids found that the nucleic content in the nucleus appeared to diminish, which was shown later to result from a large accumulation of RNA in the cytoplasm. The decrease in nuclear content of nucleic acids was thought to show the instability of DNA, so that DNA was regarded as unlikely to be the source of information for all cellular activities. And finally there was a problem of being unable to observe different base ratios in DNA, which was later found for bacterial DNA. This made people think that the composition of DNA did not vary sufficiently from organism to organism to account for the differences in information content.

For these reasons, the discovery did not immediately get as wide acceptance as that of the previous finding of Griffith and the later hypothesis of a double-helical structure for DNA by Watson and Crick. Avery has not now got the Nobel prize which in retrospect he should have. This is an indication how important an alternative independent technique and an understanding of the DNA's hereditary mechanism were to establish finally the causal path.

6.3 The Decisive Determination

In 1952, just one year before Watson and Crick published their work, Alfred Hershey and Martha Chase made another contribution to convince most of the sceptical scientists of the vital role of DNA in heredity, because their "Waring blender experiment" provided an independence and diversity of evidence for Avery's conclusion. Their experiment consisted of demonstrating that bacteriophages that infected and replicated in bacterial cells did so by injecting their DNA into the cells (Hershey and Chase, 1952; Hershey, 1965).

From the 1930s to 1950s, studies of bacteriophages, those submicroscopic agents, by the research group called "phage group," (of which Hershey was an originator), progressed fruitfully. Some phage particles could be visualized under the microscope with a technique called *dark field visualization*, while the development of electron microscopy provided a much better way of observing the viruses. The interaction between phage and bacteria was closely studied; many new techniques were developed which resulted in understanding the expression of genes and other phenomena of heredity. Fortunately, Avery's article in 1944 was greatly appreciated by the people in the group. Hershey had conceived a similar role for DNA for the infective process of bacteria.

A bacteriophage consists of a protein coat surrounding a strand of nucleic acid. A single virus can infect a bacterium, which 30 minutes later releases a shower of hundreds of new viruses, each a replica of the infecting phage. The question was which one, the protein coat or the nucleic acid, was necessary for the infective process, namely, what viral component was responsible for the self-duplication phenomenon. To decide it, Hershey and Chase made use of the knowledge that protein contains the sulphur that DNA does not, while protein does not contain the phosphorus that DNA does, and accordingly they used a method to distinguish them: the radioactive labelling which is still of great use today. In 1951, the so called "Waring blender experiment" consisted of the following work. Hershey and Chase grew phage and bacteria in a culture that contained radioactive phosphorus (^{32}P), thus tagging the nucleic acid, as well as radioactive sulphur(^{35}S), thereby labelling the protein. Then they introduced them into unlabelled bacteria suspensions. The next step was to separate the phage protein coat from the bacterium by adsorption and agitation. The final stage was to measure the percentage of each isotope that remained with the bacteria and the percentage that was lost and then try to find if the bacteria cells could still involve the infection process.

"Waring blender" was named after the machine they used to prevent attachment of the phage protein coat to a bacterium. The blender can easily break the phage coats away from the cells, as revealed by electron micrographs, and a centrifuge separated the bacteria cells from the culture liquid and discarded viral protein. Then they found the isotope ^{32}P, i.e. DNA, stayed with the cells, and ^{35}S, protein, always stayed in the liquid, and, the cells still released fully infective newly made viruses - the new phages were continuously made in the cells. This indicated that the DNA in the cells were working to produce newly synthesized bacteriophages. The experiment confirmed Avery's conclusion. See figure 6.1 (Portugal and Cohen p. 182).

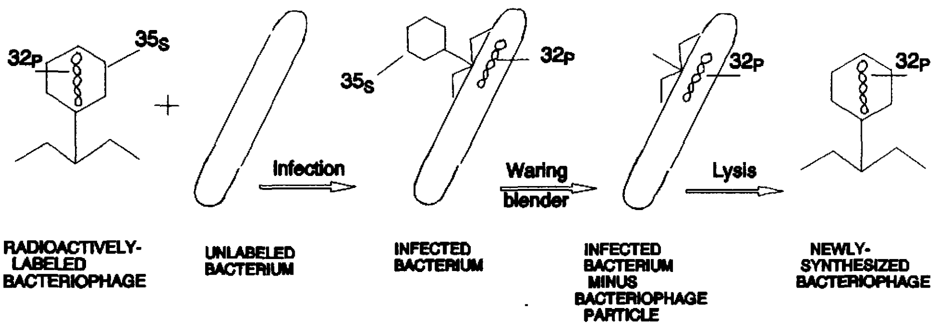

Figure 6.1. Hershey-Chase experiment

Even though a majority of the scientists in the community had accepted Avery's conclusion partly, at least, because of this experiment, it still looked inconclusive even to Hershey, who was quite conservative, for it suffered from somewhat the same problem as Avery's experiment. By measurement of the isotopes, still about 20 percent of the protein was found remaining attached to the bacteria cells. It could be argued that maybe this small percent of protein can play a significant role in making new phages in the cells. Another reason was the mechanism for the DNA to make phage replication was unknown. S. Luria, who shared the Nobel Prize with Delbruck and Hershey for their fundamental studies on bacteriophage genetics, said:

> In 1945, when the clones of phage mutants were first noted, and even in 1951, when the distribution of mutant clones was determined, the mechanism of phage reproduction was uncertain. Not only had the Watson and Crick model of DNA structure and replication not yet been proposed, but even the identification of the phage genome with phage DNA was still questionable, and was actually questioned by myself and several others....Despite the clearly recognized fact - that phage multiplication occurred by replication of subviral components followed by assembly and maturation - all biological evidence insisted, to the very last, in directing our attention to a "fission-like" rather than a "template-like" process of replication. Only with the Watson-Crick (1953)

model was the fission vs. template antinomy resolved. (S. E. Luria, 1966, p. 177-178)

So the final chapter in the history of the discovery of DNA as the basis of heredity was opened by Watson and Crick's model of the chemical structure of DNA and closed by the experimental proof of the hypothetical model. The model provided a pattern to combine all evidence into a whole picture, and, more importantly, a cause for heredity. So the model was, as Luria correctly called, not only of DNA structure, but also of replication. Here I shall just point out some aspects of the development around the model.

The search for the structure started a long time ago. In the early 1910s the conclusion made by Levene with chemical means supporting the tetranucleotide structure hypothesis of nucleic acid was only a result of deduction from the degradation products. In the beginning of the 1950s, the method of chemical synthesis, the X-ray photograph, and other techniques for interacting with DNA had developed and had already strongly suggested what the structure of DNA was like. So in addition to Watson and Crick, there were at least three people, Wilkins, Franklin and Pauling, close to the discovery of the helix structure.

As I said in chapter 5, however, Watson and Crick's hypothesis was that the helix was a double-stranded one, instead of a single-stranded one as Wilkins thought or a three-stranded one as Franklin did. They conceived a hydrogen bonding between the bases of the two strands - by which the adenine would always pair with thymine, while guanine would always pair with cytosine. This bonding shows that the DNA structure is actually a replication scheme, a "copying mechanism" inside the genetic material - the cause for heredity. The arrangement that A always pairs with T and G with C meant that the base sequences of the two intertwined chains were complementary to each other. Given the base sequence of one chain and hydrogen bonding, that of its partner would automatically be determined and formed - a single chain could be the template for the synthesis of a chain with the complementary sequence. Watson and Crick emphasized:

...our model for deoxyribonucleic acid is, in effect, a *pair* of templates, each of which is complementary to the other. We imagine that prior to duplication the hydrogen bonds are broken and the two chains unwind and separate. Each chain then acts as a template for the formation onto itself of a new companion chain so that eventually we shall have two pairs of chains, where we only had one before... Moreover the sequence of pairs of bases will have been duplicated exactly (Watson and Crick, 1953a).

Therefore if the model is correct, the internal device of DNA that initiates the causal path from the DNA to the phenomena of heredity becomes clear, genetic continuity is understood, and DNA as the cause of heredity is assured. The model also suggests a new independent approach to proving the relation between DNA and heredity phenomena: by a DNA strand's self-copying process. Very soon this approach got confirmation from M. Meselson and F. Stahl's experiment, which was called by Watson "classic" and by J. Cairns "the most beautiful experiment in biology" (Cherfas, p. 16). This was because of the novelty of the idea, the technique and the precision of the experiment. In order to know if DNA can reproduce, a way of distinguishing copy DNA from original DNA (template) is needed. Usually, the way is to labell DNA with radioactive isotopes. The distinctive feature of Meselson and Stahl's way was to use a heavy isotope of nitrogen to mark some DNA in some (ammonium chloride) cells, while using the normal, light isotope of nitrogen to mark the DNA in some other cells. After breaking the cells with detergent and extracting the DNA, putting these two sorts of DNA into tubes with a solution of caesium chloride, and spinning them round in an ultracentrifuge at about 44700 revolutions per minute for 24 hours (this was a critical method called "density-gradient centrifugation" to separate light from heavy DNA), the two sorts of DNA of different density separated out and collected. Because of different density, the light DNA strand floated at the top of the tube, while the heavy one remained at the bottom. A photograph taken by ultraviolet light showed two bands of DNA, one above another. Now if the Watson-Crick model was right, these two DNA would become two templates to form new complementary strands for each other. That is, new DNA molecules would contain one light strand and one heavy strand, have a density between the heavy and the light DNA, and therefore have a position of their band on the photograph between the two original bands. This was "exactly what Meselson and Stahl saw. The evidence was incontrovertible. DNA reproduced..."(Cherfas, p. 16).

By the early 1960s, the causal mechanism of the double-helical structure of DNA was clear. In the following years, in the light of the model and, as always, by innovations of techniques, new waves of discoveries concerning every step along the causal path from DNA to the replication of cells came. In 1961, Jacob and Monod postulated their messager RNA (mRNA) hypothesis (Jacob and Monod, 1961). Another form of RNA, transfer RNA (tRNA), was determined in 1965 (Holley, et. al., 1965).

By 1969, most of the details of the genetic code had been resolved. The structure of protein was also revealed: it has twenty subunits called amino acids; the amino acids are strung together by "peptide bonds" into the chain; and the shape, properties and function of protein are determined by the order of amino acids along the peptide chain. The relationships between the sequence of four bases in DNA and the sequence of twenty amino acids in protein being synthesized had been established. The nucleotide in the mRNA represents one amino acid. It was displayed by the stretching of the complex process of the causal path of transfer of genetic information from DNA to RNA and protein - Crick's "Central Dogma" finally became "DNA makes RNA makes protein". All these eventually comprised a detailed picture of the causal path or passage from DNA to the replication of cells, every step of it repeatedly proven real by various independent techniques:

DNA (genes) → RNA (mRNA & tRNA) → Protein (amino acids) → Cell
(Ribosomes)

That is, DNA molecules carry the hereditary message of genes in the form of triplets of nucleotide. With the aid of enzymes, the genes are converted into mRNA. The mRNA then goes away from the nucleus to the ribosomes. The tRNA brings the requisite amino acids to the ribosome and reads the code words in mRNA, thus proteins are synthesized. And finally from the protein chains cells are formed[1]. Now our knowledge about this

[1] We know now that in addition to this causal path, under special circumstances information can pass directly from DNA to proteins, but not vice versa, while information can go both way between DNA and RNA.

process is by no means complete, but the existence of DNA as the gene of heredity has become one of the most basic parts of our current empirical foundation. It had entered into the core of our most stable knowledge and would be the last thing to be challenged. It has already gone beyond the stage of reasonable doubt[1].

6.4 When a Good Reason Occurs

The discovery of DNA as the genetic substance is a typical case which completely shows what components a good reason is composed of, how the components are brought in by their interaction, and which component plays what role in this process. We can summarize the points in this chapter as follows.

(1) Clearly, as my account of good reasons, Cause Identification Condition, CIC, indicates, a good reason for acceptance of an entity is a synthesis of a model constituent (MC) and an experiment constituent (EC). MC is a causal model displaying by existing causal laws the existence of a causal connection between the entity and certain phenomena; EC is obtained from experimentation by which a viable causal path from the entity to some other objects is materialized.

(2) Although EC is a decisive element, without MC a good reason would not be obtained. For one thing, without MC the causal mechanism of the entity or its necessity in a causal chain would be unknown, and as a result, among other things, the entity could hardly be distinguished from others. For another thing, very often without the help of MC the process to establish a casual path experimentally beyond any doubt can not be finalized - MC can bring a brand new way to form an independent test and eventually solve the disputes over previous experiments. The Watson-Crick model led to and also justified the confirmative experiment for DNA as the genetic substance in which DNA's self-replication mechanism, i.e., a short causal path from original DNA to copy DNA,

[1] In the later 1960s, in the literature we can still find a few articles doubting the realistic status of the DNA's helical structure described by Watson and Crick's model (see, e.g., Hamilton, 1968). Since 1970, especially after gene engineering began, I personally have found no such objection at all.

rather than a longer path from DNA to protein or transformation phenomena, was focused and materialized. The formation of MC and EC comes from the interaction between their elements. Typically, they become mature almost at the same time, and this is the time a good reason forms.

