



THE PROGRESS AND RATIONALITY OF PHILOSOPHY
AS A COGNITIVE ENTERPRISE:
AN ESSAY ON METAPHILOSOPHY

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ABSTRACT

Throughout the history of philosophy, philosophers have commented (often with dismay), at the state of cognitive discord and lack of consensus which seemingly characterizes their discipline. As David Hume observed: "There is nothing which is not the subject of debate, and in which men of learning are not of contrary opinions. The most trivial question escapes not our controversy, and in the most momentous we are not able to give any certain decision" (Hume, 1960, p.xviii). If Hume is right, then the question should be immediately asked: 'is the Western tradition in philosophy a degenerating "research programme"?' Alternatively: 'how can philosophy as a cognitive enterprise be considered to be both *progressive* and *rational* in the face of the *problem of perennial philosophical disputes*?' (Kekes, 1980). The conflicting views of the philosophers, formed part of the *rationes dubitandi* of scepticism in antiquity (Rescher, 1978, p.217). The sceptics maintained on this basis, that philosophical knowledge was impossible. This view is held today in a variety of forms (Rorty, 1979), (Unger, 1984). It is the aim of this work to suggest a resolution to the problem of perennial philosophical disputes and show that philosophy is both a rational and progressive enterprise. Unless this can be shown, then, as I have argued in my book *Reductionism and Cultural Being* (Smith, 1984), the rationality and progressiveness of a large portion of the social sciences is also threatened.

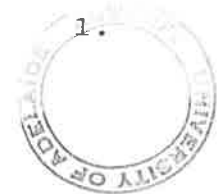
DECLARATION

1. The thesis contains no material which has been accepted for the award of any other degree or diploma in any University and that, to the best of the candidate's knowledge and belief, the thesis contains no material previously published or written by another person, except where due reference is made in the text of the thesis.

2. The author consents to the thesis being made available for photocopying and loan if applicable, if accepted for the award of the degree.

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1. STATEMENT OF THE ARGUMENT: IS PHILOSOPHY A
DEGENERATING RESEARCH PROGRAMME?

1. THE PROBLEM

Principles taken upon trust, consequences lamely deduced from them, want of coherence in the parts, and of evidence in the whole, these are everywhere to be met with in the systems of the most eminent philosophers, and seems to have drawn disgrace upon philosophy itself . . . [E]ven the rabble without doors may judge from the noise and clamour, which they hear, that all goes not well within. There is nothing which is the not subject of debate, and in which men of learning are not of contrary opinions. The most trivial question escapes not our controversy, and in the most momentous we are not able to give any certain decisions (Hume, 1960, pp. xvii-xviii).

The first to present his case seems right, till another comes forward and questions him (Proverbs, 18:17).

If Hume's description of the state of cognitive discord of the discipline of philosophy is correct, then the question should be immediately asked: 'is the Western tradition in philosophy a degenerating research programme (or research tradition) (Laudan, 1977)?' Alternatively: 'in the face of the *problem of perennial philosophical disputes* (PPPD), how can philosophy as a cognitive enterprise be considered to be both *progressive* and *rational*?' This is a major problem for anyone who understands philosophy as that discipline which attempts to formulate and justify *Weltanschauungs*, that is, world views (Kekes, 1980), (Smith, 1984).¹ Such *Weltanschauungs* attempt to give an account of the furniture of the universe, a view of human nature and an outline of the place of human beings in the scheme of the world. Philosophy unlike most literature and poetry, *must* support its conclusions by reasoned argument. It is not enough merely to describe the world, the description must be justified and shown to be more satisfactory and preferable to other competing descriptions, with respect to various cognitive standards such as truth, consistency,

comprehensibility and simplicity, among others (Rescher, 1979(a)). If philosophy is a rational enterprise, it should give us *knowledge*. If philosophy is a cognitively progressive enterprise, it should give us increasingly more knowledge over time. Yet the existence of perennial philosophical disputes seems to indicate that in its 2500 year history, philosophy has produced no knowledge and not a single philosophical problem has been satisfactorily resolved. Not only does the history of philosophy seem to be little more than the successive refutation of previous philosophical *Weltanschauungs*, but in more recent times when the bulk of published basic research in philosophy has appeared in specialized journals, it is not uncommon to find criticisms of an author who has published a paper in a particular journal, criticized in the very issue in which the target paper appeared. From an anthropological perspective, philosophy may be seen as a discipline torn by unending disputes, agonized by pluralism and unachieved consensus, and humiliated by the success of the natural sciences. The disagreements and failures of the past and present, *should* weigh like a nightmare on the brains of the present generation of philosophers.

The problem of perennial philosophical disputes is almost as old as philosophy itself, being discussed very early in philosophy's history primarily by the Sceptics. Sextus Empiricus in his *Outlines of Pyrrhonism* (1933, bk 1, Sect.178) discussed the problem in antiquity, and the 'unreliability of philosophical opinion as indicated by the disagreements of the philosophers', appears as the first *tropoi* of Agrippa's five *rationes dubitandi* (Rescher, 1978, p. 217). For the Greek Academic Sceptics, (PPPD) indicated that the philosophical enterprise itself was bankrupt and that there was no philosophical knowledge; for the Pyrrhonists the assertion of even the bankruptcy of the philosophical enterprise, was itself a knowledge claim upon which judgement must be suspended, with the attainment of a blissful state of *ataraxia* (Popkin, 1960). Almost

two thousand years later, Benson Mates (1981) has also argued that all of the major problems of philosophy are incapable of either solution or dissolution, since the reasons given on both sides of the debate can be equally good. But for Mates, this situation hardly leads to a blissful state of *ataraxia*; if anything it indicates the strong existential absurdity of the human condition. Problems such as of the nature of freedom, goodness and justice, questions of the *telos* of the world (if any) and the structure and origin of the universe, must be solved if human life is to be rich, meaningful and fully flourishing. For Mates such problems cannot be solved by philosophy and human life is all the poorer.²

In these introductory remarks I cannot adequately explicate the notion of *philosophical unsolvability* (it is not crucial to the analysis at this point in any case), and so the topic is deferred for discussion until chapter 7. At this stage of the discussion, my account of *perennial philosophical disagreements* and the thesis that philosophy is a *degenerating research programme* remains metaphorical, and whilst more detailed explications are promised for later in the work, some further guiding remarks may be added here. My use of the term 'degenerating research programme', whilst obviously taken from Lakatos' famous work (Lakatos, 1970) is once more metaphorical; I could just as easily use Laudan's term 'research tradition'. To be explicit however, in investigating the issue of whether or not philosophy is a degenerating research programme/tradition we are asking whether or not philosophy is a *progressive discipline in the sense of providing a rapid solution to problems which the discipline investigates*. The concept of a *discipline* is taken as a primitive here; the reader with a university education should have an intuitive grasp of this notion. The notion of the progressiveness of a discipline will be discussed more fully in chapter 3. For the moment I will maintain that 'progressiveness' in philosophy is best understood in the realist sense of providing increasingly

more truths or truth-like propositions.³ If however philosophy is riddled with perennial disputes - that is, disagreements that have existed not merely for decades, or hundreds of years, but possibly millennia - then it is extremely difficult to see how it could be maintained, with rational justification, that philosophy provides a rapid solution to the problems found in its domain, supplying humanity with a constant or even an accelerating flow of truths or truth-like propositions. It is a remarkable scandal that philosophers who generally pride themselves with the virtues of rigorous analysis and argument and the systematic examination and criticism of all knowledge claims, have devoted little of their logical energies to an examination of the rational status of their own discipline.

Something should be said now about what the unit of progress is in philosophy if there is such a unit. This question has seldom received any discussion in the literature dealing with perennial philosophical disputes; in Kekes' detailed discussion of this problem (Kekes, 1980) for example, we are left wondering whether it is philosophy as a whole, selected parts of philosophy, world views or philosophical theories which are said to be progressive or degenerative. This is no trivial question because Kekes introduces our problem, as I have done, globally - as a problem for philosophy as a discipline (ibid, Chp.1). However much of his discussion of the problem of perennial philosophical disputes is concerned with *world views*. The precise relationship between the *discipline* of philosophy and philosophical world views is not explicated in detail. Yet if he is to solve the problem which he set himself, it is precisely these sorts of questions which must be addressed.

In this work, the *unit* of philosophical progress is taken to be a philosophical theory. A philosophical theory is a non-null set of propositions which responds to a philosophical *problem*. In the next

chapter I shall give a formal model of this. Philosophical theories are the basic units that are either progressive or degenerative. The issue of explicating the notion of the progressiveness or degenerativeness of a theory is a complex issue that will also be dealt with in a separate chapter of its own. Now world views or philosophical systems can be simply explicated as being sets of philosophical theories. A world view usually is a set consisting of a *finite* number of philosophical theories. For example Russell's world view in the last twenty five years of his life consisted of the theories which he held on aesthetics, logic, epistemology, ontology, ethics, and so on. It is intuitively reasonable to say that a world view W_1 is *more progressive* than a world view W_2 if *given that they both are concerned with the same philosophical problems*, W_1 contains more progressive theories than W_2 . If they both contain the same number of progressive theories then W_1 and W_2 will be said to be equally progressive. If W_1 contains more progressive theories than W_2 , then W_2 shall be said to be degenerative relative to W_1 .

Is it reasonable to propose that all of the theories accepted by a subject at some time t constitute the world view of the subject at that time? Isn't this characterization somewhat counter-intuitive? I do not believe that it is. A world view is just that - a *world view* or a view of the world. Now if theories are conceptual entities which explain puzzling events in the world and describe the structure of reality, theories must therefore give us a view of the world. My basic idea is that a subject's theories are the various "pictures", "views" or "ideas" of parts of the world. A subject's aesthetics for example is his/her view or idea of the nature of aesthetic value. Put all of these "points of view" together and one has a global picture of the world - a world view. This world view may be incomplete or inconsistent, yet it is

still a broad picture of the nature of the world. I therefore reject the claim that my characterization of a world view is counter-intuitive.

Philosophy as a discipline can be analysed into sets of world views. The discipline of philosophy will be said to be progressive relative to the discipline of say physics, biology, chemistry, sociology and so on, if when we obtain the ratio of solved problems to unsolved problems (where the number of unsolved problems is non-zero) in philosophy, this ratio is greater than the ratio of solved to unsolved problems in the other discipline. If the ratios are the same then both disciplines will be said to be equally progressive, and if the ratio of solved to unsolved problems in a discipline is less than the ratio of solved to unsolved problems in another discipline, then the former discipline will be said to be relatively degenerative. This definition enables us to compare disciplines which not only may have totally different numbers of solved and unsolved problems, but which, obviously enough, have problems concerned with a totally different subject matter. These definitions are based upon the rationally appealing intuition that a mature and progressive discipline is a good problem-solver, whilst a degenerative discipline is not.

The above remarks have been made to clarify the notion of the unit of philosophical progress and also to show that it is not logically absurd to compare the progressiveness of disciplines. It therefore *does* make sense to state the problem of perennial philosophical disputes by claiming that the discipline of philosophy is a poor problem-solver relative to any of the natural sciences. This account can be challenged on Kuhnian/Hegelian grounds and we will consider such objections shortly in section 3 below. Nevertheless for the moment the only other serious objection that could be made here is that the idea of a philosophical problem itself is too vague. Little can be said to reply to someone with

such high standards of precision. Such a person must also dismiss discussions of scientific, moral and social progress as being unsatisfactorily vague as well. Therefore the vagueness objection is not one which I find particularly bothering.

Equally obvious, if we wish to compare different disciplines as a whole, then we must accept that the compared disciplines share the *same* concept of progressiveness - otherwise comparison is meaningless. Now part of the intuitive formulation of the problem of perennial philosophical disagreements relies upon a comparison between the allegedly weak problem-solving ability of philosophy and the allegedly strong problem-solving ability of mature sciences such as physics. My definition permits such a comparison even though it would be laborious to actually work out *any* ratios of progressiveness. But it is not necessary to do this to see the challenge of the problem of perennial philosophical disagreements. This is so because the extreme lack of consensus in philosophy about which problems are solved at all makes it reasonable to take philosophy's progressiveness ratio to be zero: an absolutely minimal degree of progressiveness.

The above analysis assumes, as I have already said, that philosophy shares with the sciences the same basic concept of progressiveness. This means that there are strong elements of similarity between the structure of scientific theories and philosophical theories. Both scientific and philosophical theories are answers to, or explanations of problems. Darwin's theory of natural selection for example, is a set of propositions attempting to correctly answer the question 'how did species arise?' Empiricism is an epistemological theory attempting to correctly answer the question 'what is the nature, scope and limits of human knowledge?' These examples indicate that it is not unreasonable to suppose that philosophical and scientific theories have a common *logical structure*.

However it does not follow from this that there is a methodological unity between philosophy and science. In this work and *Reason, Science and Paradox* (Smith, 1986) I shall attempt to show that philosophical theories are not a special type of scientific theory. There are philosophical methods of argumentation not generally found in the sciences, showing that there is no strict methodological unity between philosophy and the sciences. Philosophy is distinguishable from the sciences by these methods. Also both philosophical and scientific theories may be ideological. In an appendix to *Reductionism and Cultural Being* (Smith, 1984) I argued that the characteristic quality of ideology is not falsehood but its use by agents in justifying the legitimacy of the status quo. It is therefore logically possible, although unusual, to find true philosophical and scientific theories, being used to justify the ruling position of some power elite. According to the view of theory-progress to be defended in this work, an ideology is not the sort of entity which can be said to be progressive or degenerative.

It is the aim of this work to offer a defense of the progressiveness and rationality of philosophy in the light of the problem of perennial philosophical disagreements, rebutting the metaphilosophical sceptic's challenge. This is a project which I dealt with only very sketchily in my previous book *Reductionism and Cultural Being* (Smith, 1984), and this work will correct the omissions, and transcend the limitations of the former work and my earlier research papers on (PPPD). The project will break new ground in a field which is extremely underdeveloped: the methodology of philosophy. For example, as we shall see in chapter 3, discussions of cognitive progress have been centred primarily upon the natural sciences and little work has been done on developing models of cognitive progress for a philosophical subject matter. In this work both the questions of progress in philosophy and science will be addressed, and a model of cognitive

progress developed which avoids the defects of existing models.

In the remaining space of this chapter I shall explicate my principal thesis in more detail, and give an overview of the structure of the argument of the work.

2. RESPONSES TO THE PROBLEM OF PERENNIAL PHILOSOPHICAL DISAGREEMENTS

There are, elaborating on Rescher (1978, p.219), four basic alternative explanations of (PPPD):

- (A) *Eliminative Explanations*. Eliminative explanations dismiss the entire discipline as cognitively bankrupt. Philosophical questions are either outrightly incapable of solution (*solvability scepticism*) being proper pseudo-problems, or alternatively such problems are meaningful, but simply incapable of solution. Eliminative explanations may also take a weaker pragmatic bent. Philosophy is seen as a waste of energy and mind-power, human resources which could and should be channelled into alternative pursuits such as: (1) science, especially the natural sciences; (2) management science, commerce or business; (3) hedonistic pursuits such as gross sexual and drug encounters or (4) the practical business of simply living one's life.
- (B) *Causalist Explanations*. Causalist explanations see perennial philosophical disagreements as either a purely sociological, psychoanalytical or sociobiological phenomena. They are consequently fully explicable with respect to either sociological, psychoanalytic or sociobiological causal mechanisms without recourse to an examination of the arguments canvassed in any philosophical disagreement.

(C) *Methodological Explanations*. Methodological explanations seek to explain the phenomena of perennial philosophical disagreement as due to the lack of appropriate philosophical methods. This may be because of either the complete absence of such methods or because the methods have not been sufficiently developed, and have existed only in a primitive form. With careful refinement and development of philosophical methods (PPPD) can be dissolved.

(D) *Internalist Explanations*. Internalist explanations maintain that perennial philosophical disagreements are the *modus operandi* of philosophical inquiry itself; it is the natural and healthy state of the discipline itself and is not to be taken as a ground for wretched lamentations. The ground of perennial philosophical disagreements are traceable to either: (1) the problems of philosophy; (2) the methods of solving philosophical problems; (3) the structure of philosophical argument; (4) the extreme complexity of the reality which beings of finite intellectual powers seek to grasp, or in some combination of all four variables.

These explanations are not mutually exclusive and may be combined in a number of metaphilosophical models of perennial philosophical disagreements. An attempt at a more finely structured taxonomy which I gave previously is as follows (cf. Smith, 1984, chapter 2):

(1-1) Metaphilosophical Nihilism (MN)

(a) *strong thesis*: The diversity of opinion with respect to philosophical issues illustrates the bankruptcy of the philosophical enterprise itself. Philosophical problems are either (i) pseudo-problems, incapable of solution because of

their cognitive meaninglessness and obscurity or
 (ii) cognitively meaningful, but nevertheless outrightly
 unsolvable. Philosophy, as traditionally conceived, is
 incapable of resolving its central problems and is in need
 of replacement by some other discipline, such as physics,
 sociology, history or hermeneutics. (PPPD) is best understood
 causally, rather than rationally.

(b) *weaker thesis*: Whilst philosophical issues may be capable
 of resolution, the task is not worth the effort. Such a
 resolution is not in any way of value, and resolves none of
 the pressing problems of the human condition. Concern should
 therefore be directed elsewhere.

(1-2) Metaphilosophical Irrationalism (MI)

(a) *strong thesis*: It is never *reasonable* for any human subject
 to accept any philosophical theory or *Weltanschauung*, or to
 believe that any philosophical theory resolves any
 philosophical problem, on purely epistemic grounds. Reasoned
 argument, that beloved tool of the Rationalist, is subject
 to the *tu quoque* argument: a defence of Rationalism must
 appeal to argument and experience, and as such must be
 viciously circular. Just as Kant produced what he believed
 were antinomies of reason, which he directed against the *a*
priori metaphysics of his time, so we must conclude from
 (PPPD) that philosophy as conceived in the Western tradition,
 is itself subject to antinomies, thus constituting an
 effective *reductio ad absurdum* of the Western tradition.