(3) Experimental implementation of a causal path is often a combination of a negative procedure of eliminating the possibilities of other factors, paths or artifacts responsible for the phenomena to be explained, and a positive procedure of identifying the entity by its known properties from. This is a process to experimentally assure the entity as *the* cause under certain circumstances, parallel to its causal model which assures the entity in terms of various existing causal laws.

(4) The reliability of an experiment in this respect is a function of the reality of each step of the path. The justification of the reality of each step along a causal path in an experimental system can also be a combination of positive and negative procedures. Methods for the procedures, like those for checking the reliability of each part of a causal connection in a causal model, should be alternatively independent.

(5) Once MC and EC are both obtained in the case of DNA, the ontological status of DNA is finally established. Any further studies as to its composition or properties would contribute to understanding, and would surely confirm its existence, but would not be likely to substantially change its ontological status. We may say now that if a postulated theoretical emtity meets only one of MC and EC, it is in doubt; if it meets neither of them, it is surely far from being real; and if it does not meet the maturity condition over a long time, the initial reason for postulating it loses most of its scientific significance, it becomes very highly "theoretical." In the end, its postulated realistic status would have made no sense - people will widely agree that it is a fiction, even though it might still be of some use.

There is one thing I need to make clear here: the methods used in constructing a causal path may differ from those laws in the causal model. As I said, in many cases a causal path is not the same as a causal connection or chain described by its model: the causal path must be a causal process happening in an experimental system, while a causal chain in a model could be just a possible one according to existing causal laws. In building an experimental system to find a causal path to prove the existence of a modelled causal connection, we have to consider other chains consisting of causal laws by which certain experimental devices work. In Hacking's words (Hacking, 1992), we actually use another layer of modelling, (I call it "experimental modelling"), namely models of how the apparatus and instruments themselves work to realise a causal chain. In Meselson and Stahl's experiment, for example, "density-gradient centrifugation", labelling by isotope, photographing by ultraviolet light and so on were used to isolate, extract, separate, combine and visualize DNA fragments. The formation and reality of the causal path from original DNA to copy DNA depended on what techniques this experimental system was made up of, how these techniques worked, and what the system was like as a whole.

In short, therefore, the deepest basis for a good reason or for realism about an entity lies in the features of its corresponding experimental system and techniques, in those of its "experimental modelling." A scientist's belief in an entity consists in the belief that its experimental system and techniques are satisfactory. In the next chapter, I shall show when they are satisfactory to the scientists and why if they are satisfactory then the scientist will believe that she can really experience or manipulate the entity at issue. This chapter will thus provide a foundation for my account of a good reason.

Chapter 7 Experiments and Technology: Structure and Nature

In the last two chapters, I have displayed the constituents of good reasons for an entity. I have argued, meanwhile, that my notion of good reasons does fit, in particular, the practices in nuclear physics and molecular biology. But one can still ask: why should we trust that our experimental system has indeed materialized the causal relevance described by some models between the entity and some other objects? Or, how can an experiment assure us that we can really "experience" the entity just by manoeuvring the pieces of the experimental devices? This is a question about the character that such an experiment should have. This question concerns the foundation for my notion of good reasons for realism.

Now I will reveal a "tree-structure" of experiments and some properties of related technology through my own observation of two comprehensive experiments in molecular biology. I will argue that the required character is in the structure, and in turn the structure is an indicator that an experiment of this structure has indeed materialized the causal role of the entity in question. I will also show that the nature of the technology can give us some ideas why the structure of experiments may underlie a claim about the finding of the causal existence and role of such an entity as a DNA fragment both in hereditary (replicating) process and in the process of experimental investigation of it.

Finally, I conclude that after all these conceptual and experimental studies, it appears that scientists' claim (a) about the DNA molecule indeed has obtained a good reason - has met CIC. Now given RR, which also stands for the presuppositions of the realism debate which legitimate a move from individual observation to persisting existence, or, from causation to reality, we can also say, philosophically (within the debate), that claim (b) about the entity implied and the realism about it is reasonable.

7.1 The HLA Gene Haplotype projects

From December 1991 to October 1992, I frequently visited the Tissue Typing Laboratory, Pathology Department, McMaster University. I especially observed two projects the scientists and technicians in the lab were working on during this period. The first was to find polymorphisms in the genes which lead to unusual and rare HLA-DR, DQ and DP haplotypes in people[1]. The second project was to find polymorphisms in the transporter associated with the antigen processing (TAP2) gene located in the HLA class II region. Both projects had succeeded in finding new polymorphisms (see D. Singal, M. Ye, et al. 1992, 1993). Here for illustration I mainly use the first project[2].

Today, a huge number of molecular biologists are involved in experimental studies which either identify the sequences of genes in this or that area of the human genome, or, seek different structures (polymorphisms) of the genes. In either case, a micro entity, a DNA fragment with a particular structure, is studied or revealed. Like the HLA projects, all such investigations in the world almost always employ a kind of experimental system consisting of a similar structure. Generally, once based on such a system, a single experiment is good enough for identifying a specific gene structure or polymorphism. A successful finding of such a single experiment can be accepted by the international gene bank and published without waiting for confirmation by other sources.

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[1] HLA, Human Leucocyte Antigens, are cell surface proteins, encoded by a series of closely linked polymorphic genes - genes DR, DQ, and DP, each respectively presenting one HLA-DR, DQ and DP (together called HLA-D) haplotypes. The type of each gene DR, DQ, and DP, and the distribution of them, display polymorphisms.

[2] The theoretical purpose of the project is that by analysis of the distribution of DR, DQ and DP haplotypes, the discovery of the presence of certain unusual and novel haplotypes and the absence of certain alleles would demonstrate extensive polymorphisms of HLA-D region genes and suggest that occurrence of different DR, DQ and DP genes on a haplotype may also be important in generating class II haplotype diversity. Comprehensive analysis of HLA-D region genes would be useful for a precise and fine definition of these genes, for analysis of evolution of genes and for construction of a human phylogeny. Practically, the discovery will be helpful for studying relevant immune responses, clinical transplantation, and their association with disease susceptibility.

sources. Why do scientists trust the experimental system so much in making their conclusions about a mircoentity? One answer I think is in its structure.

This kind of system is a synthesis for several parallel processes of studying the object. Each process has some sub-processes for checking the performance of the process. Hence such a system could be regarded as having a "tree" structure: each main parallel process is a "trunk" and each sub-process on each main process is a "branch." Such a tree structure gives the system a "mechanism" for self-checking, self-improving and self-confirming. The projects I observed consisted of four parallel processes or trunks: 1. Serological Typing (ST), 2. DNA Typing, 3. Cloning & Sequencing, and 4. Restriction Fragment Length Polymorphism (RFLP). Let me briefly describe them now.

1. Not long ago, HLA-D genes haplotypes were usually determined by Serological Typing, a method which studies the haplotype through typing the antigens and antibodies expressed by the HLA-D genes. This method could decide general haplotypes but had some difficulties in precisely deciding sub-haplotypes which are the results of slight differences between a few bases.

2. Thanks to recent developments in technology, HLA-D alleles can be determined with greater precision in large scale population studies by DNA Typing, a new molecular typing technique. The technique defines alleles by hybridization of sequence-specific oligonucleotide (SSO) probes following the Polymerase Chain Reaction (PCR) amplification of genomic DNA. These techniques constitute one of the main processes (trunks) that were actually carried on in the two projects.

The main steps of this causal process (trunk-path) can be stated as follows. First, after obtaining samples of blood from people, genomic DNA from each of the samples of blood is extracted. Genomic DNA is the DNA macromolecule which contains all kind of genes for human heredity. Since HLA class II genes are only part of a genomic molecule, separation of the HLA-D fragment and amplification of it are needed. So the second step is to pick out and replicate the fragment into a huge number of copies from each genomic DNA by the technique PCR in order to study them. The next task is to determine the type of each HLA gene fragment by the hybridization of SSO probes - called "DNA oligo typing." Namely, the second trunk is this:

(Sampling blood) → Extracting genomes → Picking out and Amplifying HLA gene fragment by PCR → Determining haplotypes by hybridization.

When one enters a molecular biology laboratory, what one would notice first of all would be test-tubes, bottles, centrifuges, some electric instruments, computers and the noises from some big refrigerators and other machines. Microscopes will not necessarily be found in the lab. One's first impression scarcely suggests that the scientists and technicians are working on some micro-entities. The operation of experimentation is often something like calculating the density of a solution and adding it to test-tubes. Indeed, with a little training, a novice could do a lot of routine work. However, when the scientists and technicians in the lab discuss their operations in the experiment, they often refer to something like cutting or joining a small piece of nucleic acid or even one or two bases. They are actually talking about precisely manoeuvring groups of atoms! How can they be so sure about it? Let us see some details of the second main path.

Extracting Genomes. Having separated lymphocytes from sample blood, the scientists extracted genomic DNA from the cells[1]. It was followed by separating the protein, RNA, and DNA into different layers in the test-tube through centrifugation. Consequently, the DNA sample was concentrated. At this stage of the purification of DNA, scientists need to measure exactly how much DNA was present in a solution by ultraviolet absorbance spectrophotometry. The rationale of the technique is that the amount of ultraviolet radiation absorbed by a solution of DNA is directly proportional to the amount of DNA in the sample. As we pointed out, measurement is also an interaction with the object, a way of checking, by making a different causal process to interfere with the object. That is why I shall call this kind of causal path a "branch path" in contrast to the main causal paths that stretch throughout the whole experiment. Thus the tree-structure of causal paths in the experiment is like this: the tree's trunk is the

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[1] It was done by using a specific chemical to lysis the cell to release the contents of the cells; by using some deproteinizing-enzymes, proteinase K, and phenol, to precipitate proteins; and by using RNA-digesting enzyme RNase (ribonuclease) to remove RNA, leaving DNA in aqueous solution.

main causal path representing the main chain of interactions on which the experiment is built; while the tree's branches, growing from the main path, are the paths of interactions at different stages of the experiment between the entity or its causal products and other physical means or objects. Branch paths at a stage of experiment represent the various ways of checking, identifying or singling out the existence and performance of the tree-trunk, the main chain at the stage. So the formation of branches affects the way the trunk grows into the next stage.

PCR. Next, in order to select the required segment of the DNA for amplification, the region of the DNA molecule must be delimited. This can be done by annealing two short oligonucleotides (18 to 28 nucleotides in length), called primers, to the two ends of each strand of the double helix of the DNA molecule. The sequences of primers are complementary to the sequences of the bases of the two ends of the required segment (as template), so that they can attach to each other and denature the DNA. Thus only the required segment between the two ends is replicated. This is the rationale of PCR[1]. PCR can also be used to perform a similar task as the cloning of genes does, although each of them has its own advantages. Thus PCR and cloning are two alternative independent means for the same object and purpose. PCR is a great innovation in genetic engineering[2].

There are ways to confirm and analyze the work of PCR. Two new branch-paths to "touch" the DNA molecules grow from the main trunk of the experiment. One of them

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[1] The process of using PCR is this: by adding a special enzyme, DNA polymerase I enzyme, a new strand which is complementary to the strand of the template was formed. Then the reaction mixture is heated to 96°C, so that the newly synthesized strand detaches from the templates. On cooling, more primers anneal at their respective positions (including positions on the newly synthesized strand), and the polymerase I enzyme, unaffected by the heat treatment, carries out a second round of DNA synthesis. The reaction can be repeated 30 to 40 times, producing a huge number of copies of the strands of the HLA gene fragment.

[2] The operation of a PCR machine is simple. It is slightly smaller than my computer monitor; what one needs to do is just put the PCR tubes into the holes on the machine; then press buttons to set time, temperatures and numbers of the cycles of reaction; and then go away. Hours later, everything is done.

is gel electrophoresis. Gel electrophoresis can separate different DNA fragments according to their sizes[1]. It is often used for picking out a required fragment from many fragments of different size. If PCR worked, there should be one band on the gel the position of which should be on the place corresponding to the size that we expect the segment to have, indicating the required fragment has been amplified.

To see the band, we have to visualize DNA molecules in the gel. There are two main methods for this, one is staining it with ethidium bromide (EtBr); another is autoradiography of radioactively labelled DNA. We can take these two means to interact with the molecules as two new sub-branch-path growing from the branch-paths made by using gel electrophoresis[2]. In the HLA experiment, both ways were used.

SSO Hybridization. Once the production of PCR was confirmed and the required segment was singled out, the main causal path of the experiment extends to the stage of determining the sequence of the DNA segment. Two ways can be used for the purpose: one is hybridization, and the other is sequencing. The former is often used to determine a large number of DNA samples since it can apply to them at the same time; the latter is used for detailed studies of a small number of samples since it is more precise. The HLA project involved a large number of DNA samples (since it was to find polymorphisms), so the former was used here. So a method which constructs a part of a trunk can be used to form a branch of, or sub-trunk parallel to, another trunk.