(b) *weaker thesis*: It is never more *reasonable* for any human
 subject, on purely epistemic grounds, to believe that any

philosophical theory or *Weltanschauung* satisfactorily resolves any philosophical problem, than it is to believe the denial of this claim.

(1-3) Metaphilosophical Scepticism (MS)

It is not *known* by any human subject whether any philosophical theory resolves any philosophical problem.

(1-4) Metaphilosophical Relativism (MR)

There are no criteria of assessment of world views independent of those internal and peculiar to the various considered world views;

(4a) This situation is cognitively intolerable, therefore the strong thesis of metaphilosophical nihilism is true, or

(4b) This situation illustrates something fundamental about philosophical argumentation, namely its inherent inconclusiveness. Perennial disputes thus cannot be avoided, being part of the nature of philosophy itself;

(4c) This illustrates something fundamental about the nature of reality and the objects of philosophical inquiry; either

(i) there is no such reality, therefore disagreement exists because there is nothing to agree about;

(ii) there is a reality but it consists of a multitude of incommensurable "worlds";

(iii) there is a reality, but it is either unknowable (*epistemological scepticism*), or not apprehendable by any rational and evidential means, but only through art, drugs, sex, yogic consciousness or divine revelation (*transcendental irrationalism*).

(1-5) Metaphilosophical Anarchism (MA)

The Western rationalist ideal of the pursuit of the Truth by reasoned argument is an utterly obscure and mistaken ideal, and the problems facing rationality and progress by (PPPD) are illusions based upon totally misconceived notions. We should be free to choose theoretical, metaphysical and logical principles to suit our tastes and desires, as well as our political and axiological orientations. The question of the conflict of arguments, theories and world-views, begs important political questions: in a free society such a pluralism should be encouraged, not crushed under the foot of the Rationalist's jackboot.

(1-6) Metaphilosophical Monism (MM)

There is only one correct system of philosophy;

(6a) This system S_1 exists at present and all competitors $S_2, S_3, \dots S_n$, are demonstrably inadequate.

(6b) S_1 is accepted fideistically as being the correct philosophical system.

Metaphilosophical Monism may also be formulated as a position with regard to philosophical theories, as well as specific philosophical arguments advanced to solve a specific problem.

(1-7) Metaphilosophical Perspectivism (MP)(7a) Oriental Perspectivism (OP)

One and only one position is right from a given orientation, but there exist a plurality of perspectives, none of which is uniquely correct. This position accepts Metaphilosophical Relativism only between systems: from the perspective of

some probative orientation, further reasoning is possible. These perspective are value orientations, arising from cultural and personal intellectual experience. One can reason about these evaluative attitudes, but to do so, is to already adopt an orientation from which one's own orientation is retrovalidated. Therefore the existence of pluralism in philosophy is inevitable and with regard to any philosophical issue there will also exist many equally reasonable and incompatible solutions.

(7b) Monistic-Systemic Perspectivism (MSP)

While most positions grasp at least some of the truth about reality and the objects of philosophical inquiry, virtually all positions *at present* suffer defects. It is possible, in principle, to produce an ideal cognitive system which possesses all the merits of each system (consistent with various systematization constraints such as consistency) and none of the defects. It is at least possible to approach this ideal system by the piecemeal construction of a series of less problematic systems. Philosophical progress cannot be merely that of agreed achievement of solutions to problems: it must also involve in its final explication the increasingly sharper definition of problems, the exposure of error and the sealing off of blind alleys.

The aim of this work is to produce a response to the problem of perennial philosophical disputes, consistent with the Monistic-Systemic Perspectivist view of knowledge discussed in *Reductionism and Cultural Being*. To condense the discussion somewhat, the organisation of the chapters of this work will not follow precisely my older classification of

the responses to (PPPD). Rather I shall modify the classification as follows. Metaphilosophical scepticism, relativism, anarchism and nihilism will be discussed in chapter 4, along with the Orientational Perspectivism of Nicholas Rescher. Chapter 5 discusses Objectivist responses to (PPPD) within the framework of the programme of a naturalized epistemology. This is a more comprehensive category than Rescher's *Causalist Explanations* for Objectivists not only attempt to *explain* why perennial philosophical disputes exist, but they also are concerned with commenting upon the philosophical significance of perennial philosophical disputes with respect to the progress and rationality of philosophy as a cognitive enterprise. Chapter 6 discusses Internalist responses to (PPPD), a category which corresponds approximately to Rescher's *Methodological Explanations* of (PPPD). These three chapters as a whole present a comprehensive discussion of the modern literature on (PPPD). Chapter 7 explores the implications of some recent work in decision theory and mathematical logic to (PPPD). The conclusions of chapters 4 to 7 are exceedingly negative: I argue that none of the considered material presents a satisfactory response to (PPPD).

I shall now state explicitly the *principal thesis* of this work, and proceed immediately to explicate it:

(PT) The work will attempt, consistent with (MSP): (1) an explanation of *why* perennial philosophical disputes exist; (2) a critique of the major metaphilosophical and scientific responses to (PPPD) showing that they fail to secure the progressiveness and rationality of philosophy as a cognitive enterprise, and fail as well to produce satisfactorily general explanations of the existence of perennial philosophical dispute; (3) to

show that despite (PPPD) philosophy is both a progressive and rational enterprise, capable of generating knowledge-claims.

The idea of a perennial philosophical dispute has not yet been satisfactorily clarified (despite the remarks given towards the close of section 1 above), and chapter 2 will be exclusively devoted to this task. Nor has any detailed discussion of the notion of philosophical progress been given. There is a very good reason for this: no satisfactorily detailed account of this notion is currently available. Indeed, we do not have even a non-problematic notion of scientific progress available. Any realist account of scientific progress must face the conceptual difficulties and paradoxes facing the notion of *verisimilitude* (Tichý, 1974). This issue will be discussed in chapter 3 and resolved (at least I boldly propose) in chapter 9.

It is an assumption accepted by all parties to the dispute, that a justification of knowledge claims may be made by an appeal to an established consensus between ideal thinkers, most usually experts in the field under consideration, whose judgement is not swayed by non-rational factors such as personal power and academic status. Disagreements between ideal thinkers is taken to be impossible if such thinkers are purely rational, and Lehrer and Wagner (1981) have attempted to demonstrate this mathematically. In chapter 7 I will reject the proposal that it is impossible for ideal thinkers to rationally disagree. I shall go on to establish that the idea that there are no unsolvable problems in philosophy and the sciences is quite mistaken, and will reject the claim that a justification of knowledge claims may be made by an appeal to an established consensus. This gives us both an explanation and response to (PPPD) by *dissolving* the problem. Explaining the existence of

philosophical disputes is not particularly pressing if the progressiveness and rationality of philosophy is not at stake. In fact, we have no good reason to believe that any (satisfactory) unified account of the existence of perennial philosophical disputes can be given, as I attempt to show in chapters 4-7 of this work.

Despite the existence of perennial disputes I shall try to show that the following traditional view of philosophy, given by Brand Blanshard (1980, pp.211-212) is basically correct:

Philosophy, broadly conceived, is a persistent raising of the question Why? It may be objected that this is a description that does not define, that the same is done in many other disciplines also - in the formal and natural sciences, in religion, indeed in the making of countless everyday decisions. Philosophy so conceived would infiltrate every department of life. Well, so it does, and so it should. Philosophy is not a special subject like geology or history; it is rather a special kind of activity. Taken at its loftiest, it is the attempt to understand the world. But the world is inexhaustible in its extent and its variety, and the work of understanding can proceed only by the solution of numberless more specific problems that lie along the way. Philosophy, as I conceive of it, is a continual effort, made by individual minds but sustained by a congenital and racial drive, to render its world intelligible.

The aim of philosophy is to present truths about reality. How this is possible, is discussed in chapter 10, and in more detail in my *Reason, Science and Paradox* (1986).

The project will conclude with an image of cognitive life. Whilst we are encultured beings (Smith, 1984), we stand alone epistemologically, and cannot with justification rely upon social consensus as a criterion of truth. Whilst I shall cite other supporting explanations of (PPPD), the rejection of the consensus view of knowledge is my prime target in this work. To reinforce this attitude I shall in the course of this work attack some almost universally accepted knowledge claims in mathematical logic and other areas. This inquiry shall be furthered in *Essays on Ultimate Questions* (Smith, 1986) and *Reason, Science and Paradox* (Smith, 1986) which will criticize the consensus position on a number of basic

issues in contemporary physics, logic and metaphysics. My dissatisfaction with mainstream social theory and theoretical biology has already been recorded (Smith, 1984); this work supplements my previous views.

3. ATTEMPT TO DISSOLVE OUR CENTRAL PROBLEM: TWO POST-EMPIRICIST PHILOSOPHY OF SCIENCE OBJECTIONS AND A HISTORICAL REPLY

I now wish to consider two further objections to the problem of perennial philosophical disagreements which derive from post-empiricist philosophy of science. These objections attempt to dissolve our central problem by showing that there are no perennial philosophical disputes. These objections will now be outlined, and after doing this I shall try to show that both replies are unsatisfactory on historical grounds.

The first objection *loosely* derives from the work of Kuhn (1970) although some may wish to call this criticism the Hegelian response. According to this view problems and standards are historically variable, hence philosophical solutions and their adequacy are also historically variable. As a result, perennial philosophical disputes do not really occur. Philosophers living in different contexts may use the same words, but they mean quite different things by them. Stated more formally this argument is as follows:

(K₁) Philosophical problems and standards of adequacy change over time.

Therefore,

(K₂) Solutions to philosophical problems and standards of adequacy change over time.

Therefore,

(K₃) Perennial philosophical disputes do not occur.

A second objection which is a variant of the above argument has been given by Ruth Barcan Marcus (1985). Marcus argues that on historical grounds we can see that most past philosophies are displaced, so there must be substantial agreement about which philosophical theories are rejected. Philosophical theories do not endure (ibid, p.325):

"There are more than 700 Western philosophers judged worthy of notice in Paul Edwards' *Encyclopedia of Philosophy*. Many of them had a period of substantial recognition. Most have ceased to be studied or read except for occasional recondite scholarship. Very few will even fleetingly be a continuing part of any philosophical canon. Far from never having suffered rejection, most wane and are largely ignored. They fare no better than abortive or rejected scientific theories. Nor is the eclipse due to their failure to achieve the fullness and richness of holistic speculative systems as is sometimes claimed. A perusal of the literature falsifies the suggestion. I am reminded that my first awareness of speculative philosophical systems was through happening upon some imposing volumes by John Elof Boodin with titles like *The Realistic Universe, Time and Reality*. In the early years of this century Boodin was seriously studied. His post-humous papers were published as late as 1957 by the University of California Press. Where is he now?"

Boodin is of course dead, but it does not follow that the issues which he dealt with, such as the problem of realism and the nature of time are dead. The sort of historical argument employed by Marcus is insufficient to show that there are either no, or very few, perennial philosophical disagreements. What must be shown is that *all* philosophical problems, or all of the most important philosophical problems, change over time, along with the standards of adequacy. This qualification needs to be added to the argument cited above before it can be regarded as being valid. It is the soundness of this argument which I wish to question by questioning the truth of its premises.

First I shall show that there is an example of a philosophical problem which has been discussed since the dawn of Western philosophy in Greece - the problem of formulating a consistent view of change in the light of Zeno's paradoxes - which whilst changing in formulation over two thousand years remain with us still. As Wesley Salmon has put it in his introduction to a volume of essays on Zeno's paradoxes (Salmon (ed), 1970, pp.43-44):

"It would, of course, be rash to conclude that we had actually arrived at a complete resolution of all problems that come out of Zeno's paradoxes. Each age, from Aristotle on down, seems to find in the paradoxes difficulties that are roughly commensurate with the mathematical, logical, and philosophical resources then available. When more powerful tools emerge, philosophers seem willing to acknowledge deeper difficulties that would have proved insurmountable for more primitive methods. We may have resolutions which are appropriate to our present level of understanding, but they may appear quite inadequate when we have advanced further."

According to this point of view even though Zeno's original formulation of his paradoxes makes these paradoxes seem like little more than sophisms, the paradoxes have been reformulated over time to escape previous solutions and revive the difficulties which Zeno first saw with the concept of change and plurality. This means that a problem may very well change over time, as might the standards of adequacy by which solutions to the problem are judged, and yet enough continuity exist between the original problem and its reformulations to say that we are considering either the *same* problem, or the same *family* of problems. This means that the mere fact that philosophical problems are reformulated, modified and rethought does not show that there are no perennial philosophical disagreements or disputes. This fact is consistent with philosophical problems becoming *more difficult* over time, as if we have opened one door and found a thousand before us. The problem of the nature of the self has been complicated by a century of neurophysiological

discoveries; the semantical paradoxes such as the Liar paradox which puzzled most great philosophers from the dawn of Western philosophy, have been supplemented by an array of powerful logical paradoxes discovered by modern formal logicians and arguments such as the ontological argument for the existence of God have been revived through the use of new formal systems of modal logic. It would require a discussion which would be more lengthy than necessary to fully document my case by considering all of these philosophical problems. Hence I shall concentrate on the example of Zeno's paradoxes to make my point.

Zeno's paradoxes are a particularly good example to choose because they enable us to deal with this objection on its most favourable grounds: that philosophers in different contexts may use the same words, but they mean quite different things by them. Certainly concepts of space, time and motion have radically changed over the last two thousand years. But Zeno at no point attempted to define 'motion' or 'change'. He devised paradoxes which he thought, rightly or wrongly, would threaten any conceivable general metaphysical theory of motion or change. To see this I shall concentrate my discussion upon the best known of Zeno's paradoxes: *Achilles and the Tortoise*.

Aristotle in his *Physics* VI, 9, 2396 formulates Zeno's paradox of Achilles and the Tortoise in the following fashion (paraphrased): Achilles can never beat the slow Tortoise in their race, for given the Tortoise's lead Achilles must first reach the place from which the Tortoise started. But by then the Tortoise has a further lead, so that the slower Tortoise must always be in advance, however small. If this is so, then there can be no occasion when a body can reach its destination, for after any distance there will always be another distance to cover. Hence motion is impossible. This argument is based

upon the premise that space and time are *continuous* and thus is explicitly advanced as an attack upon continuous theories of space and time. Another of Zeno's paradoxes, the so-called Stadium, attacks the notion of discontinuous or atomistic space and time. Given that space and time are either continuous or discontinuous, Zeno concludes that reality is unchanging. Zeno is certainly not unaware that it appears to our senses that change occurs and that Achilles passes the Tortoise. He has taken the criterion of logical argument to override the evidence of the senses. This then is the first response which can be made to Zeno's paradox: accept the conclusion and deny the possibility of motion and change. This view has only been opted for by a handful of Western philosophers: F.H. Bradley's *Appearance and Reality* (1930), whilst making no explicit textual reference to Zeno, uses Eleatic arguments to establish that space, time and motion are contradictory appearances.

Another solution which a few philosophers have taken is to accept that Zeno's arguments are sound, and also that motion is possible, so that reality is seen to have inherent contradictions. This position was taken by Hegel in his *Lectures on the History of Philosophy* (1892, pp.261-278) when he rejected the universal validity of the law of non-contradiction. This position has also been taken by modern dialectical logicians such as Graham Priest. The dispute between Priest and Zeno could only be resolved by examining the question of the validity of the law of non-contradiction: differences in viewing the validity of this law account for the radically different metaphysical positions of these philosophers, even though both groups accept Zeno's arguments as being valid, and in fact sound.

Other philosophers attempted to use Zeno's paradoxes to prove particular speculative metaphysical theses. Henri Bergson in *Creative Evolution* (1911) for example accepts that change or becoming is

continuous, but denies that either mathematical analysis or logical reasoning are capable of enabling us to understand motion and change. This style of solution, if it can be called that at all, is certainly not the most common solution to Zeno's paradox. Many philosophers have felt that Zeno's paradoxes, especially Achilles and the Tortoise rest on simple mathematical errors. Russell (1929, pp.182-198) and Whitehead (1929, p.107) were both critical of various paradoxes of Zeno on mathematical grounds. It is worthwhile considering their respective views on the problem with Achilles and the Tortoise.

Russell reformulates the Achilles paradox mathematically and attempts to show a mathematical flaw in the argument. If Achilles overtakes the Tortoise it must be after an infinite number of instants have elapsed since the beginning of the race. Russell accepts that this is true. He then argued that Zeno's view that an infinite number of instants must make up an infinitely long time is not true, and so Zeno's conclusion is refuted. Russell at no point shows that this proposal - that an infinite number of instants add up to an infinitely long time - is either accepted by Zeno or is essential to his argument. It is hardly likely that Zeno accepted Russell's proposal, since in Zeno's paradox of plurality he considers what he might describe today as the difficulty of constructing the extended linear continuum out of unextended elements. Nor is Russell's reconstruction of Zeno's argument satisfactory in so far as it makes the source of the difficulty of the Achilles plain. We shall see the real source of this difficulty after examining Whitehead's response.

Whitehead argues that the Achilles should be analysed in the light of the modern mathematical theory of convergent series. By use of these series we can show, given the appropriate distances and times, when and where Achilles overtakes the Tortoise. According to Peirce, ". . . this

silly little catch presents no difficulty at all to a mind adequately trained in mathematics and in logic (Peirce, 1935, 6.177). Not all philosophical minds trained in mathematics and logic have agreed with Peirce. Max Black (1950-1951) has argued that the notion of a limit shows that these distances and intervals of time may *approach* zero but that they do not become zero as Zeno's original paradox seemed to require. The mathematical solution is fundamentally irrelevant.