The principle of the hybridization is that we can discover the sequences of a

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[1] This is how it works: putting a sample of the production of PCR into the gel, and placing the Gel in an electric field, we can expect that the DNA molecules, because they carry negative electric charge, will migrate towards the positive pole. The smaller the DNA molecule, the faster it can migrate through the gel; thus DNA molecules can be separated according to their size, resulting in many bands located in different positions on the gel.

[2] EtBr binds to DNA molecules by intercalating between adjacent base-pairs, causing partial unwinding of the double helix. The position of the DNA bands is clearly visible by shining an ultraviolet light on the gel, which causes the bound EtBr to fluoresce. On the other hand, if the DNA is labelled by a radioactive marker, then the DNA can be visualized by placing an X-ray-sensitive photographic film over the gel. The radioactive DNA will expose the film, revealing the banding pattern.

strand by checking if it can pair with a probe - a DNA molecule fragment the sequence of which is known. The strands which can pair with - hybridize - the probe will have the sequence complementary to it. The probe is labelled with a radioactive nucleotide or alternatively, some kinds of non-radioactive compounds, in order that the positions of the bound probe are detectable by autoradiograph. On X-ray film the strands that pair with the labelled probe will show black dots. Hence some parts of the sequence of the segment are known. Repeating this procedure will lead to the determination of each strand's genes.

Now we can see that the second main chain of the experiment was a net of various interactions among DNA molecules, nucleotide fragments and various enzymes both on the main chain and on the branch-paths.

7.2 The Complex Tree Structure

So far only half the story is told. The result of identifying the sequence of the DNA fragment by trunk 2 must be determined in some other major way, especially if some unusual and novel haplotypes were indeed found or some confusions also occurred. The third main chain, trunk 3, plays this role. This was a process from the extraction of genomic DNA to cloning, and to sequencing the HLA-D fragment.

3.Cloning. Like PCR amplification, the purpose of cloning of DNA is to obtain a huge number of copies of a required DNA fragment in order to study its sequence. Beginning with pure samples of genomic DNA molecules, the gene cloning experiment should first construct the desired recombinant DNA molecule. For this, we need the DNA molecules to be cloned and the vector, or plasmid, some kinds of small circles of DNA found in bacteria or virus chromosomes which will transport the gene into the host cells and will be responsible for its replication. The DNA molecules to be cloned and the vector must be cut at specific points and then joined together in a controlled manner. The manipulations will be done by some special purified enzymes. The enzymes for cutting are restriction endonucleases, the enzymes for joining are ligase.

The next step was to introduce the recombinant into living cells, usually bacteria, which would then grow and divide to produce clones. Therefore a large number of recombinant DNA molecules - the vector carrying the HLA-D fragment - could be produced.

Selecting and Extracting Desired HLA-D clones. There are several ways, by several causal path branches, of checking which bacteria cells take up the HLA-D molecule fragments since usually in addition to the plasmid with the desired inserted fragments, there are also those self-ligated vectors, the vectors with undesired molecules and unligated molecule fragments, all of which could be taken up by the host cells. The principle of the ways of identifying the required recombinant DNA molecules, the desired transformants, is called "insertional inactivation." The plasmid, bluescript, has an ampicillin resistance gene called *lac Z'* which codes for a part of the enzyme β-galactosidase. So if a host cell harbours a normal plasmid, the cell will be able to grow in ampicillin medium, while the cells which just have unligated molecule fragments will not. Moreover, if the site on the plasmid being cut by the *Bam* H1 is the place of gene *lac Z'*, and the HLA-D fragment is inserted into the *Bam* H1 site, the gene will be broken and will lose its ability to synthesize β-galactosidase (that is "insertional inactivation").

So the stage of checking and picking out the transformants was undertaken: plating the cells on to an ampicillin agar, then only those cells containing the plasmid could survive. And further, among those surviving cells, the scientists needed to screen which ones were or were not able to synthesize β-galactosidase. Only those that were not able to do it should contain desired recombinants - the HLA-D fragments. The screening method was this: it was known that a lactose analogue called X-gal would be broken down by β-galactosidase to a product that was coloured deep blue. So if X-gal was also added to the agar, then those non-recombinant colonies, the cells of which synthesize β-galactosidase, would be coloured blue, whereas recombinants with a disrupted *lac Z'* gene and unable to make β-galactosidase, would be white. This causal interaction helped both to check the success of the transformation and to pick out the recombinants. It is worth mentioning that there is more than one way of doing the work (see Brown, 1990, chap. 4). The plasmid from the cells that are white will be extracted.

After the extraction of the plasmid, one more job still needed to be done to obtain the desired recombinants, because some plasmids may be inserted with different DNA fragments other than the HLA-D fragments. Again, the method to identify and pick out the desired fragment was hybridization probing as above, but it was much simpler here. After this, a huge number of copies of the purified fragments was obtained.

Hence the work before sequencing of the DNA fragment consists of the following procedures:

Cloning (1) Construction of a recombinant DNA molecule → (2) Transport into the host cell → (3) Multiplication of recombinant DNA molecule → (4) Division of host cell → (5) Numerous cell divisions resulting in a clone → bacterial colonies growing on solid medium → (6) Selection of cells involving recombinant DNA → **Selecting and extracting the gene** (7) Extraction of recombinant DNA from the cells → (8) Selection of desired recombinant DNA.

Sequencing. The final stage of trunk 3 was to obtain the information of the structure of the HLA-D fragments about some particular samples which have been found to have polymorphisms on trunk 2. Perhaps DNA sequencing is the most important technique available to the molecular biologists for determining the precise order of nucleotides in a segment of DNA. Actually, besides gene sequencing, the technique of hybridization probing can also be used in the same manner, not as that simple way of selecting the desired recombinant, but as the complex one of deciding the haplotypes of a large number of samples after PCR as in the above section. Since here the purpose was to check the result of hybridization probing used in the complex manner, gene sequencing was the choice.

As we can expect, there are two alternative independent techniques for gene sequencing - the chain termination method (CT) by F. Sanger and A. R .Coulson in the UK, and the chemical degradation method (CD) by A. Maxam and W. Gilbert in the USA. The two techniques are radically different in principle and in most aspects but equally reliable. In the HLA project, CT was used.

Usually just one strand, strand a, of the recombinant molecules needs gene sequencing, for the complementary one can be thus decided. But sometimes sequencing and reading another one, strand b, is necessary. In a sample Q, when reading one strand of the recombinant molecules, the 52nd gene was found missing. If this is true, a new kind of sequence or mutation will be found. But it needs checking. So gene sequencing of strand b was done, and the 52nd gene was found there. Hence gene sequencing itself could be used for checking the success of gene sequencing[1].

4. Some other techniques for the same purpose of sequencing are also available, such as "Restriction Fragment Length Polymorphism" (RFLP). These either less convenient or less direct methods are mainly used for obtaining information on the structure of smaller pieces of DNA.

Now the experiment was completed. In principle we have at least four trunk-paths to determine the sequence of a DNA fragment. In the HLA projects, trunk-paths 2 and 3 were systematically carried out. The tree-structure of the whole experimental system is as shown in Figure 7.1. The details of trunks 1 and 4 are not mentioned in my description and not shown in the figure.

Some conclusions about the structure can be drawn immediately as follows.

The four parallel trunks stand for four major different, independent alternative ways to interact with the same entity; each way consists of many pairs of independent alternative techniques. The branches represent other alternative ways of interacting with the same microentity. The ways of interacting with the microentity are diversified not only by those radically different techniques for the same purpose, but also by the different usage of the same method, as shown by the sequencings along the a and b strands. A trunk and its branches are interdependent. A trunk could not continue without growing some branch which checks the growth of the chain. This shows that such an integrated experiment has internal self-checking and -correcting mechanisms due to the synthesis of the ways of interacting with the DNA molecule. The structure is a net of

[1] Of course, the findings of an experiment can be further checked by applying the same experiment to the samples of the parents of the people in whom some polymorphism of HLA-D gene distribution was found.

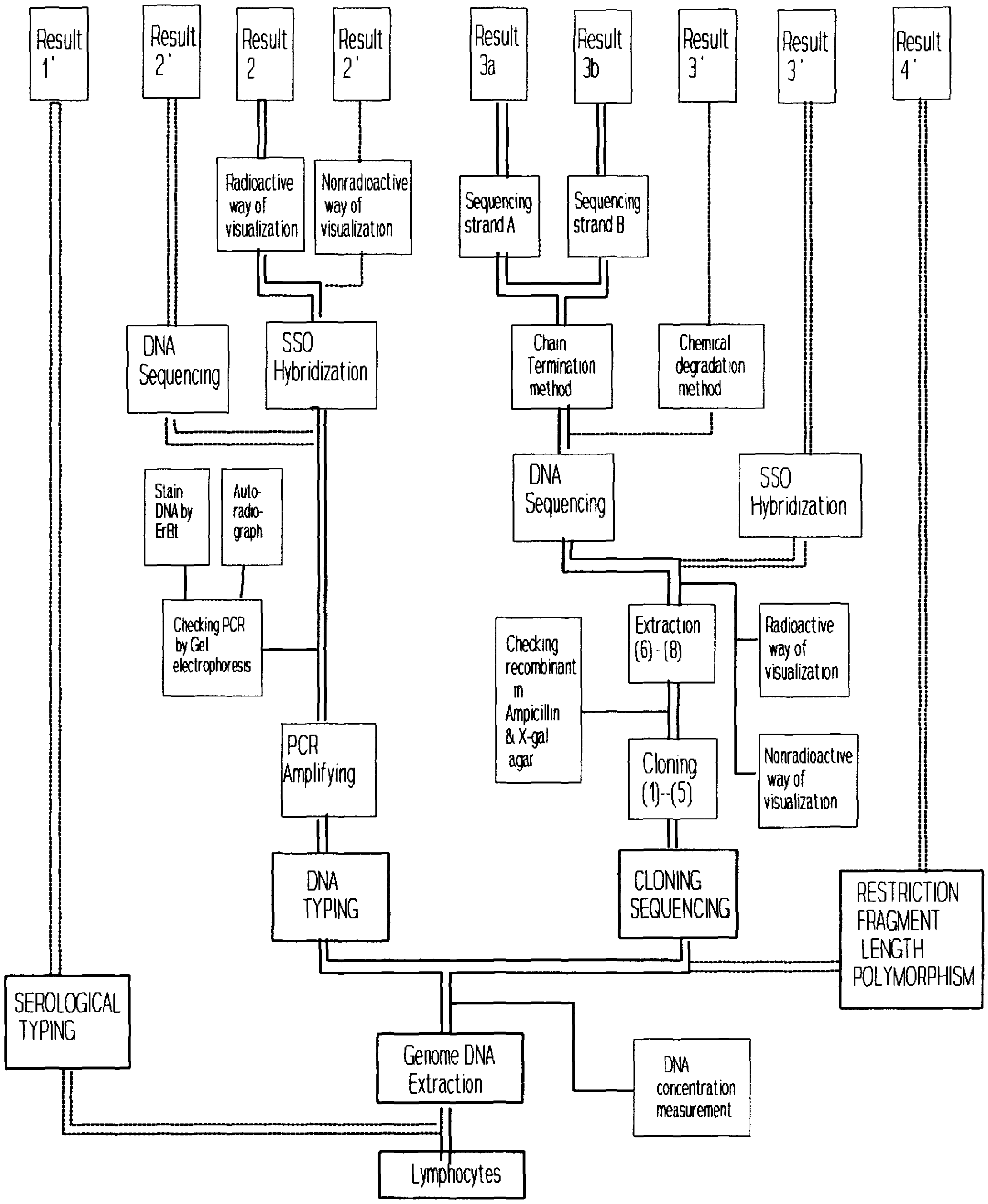

Figure 7.1 The Complex Tree-Structure of HLA Project

Here Result 1' denotes the possible result of the experiment by ST, Result 2 the result by DT, and Result 2', the possible result by DT. Result 3, the result of cloning and sequencing; Result 3', the possible result by other ways in that sequencing process; Result 4', the possible result by Restriction Fragment Length Polymorphism. The double lines denote the main causal path, the tree-trunk, of the experiment that has been implemented, the single lines, the causal path branches; and the dotted lines (double or single), other possible causal processes (trunks or branches) which have not been actually carried out in the HLA project.

these mechanisms which "covers" the entity in more than one way.

From another point of view, such an experiment is also a test of more than one aspect of the entity's causal structure or devices. In cloning and PCR, DNA molecule's ability of self-replication is both used and tested. In sequencing, not only the subtle internal chemical and physical mechanisms of the DNA molecule, like that of hydrogen bonding, base pairing, etc., are made use of to interact with enzymes in order to visualize the sequences, but also the mechanism of storing genetic information is used and confirmed. So the existence of the tree-structure both depends on and confirms the consistency among the entity's internal causal "devices," which in turn reinforces scientists' belief in the reality of the entity.

Consequently, the result of such an experiment is by no means a single one. Rather, it is the combination of those from every trunk and every branch. The possible paths which have not systematically been performed in the projects are the potential ways to eliminate doubts or confirm the actual results. The significance of the actual results could be better recognized by considering those possible results.

The structure shows that the system is not a black box, or an untouchable process of pure deduction. Branch-paths show that at almost every stage we can open the system to interfere with the entity. Every trunk is a chain of interactions. The directness of the experimental test is not a matter of the length of the trunks, but a matter of (1) the variety of the independent alternative trunks and branches; (2) the independent testability of the process along each trunk and branch; (3) the uniqueness of the causal role of the entity in each trunk (the fact that only this entity will produce those phenomena through these causal processes), and (4) the ways and the degree of manipulating the entity along the trunks and branch paths, which I will talk about next. So in turn, the existence of the tree-structure of an experiment is an indicator of the reliability of the experimental conclusions about its object.

7.3 The Way of Manipulating DNA Molecules

Traditionally biology has been a descriptive science. R. Weinberg, a molecular biologist, points out, in "describing organismic traits, or phenotypes, biologists confronted only the consequences of biological processes, not the causative forces" (Weinberg, 1985, p. 48). The observation of how a muscle contracts or an embryo develops "could not provide the clues that we needed for any real understanding of underlying mechanisms" (p. 48).

The development of microscopic and electron microscopic techniques extended the ability to observe into cells and subcellular organelles. They are visualized, and the fine structure of cells can be resolved with great precision. But these subcellular organelles and fine structures were still, said Weinberg, "structures and phenomena whose causative mechanisms remained unexplained. The explanations clearly lay with elements even smaller than the cellular components observed by microscopists" (p. 48). Yet today the scientist has no doubt that the ultimate causal mechanisms behind the biological phenomena depend on the functioning of specific molecules, DNA molecules.

Here again we see the fact that scientists take observation under electron microscopy as valid and direct "seeing" in all senses of the word, not as merely inferring from a developing embryo or whatever. These observational data, which are of course unavailable to unaided eyes, are taken as "phenomena," like those data we have about a table. Scientists have advanced the limit of both visualization and observation into the realm which the scientists before had thought of as "theoretical" or unobservable territory. Furthermore, they believe that studying DNA now is no longer like an indirect inference from its effect or like studying life as the end product of two billion and more years of evolution, even though they do not see DNA with microscopes.

This is because molecular biologists now also draw a line between "observable" and "experienceable." They think they can experience or manipulate a thing they can not see even with electron microscopy. In 1985, Weinberg wrote:" The molecular biologists who present their work here manipulate things they will never see. Yet they work with a certainty that the invisible, submicroscopic agents they study can explain, at one essential level, the complexity of life" (p. 48). If here "invisible" means invisible as directly as those cellular components observed under electron microscopy, at least some indirect ways to visualize DNA exist. In the 1990s, more and more scientists are talking about "visualizing DNA" through staining or autoradiography (Brown p. 66). It does not matter if the difference among them is merely verbal or due to views of human limitation. The point here is that "experienceable" is not equal to "visible" and visible is not equal to what our philosophers have in mind about "observable." For scientists, "visualizing" can be a very long series of interactions of different forms, finally transferring some properties of an entity into a physical trait of the form sensible to our eyes. When they say they "see" in this case, they mean that each interaction of the transferring process can be corroborated independently. This series of interactions is just one kind of physical process, with no privilege over some other cluster of interactions. A cluster of interactions might help us to interfere with an entity, so it makes the entity "experienceable," but might not transfer it into any sensible form in order to make it visible, let alone "observable." They are on a par in this sense. Whether there is a sensible form in such a cluster does not affect its reality. The criterion for distinguishing experienceable from unexperienceable is not the sensible form, but the availability of a causal chain enabling us to reach it. The only way to materialize such a chain is experiment. The better the experiment, the more probable the reality of the chain, and the more experienceable the entity. For this, an experiment with an open tree-structure is required because it contains processes by which we can control the reaction of the entity, and as a result it offers us a chance to control a causal process and the entity in a way similar, in principle, to the way we control a macro entity.

Today our capacity of experiencing DNA molecules has been so developed that now DNA can be cut apart, modified and reassembled very precisely. Any segment of it can be amplified to many copies. Changing DNA by manipulating even a single base pair can generate RNA and then protein molecules of desired sizes and constitutions. And as a result the critical elements of the biological blueprint can be changed at will. Most of this can be done in an extremely reliable, accurate, and effective way. Experimentally manoeuvring a DNA fragment today is like our moving a table: considering all relevant conditions and factors, the speed and distance of the motion of the table proportionally corresponds to the way and amount of the force we exert on it.

I would like to discuss one example of the manipulation of DNA in biological experiments, including the HLA project, that is, cutting and joining DNA for making recombinant molecules in gene cloning. T. Brown states:

> Cutting and joining are two examples of DNA manipulative techniques, a wide variety of which have been developed over the last few years. As well as being cut and joined, DNA molecules can be shortened, lengthened, copied into RNA or into new DNA molecules, and modified by the addition or removal of specific chemical groups. These manipulations, all of which can be carried out in the test-tube, provide the foundation not only for gene cloning, but also for basic studies into DNA biochemistry, gene structure, and the control of gene expression. (Brown, 1990, p. 49)

A genome DNA of a complex organism is too large to analyze at one time: a larger genome of a mammalian cell carries some 2.5 billion base pairs of information arrayed along its chromosomal DNA; and there are 50,000 to 100,000 individual genes in the genome of a mammal, each one presumably responsible for specifying the structure of a particular gene product, usually a protein. Thus molecular biologists often focus on studying or manipulating individual genes, or a small piece of DNA involving a few genes, like a HLA-D fragment.

The manipulation of genes depends firstly on the techniques of identifying them. Almost inevitably, the techniques must involve those for isolating individual genes from the whole genome molecules. As Weinberg asserted, "In the absence of effective techniques of enrichment and isolation, individual cellular genes were abstractions. Their existence was suggested by genetic analysis, but their physical substance remained inaccessible to direct biochemical analysis."(p. 50) This is another way to say that technology means manoeuvring; and manoeuvring means existence.

The technological revolution which gave rise to tools for cutting DNA - DNA cleaving enzymes called restriction endonucleases, was brought aboout by, W. Arber, H. Smith and D. Nathans. They shared a Nobel Prize in 1978 for this great breakthrough in the development of genetic engineering. Up to now more than 1200 different DNA cleaving enzymes have been characterized. The successes of protein biochemistry provide the most important tools, purified enzymes for all kinds of DNA manipulative techniques. Enzymes as tools play a great role in molecular biology today. There are five classes of enzymes used most frequently today: (1) Nucleases, (restriction endonuclease) for cutting, shortening or degrading DNA. (2) Ligases, for joining DNA molecules. (3) Polymerases for making copies of molecules (4) DNA modifying enzymes, for removing or adding chemical groups. (5) Topoisomerases, for introducing or removing supercoils from covalently closed-circular DNA.

Restriction endonucleases break internal phosphodiester bonds within a DNA molecule only at specific sequences that occur here and there along the DNA double helix. Different ones cut different sequences; this gives scientists a variety of tools for various purposes. *Bam* H1, the restriction endonuclease used in the HLA project, cuts DNA molecules at the position with the sequence GGATCC, the sequence existing at two ends of the HLA-D molecule fragments. As well, the plasmid was cut at the same sequence by *Bam* H1. But the way of cutting must be in a very precise and reproducible fashion. Each plasmid molecule must also be cleaved at a single position, to open up the circle so the HLA-D fragment can be inserted. A plasmid that is cut more than once will be broken into two or more separate fragments and will be of no use as a cloning vehicle. Each plasmid molecule must be cut at exactly the same position on the circle - at the place of gene *lac Z'* for the purpose of the selection of the recombinant later. Random cleavage at any place of the recognized sequence is not satisfactory. The choice of suitable enzymes thus is crucial. It must take into account both the sequences of the ends of the HLA-D DNA, the sequence of the plasmid, and the sequence of gene *lac Z'* on the plasmid. Namely, scientists must find the sequence near the ends of the HLA-D fragment which is the same as the sequence of the gene on the plasmid. The sequence of the fragment must also be chosen in such a way that the fragment will not be cut into too long a fragment to be carried by the plasmid. Fortunately all this can been done easily today since restriction endonucleases can be made to fit various needs. What the scientists need to do often is to phone a company producing the enzymes to synthesize a special restriction endonuclease which can cut just the sequence they find suitable on both the plasmid and the fragment.

Another aspect of cutting is of considerable importance in the design of a gene experiment. Many restriction endonucleases make a simple double-stranded cut in the middle of the recognition sequence, resulting in a blunt or flush end. In a ligation reaction, two blunt-ended fragments can be joined together, but it will not be a very efficient joint. This is because the ligase is unable to "catch hold" of the molecule thus cut, and has to wait for chance associations to bring the ends together. If the two DNA strands are not cut at exactly the same position, but the cleavage is staggered, usually by two or four nucleotides, then the resulting DNA fragments will have short single-stranded overhangs at each end - called "sticky" or cohesive ends, so base pairing between them can stick the molecule back together again easily. Some restriction endonucleases, like *Bam* H1 in the HLA project, can automatically cut in this way. Yet if the enzyme used can not make such ends, some additional work can be done to make them "sticky." One method is to make and attach some short pieces of double-stranded DNA of known nucleotide sequence which can be cut into cohensive ends, to the ends of larger blunt-ended DNA.

After thus cutting, the ligation reaction for joining DNA molecules together is performed in order to construct a recombinant DNA molecule. The enzyme for ligation is called DNA ligase; the one used in the HLA project was from E.coli bacteria. Within the cell the DNA ligase carries out the very important function of synthesizing the missing phosphodiester bond between adjacent nucleotides in the strands of DNA molecule. If the ends of the fragments are "sticky," the enzyme will synthesize most effectively the bonds between them, so the fragments are joined. Certainly, in the reaction, some opened plasmid molecules will be recirculated without the HLA fragment being inserted, some will carry undesired inserted fragments, etc. That is why checking and selecting recombinant molecules are required after cloning is finished. But once the operation of the gene cloning experiment is well controlled, some desired recombinant molecules will surely form as planned. Here, the relationship between the relationship between the input and output is proportionally or statistically regular and linear. The more efficiently the means are used and the reaction is controlled, the more desired molecules will be obtained. The experiment has been proved to be an effective and subtle manipulation of the DNA molecules by various techniques used in the tree-trunks and branches, i.e., by the successful performance of the experimental system of the tree-structure of the causal paths. So what we can claim is this: the molecules can be effectively manipulated to an extent which has no essential difference from the way in which a macro entity is controlled to move. That is, ideally, the difference between the two kinds of relationships is mainly a matter of degree.

7.4 The Nature of Technology

No doubt, the ground of our ability to experience or manoeuvre a DNA fragment is technology. Now I would like to highlight two aspects of the relation between experiment and technology: (1) the comprehensiveness and reliability of the experiments we have seen above by looking at their tree-structure is made possible primarily by the existence of those independent alternative techniques for studying the same object; (2) there exist similarities and relatedness between rationales of techniques and background knowledge of the entity to which the techniques apply. As a result, due to this kind of relation, people's confidence in technology leads to beliefs in their experiments. And the confidence in technology is well supported by the nature of it. Hence, technology offers a foundation for realism about entities.

First, as shown by the tree-structure, a variety of causal paths for interacting with an entity at different stages and with different characters finally consists in a variety of techniques available for the purpose. Just for visualizing or recognizing DNA molecule, for example, we have two main ways, staining and autoradiography, and each has more than one variation, depending on the tools and principles involved. It is these techniques that form the trunks and branch-paths of the experiment. I propose that all these techniques used in the experiment comprise a specific "tech-net" as a base for the experiment. The constituents of it include those for extracting and selecting DNA, PCR, cloning, sequencing, SSO hybridization, RFLP, gel electrophoresis, autoradiography, and so on. We see now that the formation of the tree-structure depends on the existence of such a net. Designing an experiment for materializing a causal connection is a modelling with the net of relevant techniques. The causal paths extending in the experiment are actually formed by the techniques in its tech-net.

I use a name "net" here not only because all its members are used in the same experiment, but because there exist some relationships among its members. What I have in mind is mainly that independent alternative relationship between, say, PCR and cloning, or SSO hybridization and sequencing. They are different but they pair; they are independent alternatives to each other for the same purpose. This relation makes both techniques involved in it "well developed" techniques. That is, a technique is a well developed one when it becomes an independent alternative to another one. Since this means that the result of using one technique has been confirmed as real by another different one about the same object and for the same experimental purpose, the "independent alternative" relationship connects the two so that once we accept the one technique as a "real-image-maker," we have to do that to its alternative. The ontological status of their results are transferred in this sense. As Hacking (1983) has suggested, this kind of "parallel difference" between independent alternative techniques constitutes a basis for realism about entities. A denial of the function of a technique would lead to a denial of that of its alterntive independent ways when they all result in the same outcome. Also this would be a denial of a much larger part of modern experimental biology and technology.