Max Black's paper is very interesting for its attempt to show that the expression "infinite series of (performable) acts" is self-contradictory by means of the notion of an infinity machine. It may be thought that there is no logical impossibility, but only a medical impossibility in performing an infinite number of tasks and in fact motion consists in the performance of an infinite number of tasks. There is never a time at which we are *ending* our motion, but there is a time at which we would have already reached our destination. The infinity machine is used to show that this proposed solution to Zeno's Achilles paradox cannot succeed. The following example comes from James Thomson (1970, pp.94-95):

"There are certain reading lamps that have a button in the base. If the lamp is off and you press the button the lamp goes on, and if the lamp is on and you press the button the lamp goes off. So if the lamp was originally off, and you pressed the button an odd number of times, the lamp is on, and if you pressed the button an even number of times the lamp is off. Suppose now that the lamp is off, and I succeed in pressing the button an infinite number of times, perhaps making one jab in one minute, another jab in the next half minute, and so on, according to Russell's recipe. After I have completed the whole infinite sequence of jabs, i.e. at the end of the two minutes, is the lamp on or off? It seems impossible to answer this question. It cannot be on, because I did not ever turn it on without at once turning it off. It cannot be off, because I did in the first place turn it on, and thereafter I never turned it off without at once turning it on. But the lamp must be either on or off. This is a contradiction."

The extensive literature dealing with infinity machines (Salmon (ed), 1970) shows that Zeno's Achilles paradox is far from being trivial, and far from being satisfactorily solved if space, time and motion are accepted as being continuous. I have now supplied an example of a philosophical problem which is perennial - being discussed by many great philosophers since the dawn of Western philosophy - and which through recent reformulations, has become even more difficult. So a philosophical problem can change over time, yet still be perennial. What is important is the sort of change. I have considered internal changes in a problems formulation in my attempt to refute this objection. I now wish to consider the view that perennial philosophical disagreements do not exist because *entire* problems are abandoned.

It is true historically that many philosophical problems were the product of the interest and acceptance of certain philosophical systems, and once they were abandoned, the problems went with them. A good example of this, is the problem of the nature of the Absolute: when absolute idealist philosophical systems were abandoned at the turn of this century this problem ceased to be of central interest to philosophers. But the problem of the Absolute is not a "grand" philosophical problem arising from speculation about the basic fabric of reality and the fundamental concepts involved in understanding, such as the problem of universals, the nature and justification of knowledge, the nature of causation and the freewill problem. In what follows I shall give one example of a philosophical problem which has not changed in its formulation since the dawn of Western philosophy, and where contemporary philosophers usually begin their discussion of this problem by citing and relying upon the ancient formulation of this problem.

Sextus Empiricus in *Outlines of Pyrrhonism*, II, chap.IV gave a

precise outline of the *diallelus* problem of justifying a standard of truth, justification or knowledge (Sextus Empiricus, 1933, pp.163-165):

" . . . in order to decide the dispute which has arisen about the criterion, we must possess an accepted criterion by which we shall be able to judge the dispute; and in order to possess an accepted criterion, the dispute about the criterion must first be decided. And when the argument thus reduces itself to a form of circular reasoning the discovery of the criterion becomes impracticable, since we do not allow them [the Dogmatic philosophers] to adopt a criterion by assumption, while if they offer to judge the criterion by a criterion we force them to a regress *ad infinitum*."

This argument attempts to establish that an adequately justified standard of knowledge is impossible. Given that there can be no particular knowledge claims without some general standard used to justify or show that what is claimed to be knowledge is in fact knowledge, it follows that there is no knowledge at all unless the *diallelus* can be escaped.

Richard Popkin (1964, p.xi) points out that the Pyrrhonic form of scepticism was unknown in the West until the rediscovery of the manuscripts of Sextus Empiricus in the sixteenth century. Popkin's book *The History of Scepticism from Erasmus to Descartes* is a detailed historical treatment of the impact of Sextus Empiricus' arguments upon theology and philosophy during the period, 1500-1650, which we can draw on here to develop a critical historical argument against this objection.

Popkin points out that the full impact of Sextus Empiricus' *diallelus* argument was first felt in the dispute over the proper standard of religious knowledge. The problem of finding a criterion of truth, reinforced by Sextus' argument, was later raised regarding natural knowledge provided by the new natural sciences of the day. This led to what Popkin describes as the *crise pyrrhoniennne* of the early seventeenth century (ibid, p.1).

The conflict between Martin Luther's views and his quarrel with Erasmus illustrates the difficulties raised by the *diallelus* in a theological context. In writings such as *The Appeal to the German Nobility* and *The Babylonish Captivity of the Church*, Luther denied that the Church is the criterion of religious knowledge and at the Diet of Worms, pleaded that the correct criterion of religious knowledge is conscience conditioned by a reading of the Scriptures. In outlining a new criterion of religious knowledge, Luther directly challenged the authority of the Church. This challenge was met by Catholic theologians, particularly Erasmus of Rotterdam, by a sceptical defense of the faith. Erasmus in *De Libero Arbitrio* argued that Scripture is not as clear and uncontroversial as Luther had supposed: theologians have argued about the meaning of certain Scriptural passages and the correct solution of certain theological problems for centuries without any sight of solution. Now Luther claims that *he* has within his grasp the *true* meaning of Scripture: but how do *we* know this? This whole debate is too difficult to resolve Erasmus maintained, so it is best to accept in good faith the traditional teachings of the Church. Luther replied to Erasmus in *De Sermo Arbitrio* arguing that scepticism was inconsistent with Christianity as the Scriptures are not composed of unjustified hopes, but of God-given *truths*. Some of these truths may be difficult to know, but there are basic truths which are clear and evident. These truths serve to illuminate the meaning of "darker" passages (*ibid*, pp.5-7). Nevertheless Luther left it unclear as to why that which our religious conscience convicts us in believing as true when reading the Scriptures, is in fact true rather than false. Calvin attempted to answer this problem by maintaining that our inner persuasion (if we are a Christian!) about what is true and false in religious matters is given to us by the Holy Spirit, an all-knowing and all-powerful entity who would not deceive us.

Nevertheless the Catholics argued in reply that Calvin did not escape the *diallelus* as the criterion of religious knowledge is inner persuasion, and this is authentic because it is caused by the Holy Spirit, and we know this because of inner persuasion (ibid, p.9).

The rediscovery of the manuscripts of Sextus Empiricus extended the *diallelus* problem from theology to philosophy. Gian Francesco Pico della Mirandola in *Exam Vanitatis Doctrinae Gentium* first made use of Sextus' arguments to destroy the foundations of rational philosophy which he saw supplying philosophical justification for various pagan world views. He hoped to lead the sceptically-devastated pagans to rest with the Christian revelation. This course of action was also recommended by Francisco Sanchez in *Quad nihil scitur* and by the better known Michel de Montaigne in his *Apologie de Raimond Sebond*. Montaigne gave a restatement of the *diallelus* problem which has been a particularly influential statement of this problem. Here, for example, is a paraphrase of Montaigne's French given by Roderick Chisholm (1973, p.3):

"To know whether things really are as they seem to be, we must have a *procedure* for distinguishing appearances that are true from appearances that are false. But to know whether our procedure is a good procedure, we have to know whether it really *succeeds* in distinguishing appearances that are true from appearances that are false. And we cannot know whether it does really succeed unless we already know which appearances are *true* and which ones are *false*. And so we are caught in a circle."

Montaigne's version of the *diallelus* was accepted by P. Coffey in *Epistemology or The Theory of Knowledge* (1958), part I, a work first published in 1917. Montaigne's essay is thus a crucial link between the seventeenth century considerations of the *diallelus* problem and twentieth century considerations. It is hardly possible here to discuss the *diallelus* problem in the work of all major philosophers from the seventeenth century onwards, and nor is this necessary to rebut this

objection and historically illustrate the perennality of a major philosophical problem. Rather all that we need to show is that the *diallelus* problem was discussed in both the seventeenth century and the twentieth century. Even if it was not given the same intensive discussion in the eighteenth and nineteenth centuries, it does not follow that the *diallelus* problem is not a perennial philosophical problem. All that this shows is that at certain times philosophers do not discuss certain problems: it does not show that the problems have been either rationally resolved or rationally abandoned.

Coffey felt that the *diallelus* argument was based upon an equivocation, although he by no means felt that this problem was a trivial one. The argument assumed that the criterion of truth must always be extrinsic to the judgement the truth of which it is the test, but (*ibid*, p.144):

" . . . since we have the power of reflecting on our judgements, what if we find that some judgements contain *in themselves* and *inseparable from themselves*, a characteristic which is the test, or criterion, of their own truth: so that by one and the same intuition we see the *truth of the judgement*, and simultaneously, - not antecedently, or subsequently or by a distinct judicial act, - the *validity of the criterion*?"

Another philosopher, Leonard Nelson, writing a few years before the publication of Coffey's book, was not confident that the *diallelus* argument could be solved in this fashion. Arguing before the Fourth International Congress for Philosophy at Bologna in 1911, Nelson maintained that it was impossible to give reason to believe that our knowledge is objective. He gave the following version of the *diallelus* to show this (Nelson, 1973, p.6):

"In order to solve this problem, we should have to have a criterion by the application of which we could decide whether or not a cognition is true: I shall call it briefly the "validity criterion". This criterion would itself either be nor not be a cognition. If it be a cognition, it would fall

within the area of what is problematic, the validity of which is first to be solved with the aid of our criterion. Accordingly, it cannot itself be a cognition. But if the criterion be not a cognition, it would nevertheless, in order to be applicable, have to be known, i.e., we should have to know that it is a criterion of the truth. But in order to gain this knowledge of the criterion, we should already have had to apply it. In both cases, therefore, we encounter a contradiction. A "validity criterion" is consequently impossible, and hence there can be no "theory of knowledge".

The above statement of the *diallelus* argument is virtually a paraphrase of Sextus Empiricus' ancient formulation, although Nelson nowhere acknowledges this. The only difference is Nelson's use of the term 'cognition', which means 'true judgement' or 'true proposition'.

In the writings of Chisholm (1973) and Rescher (1973(b); 1979(a); 1980(a)) the formulation of the *diallelus* also follows the classical sources of Sextus Empiricus and Montaigne. Both authors, who have considered the *diallelus* problem in more detail than any other modern philosophers, also add refinements to the classical argument to make the argument clearer. This, as we shall now see, does not materially change the problem, but rather makes the difficulties posed by the *diallelus* argument more evident.

Rescher in *The Coherence Theory of Truth* (1973(b)) gives the following analysis of the *diallelus* argument after citing Sextus' formulation. This clearly indicates that Rescher believes that he is dealing with the *same* problem that Sextus dealt with. A criterion of truth (or knowledge (Rescher, 1980)) is of the form: whenever a proposition p meets the requirement R , then p is true:

$$(C) \quad (\forall p) (R(p) \rightarrow T(p)).$$

Now to establish the truth of p is to give a deductively sound argument: $C \ \& \ R(p) \rightarrow T(p)$. Now if this argument is sound, then it must be both formally valid, and have true premises. Consequently, if $C \ \& \ R(p)$ is to

be established as true, we must establish $T(C)$. To give a deductively sound argument for $T(C)$ by taking C to be self-applicable, is to give an argument of the form $C \ \& \ R(C) \rightarrow T(C)$. To establish $T(C)$, it is necessary that the truth of C is established, i.e. $T(C)$, and we thereby fall into vicious circularity. If C is not self-applicable, then $T(C)$ is established by use of another criterion C_1 by an argument $C_1 \ \& \ R_1(C) \rightarrow T(C)$. For this argument to be accepted, it must be established to be deductively sound. To do this, we must establish that $T(C_1)$. To appeal to another criterion C_2 leads us into an infinite regress.

Roderick Chisholm (1973) also begins his discussion of the *diallelus* by citing a classical source, this time Montaigne. He then adds the following analysis of the problem. We may distinguish between two fundamental epistemological questions: (A) What is the *extent* of our knowledge? What do we know? and (B) What are the *criteria* of our knowledge? How are we to decide *whether* in fact we do know? *Methodists* claim to be able to answer question (B) and on the basis of this provide an answer to question (A). *Particularists* claim to be able to answer question (A), and on the basis of this answer question (B). Sceptics on the other hand argue that (1) it is necessary to *first* solve the question of the extent of our knowledge in order to solve the question of the criteria of our knowledge and (2) it is *also* necessary to *first* solve the question of the criteria of our knowledge before we can solve the question of the extent of our knowledge. This however is a vicious circle. Consequently, the sceptic concludes, knowledge is impossible.

Chisholm and Rescher both supply elaborate responses to the sceptic's *diallelus* argument. It is too much of a tangent to consider their solutions here, just as it is an unacceptable tangent to detail the

importance of the criterion problem for other leading philosophers in this century such as Russell and Popper. Our aim has been to outline one alleged perennial philosophical dispute and show that this problem has been discussed since the dawn of Western philosophy in much the same form as it was originally stated. Whilst I do not pretend to have written a historical treatise on this issue, the reader should be able to see a broad historical link between figures such as Sextus Empiricus, Montaigne and Rescher and Chisholm. This places the burden of proof upon the critic to show that despite the use of common formulations of a problem which they believe is the same, these thinkers are really dealing with different problems. Therefore I conclude on the basis of these historical examples of "deep" philosophical problems, that the Hegelian argument for the non-existence of perennial philosophical disputes, fails.

1. NOTES

1. The metaphilosophical question 'what is philosophy?' arises at this point, and I have no wish to explicitly answer it immediately, although chapters 11 and 12 will outline *my own* metaphilosophy, what *I* take philosophy to be. Here I offer a very broad conception of philosophy which is particularly troubled by (PPPD); no attempt is made to present anything approaching a comprehensive and coherent definition of 'philosophy' which might be applicable to all schools, capturing the *essence* of philosophical inquiry. It is difficult to see what could possibly be the essence of any discipline which would unite the writings of Zen Buddhists and Logical Positivists, Bernard-Henri Lévy and Quine, Heidegger and Dummett.

2. This leaves us in a position which can only be a generalization of Sartre's plight in his *Being and Nothingness* (1956, pp. 38-39):

As a being by whom values exist, I am unjustifiable.
 My freedom is anguished at being the foundation of values
 while itself without foundation ... I do not have nor can
 I have recourse to any value against the fact that it is
 I who sustain values in being .

3. The notion of *progressiveness* in philosophy is very difficult to understand in any other sense but a realist sense, involving the provision of increasingly more truths or truth-like propositions. It is difficult for me to see how any pragmatist or instrumentalist account of progressiveness could satisfactorily operate for very general metaphysical and epistemological theories. This objection will be developed in my criticism of Kekes' (1980) extremely interesting solution to the problem of perennial philosophical disputes to be discussed in chapter 6 below.

2. DIALECTICS, CONTROVERSY AND PHILOSOPHICAL DISAGREEMENTS

1. STATEMENT OF THE ARGUMENT

In the previous chapter an introductory sketch of the principal thesis of this work was given. In this chapter I attempt to clarify key terms of the principal thesis. I shall do this by analyzing the question 'What is a perennial philosophical dispute?' This analysis I take to be comprised of two sub-tasks: (1) to state what a philosophical dispute consists of and (2) to analyze the notion of perennality.

Previous works on the problem of perennial philosophical disagreements such as (Rescher, 1978), (Kekes, 1980) devote little space to any analysis of 'perennality' and 'philosophical dispute', seemingly taking these expressions to wear the clarity of their meaning upon their faces.¹ Kekes, whilst saying little about what a philosophical dispute consists of, on the matter of perennality, in a brief passage has this to say (ibid., p. 20):

... though perennial arguments may be external or internal, what does it mean to say that they are *perennial*? *Perennial* carries the suggestion of being endless, long-standing, recurrent, enduring; and I do mean that the arguments I am concerned with are endless and recurrent. But I want to underplay the implication that perennial arguments must have a long history. For the life-span of perennial arguments depends on the life-span of the ideal which is argued about. Some of the ideals are very old indeed; knowledge, morality, logical consistency, and rationality have at least as long a history as our civilization. But others, such as culture, scientific understanding, or freedom are quite recent. In my use of *perennial* I do not want to exclude relatively recent ideals. Therefore, it is not their duration, but their lack of finality and recurrence which I take to constitute their perennial aspects.

I agree with this characterization of the notion of 'perennality', and hope to be able to say more about this notion than has been said by Kekes. Surely the first step in solving a problem is to be clear

about what the problem is. Thus let us now turn to the explication of the expression 'philosophical dispute'. In sections 2 and 3 I will consider some logical formalizations which significantly clarify the expression 'philosophical dispute'. This is not a case of formal precision for the sake of formal precision; as I have argued elsewhere, in a paper on the relevance of games theory to the problem of perennial philosophical disputes (Smith 1983(a)), such formal models are needed if we are to examine the relevance of mathematical theories to our principal problem. On this matter I shall have more to say in chapter 7.²

The structure of my argument in this chapter is as follows. In section 2 I shall outline in some detail Rescher's theory of dialectics as developed in (Rescher, 1977(a)). This exposition is given not merely because Rescher's theory is of interest in itself (and that a comprehensive treatment of my subject matter requires its mention), but because this theory is the best answer to the target question 'what is rational disputation?' The critic who feels that even this cannot satisfy his/her demands for clarity, will find the bulk of this work objectionable in its methodology and choice of subject matter. Such a critic would also find the bulk of standard philosophical discourse objectionable because of its ultimate obscurity. This critic can however be refuted by a *tu quoque* argument: the critic him/herself is also using terms which are very far from precise, the term 'vague' for example is itself vague. If the critic's own position is infected with the same cognitive defect, as allegedly our own position is thought to be, then we need not bother further with this criticism. Other reviewers seem to find that all expositions of other writer's positions are of little value, and no doubt they will find this problem again here. In my defense I shall point out that if the attitude that expositions of

other writer's positions are of little value is taken as a serious objection to the structure of the argumentation, then the critic seems to have committed him/herself to the necessity of arguing through every point first hand, and to have eliminated in one swoop, an important academic preoccupation. Against this view I maintain that it is of value to distil relevant and worthwhile material from other authors. Why? Because what they say may be substantially correct and satisfactorily answer a question of interest. Non-trivial interesting truths are well worth hearing.

After outlining Rescher's theory of dialectics, and thus answering the question 'what is a philosophical dispute?' I add a further refinement to this analysis by discussing some mathematical models of dialogue. The motivation for this discussion is as follows. First it can be very easily shown that Rescher's dialectics can be reconciled with the to-be-discussed mathematical models of dialogue. The point seems to be worth making and is surely relevant to our interest in dialectics. Having said this, however, I must also point out that I am not committed to operating with such formal models at any other point in this work. To do so *would be* to introduce unnecessary technicalities and formalism. I choose only to show how some of the basic concepts of my topic may be clarified. This point leads me immediately to the second motivation for my exposition: I shall use concepts from the mathematical models of dialogue to explicate the notion of *perenniality* in section 4, which commits me to presenting an exposition of the required and related concepts.

I alight then from the dock and begin my study of Rescher's theory of dialectics.

2. RESCHER ON DIALECTICS

Rescher (1977(a)) has presented a theory of dialectics, the theory of rational disputation, debate and controversy, and has argued that this topic, neglected by contemporary epistemology, can have profound implications for the theory of knowledge. Rescher attempts to develop a dialectical model for the rationalization of cognitive methodology, specifically scientific inquiry. Included in his work is a dialectical criticism of epistemological scepticism. Our interests here remain with Rescher's account of disputation, rather with more controversial matters, such as his theory of *petitio principii*.³

Disputation is taken to involve three parties, the *proponent* and the *opponent*, two disputing adversaries, and the *determiner* who judges the dispute. The proponent is to defend a thesis T , and opens the debate by advancing T and offering grounds G_i in support of T . The opponent advances counterarguments in rebuttal of these grounds R_{ij} , and the proponent in turn offers a rebuttal R_{ijk} of the opponent's R_{ij} . This exfoliating tree process of the continuing elaboration of reasons *pro* and *con* for T , is shown by a *connected graph*. A connected graph consists of a set of points known as *nodes*, with branches (line segments) between certain pairs of nodes such that a path can be traced out from each point to every other point. Connected graphs with no closed loops (i.e. crossing branches) are called *trees*, whilst the graph of a game is called a *game tree*.⁴ The isomorphism between the structure of Rescher's model of formal disputation and that of the games tree is quite suggestive of the relevance of games theory as a mathematical model of processes of formal disputation. We will consider this matter in chapter 7. Rescher's account of formal disputation is diagrammatically represented in figure 2.1.

FIGURE 2.1

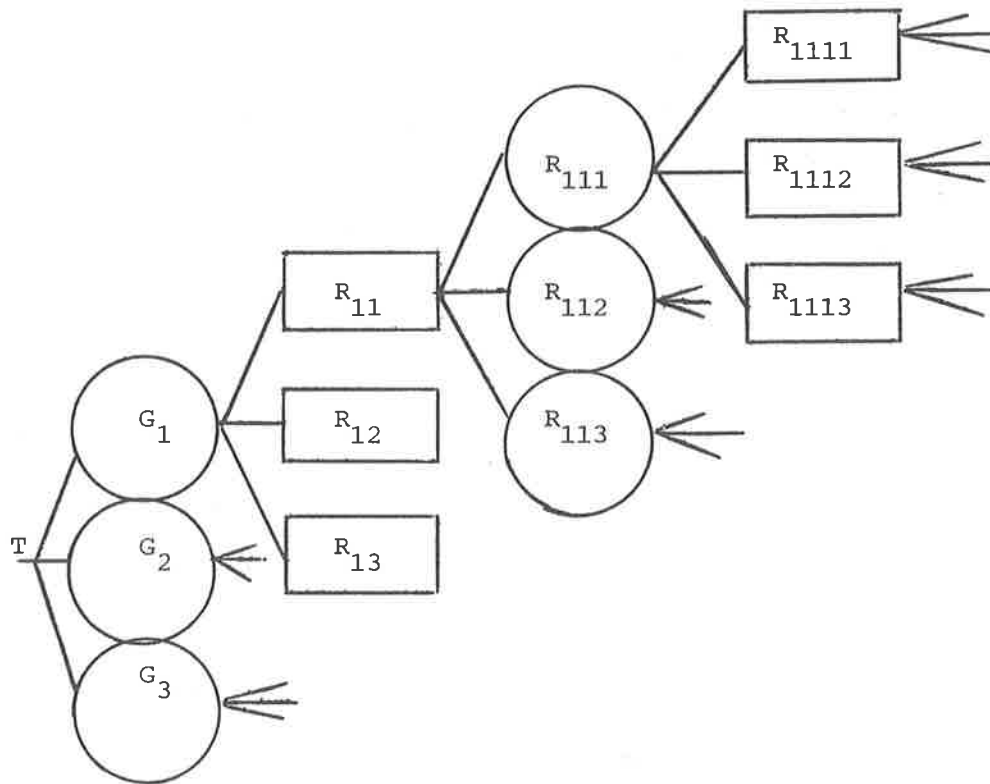
RESCHER'S MODEL OF FORMAL DISPUTATION

Figure 2.1 represents only one branch of the exfoliating tree process of disputation, and thus is an oversimplification. Complications will also arise once we recognize that for some node, the opponent may need to argue for some sub-thesis T^* as one part of his/her general attack on T .⁵ This results in a branch arising from any of the opponent's nodes in figure 2.1. This situation results in a structure which is 3-dimensional, like a neural net or cellular system. Since we permit exfoliation trees to arise from any of the nodes of this tree, it is convenient to represent disputation by a game tree in n -dimensional space.

Rescher allows three basic types of moves to the proponent:
 (1) *categorical assertion*, '!P' for 'P is the case', or 'it is maintained (by the assertor) that P'; (2) *cautious assertion*, '!p' for

'P is the case for all that you (the adversary) have shown' or 'P's being the case is compatible with your argument'; (3) *provisioed* assertion, 'P/Q' for 'P generally obtains provided that Q', or 'Q constitutes *prima facie* evidence for P?'⁶ Moves of the !-type can be made only by the proponent, and those for the †-type only by the opponent. If P/Q is made, then either !Q or †Q must also be made. Since P/Q is consistent with $\sim P(Q \& R)$, and also because neither *modus ponens* nor transitivity hold as principles of dialectical logic, P/Q is not an implication relation. It is a much weaker relation, that of *presumption* (ibid., p. 8).

Rescher in chapter 4 of dialectics states that in dialectical logic the law of non-contradiction is "abandoned", or "fails" (ibid., pp. 61-68). Rescher goes on (pp. 69-72) to locate his position within deviant logics, such as paraconsistent logics. S.E. Hughes (private communication) has argued that Rescher is mistaken in doing this. If a relationship is defined between propositions, such as the slash relationship, which is a weaker relationship than implication, then logical laws such as *modus ponens* are not strictly speaking expressible in the system, and so can hardly fail. Hence dialectics does not presuppose any particular type of deductive logic.

Some other properties of the slash-relation are now listed. Transitivity does not "hold" for the slash-relation. If P holds in most cases where Q does, and R holds in most cases where P does, it is not necessarily the case that R holds in most cases where Q does. The law of non-contradiction, excluded middle and double negation, also do not hold (ibid., pp. 62-68). Whilst we do not have both P/Q and $\sim P/Q$ (at the price of generating a "dialectical antinomy") we can have both P/Q and $\sim P/(Q \& R)$. In such a situation, the evidence at our disposal is too strong (ibid., p. 66) and the law of non-contradiction does not hold. The law of excluded middle does not hold in dialectics allowing information underdetermination.

The principle of double negation is rejected as being a principle of dialectics because negation is not viewed in dialectics as the mere denial of a thesis, but "[when] P/Q is succeeded by $\sim P/(Q \& R)$, there is not just the displacement of the transition from P to $\sim P$, but also the refinement (amplification, improvement) of the transition from Q to $(Q \& R)$ " (ibid., p. 66).

Rescher supposes for simplicity of exposition that the disputants cannot make erroneous claims regarding purely evidential relationships. Thus an exchange such as:

	proponent		opponent
1.	$!P$		$\sim P/Q \& \dagger Q$
2.	$P/Q \& !Q$		

is ruled out by Rescher. To do so however severely limits his account of dialectics in any philosophical domain, especially in the philosophy of logic and philosophy of science, where arguments about such purely evidential relationships do occur. Thus we have no need for such a restriction.

Finally we note various dialectical countermoves to fundamental moves. In reply to $!P$, the opponent may (1) make challenge or cautious denial $\dagger \sim P$ or (2) make a provisoed denial $\sim P/Q \& \dagger Q$ for some Q . As a formal disputation always begins with $!P$ by the proponent, a formal disputation always opens with one of the following patterns:

Pattern I		Pattern II	
	proponent		opponent
1.	$!P$		$\dagger \sim P$
2.	$P/Q \& !Q$		$\sim P/Q \& \dagger Q$

Countermove to $\dagger P$ are (1) categorical counterassertion $! \sim P$ or (2) provisoed counterassertion $\sim P/Q \ \& \ !Q$ for some Q . Rescher compiles a long and complicated list of other countermove which will not be summarized here (ibid., pp. 11-17).

This system of dialectics readily yields a definition of a philosophical dispute. In a philosophical dispute, T^* is an element of the set of philosophical theses and this situation occurs:

proponent of T^*	opponent of T^*
1. $!P$	$\dagger \sim p$
2. $P/Q \ \& \ !Q$	

or this situation occurs:

proponent of T^*	opponent of T^*
$!P$	$\sim P/Q \ \& \ tQ$

A ground of disagreement which I have expressed with Rescher (Smith, 1983(a), p.13) is over his assumption that disputation involves a *determiner*. There is no determiner of a philosophical dispute, unless trivially we take the determiner to be the ideal entity <proponent, opponent>. To this one may reply that the determiner of a dispute is a judge who is not actually a debating party at the dispute, but studies the dispute and makes an adjudication. Whilst this seems plausible for legal debate, it is not so plausible when applied to philosophical disputation. The determiner is simply a part of the philosophical dispute accepting either $!P$ or $\sim !P$, $! \sim P$ or $\sim ! \sim P$, or sceptically $\sim (!P \vee ! \sim P)$. The determiner is an element of Rescher's

dialectics which can be eliminated when we consider philosophical disputations.

This completes my survey and suggested modifications of Rescher's theory of dialectics, and the first question which we set ourself to answer in section 1 is answered. This model captures, I believe, the formal structure of philosophical disputation. The model is however capable of further refinement, by embodying it in a general mathematical model of dialogue. Rescher has made no step towards doing this, and an inquiry into the possibility of extending the theory in this direction is of value. In overview then, sections 2 and 3 of this chapter will provide a formal answer to the first question asked in section 1 above 'what does a philosophical debate consist in?' I shall say nothing in this chapter about the nature of philosophical argument itself, addressing this issue in chapter 12 below where I argue for the rationality of the philosophical enterprise. The second question, asking us to analyze the notion of perennality, will be addressed in section 4 below.

3. HAMBLIN AND MACKENZIE ON THE LOGIC OF DIALOGUE

Attempts to provide a formalism for the analysis of dialogue have been made by both Hamblin (1971) and Mackenzie (1979(a), (b); 1981).⁷ Not all aspects of these positions are of interest to us here, so the following description will be selective. Hamblin takes as primitive the notions of *participants* and *locutions*. Let P be a set of participants and L a set of locutions, then a *locution-act* is a member of $P \times L$, $\langle P_n, L_n \rangle$. A dialogue is a member of the set:

$$(2-1) \quad D = \bigcup_n (P \times L)^n \quad (n \in \mathbb{N})$$

of dialogues of any *length*. The length n of a dialogue is a member of the set $(P \times L)^n$ of sequences of n locution acts. Thus a dialogue

may be represented by the triple $\langle n, p, l \rangle$ where $n \in N, p \in P, l \in L$. The set $E = N \times P \times L$ is the set of *locution-events*. A dialogue is a set of locution-events such that the first members of the various locution events are in consecutive numerical sequence. The notions of *monologue*, *subdialogue* and *submonologue* of p are defined as follows:

$$(2-2) \quad \text{Mon}_p = \text{df } \bigcup_n (\{p\} \times L)^n$$

$$(2-3) \quad \text{Subd} = \text{df } \{x: (\exists d \in D) (x \cap d = \emptyset. x \cup d \in D)\}$$

$$(2-4) \quad \text{Subm}_p = \text{df } \text{Subd} \cap \{x: (\exists y \in \text{Mon}_p) (x \subset y)\}$$

A *contribution* of p to dialogue d is defined as follows:

$$(2-5) \quad \text{Cont}_{p,d} = \text{df } \text{Subm}_p \cap d \cap \{x: (\forall y \in d) [(y \in \text{Subm}_p. x \subset y) \supset x = y]\}.$$

Hamblin uses a possible world semantics for the semantics of locutions. If W is the set of possible worlds, then a statement is a member of the set $S = \{s: s \subset W\}$ and the negation of s , the conjunction of s and t and the disjunction of s and t are the sets $W-s, s \cap t$ and $s \cup t$ respectively. A set of statements Σ is said to imply s if $\bigcap \Sigma \subset s$. If W_0 is the actual world, s is true if $W_0 \in s$, and false if $W_0 \notin s$. A question is a member of the set $Q = \{q: q \subset S\} - \{\emptyset\}$, and s is an answer to q if $s \in q$. The *presumption* of a question q is the disjunction of its answers or equivalently $\bigcup q$, and a *verisumptive* question q is one with true presumption $W_0 \in \bigcup q$. We write ' l . Den z ' for ' l denotes z ', where ' l ' is a locution and z is any member of the set $S \cup Q$ of statements and questions. Hamblin assumes that each locution denotes at most one semantic entity.

Within D there is a subset K of *legal dialogues*. Hamblin defines rules as sets of prohibited dialogues $r \cap K = \emptyset$. The set R of rules applicable to a particular system can be used to define K as follows: $K = D - \bigcup R$. *Commitments* are taken not necessarily to be beliefs, but rather a function of the locution-events which have occurred. A commit-

ment-state $C_{d,n,p}$ where $d \in D$, $n \in N$ and $p \in P$, is a set of locutions, representing the current commitments of a person p after the n th locution of dialogue d . A system of dialogue then is quintuple $\langle P, L, K, W, Den \rangle$.

An example of a system of dialogue given by Hamblin (1970, p. 265) is the 'Why-Because-System-with-Questions' (WBSWQ). There are two participants, each of whom has a commitment-store containing a finite number of statements, and each participant must add or delete commitments according to rules of commitment-store operation. Let S_1, S_2, \dots, S_n be variables of the metalanguage of WBSWQ ranging over statements. Locutions then may consist of the following types of statements: (1) \ulcorner Statements $S_1, S_2, \dots, S_n \urcorner$; (2) \ulcorner No commitment $S_1, S_2, \dots, S_n \urcorner$ for one or more of S_1, S_2, \dots, S_n ; (3) \ulcorner Question $S_1, S_2, \dots, S_n \urcorner$ for one or more of S_1, S_2, \dots, S_n ; (4) \ulcorner Why $S_k \urcorner$ for any statement S_k other than a substitution-instance of an axiom and (5) \ulcorner Resolve $R \urcorner$. Five *locution rules* and five *commitment-store operations* may then be stated; here it is sufficient to cite one example of each by way of illustration. Corresponding to locution type (4) is the locution rule:

(LR4) \ulcorner Why $S_k \urcorner$ must be followed by \ulcorner Statement $\vee S_k \urcorner \vee$
 \ulcorner No Commitment $S_k \urcorner \vee \ulcorner$ Statement $S_L \urcorner$ where
 $S_k = \text{df } S_L \vee \ulcorner$ Statements $S_L, S_L \rightarrow S_k \urcorner$ for any S_L .

Corresponding to locution type (4) is the commitment-store operation:

(CSO4) \ulcorner Why $S_k \urcorner$ places S_k in the hearer's store unless it is there already, or unless he/she replies \ulcorner Statement $\vee S_k \urcorner$ or \ulcorner No Commitment $S_k \urcorner$.

Mackenzie (1979(a), (b); 1981) in defining the notion of a *system of dialogue* accepts Hamblin's primitives, the notion of 'locution-act', 'dialogue of length n ', 'dialogue' and 'locution-events'. He defines

a system of dialogue as a triple $\langle P, L, R \rangle$ (or given a semantics, $\langle P, L, R, W, \text{Den} \rangle$) by contrast to Hamblin's $\langle P, L, K \rangle$ (or given a semantics $\langle P, L, K, W, \text{Den} \rangle$). Mackenzie's account of locutions also differs from that of Hamblin's. Rather than taking L to be the union of various sets, Mackenzie takes L to be generated from the set S of statements of the propositional calculus, comprising: (a) the *negation* N 's of any statement $s \in S$; (b) the *conditional* $C \langle s, t \rangle$ of any $s, t \in S$ and (c) the alphabetically ordered *conjunction* $K'T$ of any non-empty set of statements $T \subseteq S$, where $T = \{s\}$, $K'T = s$. Mackenzie also extends Hamblin's systems by explicit use of *locution modifiers*; a k -adic locution modifier with k locutions forms a locution. In the system DT, a locution modifier is an expression which with a statement forms a locution other than a statement. The set L of locutions for DT are specified in figure 2.2.