The second aspect of the relatedness between the tech-net and the experiments is a "vertical" similarity between the techniques and the entity in the experiments of the tree-structure, i.e., the connection or commonality between the rationales of techniques, like cloning, sequencing, PCR or DNA typing, and the causal or empirical laws about the objects of the experiments, like those in the model of DNA structure, knowledge of transmission of genes, etc. The models of the operation of the techniques, like PCR, for cutting and replicating DNA fragments, are based on the same principles as the background theory of its object.

The invention of the PCR technique shows this typically[1]. What Kary B. Mullis, the inventor, was thinking about was an experiment using a modified version of Sanger's

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[1] The relation between the rationales for PCR and the causal laws for DNA is fully shown in the recollections by the inventor (K. B. Mullis, 1990).

dideoxy sequencing to identify particular base pairs in a DNA molecule. The inference involved empirical knowledge about the structure of DNA (three-prime end and five-prime end of each strand; the pairing of the two strands; etc.); the function of DNA polymerases; and knowledge and techniques necessary for Sanger's dideoxy sequencing method. In the imagined experimental operation, Mullis suddenly realized that the resultant strands of DNA in the template and the extended primers - oligonucleotides - would have the same base sequence, i.e., the mock reaction would have doubled the number of DNA target in the sample. Repeating this reaction would result in the same strands. The inference from the sequencing experiment thus led to the birth of PCR - a revolutionary technique in genetic engineering.

This is a reflection of a general practice. Today research in labs is heavily dependent on the gene industry providing materials and tools. A scientist may, for example, ask a company to synthesize a piece of nucleotide of a certain sequence that she wants to be a primer for making a DNA fragment. The techniques or the tools sold by the industry are themselves the products of molecular synthesis or extraction of the entities, like DNA or protein molecules. These manufacturing processes make use of the knowledge of DNA and genetic methods for reading and using genetic codes to manufacture protein products. That is, many causal laws and entities involved in the technique are the same as those for understanding DNA. It would be very hard for scientists to separate the acceptance of the reality of the causal processes in making tools from the acceptance of those in their experiments using the tools.

It is a fact that if something becomes a tool-product of the biology industry, scientists hardly challenge the reality of it and the reality of the process of manufacturing it, no matter what kinds of entities it contains, what kinds of rationales it is based on, or how difficult to imagine before it was invented. This is because otherwise they must also reject the reality of the products and the process of manufacturing them. This will cost too much: they may either be led to deny both the products and the process of manufacturing them as real, or be inclined to deny just the causal reactions in the process as real. But the former choice sounds as ridiculous as the denial of the existence of a table; and the latter one will be both inconsistent and equivalent to claiming that the industry is making its profit by a fictitious agent. So, the beliefs in the reality of the tools generate the beliefs in the reality of the entities. This *vertical commonality* in the tree-structure constitutes another basis for scientists' belief in the reality of DNA molecules. Realism about DNA molecules is grounded in the manipulation of them and depends in turn on realism about the function of technology.

And in turn, this basis is reinforced by the nature of the technology, such as its progressiveness. That is, its development is greatly different from that of theories in the sense that "technological revolutions" are progressive ones. Indeed, scientists and technologists use the phrase "technological revolution" in similar senses to that of the word in the case of theoretical revolutions. For one thing, this has something to do with the degree to which the new technology changes an older way of research. A revolutionary technique might make some impossible manoeuvring viable or make some difficult work incredibly simple[1]. For another thing, a breakthrough in interfering ability often means an ability to "go beyond simply interpreting and understanding what one sees and to interfere purposefully" (Cherfas, p. 74). And the term also refers to creativity, ingenuity, and sometimes, conceptual beauty. One could not help being impressed by the marvellous trick in DNA sequencing technology which enables us to "read" genes.

Yet so far there has been no technological revolution which has ever led to eliminating any previously well developed techniques - in the sense stipulated above - as fictitious, fallacious or illusory. Technology revolutions are aimed at, and actually have always been, improving, refining, extending previous technology; exploring newer and deeper aspects of the world. The new discoveries resulting from the revolutions in technology will not be likely to lead to the conclusion that older techniques were making fictitious objects. If the reality of an entity was regarded as the one that some alternative independent techniques could reach, it will hardly be changed to non-real by any kind of new means.

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[1] For example, with the catalogue of known enzymes, a specific point on a sequence can be selected to cut in order not only to have a sticky end or to obtain the desired fragment, but also to reach the goal of selecting it later by its antibiotic resistance genes. Simple, convenient, ingenious and productive!

Now we can list some features of our tech-net or technology as a whole as follows.

(1) Such a tech-net in biology is a realistically holistic one. What I mean here is that neither the whole net nor any of the member of it is an "illusion-maker." So in this sense, the net is consistent, coherent and interrelated. A doubt about the reality of a well developed element will very likely become a doubt about the reality of the whole net. Surely the possibility that the whole net would be taken as an illusion-maker or the whole experience of the DNA molecules resulting from the net as a fiction will be extremely low.

(2) The *parallel difference* between pairs of independent alternative members of the net means various "accesses" to interfere with the same objects. This makes the argument "no independent access" (Fine, 1984) very weak.

(3) In contrast, the *vertical commonality* between the rationales of the net and the body of background knowledge for the entities of the experiment offers another ground for the reality of the entity if we accept the reality of the technology. The net and experiment are tied as flesh and blood in this way.

(4) The tech-net is open and progressive. It grows and matures continuously. New elements will enter into the net from all directions. A tech-net can consist of both older and newer elements. Even though some older ones cease to be actual members of the net - no longer remembered as alternatives, they are by no means abandoned as "false." Any conflict among the products of older and newer techniques will primarily be regarded as the result of different degrees of precision or errors, not as a conflict between true and untrue experiences. The result of older or less precise techniques can be regarded as a significant reference for considering and checking the result of newer techniques. The value of the past elements lasts for quite a while; the reality of the experience from them remains unchanged whatever new elements are added to the net. That is why the "pessimistic induction from history" concerning our experience has no scientific or historical base.

Given all the discussions about the DNA model, the history of the experimental search for the causal role of the DNA molecule in the hereditary process, we found that scientists' belief in it has met CIC. Furthermore, by the detailed studies of today's experiments about DNA fragments and technology, we can see that the entity is manipulated both because of the synthesis of independent and different ways, and because of the nature of technology which becomes a base for the belief through its relationship with experiments. In the meantime, I have argued that the manipulation of the entity occurs also because in experiments having tree-structure the way and degree of controlling the effect will be close to the way we control a macro entity. Now it can be concluded that for such an "unobservable" entity as the DNA molecule, it is more reasonable to believe in its reality than to deny it. Or, given RR, realism about the entity is reasonable philosophically.

Chapter 8 Neutrino and Ether

In this chapter I will compare and explain two actual cases in science in terms of my account of good reasons. I will argue that according to the account, the acceptance of the neutrino could be regarded as reasonable, while the popular acceptance of the ether in the last century was not, so the final rejection of it as real before the end of the last century was no surprise. And by these cases I hope to have eventually met the challenge posed by Laudan against realism.

8.1 The Models of Neutrino and Beta Decay

In 1930, Pauli proposed in a letter to Geiger that in β-decay, a new, neutral particle called by him the "neutron" was emitted. Yet right in the letter he admitted that his proposal might look very unlikely because "one would have seen these neutrons long ago if they really were to exist." He felt so uncertain about the idea that he was very reluctant to publish it formally.

Why, however, did he invent it after all? During the 1920s, one of the riddles facing physicists was the phenomenon of beta-decay. The energy spectrum of beta particles is completely different from that of alpha particles: it is not composed of discrete energy values, as the quantum theory required, but rather is a continuous curve. Experiments showed also that in beta decay the radioactive substance (actually it is a neutron, n, found by Chadwick in 1932) is converted into a proton, p, and an electron, e. The reaction is:

$$n \rightarrow p + e \qquad (8.1)$$

The problem was that the total energy of proton p and electron e was found less than that of neutron n. It seems that some energy disappears. Also, the total spin of p and e is not equal to that of n. All these represented violations of the laws of the conservation of energy, momentum and angular momentum.

The ways out suggested by this problem-situation seemed to be in principle two: either believing that the laws of conservation are not true of the quantum world, or, suspecting that there was something we did not detect carrying away the missing energy, momentum and angular momentum. That was why Pauli made his proposal - an unknown particle is emitted in the beta decay reaction. It was required that the causal reaction be thought of as this (in the form given by Heisenberg in 1932, writing Pauli's particle as ν, called the neutrino by Fermi later):

$$n \rightarrow p + e + \nu \qquad (8.2)$$

(It was found here ν should be an antineutrino.) Accordingly, the physical character of the neutrino was specified in terms of the requirement of the problem-situation to be solved: the particle should carry part of the energy liberated in the process; have 1/2 spin, in order to make the spins of two sides of the equation equal; have no electrical charge and very small or even zero rest mass.

Now a model describing the character of the entity and its unique role in a causal process was formed. The question then is whether it could become "mature."

Despite its lack of testability, in about twenty years, scientific ground for the model continuously strengthened in the following ways. (1) The model of the neutrino was embedded into a broader net of accepted empirical knowledge; and the causal connection or chain between the neutrino model and some other parts of the physical world was revealed. As a result new viable causal chains or paths were found - alternative independent means were thus available for directly detecting the entity. And (2) various reasons and evidence against the internal and external feature of the model had been gradually ruled out. These developments led to the belief before the detection of the entity in 1956 that the neutrino model was a well-founded one. Now let me mention some of these developments.

As we see, the model postulated and portrayed by known physical laws a causal relation existing in that specific problem-situation. The design of the character and relation was never thought of as awkward, artificial or inconsistent. Each property of it was not invented just for this purpose; there was no auxiliary hypothesis in the model postulated for the first time. The concept of a particle without charge or mass had occurred before 1930; spin as an essential property of reality meanwhile had been developing along different lines. No reason existed then for thinking that there was any contradiction between the property of, say, zero-mass or charge and that of 1/2 spin so they could not exist in one and the same particle. The internal consistency was not in doubt according to known physical laws.

At the beginning of the 1930s there were two theoretical tendencies which led to objections to the model. One was that Bohr and many other scientists tended to believe that in nuclear reactions energy and momentum might be just statistically, not strictly, conserved; another was that partly from the consideration of simplicity, many scientists, including Bohr, held that the physical world consists of only three kinds of particles known then: protons, electrons, and photons. They thought that if the number of fundamental particles were more than three then the number would be infinite and that would be unacceptable.

A series of discoveries in different fields soon made the two objections much weaker. With discoveries of the neutron by Chadwick and the positron by Anderson in 1932, two more particles entered the physical structure of the world. Furthermore, the discovery of the neutron, which had been somewhat predicted by Rutherford 12 years ago, strengthened the status of the conservation laws. The neutron resolved all the apparent contradictions in experimental results of alpha particles; one was just that the estimated gamma ray energies did not tally with the masses of the nuclei and the alpha particle involved. The strategy of keeping the conservation laws of energy and momentum in the nuclear reaction turned out to be correct in the discovery of the neutron.

Another important support for the neutrino postulate came from Ellis' and Mott's research on the upper energy limit of the beta spectrum, which indicated that even Bohr's statistical version of the conservation laws could not explain the beta decay phenomenon. Advancing the earlier work on the thorium C branching problem and the upper limit of beta spectra, Ellis and Mott proposed a new assumption that the energy difference between two nuclei linked by beta decay was equal to the upper energy limit of the beta decay in which, they argued, the conservation of energy was maintained: "if the energy merely disappears, implying a breakdown of the principle of energy conservation, then in a beta decay energy is not even statistically conserved. Our hypothesis is, of course, also consistent with the suggestion of Pauli that the excess energy is carried off by particles of great penetrating power such as a neutron of electronic mass." (Ellis and Mott, 1933). Their assumption was soon proven by the tests of Henderson.

This success made Pauli much more confident about his idea. Besides, his confidence became stronger when in 1931 he was told by some physicists of the possibility of using his neutrino to solve some difficulties about cosmic radiation. In the Solvay Conference in October 1933, he gave for the first time a full account of his postulate, and allowed it to be published. Meanwhile he argued against Bohr's hypothesis of breaking down those conservation laws in nuclear reaction:

> This hypothesis does not appear satisfactory to me, nor even plausible. First electric charge is conserved in the process and I do not see why the conservation of charge should be more basic than that of energy and momentum. In the second place, it is precisely the energy relations which regulate several characteristics of beta-spectra (existence of an upper limit and agreement with gamma-spectra, Heisenberg's stability criterion). If the conservation laws were not valid, it would be necessary to conclude from these relations that a beta-disintegration is always accompanied by a loss of energy and a gain; this conclusion implies an irreversibility of the process with regard to time, which too is scarcely acceptable (see Morton, 1983, pp. 178-9).