FIGURE 2.2

LOCUTIONS FOR DT
(Mackenzie, 1979(b), p. 707)

NAME	FORM	READING	FUNCTION TO S
Statement S	'p'	'p'	I
Withdrawals	'w p'	'I'm not sure that p'	W
Questions	'? p'	'Is it the case that p?'	Q
Resolution demands	'r p'	'Resolve whether p'	R

$$L = \text{df } S \cup \{ \ell : (\exists s \in S) (\ell Ws \vee \ell Qs \vee \ell Rs) \}$$

There is a commitment function, defined inductively by specifying the initial commitment of each participant and the effect of each kind of locution event on its speaker's commitment and on its hearer's commitment, from $N \times P$ to the power set $P(L)$. This assigns a set of locutions

as the commitment $C_n(p)$ of each participant p at each stage n of each dialogue $d \in K$. These commitments are used in stating the rules R of DT. These rules are formulated also by reference to other syntactical relationships such as 'immediate consequence', which we have not summarized here to simplify exposition. The first member of R is this:

$$R_{\text{Gram}}: \text{ No legal dialogue contains an event } \langle n, A, l \rangle \\ \text{ unless } l \in L.$$

The only rule restricting the use of statements in DT is:

$$R_{\text{Repstat}}: \text{ No legal dialogue contains an event } \langle n, A, s \rangle, \\ s \in S, \text{ such that } \{s\} \subseteq C_n(A) \cap C_n(B)$$

The commitment rule for statements is:

$$CR_S: \text{ After } \langle n, A, s \rangle, s \in S: \\ C_{n+1}(A) = C_n(A) \cup \{s\} \\ C_{n+1}(B) = C_n(B) \cup \{s\}$$

According to this rule, a rule also found in Mackenzie's system DC, 'p' is included in B's commitment after A says 'p'. This, whilst seeming counterintuitive is nothing more than a formalization of the intuitive principle that *silence means assent*, and in any case B can immediately withdraw the statement if he/she does not want to be committed to it.

This leads us to the commitment rule for withdrawals:

$$CR_W: \text{ After } \langle n, A, W's \rangle: \\ C_{n+1}(A) = C_n(A) - \{s\} \\ C_{n+1}(B) = C_n(B),$$

with the following restriction on the withdrawals:

$$R_{\text{incom}}: \text{ No legal dialogue contains an event } \\ \langle n, A, W's \rangle \text{ where } s \in \lambda, \lambda \text{ being the} \\ \text{ set of logicians' conditionals.}$$

Mackenzie maintains that DT has a clear advantage over Hamblin's systems, insofar as he can solve Carroll's problem of the tortoise and Achilles

(Carrol, 1895). This is done by use of a rule of resolution demands which Hamblin's system lacks. For simplicity of exposition we shall say no more on this matter except to note that the tortoise problem can be simply answered by arguing that if the tortoise is committed to 'p' and ' $p \supset q$ ', then refusing to accept 'q', may be viewed as being committed to 'q' through CR_s and immediately withdrawing 'q'. But since $q \in \lambda$ the tortoise violates R_{incom} , so that Carroll's problem is dissolved in DT.

It is evident that Rescher's theory of dialectics can be fused with current mathematical models of dialogue. Philosophical disputes, which are nothing more than cognitive disputations about philosophical matters are *dialogues* about some philosophical problem PP where $p = \{\text{proponent, opponent}\}$. The point to be made is not the trivial one that because philosophical disputes are dialogues, then dialectics can be readily fused with current mathematical models of dialogues. Rather, it seems that systems of dialogue such as Mackenzie's DT, are more appropriate for the study of philosophical dialogues than they are for natural language dialogues about mundane topics. Resolution demands, ' r_p ', the principle that silence means assent, and the whole matter of talking about the assertion and withdrawal of theses and commitment to assumptions, are characterizing properties of philosophical debates. Natural language dialogues with their metaphorical aspect do not satisfactorily fit the current mathematical models of dialogue.

Researchers in this field, have in general, only developed their models to analyse a particular concept of informal logic and metaphilosophy, the *petitio principii*. There are two general approaches to understanding this fallacy, both of which place this research squarely in the field of dialectics. The first approach considers the fallacy in the environ-

ment of contentious debate, where one participant assumes a premise which is the subject of the dispute. This is clearly a dialectical conception. The epistemic approach says that a person begs the question by using a circular argument to defend a controversial thesis. Here as well *petitio principii* is a pragmatic notion which must also be understood by reference to disputations. If the target thesis was not controversial, if there was no question to beg, then this fallacy could not occur. Hence contemporary mathematical models of dialogue in the Hamblin-Mackenzie-Woods and Walton tradition are more appropriately viewed as a study of philosophical dialectics.

Enough has been said for the purposes of this work to characterize philosophical disputes in a formal way. The nature of philosophical argumentation will be discussed in chapter 12. In this chapter I have merely attempted to demonstrate the relevance of a field of research which metaphilosophers have seldom discussed. I shall now discuss and analyze the concept of *perenniality*. This concept can also be clarified by the formal tools now at our disposal.

4. THE IDEA OF PERENNIALITY

The citation in the text from Kekes (1980, p. 20), rejects the notion that *perenniality* involves the notion of historical longevity. Certainly there are philosophical problems which are quite ancient: the problem of universals and abstract reference, the definition and criteria of truth, and the problem of the criterion. Further, even though many ancient philosophical problems are restated today in the light of contemporary theories, at least some old philosophical problems are discussed by modern authors. For example, the problem of the criterion or *diallelus* as formulated by Sextus Empiricus and Montaigne, appears in the work which Rescher has done on this problem (e.g.

Rescher, 1979(a), pp.91-93) virtually unchanged. The criterion argument is both simple, powerful - and extremely difficult to refute (Smith, 1982(a)).

Nevertheless, many philosophical problems are of quite "recent" origin: the Gettier problem, the preface and lottery paradoxes being uncontroversial examples. However one feels on the basis of current controversies, that is, if there is still a world where such topics can be discussed, that debate on such topics will be endless and recurrent. The games trees for such debates do not have terminal nodes. Even if some argument sequence A_1, A_2, \dots, A_n is taken to be refuted by even the proponent, nothing prevents these arguments from being restated by some other proponent, perhaps with more force, in the light of results from some other subject field. Philosophy then is a non-cumulative enterprise *par excellence*. The idea of cumulativeness, is that in a cumulative system, once a proposition is established it remains so. Woods and Walton (1978) give a definition of cumulativeness in terms of a set of points, $w_i \in W$, an ordering relation $<$ on the W_i , a language L with $A, B, C, \dots \in L$ for statements A, B, C, \dots and a function f that maps $\langle w_i, A \rangle$ onto $\{1, 0\}$. A system $\langle W, <, L, f \rangle$ is *cumulative* if and only if for any two points $w_i, w_j \in W$, for any statement A , if A has a given value $\in \{1, 0\}$ at w_i , then A has the same value at w_j if $w_i < w_j$. A system is *non-cumulative* if and only if this condition does not hold. 'W' is taken to be the stages of the dialogue, '<' as the relation 'occurs before', 'L' as the set of statements, and f as the commitment function for one participant. An ideal model of philosophical disputation is a strong non-cumulative system such as Mackenzie's DD (Mackenzie, 1979(a)) which is non-cumulative with respect to both statements and challenges.

Perennial arguments then are non-cumulative (argumentative) dialogues, or potentially infinite exfoliating games trees; this is the first element which I take to characterize perennial arguments. From an

intuitive perspective, a cumulative system is one where once a proposition is established as being reasonable to be believed to be true it remains so. Philosophical disputes are non-cumulative because any thesis in philosophy, no matter how rigorously defended, always motivates a rejoinder by another philosopher, who attempts to undermine the arguments given in defense of the target thesis. Philosophical theses are like sand castles built on the shoreline of cognitive criticism: eroded by each new wave, crushed by the forces of the inevitable tide.

The second element constituting perennial arguments is *controversy*. The work of Kuhn and Feyerabend, despite strong critical response to their more extreme statements, indicates that empirical science is not as cumulative (in the intuitive sense given above) as the logical empiricist metascientist believed that it was. Nevertheless, scientists still work within 'paradigms', and such paradigms exhibit consensus about basic theoretical assumptions of the field, research strategies, scopes of problems and methodologies. If controversy occurs in the empirical sciences then it is not as *intense* and as *comprehensive* as controversy in philosophy. This is at least a widely held position on this matter. It is difficult to operationalize 'intensity of a debate' and 'comprehensiveness of a controversy', and I am not aware of any substantial research on this topic. But this much can be said by way of clarification of these expressions; a debate is certainly intense if in some time period λ , there are a large number of papers both supporting some thesis T and also a large number of papers criticizing T , i.e. supporting $\sim T$. A debate is comprehensive if there are a wide range of types of supporting arguments given for T , as well as critical arguments advanced against T . We take a 'paper' to be simply a publicly available expression of someone's

point of view.

These characterizations do not tell us precisely how many proponent papers and opponent papers are needed to make a dispute intensive and/or comprehensive, but nor do we need such absolute quantities. Our characterization is advanced only to give us a rough estimate of the intensity and comprehensiveness of a debate, as well as a relative measure of the intensity and comprehensiveness of disputation.⁸ For example, suppose that we consider two debates, P_1 in philosophy about problem ϕ , and S_1 in molecular biology about problem ψ . We then can give a rough estimate of the intensity of the debate within each discipline by calculating the number of papers for T and multiplying this figure by the number of papers against T, and likewise for an estimate of the comprehensiveness of debate. Note that if there are 0 papers against T, then we obtain an intensity of disputation value of 0, as we would expect. A relative measure of the intensity of disputation between P_1 and S_1 is obtained by calculating the absolute value of the difference between the intensity of disputation scores for each field, and likewise for the corresponding notion of relative comprehensiveness.

Philosophy then is seen to be characterized by non-cumulative (argumentative) dialogues and by intensity and comprehensiveness of disputation greater than that found in the empirical sciences. If this is actually the case, then how can philosophy make any justifiable claim to be a rational and progressive cognitive enterprise? The exploration of this problem is the principal thesis of this work.

5. CONCLUSION: STATE OF THE ARGUMENT

This chapter has attempted to clarify key terms in the principle thesis of this work, as stated in chapter 1. Hardly any attempts have

been made by researchers interested in the problem of perennial philosophical disputes to offer more than metaphorical accounts of key expressions such as 'philosophical dispute' and 'perenniality'. Here I have attempted to improve upon this state of affairs by outlining what a general theory of disputation consists in, as presented in Rescher's theory of dialectics, and showing how the theory may be enriched by augmentation with current mathematical models of dialogue. Whilst this discussion may be dismissed by some as irrelevant, I object that in today's Anglo-American philosophical climate, any topic which cannot be treated by the tools of mathematics and formal logic, whilst perhaps not being viewed with the same degree of academic suspicion which the positivists viewed such fields, is viewed nonetheless with suspicion. Metaphilosophy is however quite open to being examined in a formal way, and my subject matter quite open to mathematical treatment. Further development of the theories treated here must be left to others; sufficient background material has been given here for a consideration of some mathematical responses to the problem of perennial philosophical disputes in chapter 7.

In the following chapter an investigation of some outstanding problems in the theory of cognitive progress will be conducted. After all, if philosophy is to be shown to be a progressive enterprise, then we must outline a theory of progress. Standard "realist" accounts of cognitive progress face a very serious problem which has yet to be solved. Further, the problems which the perennial nature of philosophical disputations raise for any justified claim of the progressiveness of philosophy must also be examined.

2. NOTES

1. For a discussion of Rescher's views on the problem of perennial philosophical disputes c.f. chapter 4 below; for a discussion of Kekes' views on this problem c.f. chapter 6 below.
2. No discussion of the vast literature centred around Ch. Perelman and L. Olbrechts-Tyteca's *Traité de l'argumentation: La Nouvelle Rhétorique* (1970) is given here. Perelman and Olbrechts-Tyteca do not set for themselves the task of giving *general* characterizations of *argumentation*, *dispute* and other dialectical concepts. For a discussion of their work c.f. (Apostel, 1979(a), (b)) (Kluback, and Becker, 1979), (Maneli, 1979), (van Noorden, 1979), (Zyskind, 1979). For a discussion of the general notion of argumentation c.f. (Hamblin, 1970, pp. 224-252), (Woods and Walton, 1977).
3. For a criticism of Rescher's theory of *petitio principii* c.f. (Woods and Walton, 1982(a)). Woods and Walton object to Rescher's dialectical account of *petitio principii* on the grounds that he has formulated no "blockage rules" to stop circular reasoning such as:

proponent	opponent
!P	† ~ P
P/Q & !Q	† ~ Q
Q/P & !P	

Woods and Walton object to the dialectical systems of Hamblin and Mackenzie on precisely the same grounds, and in addition criticize these authors for failing to state what is wrong with arguing in a circle (*ibid.*, p. 592). Now a satisfactory response to this objection is to deny that such dialectical systems need blockage rules which prevent such circles from arising. If dialectics seeks to model actual argumentative practice, then we must allow such sequences to occur. Nevertheless in a situation where the proponent does commit the fallacy of *petitio principii*, he/she has not succeeded in defending P. Philosophical systems are, to use a term to be defined in chapter 2, section 4, *non-cumulative*. Thus I do not believe that the systems of Rescher, Hamblin and Mackenzie are rendered problematic by Woods and Walton's objection, and thus we are free to use this work for our present purposes.

4. It may be objected here that if Rescher followed the responses to the objection to his account of *petitio principii* given by Woods and Walton as discussed in note 3 above, then formal disputation could not be modelled by a game tree because we would immediately have closed loops. But this is not so. 'Closed loops' in games trees are crossing branches. There is no necessity to model even circular reasoning by a closed loop. Rather, in circular reasoning, we have a branch $R \rightarrow R_1 \rightarrow R_2 \rightarrow R_3 \rightarrow \dots \rightarrow R_n \rightarrow R$ where ' \rightarrow ' is a relation of epistemic support, and where $R_0, R_1, R_2, \dots, R_n$ are reasons, such that the terminal node and the initial node are identical. We can mathematically model circular reasoning by

making use of the metaphor that in such reasoning, the sequence of justificatory responses *loops back to the very point from which it started*.

5. This point is not explicitly recognized by Rescher.
6. Nothing of course prevents the proponent and opponent in Rescher's model of formal disputation being groups of arguers, or even ideal (fictitious) arguers (Smith, 1983(a), p.13).
7. For further discussions of formal models of communication c.f. (Vaina and Hintikka (eds.), 1984).
8. I speak of an estimate here, because both the concepts of the *intensity* of a disputation, and the *comprehensiveness* of a disputation are quite vague. The best which can be achieved here, is to make these concepts less vague than they are when found in their natural environment of contemporary debates in metaphilosophy.

3. THE PROBLEMS AND PARADOXES OF COGNITIVE PROGRESS

1. STATEMENT OF THE ARGUMENT

Science, we are told, is distinguishable from other cognitive enterprises by its "progressiveness"; scientific knowledge by contrast to philosophical and theological knowledge (if these latter subjects are taken to make cognitively meaningful knowledge claims at all), exhibits "continued growth" (Popper, 1963, p. 125). Science is taken to be cumulative regardless of crisis and revolution and hence "capable of unrestricted growth towards universal coerciveness of argument and evidence" (Quay, 1974, p. 154). Few deny that science is in some sense "progressive" - even Paul Feyerabend claims that his epistemological anarchism "helps to achieve progress in any one of the senses one cares to choose" (Feyerabend, 1975, p. 27).¹ For Feyerabend it is the metascience of contemporary philosophy of science, which if consistently adhered to, would evaporate or stagnate scientific progress (Feyerabend, 1979).

Various philosophical questions can be asked about the concept of *scientific progress*. First is the *semantical* and *conceptual* question of how the expression 'scientific progress' is to be explicated. Second is the *epistemological* question of how, given an explication of the expression 'scientific progress', are we to identify progressive theories in science? What are the criteria by which progressive scientific theories are distinguished from non-progressive or degenerating scientific theories? The logical empiricist answer to these questions has been considered and rejected elsewhere (Smith, 1984)², and I shall not flog dead horses here. If the logical empiricist account of scientific progress is inadequate, then *mutatis mutandis* so must be their views of philosophical progress, for such views are premised upon their metascience (Smith, 1984, chap. 2).

Philosophical writings on the subject of cognitive progress have been concerned primarily with the phenomenon of scientific progress. This is so because science, most notably the natural sciences, have been taken as the progressive enterprise *par excellence*. However, not only are there grave philosophical and logical difficulties in extending most of the models of scientific progress to philosophy and even the social sciences, but it is no news to state that contemporary metascience is in a state of severe "epistemological crisis". This is nowhere seen more clearly in the challenges facing any realist approach to scientific and cognitive progress in arguments which establish that the notion of truth-likeness or verisimilitude is incoherent. As I wish to understand philosophical progress as consisting in the production of theories which are more verisimilar than previous theories, the problem of verisimilitude must be solved. I shall attempt to solve this problem in chapter 11. The bulk of this chapter will consist of an exposition of the inadequacies of received views on the nature of verisimilitude, and it will be argued that the principal contemporary accounts of verisimilitude suffer from major logical defects. Note that even though my principal concern in this work is with philosophical progress, this negative conclusion has immediate and vital relevance for such a concern, so that no specific examination of the applicability of any specific account of verisimilitude to any philosophical data is needed here. *It is obvious that if we cannot even offer a satisfactory explication of the concept of verisimilitude, then a realist account of cognitive progress in general, and hence of philosophical progress in particular, is logically crippled.*

It may then be argued that this is so much the worse for realist accounts of cognitive progress, and that alternative non-realist theories must be examined. I shall now argue that the leading alternative

approaches which may be made to the issue of philosophical progress are quite unacceptable. No discussion is given here of the structuralist account of progress, associated with the work of Sneed (1971), Stegmüller (1976)(1979) and others (cf. (Przelecki, 1974); (Feyerabend, 1977), (Rantala, 1978; 1980); (Dilworth, 1981, chap 11), as I have criticized this approach elsewhere, (Smith, 1984, chap, 3). The approach to cognitive progress adopted by the "Sydney neo-Althusserians" (Chalmers, 1978; 1979); (Curthoys and Suchting, 1977) is discussed elsewhere (Smith, 198+(b)).

The non-realist accounts of scientific progress which are in my assessment generalizable to the subject matter of philosophy are Lakatos' model of scientific progress, which is a part of his general methodology of scientific research programmes (Lakatos, 1970), and Laudan's theory of progress (Laudan, 1977). Both accounts of scientific progress have been strongly criticized (e.g. Lakatos is criticized by (Suppe (ed), 1977, pp. 664-670); (Laudan, 1977, pp. 77-78); (Newton-Smith, 1981, pp. 77-101); (Derr, 1981); (Stove, 1982) and Laudan by (Krips, 1980); (Newton-Smith, 1981, pp. 185-195); (Baigrie and Hattiangadi, 1981); (Doppelt, 1981); (Feyerabend, 1981)), which undermines one's confidence in these metascientific positions as even presenting a satisfactory account of scientific progress, let alone philosophical progress. It is instructive to consider here however the problems which arise in the application of Lakatos and Laudan's respective models of progress to philosophy.

According to Lakatos (1970, p. 118) a series of theories $T_1, T_2, T_3, \dots, T_n$ is said to be theoretically progressive, or to constitute a theoretically progressive problem-shift if each new theory has some excess empirical content over its predecessor by predicting

some novel, hitherto unexpected fact. If some of this excess empirical content is corroborated, then the theoretically progressive series of theories is also said to be empirically progressive or to constitute an empirically progressive problem-shift. A problem-shift is said to be progressive if it is both theoretically and empirically progressive, and degenerating if it is not. Whilst Lakatos takes his "sophisticated falsificationism" to shift the focus of theory appraisal from the single theory to a series of theories, he nevertheless accepts that a *single* theory may be falsified, so long as our appraisal recognizes that the theory is an outcome of a specific historical development (ibid., p. 116). Lakatos' theory is classified as being non-realist because it does not depend upon the notion of truth; corroboration is understood in Popper's sense (Popper, 1968, p. 251), that the theory has demonstrated its fitness to survive by standing up to critical tests.

To apply Lakatos' theory of scientific progress to philosophy, it is necessary to specify that the content of the examined theories does not involve the *prediction* of some unexpected fact, but rather the *explanation* of some problematic aspect of experience, or the solution of some problem or paradox which its predecessor does not solve. Abandoning talk of the discovery of new facts must also involve us collapsing the distinction between the theoretically progressive and empirically progressive problem-shifts for philosophical theories. I do not anticipate that this in itself will cause any significant problems.

There is however a major problem with this account of philosophical progress. In philosophy it is intuitively plausible to propose that even if philosophical theory P_2 solves philosophical problems and paradoxes which P_1 does not solve, if P_2 in turn faces conceptual or philosophical

problems (which may or may not face P_1 as well) then P_2 is unacceptable as a rational theory, even if it is progressive in Lakatos' sense. A sequence of theories $P_1, P_2, P_3 \dots P_n$ may be progressive in Lakatos' sense, even if each theory of the sequence is untenable. The point of the problem of perennial philosophical disputes is a sharp one: all philosophical theories from the perspective of at least one other theory, are taken to suffer from conceptual or logical problems. Hence if we accept the Lakatosian account of philosophical progress, we may be able to show that philosophy is a progressive enterprise, and a significantly large number (how large?) of present theories are more progressive than previous theories, but we cannot show that present theories are more rational (in an intuitively understood sense).

A further difficulty which faces the Lakatosian account of philosophical progress is that new problems which P_2 may well solve, may be taken by the proponents of P_1 as being spurious. P_2 may be a comprehensive theistic creationist view of the world, which has solved many major problems in the philosophy of religion - such as the paradox of omnipotence and the problem of evil. But proponents of a physicalist position P_p will be unimpressed. A transcendental God is not an object of their ontology and they will not regard the solution of the problems of religion as impressive conquests at all, although they may regard the failure to provide satisfactory solutions to such problems as sound points against theism. Thus it seems that the solution of excess problems by P_2 compared to P_p is not a good reason to believe that P_2 is (intuitively) more progressive than P_p . To do this we need to show that the problems solved by P_2 and not by P_p are not spurious problems or pseudo-problems. To do this requires a demonstration of the rationality of P_2 , and to the perennial debate about the existence or non-existence of God. Hence the Lakatosian theory of philosophical

progress could only give us intuitively satisfactory progress rankings, if first the problem of perennial philosophical disputes were solved.

Thus it is useless for our purposes.

Laudan (1977, p. 13) has claimed that his general model of scientific progress is applicable, with some qualifications, to all intellectual disciplines. *Progress and Its Problems* attempts to analyze scientific rationality without recourse to the concepts of truth and verisimilitude, through viewing science as a problem-solving activity. Both conceptual and empirical problems face scientific theories. In applying Laudan's model to a philosophical subject matter our interest is with conceptual problems. Conceptual problems are taken to arise for a theory T_1 in one of two ways: (a) through *internal* conceptual problems involving either the inconsistency of T_1 or the vagueness, obscurity or circularity of definition of central concepts, or (b) through external conceptual problems involving the conflict between T_1 and some other theory T_r , the latter of which is taken to be a reasonable theory (ibid., p. 49). For scientific theories, the overall problem-solving effectiveness of such theories is determined "by assessing the number and importance of the empirical problems which the theory solves and deducting therefrom the number and importance of the anomalies and conceptual problems which the theory generates" (ibid., p. 68). For philosophical theories, concerned primarily with conceptual problems, I propose that the problem-solving effectiveness of such theories be determined solely by the number of anomalies and conceptual problems that the theory solves.

On Laudan's model of philosophical progress, the problem of perennial philosophical disputes raises problems for any attempt to warrantably assert that either philosophical theories themselves, or philosophy as a discipline, is progressive. What is precisely raised by this

problem is whether a philosophical theory P_1 can be correctly said to have solved some problem P^* when there are other philosophical theories $P_2, P_3, P_4, \dots, P_n$ such that $P_1 \& P_2, P_1 \& P_3, P_1 \& P_4, \dots, P_2 \& P_3, P_2 \& P_4, \dots, P_{n-1} \& P_n$ are all contrary-conjuncts, that is for any P_i, P_k , both P_i and P_k are false, or only one of P_i, P_k are true. From the perspective of P_2, P_1 does not solve P^* at all, its problem-solving effectiveness is zero. But the same applies to any other conjunct. Generalizing for all philosophical problems $P_1^*, P_2^*, P_3^*, \dots, P_n^*, \dots$ it follows that in the light of the problem of perennial philosophical disputes, that the problem-solving effectiveness of philosophy as a discipline is zero. If Laudan's model is to give us intuitively satisfactory progress rankings, then the problem of perennial philosophical disputes must first be given a non-negative solution.

The criticisms which have been made of the considered non-realist accounts of philosophical progress render these approaches untenable in my opinion. If sense is to be made of the idea of cognitive progress, then we are best to operate with the traditional realist notion of increasing verisimilitude. The remainder of this chapter will document the equally grave difficulties facing all major theories of verisimilitude. Unless these difficulties can be resolved then it seems that this work cannot be regarded as more than an inquiry into a pseudo-problem.

2. THE PROBLEMS AND PARADOXES OF VERISIMILITUDE

A realist account of scientific and philosophical progress proposes that the answer to the semantical and conceptual question of scientific and philosophical progress is that *if* scientific and philosophical progress is taken to occur between the theories T_n and T_{n-1} , T_n is

closer to the truth than T_{n-1} .³ That is to say, T_n has a *higher verisimilitude* than T_{n-1} .⁴ This is the view of progress which I wish to defend in this work.

The idea of verisimilitude was introduced by Popper to explicate the intuitive idea, that a theory T_2 , even though it was strictly speaking false, may still be closer to the truth than a competitor T_1 . In *Conjectures and Refutations* Popper wrote (Popper, 1963, p. 235):

Ultimately, the idea of verisimilitude is most important in cases where we know that we have to work with theories which are at best approximations - that is to say, theories of which we actually know that they cannot be true. In these cases we can still speak of better or worse approximation to the truth (and we therefore do not need to interpret these cases in an instrumentalist sense).

However, as is now well known, the results of Pavel Tichý (1974), John Harris (1974) and David Miller (1974, (a);(b)), (henceforth denoted by the expression 'the Miller-Tichý-Harris Theorem') establish that a theory T_2 could be closer to 'the truth' than another theory T_1 on Popper's qualitative theory of verisimilitude only if T_2 contains no false sentences. This result has been universally taken to demonstrate the inadequacy of Popper's qualitative theory of verisimilitude.⁵

Since it has been proposed by Chris Mortensen (1978), (1983(a)), that the Miller-Tichý-Harris Theorem can be escaped while retaining Popper's original theory of qualitative verisimilitude, by modifying the classical logical base on which the results depend, a more general formulation of the Miller-Tichý-Harris Theorem is required. The present proof follows that of Mortensen (1978). Consider a first order formal system L , which may have a denumerable number of constants, predicates and variables, such that the set of wffs of L are closed under conjunction '&', disjunction ' \vee ', negation ' \sim ' and implication ' \rightarrow '. The usual syntactical

formation rules are presupposed, as is a rich metalanguage containing set-theoretical signs of a standard set-theory (e.g. Zermelo-Fraenkel set-theory). L may also contain modal operators, various types of functors and any number of special predicates - whether it does or does not will not be of interest to our argument here. The logic L_0 of L is a subset of the set of wffs of L closed under the rule of uniform substitution. If $\phi \in L_0$ then ϕ is said to be a theorem of L_0 , written as ' $\vdash_{L_0} \phi$ '. L_0 is said to be an implication logic if and only if:

$$(3-1) \quad \text{If } \vdash_{L_0} \phi \text{ and } \vdash_{L_0} \phi \rightarrow \beta, \text{ then } \vdash_{L_0} \beta.$$

L_0 is said to be an L_0 -theory relative to logic L_0 if and only if both (3-2) and (3-3) hold:

$$(3-2) \quad \text{If } A \subseteq L \text{ and } \phi \in A \text{ and } \vdash_{L_0} \phi \rightarrow \beta, \text{ then } \beta \in A.$$

$$(3-3) \quad \text{If } A \subseteq L \text{ and } \phi \in A \text{ and } \beta \in A, \text{ then } \phi \ \& \ \beta \in A.$$

If L_0 is classical logic, then A is said to be a *classical theory*.⁶ A is *inconsistent* if and only if for some ϕ , $\phi \in A$ and $\sim \phi \in A$. A is *trivially inconsistent* if and only if $A = L$. A is *incomplete* if and only if for some ϕ , both $\phi \notin A$ and $\sim \phi \notin A$ and is *complete* if and only if it is not incomplete. The rule \forall holds for A if and only if $\phi \in A$ and $\sim \phi \vee \beta \in A$, then $\beta \in A$. A is *prime* if \forall holds and *non-prime* if it does not.

Let A and B be classical L_0 theories and let T be the set of true sentences of L and F the set of false sentences of L and $T \cup F = L$. A_T is the set of true sentences of A , and B_T is the set of true sentences of B . A_F is the set of false sentences of A and B_F the set of false sentences of B . Then Popper's qualitative definition of verisimilitude is as follows:

(PQDV) A has a greater verisimilitude than B, i.e.,

$$A >_V B = \text{df. } (B_T \subset A_T) \ \& \ (A_F \subseteq B_F) \vee (B_T \subseteq A_T) \ \& \ (A_F \subset B_F).$$

The Miller-Tichý-Harris Theorem is now stated and proved:

(MTHT) If A is false (i.e. $(\exists \phi)(\phi \in A)(\phi \in F)$) then

$$\sim(A >_V B), \text{ i.e., } A >_V B \text{ then } A \subseteq T.$$

Lemma 1:

If A and B are classical L_O -theories and $\frac{|}{L_O} (a \& b) \rightarrow a$ and if $a \in F$ then $a \& b \in F$, then if $(B_T \subset A_T) \ \& \ (A_F \subseteq B_F)$ then $A \subseteq T$.

Proof of Lemma 1:

Suppose that $A \not\subseteq T$ for *reductio ad absurdum*. Let $f \in A$ and $f \in F$ and let $a \in A_T - B_T$, so that $a \in A$, $a \in T$ and $a \notin B$. Since $a \in A$ and $f \in A$, then $a \& f \in A$ as A is an L_O -theory. Since $f \in F$, $a \& f \in F$ and $a \& f \in A_F$. But since $a \notin B$, then $a \& f \notin B$ and hence $a \& f \notin B_F$. But as $a \& f \notin B_F$ and $a \& f \in A_F$, we obtain a contradiction from the assumption that $A \subseteq T$, for $(A_F \subseteq B_F) \rightarrow ((a \& f \in A_F) \rightarrow (a \& f \in B_F))$. Hence there is no $(f \in A) \ \& \ (f \in F)$. Hence $A \subseteq T$.

Lemma 2:

If A and B are classical L_O -theories and $\frac{|}{L_O} a \rightarrow (a \vee b)$; (2) \forall holds for A; (3) if $a \in T$ then $a \vee b \in T$, then if $(B_T \subseteq A_T) \ \& \ (A_F \subset B_F)$ then $A \subseteq T$.

Proof of Lemma 2:

Suppose that $A \not\subseteq T$ for *reductio ad absurdum*. Let $f \in A$ and $f \in F$. Let $b \in B_F - A_F$ so that $b \in B$, $b \in F$ and $b \notin A$. Since $b \in B$, $\forall f \vee b \in B$. Since $f \in F$, then $\forall f \vee b \in T$, hence $\forall f \vee b \in B_T$. But $b \in A$ and

$f \in A$ so $\sim f \vee b \notin A$. Hence $\sim f \vee b \notin A_T$. But $\sim f \vee b \notin A_T$ and $f \vee b \in B_T$ contradicts $B_T \subseteq A_T$. Hence the assumption that $A \not\subseteq T$ leads to contradiction and there is no $f \in A$ and $f \in F$. Hence $A \subseteq T$.

Proof of (MHT): The proof is immediate from Lemmas 1 and 2,

The result is, as I have said, devastating for Popper's original account of verisimilitude. I shall discuss in this chapter the attempts of Tichý, Tuomela and Niiniluoto, Perry, Mott, Bunge, Wójcicki, Krajewski and Rosenkrantz, Mortensen, Newton-Smith and Agassi,⁷ to avoid the difficulties facing Popper's original notion of verisimilitude. Each of these accounts, it will be argued, is seriously defective in a number of ways (many being open to straightforward counter-examples).

3. TICHÝ'S ACCOUNT OF VERISIMILITUDE

Pavel Tichý, in criticizing Popper's probabilistic theory of verisimilitude, considered an elementary weather language L_W containing no predicates and only three primitive sentences, 'it is raining', 'it is windy' and 'it is warm', abbreviated as 'p', 'q' and 'r' respectively (Tichý, 1974). Tichý proposed for a simple language like L_W based only on propositional logic, that the distance between two constituents be defined as the number of primitive sentences negated in one of the constituents, but not in the other. The verisimilitude of an arbitrary sentence 'a' can be defined as the arithmetic mean of the distances between the true constituent t and the constituents appearing in the disjunctive normal form of a .

David Miller (1974(a)), criticized Tichý's proposal for being "language-dependent". Stated more precisely, Tichý's proposed orderings

by truthlikeness can be reversed by simple linguistic reformulations.⁸ In Tichý's example, p , q and r are three independent sentences of L_W , all of which are true. There are eight maximally consistent sentences of the sentence algebra of L_W , only one of which $p \& q \& r$ is true. According to Tichý, the maximally consistent sentence $\neg p \& q \& r$ is closer to the truth than is $\neg p \& \neg q \& \neg r$. But let $d = p \leftrightarrow q$ and $e = p \leftrightarrow r$. Consider the sentence algebra of the maximally consistent sentences generated from $\{p, d, e\}$. Then the true maximally consistent sentence is $p \& d \& e$, with the first false maximally consistent sentence now as $\neg p \& \neg d \& \neg e$, and the second as $\neg p \& d \& e$, which reverses Tichý's verisimilitude ordering.

Stated more generally, Popper's definition of verisimilitude is not invariant in its orderings with respect to logically equivalent ways of representing the two theories A and B . A simple argument for this, formulated by Chris Mortensen is as follows. Let $A = \{a_1, a_1, \dots\}$, $B = \{b_1, b_2, \dots\}$, $A_1 = \{a_1, a_2, \dots\}$, $B_1 = \{b_1, b_2, \dots\}$ and let $A >_V B$ just in case $A_1 >_V B_1$. Consider the claim that if $A >_V B \rightarrow (B_T \subset A_T) \& (A_F \subseteq B_F)$. Then $A_1 >_V B_1 \rightarrow (B_{T_1} \subset A_{T_1}) \& (A_{F_1} \subseteq B_{F_1})$. If $A_1 \subseteq T$, then there exists an $f \in A_{T_1}$. Also $a_i \in A_{T-B_T}$, so that $A_1 = \{f, a_1, a_2, \dots, a_i, \dots\}$. Then $A_1^* = \{f \& a_1, a_1, a_2, \dots\}$. Hence A_1 and A_1^* are logically equivalent. But it is provable that $\neg[(B_{T_1} \subset A_{T_1}^*) \& (A_{F_1}^* \subseteq B_{F_1})]$, since by the Miller-Tichý-Harris Theorem, $f \& a_i \in A_F$ but $f \& a_i \notin B_F$. Miller is correct in my assessment in criticizing Tichý's initial proposals for their failure of invariance of verisimilitude orderings with respect to logically equivalent representations of theories. Ideally, if the concept of truth is invariant with respect to logically equivalent representations of theories, so ought the concept of verisimilitude, and an account of verisimilitude which does not preserve this intuition is to be regarded as defective.