It was the birth of Fermi's theory of beta decay in 1934 that embedded the postulated model of the neutrino into a broader model in which a causal relation connected the entity with some other objects in a longer reaction. As we can expect, the success of the effort marked a crucial point in convincing others of the maturity of Pauli's model.

Fermi's theory in 1934 was a quantitative one of beta decay. The theory assumed the existence of the neutrino. He said that in his theory "the hypothesis regarding the neutrino is fundamental" (Fermi, 1934) By analogy with the theory of the emission of a light quantum from an excited atom where the total number of light particles is not constant, Fermi proposed that beta decay is this kind of causal process: the heavy particles, neutrons and protons could be regarded as two internal quantum states of the same heavy particle. Beta decay is a process of the transformation of the particles from their neutron state to their proton state. In the action of the transformation, electrons or neutrinos are created. And if the transformation is a reverse one, from a proton to a neutron, an electron and a neutrino will disappear.

Based on these postulates, Fermi tried to formulate mathematically the transition of heavy particles, the creation and annihilation of light particles, and the interaction between the nucleon, electron and neutrino. For this he used the Hamiltonian method. The mathematical formulation was strictly constructed according to the postulates of the physical process. Various operators were arranged to stand for the physical elements, like the creation and annihilation of a particle (called creation operators and annihilation operators) and for the change of the states of the nucleon from the neutron to the proton or vice versa. The interaction between the nucleon and the electron and neutrino field was treated by the quantum theory of transition probabilities. The electron states and neutrino states were described respectively by their momentum values and spin states, and so on. As a result, the Hamiltonian operator H (or energy operator) comprises three main parts: H_{heavy}(now H_1), relating to the nucleons; H_{light} (H_2) relating to the electron and neutrino fields, and an interaction H (now H_{int}) between the electron-neutrino field and the nucleons. Thus operator H_{int} represents the process of a neutron changing into a proton, the creation of an electron and the creation of a neutrino.

By this arrangement of these operators to reflect the subtle elements and aspects of the physical process of beta decay, the transformation of a nucleon from, say, a neutron to a proton state while at the same time an electron and an antineutrino are created could be mathematically described, and results about various properties of the transitions could be drawn from the description, such as lifetime and form of the distribution curve for some ("allowed") transitions. Very soon those results were well confirmed. The theory was later expanded into a theory of weak interaction, with a development from Gamow-Teller about the effect of a nucleon on the rest of its wave function during transition. Of course there were still criticisms at that time, yet they were not aimed at the basis of it like the neutrino postulate[1].

It is worth stressing that Fermi's theory was a description of a new kind of causal process which takes the existence of the neutrino as a necessary part. Without the operator describing the state of the neutrino, the other operators for the nucleons, electron and the interaction operator could not be specified in the way they were. Nor could those confirmed results be drawn.

Indeed there was a rival theory to Fermi's with respect to the neutrino postulate: G.Beck's model. Beck said that the very reason for introducing the neutrino was to maintain the conservation laws in nuclear processes. His main argument was very "theoretical": "it is not possible at present to find a general theoretical argument whether or not the conservation law should apply within dimensions of the order of the radius of the electron"(Morton, 1983, p. 242). However the advantage of Fermi's theory over Beck's was soon seen as obvious. In 1935, Beck himself came to accept the neutrino and reject his own theory. In 1936, by studying the recoiling nuclei during beta decay, Leipunski, a critic of Fermi, asserted, "The only conclusion that may be drawn is that these results are in favour of the emission of the neutrino during β-decay" (Leipunski, 1936). Bohr, in 1936, made a decisive shift from the hypothesis he held in the period 1930-1933. He announced:

...the ground for serious doubts as regards to strict validity of the conservation laws in the problem of the emission of beta-ray from atomic nuclei are now largely removed by the suggestive agreement between the rapidly increasing experimental evidence regarding beta-ray phenomena and the consequence of the neutrino hypothesis of Pauli so remarkably developed in Fermi's theory (Bohr, 1936, p. 26).

[1] See, for example, Konopinski and Uhlenbeck, 1935, pp. 7-12; Nordsieck 1934, pp. 234-5; Tamm, 1934, p. 981; and Iwanenko, 1934, pp. 981-2.

The increase of the evidence for the neutrino continued. From their observation of the recoiling nucleus and the decay electron in beta decay in 1939, Crane and Halpern argued in favour of the existence of the neutrino. (Crane and Halper 1939, 232-7). Also examining recoil nuclei, Allen concluded, "the recoils were caused by the emission of a neutrino and not by the emission of a gamma ray" (Allen, 1942, 692-7). At about the same time, Konopinski and Uhlenbeck "recognized that the new experimental results on both spectra and on the maximum electron energy had removed the basis for their criticism of Fermi's original theory" (Franklin, 1990, p. 20) It is clear that at least in the early 1940s, before first direct evidence for the entity occurred in the 1950s, the successes of Fermi's theory and other efforts which successfully made the neutrino an inevitable component of the interaction system of beta decay had been taken as a sign that the neutrino model had been well embedded in the existing network of empirical knowledge. The causal and descriptive model was taken as mature. Condition MC for realism about the neutrino had been met.

8.2 The Direct Test of the Neutrino

At the same time, these developments in physical science regarding the neutrino cumulatively provided more and more means to detect the neutrino directly. "Direct" here means, as we can expect, (a) there is an experimental implementation of a causal chain in which the entity is a link - both as a result and as a cause of the continuity of the chain; and (b) the experimental system is a complex net of various alternative independent means to identify, isolate, measure and transfer the chain; that is, the experimental system has a tree-structure.

Actually Crane and Halpern's work in 1939 of measuring the momentum and energy of the emitted beta ray and the recoil atom with a cloud chamber to decide the ionization effect of the recoil atom offered a way to detect the neutrino, although it was still indirect. In 1941, K.C. Wang considered a different method of detecting the neutrino. He pointed out "When a β^{+}-radioactive atom captures a K electron instead of emitting a positron, the recoil energy and momentum of the resulting atom will depend *solely* upon the emitted neutrino, the effect of the extra-nuclear electron being negligible."(K.C. Wang, 1941, p. 97, italics original)

The most important task was to find the interaction of the neutrino with some other objects and implement it experimentally. Such an interaction was conceived in the 1940s: an inverse beta decay, a process in which a neutrino or anti-neutrino is captured by a nucleus and an electron or positron of definite energy is emitted.

The capture reaction - the inverse beta decay reaction - is

$$\bar{\nu} + p \rightarrow n + e^{+} \qquad (8.3)$$

Cowan and Reines performed their first successful experiment in 1953. They made use of an intense neutrino flux from fission-fragment decay in a large reactor at Hanford, using a detector containing many target protons in a hydrogenous liquid scintillator to receive the flux. In the experiment, in order to identify the observed signal as neutrino-induced, they successfully ruled out by many methods reactor neutrons and gamma radiation as causes of the signal. Cosmic radiation was not ruled out yet.

From 1954, they started repeating the experiment in a more effective way. They placed a very large detector near a nuclear reactor at the Los Alamos Scientific Laboratory in South Carolina as a source of neutrinos. We can see the whole reaction actually started from the nuclear reaction in the reactor. The neutrino is the result of the nuclear reaction and in turn is the cause of the reaction in the detector. For this reason we will regard the whole system of the experiment as consisting of two main parts, the information source, the reactor; and the receptor of the information, the detecting system. In the experiment, the operation and shut-down of the reactor was a way of deciding that the antineutrino was the cause of the reaction in the detecting system.

The process of the experiment can simply stated as follows. If the reaction in the nuclear reactor really produced an antineutrino, then when an antineutrino hit and penetrated the target tanks, it would be absorbed by a proton in the water in a tank and then emitted a positron. In about 10^{-9}second, the positron would undergo annihilation with an adjacent electron (β^{+} annihilation) and produce two 511 keV gamma photons γ

travelling in opposite directions. Next, a neutron would be produced in the interaction between the antineutrino and the proton. The recoiling neutron would slow down by a number of collisions with the protons in the water and would be captured by a cadmium nucleus Cd(n, γ)Cd*, also producing a number of gamma photons γ of total energy 9 MeV. This capture process would take about 10^{-6}s. So the process of the emission of two simultaneous 511 keV γ photons, followed by a number of γ photons of total energy about 9 MeV a few microseconds later indicates the happening of the inverse beta decay reaction as shown in (8.3). If the photomultipliers around the scintillator which were installed above and below the tanks detect two simultaneous pulses (photons) and a few thousandths of a second later a number of pulses again, "it is clear beyond all doubt that an anti-neutrino has penetrated the container, like a thief who breaks into a house leaves behind fingerprints which are unmistakably his" (Ne'eman, 1986, p. 72).

We can see the whole process of the reaction which is the main trunk of the experiment in 1956 will be:

(1) Nuclear reaction in the reactor $\rightarrow \bar{\nu} \rightarrow$
(2) $\rightarrow \bar{\nu} + p \rightarrow n + e^+$
(3) $\quad\quad\quad$ | └──── $e^+ + e^- \rightarrow 2\gamma$
(4) $\quad\quad\quad$ └──── $n + Cd \rightarrow Cd + \gamma + \gamma + \ldots$

The detecting system consisted of a multiple-layer arrangement of scintillation counters and target tanks (sandwich). The three "bread" layers of the sandwich were three scintillation detectors, while the two "meat" layers were two target tanks full of water containing protons and cadmium chloride ($CdCl_2$) solution. This kind of arrangement "provides two essentially independent 'triad' detectors, the central scintillation detector being common to both triads." (Cowan and et al. 1956) And accordingly "Two independent sets of equipment were used to analyze and record the operation of the two triad detectors. Linear amplifiers fed the signals to pulse-height selection gates and coincidences circuits. When the required pulse amplitudes and coincidences (prompt and delayed) were satisfied, the sweeps of two triple-beam oscilloscopes were triggered, and the pulses from the complete event were recorded photographically. The three beams of both oscilloscopes recorded signals from their respective scintillation tanks independently" (1956).

For distinguishing between the neutrino flux and background radiation from the reactor and cosmic rays, the whole set-up was surrounded with a thick layer of earth and metal, located in an underground room, which shielded the experiment set-up from penetration by all other particles than neutrinos. The multiple-layer arrangement of the tanks and the two independent triad detectors also could be taken as a means to reject "pseudo events due to the penetrating cosmic rays." In the course of the experiment, some other means were also used to distinguish the effects of fast neutrons from those of the antineutrino (Cowan, et al. 1956) Figure 8.1 showing one "meat" layer of the "sandwich":

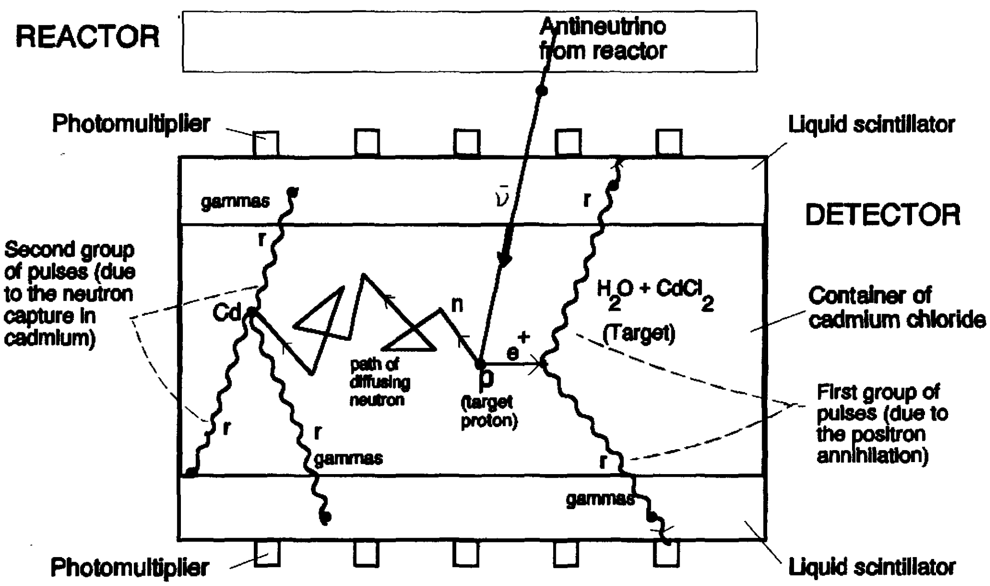

Figure 8.1 A schematic diagram of the process in the neutrino detector

After three years of work, in 1956, the events consisting of two groups of pulses and the time-delay between the pulses were recorded. By adjusting the experimental devices, the reaction in (8.3) was detected in different ways and all of these confirmed the expectation. For example, The time-delay between these pulses was regarded as depending on the number of Cd atoms in the water target. "[A]nalysis of the time-decay spectrum yielded excellent agreement with that expected for the cadmium concentration used in the target water. Doubling of the cadmium concentration produced the expected shift in the time-delay spectrum without increasing the signal rate. Removal of the cadmium from the target water resulted in disappearance of the reactor signal."(1956) This becomes another self-checking mechanism, a branch path[1].

It is clear that the directness of Cowan's experiment consists in the fact that the antineutrino was identified as a necessary cause for part of the reaction chain in the detecting system by a variety of alternative independent means. So the experiment in 1956 has a tree-structure. The detector part of the tree-structure can be briefly shown as this:

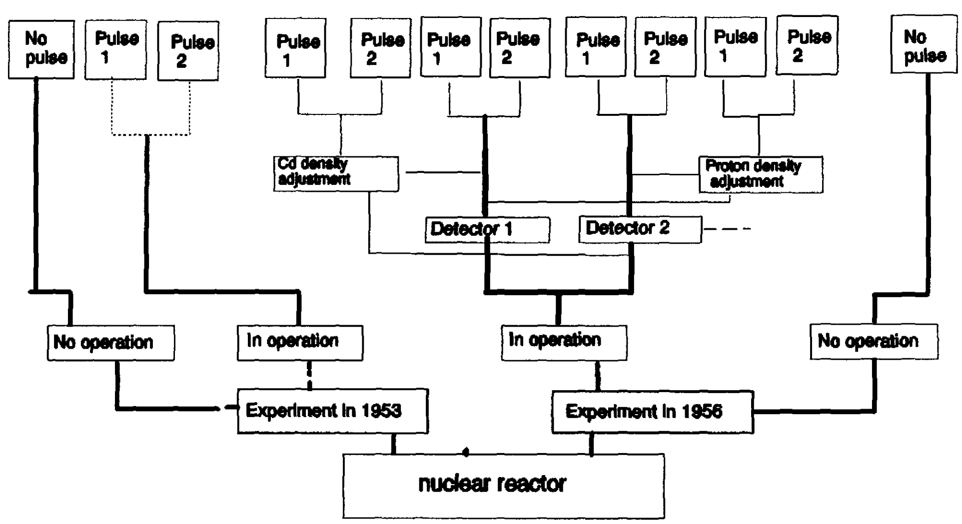

Figure 8.2 Tree-Structure of the Neutrino Experiment

[1] The probability for neutrino capture is directly related to that of ordinary beta decay and is given by Fermi's golden rule. The expected rate of the event of detecting the neutrino is $FN\sigma_c\epsilon$. F is the antineutrino flux, N, the number of protons, σ_c the cross-section (the area within which a neutrino will be captured) and ϵ the efficiency of the detector. If $F \approx 10^{17}m^{-2}s^{-1}$, $N \approx 10^{28}$, $\sigma_c \approx 10^{-47}m^2$ and $\epsilon \approx 3 \times 10^{-2}$, the expected rate is about 1 event per hour. Changing any of the factors will result in a different capture rate. The results of these manoeuvres were all in agreement with the calculated rate. When the reactor was not in operation, the number of detected reactions dropped to zero (Jelley, 1990, pp. 105 - 106).

(Here "Pulse 1," denotes the observation of the first group of pulses due to the positron annihilation (β^+); and "Pulse 2," that of the second group of pulses due to the neutron capture by Cd). The work in 1953 stands for a previously performed trunk for comparison. The choice that the reactor was shut down was another main trunk in comparison. Each trunk had some branch paths. On the trunk of the 1956 experiment, independent triad detectors represented two independent processes of detecting, so they should be taken as two branch paths. On these two branch paths, the observations of two groups of pulses of photons may be shown as some further sub-branch paths. The additions, changes and adjustments of the experiment devices, like the adjustment of the Cd concentration, happened to both devices so are the branch paths of both of them.

Now comes the confirmation of the model by an experimental system which makes use of alternative independent methods to identify the entity as a link in a different causal process from that of the original beta decay reaction. The system is of a complex tree-structure, where every stage and detail of the interactions in the system is checked and proved by various means. I conclude that the acceptance of claim (a) about the existence of the neutrino is reasonable and claim (b) about the entity is thus implied since both MC and EC have been met.

8.3 The Case of Ether: Why was it Required?

Now let us turn to the case of the ether. The core question is: had it ever had any "good reason" or "empirical support" for its existence? According to my account of good reasons or empirical support, this is to check if it has met CIC.

As we know, partly due to the inability of technology to test for such a subtle substance in the last century, until the famous Michelson-Morley experiment near the end of the century, measuring the relative motion of matter through the "ether-sea," no experiment of that character existed for detecting such an interaction. So the condition EC had never been met, at least for most of the last century. So let us just focus on the MC aspect.

The problems arising from the aspect of model construction were by no means less remarkable. There had been enormous efforts to construct a variety of models from the beginning of the last century[1]. Yet none of them were successful even in the judgment of the constructors themselves. Their fatal weaknesses had been well stated by Einstein and Infeld (1938, pp. 119-120). That is, guided by the mechanical point of view, the determination of the mechanical properties of the ether is essential for solution of the problems such as whether light waves in the ether are longitudinal or transverse, and so one. After light was found to be transverse, a model of the ether as a jelly-like mechanical substance was required. But for this, without exception, physicists had to make some highly artificial and unnatural assumptions, many of which were complicated, ad hoc, and even self-contradictory. (For example, in order to account for optical phenomena we have to assume interaction between the ether and matter, yet at the same time we have to assume no interaction between the ether and matter in order to account for mechanical motion of matter in the ether).

Hence every attempt to construct a mature ether model was fruitless. This was another reason for failing to test the ether: since no model could describe a kind of causal interaction involving the ether without some fundamental internal or external inconsistency with some known physical fact or assumption, a test of the model could hardly be taken as a test of a real causal factor; or what it really tested could hardly be known.

Thus, "the artificial character of all these assumptions, the necessity for introducing so many of them all quite independently of each other," the contradiction, "as well as the other objections, seem to indicate that the fault lies in the fundamental

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[1] Just look how many kinds of ether models had been tried in history: ether was modelled as, to name a few, elastic fluid, elastic solid, gas-like, soft jelly, vortex, vortex-sponge, gyroscopic, perfect liquid, quasi-rigid, quasi-elastic, foam-ether, quasi-labile ether, contractile-ether or purely mathematic ether, etc. Many models were the combinations of these features. G.G. Stokes, for instance, conceived that the ether might be something like pitch and shoemaker's wax, a combination of two kind of different qualities in an extreme degree: rigid enough like an elastic solid for vibrations as rapid as those of light, and yet plastic enough like a fluid to the much slower progressive motions of other bodies like the planets.

assumption that it is possible to explain all events in nature from a mechanical point of view." (Einstein and Infeld, p. 119 and p. 121)

Of course this is what we know today. Indeed, this kind of conclusion from the troubles with all those models was not easily seen for most scientists in the last century. On the contrary, for the majority of them, the reasons for requiring a mechanical ether were overwhelming. Simply speaking, according to the mechanical point of view, it was inconceivable for light as waves to travel in space without a medium. And the mechanical view of the world was so successful that it was very unlikely that the scientists, who did not have as much "historical awareness" about the truth of theories as we do, would challenge the fundamental view. So this mechanical view alone was strong enough to drive the majority to struggle with endless difficulties of the ether idea. We might understand more about how strong the fundamental demand was when we realize that the ether was even required and used by theology in the last century (see, Cantor and Hodge). As a result, for those mechanical-minded people, ether was the accepted philosophy and language of the day. Maxwell's friend, W. Thomson kept looking for improved ether models until the very end of his life, even after Einstein's theory was widely accepted. This kind of phenomena is not completely intelligible unless some social and philosophical factors are taken into account.

Interestingly, in contrast to this kind of demand for the ether, a few scientists then doubted the ether assumption in terms of concrete evidence. Worrall (1990) has revealed such a case. Sir David Brewster, an important scientist, accepted that the wave theory of light was empirically successful. But, he refused to believe that the success meant more than it should. It meant that some, but not all, assumptions, in the theory might have reflected "the real producing cause of the phenomena of light" (Brewster, 1838a, p. 306, Worrall, p. 322). Especially, he refused to be led by the success of the wave theory to accept a fully realistically interpreted claim about "an ether, invisible, intangible, imponderable, inseparable from all bodies, and extending from our eye to the remotest verge of the starry heavens"(ibid).

Brewster's reason for this non-global view of confirmation was concrete: for the wave theory had to assume that the ether within that gas:

freely undulates to a red ray whose index of refraction, in flint glass, is 1.6272. and also to another red ray whose index is 1.6274; while...its ether will not undulate at all to a red ray of intermediate refrangibility whose index is 1.6273!(1833b, p. 362, Worrall, 1990, p. 323)

That means, Worrall interprets, "a tiny difference in the length of a wave must be supposed to produce a black-and white change from free passage through the ether within the gas to no passage at all"(p. 323). Brewster pointed out the problems with this strange arrangement:

There is no fact analogous to this phenomena of sound, and I can form no conception of a simple elastic medium so modified by the particles of the body which contains it, as to make such an extraordinary selection of the undulations which it stops or transmits...(1833a, p. 321, Worrall, 1990, p. 323)

Brewster was not alone. G. B. Airy and B. Powell enthusiastically supported the theory as "certainly true," and on the other hand, maintained that the ether hypothesis in it was doubtful (see Worrall, 1990, p.343)

It is true that although the recognition of the implication of the ether problems was not common until Einstein's revolution, scientists then had been well aware of problems with this or that ether model. Yet just because of their fundamental view of the world, they had to keep trying and sometimes prematurely declared this or that model had been successful, although they soon found it wrong.

8.4 Does Fresnel's Success Support the Ether?

Now let me discuss two particular ether models. One is in Fresnel's theory, the other in Maxwell's. They are important both because the two theories' successes were among the greatest in their fields and because Laudan picked them out as his evidence that terms in "successful" theories may not refer.

Unlike Brewster, Laudan takes it for granted that the wave theory's success, in particular, the success of Fresnel's theory of diffraction, had indeed given evidential support to the ether assumption[1]. Arguing against realism, he says: "If that [Fresnel's prediction of the 'white spot'] does not count as empirical success, nothing does"... (Laudan, 1981).

When can a predictive success of a complex theory involving an ether hypothesis reasonably be taken as a success of the hypothesis as well? Laudan does not give any answer to this. This is natural, since he in his (1981) requires realists to take "working well" as the indicator of their "success." I have pointed out this is inadequate. I have argued that evidential relevance should be causal relevance and I have laid out the two conditions, MC and EC, for deciding when such causal relevance has been found between certain results of experiment and an existential claim. I mainly treat the conditions together as sufficient rather than necessary for a reasonable belief. But if an existential claim is far from meeting these conditions, meeting neither MC nor EC, then I would say that there is no evidential relevance whatsoever between the entity and any other objects. Now let us check if such evidential relevance exists between the success of the wave theory and the ether model.

Closely looking at Fresnel's ether model, it can be determined that it does not escape those defects that other ether models had. And the point is, contrary to Laudan, that the model, even if it was good, had little to do with the theory's success.

It has been widely realized now that in most of the period when Fresnel studied the problems of diffraction, double refraction and polarisation and made his mathematical and predictive achievements, not only did he not really strictly depend on a model of ether, but such a model did not develop into one that presents the properties of the ether in detail so that he could make use of it to deduce the needed theory of the optical phenomena. He had his most remarkable success before he had an ether model. When he finally developed a detailed and better-looking ether model, after many dramatic

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[1] Worrall (1989) conceded that the ether of classical physics is the sharpest challenge to the realist exactly because he shared the same view as Laudan's concerning the case of Fresnel. I believe this is because they both, like many others, are affected by the Duhem-Quine holist view about theory and confirmation.

revisions in its development, he had already established his theory of light. He found the model still problematic and hardly realistically believable. He knew many hypotheses in the model were realistically incorrect. So in the relation between his ether model and his theory of optical phenomena, the former was based on the latter, not vice versa. It is inadequate to take any of the achievements of the latter as the confirmation of the former[1].

Fresnel began by studying diffraction. At this time he believed that the light is vibrations of a special fluid which extends throughout space. The elastic fluid medium can easily allow longitudinal waves, like sound waves, but not transverse ones. When Fresnel heard and accepted Young's proposal that light is transverse waves in 1818, he realized that an elastic solid, instead of an elastic fluid, ether must be proposed since only solids can allow transverse waves to propagate. However, Fresnel's great memoir on diffraction was already written around 1816. It was based on the older longitudinal wave theory of light and the belief in the elastic fluid ether. That is, in 1816, when the existence of a bright spot at the centre of the shadow of a circular screen was recognized by Poisson in the sketch of the memoir which was presented to the French Academy of Science for the Academy's prize for 1818, the ether assumption in it was soon recognized as wrong and discarded. The success of the prediction of the 'white spot' could not count as an empirical success of the idea of the elastic fluid ether[2].

There was another reason that can be expected in our view: there was no descriptive model of the ether at that time. Buchwald claims that "Fresnel's memoir was only superficially founded on the dynamics of the wave-propagating medium. In his prizewinning memoir on diffraction, Fresnel used an expression for the force on an ether particle in order to calculate the wave velocity. However, he did not at this time deduce

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[1] Buchwald asserts that Fresnel in fact never directly used properties of the ether to generate a quantitative theory and he created an ether dynamics for polarization only well after he had already built the theory (Buchwald, 1989, p. 307; also see Whittaker, 1951, p. 125).