A second line of criticism of Tichý's proposal was given by Karl Popper (1976), who presented what he took to be counter-examples to the position. Consider once more Tichý's elementary weather language. In order to determine the distance of a sentence a from the truth, i.e. $d_T(a)$, we first put a into disjunctive normal form, count the negation signs and divide by the number of conjunctive constituents of the disjunction, i.e., the number of the disjunctive signs plus 1. If we consider $a = (p \ \& \ q \ \& \ r) \vee (\sim p \ \& \ \sim q \ \& \ \sim r)$, $b = a \vee (p \ \& \ q \ \& \ \sim r)$, $c = b \vee (p \ \& \ \sim q \ \& \ r)$ and $d = c \vee (\sim p \ \& \ q \ \& \ r)$, then we obtain $d_T(a) = 1.5$, $d_T(b) = 1.3$, $d_T(c) = 1.25$ and $d_T(d) = 1.20$. The distances from the truth of these statements declines with declining logical strength, although according to Popper's intuition, they ought to increase.

Tichý's account runs into further difficulties. The sentence p is true. Hence its distance from the truth is zero. But let us suppose that $p \leftrightarrow \sim h$. Hence $\sim h$ is true and $d_T(\sim h) = 0$. But $\sim h$ is a degenerate disjunction of a degenerate conjunction. Hence $d_T(\sim h) = 1/1 = 1$. Also $p \leftrightarrow \sim^n p$, where n is an even natural number, and we may repeat this argument. It is no good arguing here that since $p \leftrightarrow \sim p$, this claim collapses, for we may simply replace $\sim p$ by \emptyset and repeat the first argument. This of course, is an example of the variance of Tichý's verisimilitude ordering with respect to the relation of logical equivalence.

Consider a statement $e = (p \ \& \ \sim p) \vee (q \ \& \ \sim q)$ which has $d_T(e) = 2/2 = 1$. We would expect given classical logical intuitions that e would be of maximal distance from the truth. However for a statement f , such that $f = (\sim p \ \& \ \sim q \ \& \ \sim r) \vee \sim(\sim p \ \& \ \sim q \ \& \ \sim r)$, $d_T(f) = 7/2$. Hence a logical truth may be further from the truth than a contradiction, which is strongly counter-intuitive. Tichý will no doubt regard logical truths as uninformative, having zero information.⁹ Nevertheless, they are true statements, and their distance from the truth must be zero. For $g = p \vee \sim p$, $d_T(g) = 1/2$ so even the distance from the truth of various

tautologies varies.

In a later paper (Tichý, 1976), Tichý responds to Miller's criticism of the failure of invariance of verisimilitude orderings by translation into logically equivalent languages (*ibid.*, p. 35). In the case of Tichý's weather language with sentences p , q , r and where $m = p \leftrightarrow q$ and $a = p \leftrightarrow r$, Tichý denies that $p \ \& \ m \ \& \ a$ and $p \ \& \ q \ \& \ r$ are equivalent theories. His counter-argument is as follows. Two statements are equivalent if they have the same affirmative force. The affirmative force of a statement is the range of a statement, i.e., the class of possible states of affairs in which the statement holds true. Two statements are equivalent just in case they have the same range. A 'possible state of affairs' is a function which maps atomic propositions to truth-values and the totality of such functions is known as the logical space of the language. Tichý claims that $\{p, q, r\}$ and $\{p, m, a\}$ are two distinct sets of propositions, so no function on the former set can be identical with one defined on the latter (*ibid.*, p. 35). Tichý's claim here is a *petitio principii* against Miller, since there is no demonstration of the claim that $\{p, m, a\}$ is in "another language". Thus whilst Miller was quite right to point out in his rejoinder to Tichý (Miller, 1976) that Tichý's argument does not show that sentences in different languages may not have the same assertive power (*ibid.*, pp. 364-365), a more direct rejoinder is in order. We need only present a formalized metalanguage for Tichý's weather language and reformulate Miller's argument in our metalanguage. Alternatively we may grant Tichý his point and simply construct another language in which $\{p, m, a\}$ does feature in our object-language. Either of these strategies will refute Tichý's later rejoinder to Miller, that Miller's argument involves a surreptitious shift from the object-language to the metalanguage of L_W (Tichý, 1978

We will now turn to a consideration of Tichý's more extensive account of verisimilitude. To do so we will first need to define a number of concepts. Consider a first-order language L^* , with a finite vocabulary V . Then a wff of L^* , w_i is said to be of depth d if its largest string of nested quantifiers (this in turn being a sequence whose every member stands within the scope of each of its predecessors) is of depth d (Hintikka, 1973). Formulas of level 0 are those atomic formulas constructible from nothing but the members of V . Atomic formulas constructible from members of V and variables $x_1, x_2, x_3, \dots, x_{\lambda+1}$, none of which are of the levels $0, 1, \dots, \lambda$, are called formulas of level $\lambda + 1$. Let $q(\lambda)$ be the number of formulas of level λ . A conjunction of level λ is any $q(\lambda)$ way conjunction whose i -th conjunct is either the i -th formula of level λ in the lexicographic ordering of formula of level λ , or its negation. The $m = 2^{q(\lambda)}$ conjunctions of level λ are referred to as $\beta_1^\lambda, \beta_2^\lambda, \dots, \beta_m^\lambda$ in lexicographic order (with the negation sign last in the alphabet).

Any formula of level d is called a d -subtree of level d . A d -subtree of level 0 is called a d -tree. Such formula are capable of being given representation in a tree-diagram. Any formula represented by a d -tree is called a d -constituent. A node of level λ of a d -tree, is any occurrence of β_i^λ in a d -tree.

Let c be a d -constituent, then the worlds in which c is true are known as c -worlds, and for any world w^* , there is a unique d -constituent c such that w^* is a c -world. If F is an arbitrary consistent formula of depth $\leq d$, then there are consistent d -constituents $c_1 \vee c_2 \vee c_3 \vee \dots \vee c_k$, which is the distributive normal form of depth d of F . F asserts in effect, that the actual world is a c_1 -world or a c_2 -world, ... or a c_k world.

The measure $\xi(n_1, n_2)$ is the number of formulas of level λ which appear unnegated in one of the nodes and negated in the other. Let c and c' be d -trees and C and C' be the respective classes of their nodes. A linkage relation L between c and c' exists if: (1) C is the domain and C' the range of L ; (2) if n_1 and n_2 are arbitrary members of C (resp. C') such that n_2 is directly subordinated to n_1 and $n_1 Ln_2$ (resp. $n Ln_1$), then there is an n_2' such that $n_2 Ln_2'$ (resp. $n_2' Ln_2$) and n_2' is directly subordinated to n_1' ; (3) there is no subrelation S of L which satisfies (i) and (ii). The *breadth* of L is the ratio of the number of actual divergences between linked nodes and the number of all possible such divergences. The distance $\delta(c, c')$ between two d -trees c and c' , is the breadth of the narrowest linkage between c and c' . If T is a consistent theory of depth less than or equal to d , and $c_1 \vee c_2 \vee \dots \vee c_k$ is the distributive normal form of depth d of T and c^* is a true d -constituent, then the d -distance $\Delta_d(T)$ of T from the truth is:

$$(TDV) \quad \sum_{i=1}^k \delta(c^*, c_i).$$

Consider a propositional language L_A with three primitive symbols h , r and w , such that h is true in the actual world if it is hot, r if it is raining and w if it is windy. Thus for $A = h \ \& \ r \ \& \ w$, $\Delta_d(A) = 0/3 = 0$. For $D = \sim h \ \& \ \sim r \ \& \ \sim w$, $\Delta_d(D) = 3/3 = 1$. The truth-likeness of a sentence ϕ , $m(\phi)$ is $1 - \Delta_d(\phi)$, so that $M(A) = 1$ and $M(D) = 0$. Now $A \vee D$ however $= (h \ \& \ r \ \& \ w) \vee (\sim h \ \& \ \sim r \ \& \ \sim w)$ and $\Delta_d(A \vee D) = 0.5$. On this account however $\Delta_d(A \vee \sim A) = 0.5$, whereas for a contingent statement $B = h \ \& \ r \ \& \ \sim w$, $\Delta_d(B) = 0.33$. This is strongly counter-intuitive. Further the measure ' Δ_d ' violates what one would take to be an intuitive criterion of adequacy for a verisimilitude ordering: if $\vdash p \rightarrow q$, then $\Delta_d(p) \leq \Delta_d(q)$. This is to say that q may be equal to, but not closer to, the truth than p . In the case of

$\vdash A \rightarrow A \vee D$, $\Delta_d(A) = 0$ and $\Delta_d(A \vee D) = 0.5$ which is satisfactory. But for $\vdash A \vee D \rightarrow (A \vee D) \vee B$, $\Delta_d(A \vee D) = 0.5$, whilst $\Delta_d(A \vee D \vee B) = 0.44$ and $0.5 > 0.44$. This is also strongly counter-intuitive.

Tichý's response to Popper's alleged counter-examples to his position (Tichý, 1978(a), p.189), indicate that he would not find that the failure of verisimilitude orderings to be invariant with respect to the relations of either logical equivalence or implication, to be at all problematic. However the example which he offers in reply to Popper, does not at all challenge our intuitions (ibid., p. 189). Tichý asks us to consider three statements, p , 'Snow is white', q , 'Grass is green', and r , 'the Moon is made of green cheese'. Then from $p \vee (q \ \& \ r)$ we can infer $p \vee q$. On my position $\Delta_d(p \vee (q \ \& \ r)) = \Delta_d((p \vee q) \ \& \ (p \vee r)) \leq \Delta_d(p \vee q)$. Now it may well be taken to be counter-intuitive to claim that $\Delta_d(p \vee (q \ \& \ r)) < \Delta_d(p \vee q)$, but the claim that $\Delta_d(p \vee (q \ \& \ r)) = \Delta_d(p \vee q)$ is not. In the propositional logic, if p and q are true, then the falsity of r makes *no difference* to the truth value of the whole disjunct, and if this is so, it is quite plausible to claim that under these conditions $\Delta_d(p \vee (q \ \& \ r)) = \Delta_d(p \vee q)$.

It is concluded that Tichý's position stands open to a number of counter-examples, which indicate the inadequacy of his position.

4. TUOMELA AND NIINILUOTO ON VERISIMILITUDE

The approaches to the problem of explicating the notion of verisimilitude adopted both by Ikka Niiniluoto (1978(a); 1978(b); 1982) and Raimo Tuomela (1978(a); 1978(b)), make use of Hintikka's notion of constituents to define a quantitative distance between constituents, which is used in turn to define the notion of verisimilitude. These approaches will now be outlined.

Consider a first order language L_{TN} with a finite vocabulary, but no individual constants. Each generalization g in L_{TN} can be expressed as a finite disjunction of mutually exclusive constituents at depth d . L_{TN} is a monadic language with logically independent primitive predicates O_1, O_2, \dots, O_k . Constituents C_i of L_{TN} are expressions of the following form:

$$(3-4) \quad C_i = (\overset{\pm}{\neg})(\exists x) Ct_1(x) \ \&\dots\& \ (\exists x) Ct_k(x),$$

where ' $\overset{\pm}{\neg}$ ' is to be replaced by the negation sign (\neg) or else by no sign at all, and where $K = 2^k$. The constituents are sentences which claim that certain Q -predicates are empty, whilst others are non-empty.

The Ct -predicates of (3-4) are conjunctions of the form:

$$(3-5) \quad Ct_j(x) = (\overset{\pm}{\neg})O_1(x) \ \&\dots\& \ (\overset{\pm}{\neg})O_k(x).$$

Assume that the language L_{TN} is interpreted in a domain D_* , which represents the actual world. Then only one of the constituents C_i is true in D_* ; let this constituent be C_* .

Niiniluoto defines the *Clifford-measure* (cf (Jevons, 1958, pp. 143-145)) for the distance between monadic constituents C_i and C_j as follows:

$$(3-6) \quad d_c(C_i, C_j) = \frac{|CT_i \Delta CT_j|}{K}$$

where ' Δ ' denotes the relation of symmetric difference, ' K ' is the total number of Q -predicates in L_{TN} , ' CT_i ' denotes the set of C_i 's Q -predicates, and ' CT_j ' denotes the set of C_j 's Q -predicates. Given the distance $d_c(C_i, C_j)$ between two constituents C_i and C_j , the distance of a generalization g from C_* is $d_c(g, C_*)$ and the truthlikeness or verisimilitude $M(g, C_*)$ of g is $1 - d_c(g, C_*)$.

If we take account of the errors made by C_i with respect to C_j we have:

$$(3-7) \quad M(g, C_*) = 1 - (\gamma_{m_*}(g, C_*) + (1 - \gamma) m^*(g, C_*))$$

where ' $1 - m_*(g, C_*)$ ' denotes the measure relative to C_* of the degree of truth in g , ' $1 - m^*(g, C_*)$ ' denotes the measure of the degree of information about the truth in g , and γ is a weight parameter for these two factors, such that $0 < \gamma < 1$.

To outline Tuomela's position, we will still operate with the language L_{TN} . Consider two sentences $T_1 = C_1 \vee C_2 \vee \dots \vee C_{n_1}$ and $T_2 = C_1 \vee C_2 \vee \dots \vee C_{n_2}$. Then $d_c(T_1, T_2)$ is defined as follows:

$$\begin{aligned}
 (3-8) \quad d_c(T_1, T_2) &= \gamma \frac{1}{K} \text{wcard}((CT_1 \Delta CT_2)) + \\
 &\alpha_1 \left(\frac{1}{2} \left(\frac{1}{mn} \sum_{i,j=1}^{mn} d_c(CT_i, CT_j) + \frac{1}{rs} \sum_{i,j=1}^{r,s} d_c(CT_i, CT_j) \right) \right) + \\
 &\alpha_2 \left(\frac{1}{2} \left(\frac{1}{k\ell} \sum_{i,j=1}^{k,\ell} d_c(CT_i, CT_j) + \frac{1}{t\nu} \sum_{i,j=1}^{t,\nu} d_c(CT_i, CT_j) \right) \right) + \\
 &\alpha_3 \left(\beta \frac{\text{wcard}(CT_1 \Delta CT_2)}{\text{wcard}(CT_1 \cup CT_2)} + (1-\beta) \frac{\text{wcard}(CT'_1 \Delta CT'_2)}{\text{wcard}(CT'_1 \cup CT'_2)} \right)
 \end{aligned}$$

In (3-8) 'wcard' means 'weighted cardinality', these being weights associated with each Ct predicate, representing the importance of that Ct-predicate for theory distance, and summing up to K ; ' γ ' is the name of a parameter $0 \leq \gamma \leq 1$ reflecting the relative importance of the whole error-factor in the characterization of theory-distance. The parameters sum up to one, i.e., $\gamma + \alpha_1 + \alpha_2 + \alpha_3 = 1$, and $0 \leq \beta \leq 1$. In (3-8) m is the number of Ct-predicates in $CT_1 \sim CT_2$, $\frac{n}{1}$ the number in CT_2 , and r and s those in $CT_2 \sim CT_1$ and CT_1 respectively; k, ℓ, t and ν give the cardinalities of $CT'_1 \sim CT'_2$, CT'_2 , $CT'_2 \sim CT'_1$ and CT'_1 respectively. The distance $d_c(CT_c, CT_j) = 1/k$ consists of a weighted number of predicates $O_i, i=1, \dots, k$ which have a different sign in CT_i and CT_j , assigned on the basis of the importance of O_i for the comparison of distance. The concept of verisimilitude is defined as follows:

$$(3-9) \quad v(T^{(d)}) = 1 - d_c(T^{(d)}, C_t^{(d)}),$$

where $C_t^{(d)}$ is the constituent of L_{TN} at depth d representing the truth.

The proposals of Niiniluoto and Tuomela are based upon the intuition that the truthlikeness of some theory can be determined by distance comparisons *within* the theory. However the language-relativity (which is fully conceded by Niiniluoto (1978(a), p. 255) renders these accounts less than adequate. Let L_1 and L_2 be two essentially different first-order languages, and let C_*^1 and C_*^2 be the true constituents of L_1 and L_2 respectively. If g and g' are generalizations in L_1 and L_2 respectively, then it is possible that $M(g, C_*^1) < M(g', C_*^1)$, even if $M(g, C_*^2) > M(g', C_*^2)$. If g and g' respectively, are not expressed in L_2 and L_1 respectively, then it is impossible for distance comparisons to be made at all in any non-problematic fashion. The suggestion that we consider a common extension of L_1 and L_2 , namely L_3 with vocabulary $\lambda_3 = \lambda_1 \cup \lambda_2$ also meets difficulties, if T_1 and T_2 are conflicting and mutually inconsistent, for either we would outrightly fail to obtain a true constituent C_* , or else the overall verisimilitude of T_3 will be quite low due to the large numbers of *inconsistent* sentences in it. Any satisfactory account of verisimilitude should avoid this problem: Tuomela and Niiniluoto's accounts do not.