[2] Giere also realizes what the "white spot" really supported in Fresnel's theory (see Giere, 1983).

the expression from the ether's mechanical properties; he simply assumed it. Indeed, Fresnel did not even describe the ether's properties in detail" (Buchwald, 1981, p220). Schaffner also points out that Fresnel's shift from elastic fluid ether to elastic solid ether did not vitiate his diffraction theory because his inquiry was "not concerned with the true motions of the aethereal medium"(Schaffner, 1972, pp. 14-15)

Until 1821 when Fresnel solved the problem of polarisation and double refraction, his model of ether was still not well developed. He conceived that the ether consists of a huge number of very small molecules in the Laplacian sense, between which forces acted, so as to permit transverse waves to be propagated through it. But again, Fresnel's most stunning mathematical achievement - the wave surface of the biaxial crystal - "probably did not rely directly on the dynamics of ether"(Buchwald, 1981, p. 222). Meanwhile, Fresnel made four additional hypotheses about an elastic medium. Three of them had been realized as "somewhat artificial and even inconsistent with a true elastic solid theory." And some of them were admitted by Fresnel himself as in fact not correct, they were purely tools for his theory (Schaffner, 1972, pp. 14-16)[1].

It is clear that although Fresnel believed in the existence of a material ether, and although he had tried to construct a satisfactory model of it which could be taken realistically and which could show a causal process from the ether to optical phenomena, he failed to reach this purpose. The model itself was ill-defined, unclear, internally inconsistent, *ad hoc* and consisted of some realized untrue assumptions. After facing endless problems, Fresnel himself was finally forced to give up the concept of ether as a real thing in the physical world. The conclusion is obvious: Fresnel's ether did not

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[1] These inconsistent, untrue or irrelevant problems with the ether were with him all the time. For example, he made some hypotheses which meant that the speed of the ether in a material body is fixed. But since the speed also depends on the partial dragging coefficient ϕ and so on the refractive index μ, and since rays of light of different color (frequency) have different refractive indices, then ϕ for each color has different a value; this implies that the ether would have to flow with a different velocity in the body according to the color, and furthermore, that there would be just as many ethers as there are colors. Fresnel's theory of aberration, Born concedes, was founded on the result of experiment without regard to the mechanical interpretation by Fresnel involving ether assumptions (M. Born, 1962, p. 139, also see Whittaker, 1951, p. 111).

have any relevant evidence from his theory's successes. A model of ether eligible for receiving that evidential support just did not exist.

8.5 What Was Confirmed According to Maxwell?

If there was any great scientific theory which had really used a detailed model of ether, it would be Maxwell's theory of electromagnetism in 1861-2. People who have more or less sympathy for the fortune of ether often take it for granted that Maxwell's success had indeed offered a most convincing support for the ether (see, e.g., Cantor & Hodge, 1980). Maxwell was quoted by Laudan as saying that ether has firm confirmations. Such a non-referring but successful concept is believed to be the strongest challenge to realism.

But the fact in the history is that Maxwell himself did not believe that his theory could offer a ground for accepting his ether model as a description of anything really going on in nature. He did believe in the existence of ether, but he did not believe it was like what his model of ether postulated even though it was useful for his theory of electromagnetism. He was much more cautious than his contemporaries who claimed that the ether had been confirmed due to his work. Many researchers on Maxwell have pointed this out[1].

Maxwell's distinctive attitude toward ether is rooted in his inductivist methodology and his dynamistic view of nature. He believed that the laws of dynamics were equally applicable to all aspects of the physical world and so served to unify physical science. Although Maxwell followed others in believing the existence of ether, and used analogy and mathematical expressions to construct a mechanical model of ether in exploring electromagnetism, he did not believe that the confirmation of the ether could be given by the success of the use of analogy, formal method or mechanical model. Contrary to what Laudan suggested (1980), the wave theory of light did not bring about

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[1] See, for example, Torrance, 1982; Hendry, 1986; Tolstoy, 1981; Goldman, 1983; etc.

a dramatic change in the 19th century's empiricist tradition. Faraday, Maxwell and many others were still in this tradition: a mature theory must be based on known facts, or it would be a "hypothesis," no matter how useful it is. Interestingly, when dealing with the aim and truth of scientific theory, Maxwell, an outstanding mathematician, believed that formal methods may be irrelevant to the epistemological status of the physical theory since they might have nothing to do with any physical content (Maxwell, 1855-6, pp. 156-7). Formal analogies are just partial, temporary or speculative tools; they did not account for anything (p. 156, p. 207). What Maxwell was seeking is a "true physical theory," "a mature theory, in which physical facts will be physically explained" in the sense of finding physical causes of physical phenomena. Following Faraday, he insisted that physics was an experimental science, in which the interrogation of nature led to the establishment of physical connections (pp. 156-9, p. 189, and pp. 207-8).

We can see that Maxwell was very much like a realist going along a causal or experimental approach concerning the issue of evidential relevance. A physical phenomenon must be accounted for by its physical cause. The confirmation of the finding of a physical cause is basically a matter of experience, with the help of a theoretical description of its causal connection, consisting of physical (dynamical) laws. We can recognize that this view has nothing significantly different from the criteria of confirmation held by contemporary scientists, which I summarise by my CIC. Hence presumably Maxwell did not realistically believe his model of ether because it had not met the criteria. The model was useful by means of analogy and formal relations for deducing his theory, but, after all, it was no more than a useful tool.

Maxwell's seeking for a theory of electromagnetism underwent three stages (1855-6, 1861-2, and 1864). The major construction of his theory was done at the second stage where the ether model was used to help to draw "Maxwell's equations," the basic laws of the theory. The third stage was a reorganization of the theory in order to get rid of the "hypothesis," the ether model, as its component. So we just need to consider the form of his theory at the second stage, the theory of molecular vortices.

At the beginning, the ether model was constructed by an analogy with the molecular vortex theory of gas and heat. Maxwell conceived the magneto-electric medium as a *fluid*. In a region of nonzero magnetic field, this medium would be filled with innumerable small vortex tubes or filaments, corresponding to the magnetic field lines. Later, in order to establish a connection between the vortices model and electricity, the magneto-electric medium was to be conceived as a cellular medium, each cell consisting of a molecular vortex surrounded by a cell wall consisting of a monolayer of small spherical particles, which roll without slipping between adjacent vortices, functioning as idle wheels or ball bearings. He imagined that "a great many vortices, with their surrounding particles, are contained in a single complete molecule of the medium" (Maxwell, 1855-6, p. 471). A string of vortex cells was now taken to correspond to a magnetic field line, and a flux of the particles to an electric current. When the particles flow, the vortices would rotate, and the change of the rotation of the vortices would exert force on the flow of the particles and bring about its change. This corresponded to the interaction between electric current and magnetic field (see p. 475, equation (54) also chapter 5).

Soon, Maxwell found himself required to incorporate dielectric media in the vortex model by a mechanism for creating and maintaining a differential displacement of the particles in a molecule. For this he had to change the nature of the vortex medium from "fluid or mobile" to elastic *solid*, which allows for such a displacement. He said:

According to our hypothesis, the magnetic medium is divided into cells, separated by partitions formed of a stratum of particles which play the part of electricity. When the electric particles are urged in any direction, they will, by their tangential action on the elastic substance of the cells, distort each cell, and call into play an equal and opposite force arising from the elasticity of the cells. When the force is moved, the cells will recover their form, and the electricity will return to its former position(Ibid., p. 492).

In this way a flux of the idle-wheel particles is produced in this model, corresponding to a displacement electric current in the dielectric. The model was thus finished; most laws about electromagnetic phenomena were drawn from it. It can be seen that these two processes were intertwined, the model was constructed according to the known properties of electromagnetism, and in turn it helped to deduce some new laws about electromagnetism.

Yet Maxwell did not hold the model realistically, due to its analogical character and its implausibility. He did not believe that electromagnetic phenomena were really causally produced by those molecular vortices and idle wheels. He declared explicitly in his (1861-2):

The conception of a particle having its motion connected with that of a vortex by perfect rolling contact may appear somewhat awkward. I do not bring it forward as a mode of connection existing in nature, or even as that which I would willingly assent to as an electrical hypothesis. It is, however, a mode of connection which is mechanically conceivable, and easily investigated, and it serves to bring out the actual mechanical connections between the known electromagnetic phenomena; so that I venture to say that anyone who understands that provisional and temporary character of this hypothesis, will find himself rather helped than hindered by it in his search after the true interpretation of the phenomena (p. 486).

The point that his model of ether was just a working model has been reinforced again and again. As Tolstoy said, "Maxwell, of course, believes in an aether... But whatever its nature, this aether only behaves analogously to his model. The system of vortices and idle wheel is not a true picture - it merely has analogical value, as a kind of crutch for one's thinking" (Tolstoy, 1981, p. 122). That is why Maxwell did not mind keeping on modifying it by introducing such new and strange hypotheses as "idle wheels", "elastic solid medium", and Weber's theory of magnetism without worrying about the inconsistencies they had created in some other parts of the model (like being both fluid and solid)[1].

It can be expected, therefore, that Maxwell, who had been pursuing a mature, physical theory, would try to abandon the model as soon as possible. In 1864 came his "great memoir," *A dynamical Theory of the Electromagnetic Field,* in which there was no need for any ether model[2]. What was left was a belief in the ether simply because

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[1] This has also been suggested by Hendry (Hendry, 1986, p. 189).

[2] Siegel wrote: "Very soon, beginning in 1864, he retreated from that definite, concrete, and realistically intended mechanical account of the universal ether,..." (Siegel, 1980, p. 259). We know that the model was never "realistically" intended and taken; it was not a "retreat".

there must be a seat for energy[1]. But it was by no means a model. The concrete and definite model of the medium had been discarded. This indicated that the successful theory in his (1861-2) did not have to stand on that mechanical model of ether. So we can see that actually the reason for him to believe in the ether was just that general one: the transmission of light, heat and energy as waves would be inconceivable without an aethereal medium. He repeatedly indicated that he went in a different direction from the theory, based on the hypothesis of action-at-a-distance, not because the theory was not able to account for electromagnetic phenomena (actually it was, he conceded), but because he was dissatisfied with the hypothesis itself. In a letter to Bentley, he admitted that he just could not conceive of action at a distance through a vacuum, without some sort of mediation. Thus "Maxwell also required a medium while at the same time remaining strictly agnostic as to its nature" (Hendry, 1986, p. 249). (Also see Maxwell, p. 323, p. 775.)

In our terms, therefore, Maxwell needed the ether, as many others did, but he found himself having few reasons to say that it had confirmation. This was both because no realistically plausible model of it had been developed, and because Maxwell saw no possibility of detecting any causal effect of it in the near future. He noted, for example, that "the whole question of the state of the luminiferous medium near the earth, and of its connexion with gross matter, is very far as yet from being settled by experiment" (Maxwell, p. 770). He also had doubts about Michelson's effort to test the ether's wind.

In his *A Treatise on Electricity and Magnetism*, the most comprehensive and important report on his mature thought on electromagnetism in 1873, his model of 1861 was mentioned once, only as a sequel to some remarks by W. Thomson, referred to as

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[1] Hendry notes: "Maxwell was quite clear that his medium was nothing more than the recipient of energetic properties. It could be interpreted as a mechanical aether, but such a interpretation was not strictly part of Maxwell's theory, and even though he talked of a medium, what the concept entailed was essentially that of the modern field" (Hendry, 1986, p. 267).

"a working model," which "must be taken for no more than it really is" (Niven, vol. 2, p. 470). Tolstoy found that "in the *Treatise* the word aether is mentioned only once" (p. 149). The ether was no longer involved in any problem-solving procedure in the theory of electromagnetism.

Now we see, from Fresnel to Maxwell, various problems with finding a good model of a material ether remained unsolved. Even in Fresnel, the programme of the ether began regressing. After Maxwell, the regression became even faster and more remarkable. The artificiality, strangeness, inconsistency and other inadequacies of its models became more and more intolerable. As a result, model construction was no longer commonly realistically intended[1]. By the close of the 19th century, "it came to be generally recognized that the aether is an immaterial medium, *sui generis*, not composed of identifiable elements having definite locations in absolute space" (Whittaker, 1951, p. 303). That is, before Einstein, the ether as a material thing had already been close to dying for most people in terms of the standards of its own framework.

Conclusion

My final conclusion is that as far as I know, none of the standard cases, either contemporary or historical, can prove that scientists' acceptance of an entity based on good reasons in my sense is an acceptance of a fiction. The ether case, the strongest challenge, can not make the pessimistic induction from history stand; Laudan's argument can be ruled out from the debate concerning realism about entities. That realism, within the domain of some branches of natural science, is not only surviving, but even thriving.

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[1] These models included Fitzgerald's in 1885, W. Thomson's in the end of 19th century and Larmor's (see Siegel, 1980).

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