To conclude this section, I shall advance some counter-examples to the accounts of Tuomela and Niiniluoto. Let us consider the situation where $\forall = 1$ and $\alpha_1 = \alpha_2 = \alpha_3 = 0$ in Tuomela's model. Then his measure of theory distance is equivalent to Niiniluoto's. Consider a simple language L_s , which has only 2 predicates $\{O_1, O_2\}$ in its vocabulary, and one variable, x . Then the Q -predicates or CT-predicates will be:

$$(3-10) \quad Ct_1(x) = (\pm)O_1(x)$$

$$Ct_2(x) = (\pm)O_1(x) \ \& \ (\pm)O_2(x).$$

The constituents of L_s are expressions of the form:

$$(3-11) \quad C_i = (\pm)(\exists x) (\pm)O_1(x) \ \& \ (\pm)(\exists x) [(\pm)O_1(x) \ \& \ (\pm)O_2(x)].$$

Consider $C_* = (\exists x)O_1(x) \ \& \ (\exists x)[O_1(x) \ \& \ O_2(x)]$, which is taken to be true. Then $d_i(C_*, C_*) = |CT_* \Delta CT_*|/K$. Now CT_* is the set of Ct_* -predicates of C_* which is $\{O_1(x), O_1(x) \ \& \ O_2(x)\}$. Now $CT_* \cap CT_* = \{O_1(x), O_1(x) \ \& \ O_2(x)\}$ and $(CT_* \cap CT_*)^c = \{\emptyset\}$, i.e. $CT_* \Delta CT_* = \{\emptyset\}$ and $|CT_* \Delta CT_*| = 0$. Hence $d_i(C_*, C_*) = 0$. Thus $M(C_*, C_*) = 1 - 0 = 1$. This is as we would expect. But for a counter-example we need only consider a C_i such that CT_i has the same elements as CT_* , e.g. $C_i = \sim(\exists x)O_1(x) \ \& \ \sim(\exists x)[O_1(x) \ \& \ O_2(x)]$. C_i and C_* differ only by virtue of the external negation signs, although their Q -predicates are identical. Hence $|CT_i \Delta CT_*| = 0$, and $d_i(C_i, C_*) = 0$ and $M(C_i, C_*) = 1$. This is strongly counter-intuitive, and a defect in the proposal.

5. VERISIMILITUDE AND SHARED TESTS

Clifton Perry (1982) has argued that the Miller-Tichý-Harris Theorem may be questioned if the claim that the comparability of truth and falsity contents with respect to empirical content is in turn questioned. If the comparisons between two theories with respect to a given variable is to be meaningful, then both theories must be measured in terms of that variable. He continues (*ibid.*, pp. 608-609):

It may therefore be suggested that although competing theories may be commensurate, it does not follow that, taken as a whole, the truth and falsity contents of a theory with greater empirical content are comparable to the truth and falsity contents of a theory with less empirical content. Insofar as the theory with greater empirical content has potentially more true or false logical consequences which are not also consequences of

the theory with less empirical content, reference to differences in empirical content between theories in the ascription of verisimilitude would fail to compare adequately only those consequences which were relevant to both theories. Reference to the differences in empirical content in the ascription of verisimilitude to two theories shall be referred to as ascriptions of 'absolute' verisimilitude. Appraisals of verisimilitude which are restricted to those test situations which are applicable to both theories in question and consequently obviate reference to the different degrees of empirical content in the comparison of truth and falsity contents shall be referred to as ascriptions of 'relative' verisimilitude. It may be said, therefore, that theory B possesses more relative verisimilitude than theory A if and only if B's falsity content is a sub-set of A's falsity content, A's truth content consequently being a sub-set of B's truth content. The above formulation differs from Popper's in that reference to the difference in empirical content in the comparison of truth and falsity contents is omitted.

Following the conventions adopted earlier in the paper, we shall represent Perry's definition as follows:

$$(PDRV) \quad A >_{RV} B = \text{df } (A_F \subset B_F) \ \& \ (B_T \subset A_T).$$

This weakened view of verisimilitude is also subject to a variant of the Miller-Tichý-Harris Theorem:

$$(MTHT)* \quad A >_{RV} B \rightarrow A \subset T.$$

Lemma 1: $(A_F \subset B_F) \rightarrow A \subset T.$

Proof of Lemma 1: Consider $A_F \subset B_F$ and suppose for *reductio ad absurdum* that $A \not\subset T$. Let $f \in A$ and $f \in F$. Let $a \in A_T - B_T$ so that $a \in A$, $a \in T$ and $a \notin B$. Since $a \in A$ and $f \in A$, then $a \ \& \ f \in A$. Also $f \in F$, so $a \ \& \ f \in F$. Hence $a \ \& \ f \in A_F$. Now since $a \notin B$, then $a \ \& \ f \notin B$. Hence $a \ \& \ f \notin B_F$. But $a \ \& \ f \in A_F$ and $a \ \& \ f \notin B_F$ contradict $A_F \subset B_F$. Hence $\sim(A \not\subset T)$, i.e. $A \subset T$.

Lemma 2: $(B_T \subset A_T) \rightarrow A \subset T.$

Proof of Lemma 2: Consider $B_T \subset A_T$ and suppose for *reductio ad absurdum* that $A \not\subset T$. Let $f \in A$ and $f \notin T$. Consider $b \in B_T - A_T$, $b \in B$, $b \in F$ and $b \notin A$. Since $b \in B$, $\sim f \vee b \in B$. Since $f \in F$, $\sim f \in T$, so $\sim f \vee b \in T$, so $\sim f \vee b \in B_T$. But $\sim f \vee b \notin A_T$, as $b \notin A$, but $f \in A$, so $\sim f \vee b \in A$. Now $\sim f \vee b \in B_T$ and $\sim f \vee b \notin A_T$ contradicts $B_T \subset A_T$. Hence $\sim(A \not\subset T)$. Therefore $A \subset T$.

(MTHT)* follows immediately from Lemmas 1 and 2. Perry's definition does not escape a variant of the Miller-Tichý-Harris Theorem. It too is inadequate.

6. VERISIMILITUDE AND SHORT THEOREMS

Peter Mott (1978) has proposed that since the Miller-Tichý-Harris Theorem relies upon sentences that are not 'genuine' theorems in the sense of being characteristic of the theory, then this limitation theorem may be avoided by use of the notion of 'short theorems'. The notion is based upon the idea of *organicity*: an axiom is organic with respect to a first-order system X if it contains no segment which is in turn a theorem of X , or becomes a theorem of X as soon as open variables are bound by any type of quantifier so that a wff of X is produced (Sobociński, 1955-56, p. 65). Mott however offers the following definition of shortness: "a disjunction of prime formulas, A , is short in X produced there is no disjunction A' in X obtained from A by replacing throughout, P by $\sim P$ or $\sim P$ by P " (Mott, 1978, p. 256).

There are in the first-order system X denumerably many sentence letters P_1, \dots, P_m, \dots and the usual logical connectives. For any sentence letter P_i , P_i and $\sim P_i$ are prime formulas in P_i . The verisimilitude of Y

is greater than X just when it preserves the short truths of X and adds new short truths, i.e. $X <_V Y$ iff $(X_{ST} \subseteq Y) \ \& \ \sim(Y_{ST} \subseteq X)$.

Mott recognizes that his account of verisimilitude only preserves transitivity orderings for a set of theories S which is a chain with respect to the relation of \subseteq . On this failure for sets of theories S* which are not chains, but may well be given an intuitively correct verisimilitude ordering he states (ibid., p. 263):

Intuition would have it that verisimilitude is transitive - but then intuition might be conditioned by nothing more substantial than the phrase 'nearer the truth'. On reflection there seems to be no prima facie reason why verisimilitude should not be more like '... is indistinguishable from ...' than '... is identical to ...'. Perhaps as theories evolve they gradually drift apart, so that though each improved upon its predecessor, the last is not comparable with the first. Perhaps an early cosmology might contain all mixed up together religious and cosmological truths. Later theories may preserve the secular while forgetting the divine insights. The very subject matter of the theories may gradually drift finally rendering the first and the last about almost entirely different things, though each handles the problems, or most of the problems of its predecessor. In sum, it may be that there are decisive arguments to show that verisimilitude is transitive, but if so they are not known to the writer .

Such an argument for the plausibility of taking the relation of verisimilitude to be transitive may be either based on the transitivity of truth assessments, or else upon considering the transitivity of Popper's original definition of verisimilitude. Insofar as this account captures many of our intuitive beliefs about verisimilitude (not withstanding of course the Miller-Tichý-Harris Theorem), the transitivity of verisimilitude orderings is quite marked. If $(A >_V B) \ \& \ (B >_V C)$, then it is provable that $A >_V C$ If we expand out ' $(A >_V B) \ \& \ (B >_V C)$ ', we obtain:

$$(3-12) \left\{ (B_T \subset A_T) \ \& \ (A_F \subseteq B_F) \vee (B_T \subseteq A_T) \ \& \ (A_F \subset B_F) \right\} \\ \ \& \ \left\{ (C_T \subset B_T) \ \& \ (B_F \subseteq C_F) \vee (C_T \subseteq B_T) \ \& \ (B_F \subset C_F) \right\}$$

which by use of distribution rules gives:

$$\begin{aligned}
 (3-13) \{ & (B_T \subset A_T) \ \& \ (A_F \subseteq B_F) \ \& \ (C_T \subset B_T) \ \& \ (B_F \subseteq C_F) \} \\
 & \vee \{ (B_T \subseteq A_T) \ \& \ (A_F \subset B_F) \ \& \ (C_T \subset B_T) \ \& \ (B_F \subseteq C_F) \} \\
 & \vee \{ (B_T \subseteq A_T) \ \& \ (A_F \subseteq B_F) \ \& \ (C_T \subseteq B_T) \ \& \ (B_F \subset C_F) \} \\
 & \vee \{ (B_T \subseteq A_T) \ \& \ (A_F \subset B_F) \ \& \ (C_T \subseteq B_T) \ \& \ (B_F \subset C_F) \}.
 \end{aligned}$$

Now (3-13) can be readily shown to imply:

$$(3-14) (C_T \subset A_T) \ \& \ (A_F \subseteq C_F) \ \vee \ (C_T \subseteq A_T) \ \& \ (A_F \subset C_F)$$

i.e. $A >_{\vee} C$. Hence Popper's original verisimilitude relationship is transitive. We should note that such a relationship concerns the relationship between the sets of true and false consequences of theories. Mott's remarks about cosmological theories "drifting apart" will either mean that many of the religious claims of the early cosmology will be taken to be false (e.g. the world was created less than 10,000 years ago) or else the two cosmologies cannot be compared. Mott requires that sub-set relationships still hold between the respective sets of short theorems, but if the theories are *incomparable*, then this relationship will not hold i.e. Y will not preserve some of the 'short religious truths of X' ' as such truths will not be theorems of Y . In conclusion, Mott's rejection of the transitivity of verisimilitude is not supported by a satisfactory argument, and this is all the worse for his account of verisimilitude.

Mott's account of verisimilitude also fails to avoid one horn of the Miller-Tichý-Harris Theorem, this being the claim that if $(B_T \subset A_T) \ \& \ (A_F \subseteq B_F)$ then $A \subseteq T$. The proof of this can be readily given by taking a $\in A_T - B_T$ and $f \in A$ and $f \in F$. The problem is that $a \ \& \ f \in A_F$ but $a \ \& \ f \notin B_F$ which contradicts $A_F \subseteq B_F$. We can readily let a be a short theorem. As long as $f \in A$ and $f \in F$ this result will follow. Mott's

original motivation for his account was based upon the view that if $b \in X$, then $\neg a \vee b$ is logical baggage. This is of course a product of the rule of inference $a \rightarrow a \vee b$. But Mott's claim ignores one part of the Miller-Tichý-Harris Theorem as it has been stated and proved here, where reliance is placed upon $a \& b \rightarrow a$. Since f is not a short truth of A as $f \notin F$, Mott's programme is beside the point. It fails to resolve the problem posed by the Miller-Tichý-Harris Theorem.

7. BUNGE'S THEORY OF PARTIAL TRUTH

Mario Bunge has argued that the notion of 'degree of truth' is extensively employed in applied mathematics and factual science (Bunge, 1963, 1974). David Miller (1977(c)) has established the untenability of Bunge's earlier account. After briefly reviewing Miller's criticism of Bunge's earlier position, his more recent theory of partial truth shall be reviewed, criticized and rejected.

Bunge writes ' $V(p) = r$ ' for the degree of truth of the proposition p ; if p is true, then $V(p) = 1$, if p is false $V(p) = -1$ and if p is either meaningless or undecidable, $V(p) = 0$.³⁸ V is, we shall suppose, a real valued function satisfying the following axioms:

$$(A1) \quad -1 \leq V(p) \leq 1.$$

$$(A2) \quad V(\sim p) = -(V(p))$$

$$(A3) \quad V(p \& q) = \begin{cases} 0 & \text{if } V(p) = -V(q) = 0 \\ -1 & \text{if } V(p) = -V(q) \neq 0 \\ \frac{V^3(p) + V^3(q)}{V^2(p) + V^2(q)} & \text{if } V(p) \neq -V(q) \end{cases}$$

On this account if t is a tautology, $V(t) = 1$. Consider now $V(p \& t)$, which should equal $V(p)$. If $V(p) \neq -1$, then

$$(B1) \quad V(p \& t) = V(p) = \frac{V^3(p) + 1}{V^2(p) + 1}$$

(B1) is satisfied only if $V(p) = 1$. Hence if $V(p) \neq -1$, then $V(p) = 1$. Hence Bunge's theory of partial truth only permits at most two degrees of truth.

Bunge also sets out some presystematic ideas, which play the role of desiderata to be fulfilled by the theory of partial truth. Among these are:

(b) If $p \leftrightarrow q$ then $V(p \& q) = V(p \vee q) = V(p)$.

(c) $V(p \& \sim p) = -1$, $V(p \vee \sim p) = 1$.

Consider the following contradiction $p \leftrightarrow \sim p$, which should receive value -1 . Then by (b) above $V(p \& \sim p) = V(p \vee \sim p) = V(p)$. But $V(p)$ may well be in the range $0 < V(p) \leq 1$ or $-1 < V(p) < 0$. Consider also a conditional $p \leftrightarrow p$ such that $V(p \leftrightarrow p) = 1$. Then by (b) above, $V(p \leftrightarrow p) = V(p \& p) = V(p \vee p) = V(p)$. Now $V(p \leftrightarrow p)$ is surely 1, but $V(p)$ need not be 1 at all. Compare these results with the results which can be obtained from Bunge's Theorem 3, which asserts that:

$$(T3) \quad V(p \leftrightarrow q) = \begin{cases} 1 & \text{if } V(p) = V(q) \\ 0 & \text{otherwise} \end{cases}$$

Here $V(p \leftrightarrow \sim p) = 0$, which is the claim that a genuine contradiction is meaningless or undecidable. This is also strongly counter-intuitive.

We will now review Bunge's more recent theory of partial truth. Consider the structure $B = \langle S, S_D, [S], \square, \sqsupset, \cup, \cap, -, v \rangle$, where S is a non-empty set, S_D a subset of S , $[S]$ the quotient of S by the relation \leftrightarrow of logical equivalence, \square and \sqsupset distinguished elements of $[S]$, \cup and \cap binary operations on $[S]$, $-$ an unary operation on $[S]$ and v a value function on S_D . B is a metric Boolean algebra of statements if and only if V is a Boolean algebra with null element \square , universal element \sqsupset , and v is a real valued function on $S_D \subset S$, such that for any elements p and q of S_D :

- (3-15) (a) $V(p \& q) + V(p \vee q) = V(p) + V(q)$
 (b) $V(p) = 0$ for all $p \in \square$
 (c) $V(p) = 1$ for all $p \in \sqsupset$

Further, for any p, q, r in S :

- (3-16) $[\bar{q}] = [p]$ iff $\lceil p \leftrightarrow q \rceil$ is a tautology
 $[q] \cup [r] = [p]$ iff $\lceil p \leftrightarrow q \vee r \rceil$ is a tautology
 $[q] \cap [r] = [p]$ iff $\lceil p \leftrightarrow q \& r \rceil$ is a tautology,

and:

- (3-17) $\square = \{(p \in S) \& (q \in S) \mid p \leftrightarrow q \& \sim q\}$
 $\sqsupset = \{(p \in S) \& (q \in S) \mid p \leftrightarrow q \vee \sim q\}$

The function $\delta_- : S_D \times S_D \rightarrow [0, 1]$ assigns to each pair of propositions $p, q \in S_D$ a real number between 0 and 1, such that $\delta_-(p, q) = |V(p) - V(q)|$; this is to be known as the *horizontal distance*. The distance function δ_- satisfies the following axioms:

- (3-18) (1) $\delta_-(p, q) = \delta_-(q, p)$
 (2) $\delta_-(p, q) + \delta_-(q, r) \geq \delta_-(p, r)$
 (3) $\delta_-(p, q) = 0$ iff $V(p) = V(q)$, for any p, q and r in S_D .

δ_- defines a topology in the space S_D . An open ϵ -neighbourhood of $p \in S_D$ is the set:

$$(3-19) \mathcal{U}_\epsilon(p) = \{q \in S_D \mid |V(p) - V(q)| < \epsilon\} \text{ for } 0 \leq \epsilon \leq 1.$$

All statements that agree with p to within the tolerance error ϵ are in $\mathcal{U}_\epsilon(p)$. As the distance between equivalent statements is nil, equivalent statements agree with one another.

A second distance function $\delta_1 : S_D \times S_D \rightarrow [0, 1]$, which to each pair of propositions $p, q \in S_D$ is assigned a real number between 0 and 1 such

that $\delta_1(p, q) = |V(p \vee q) - V(p \& q)|$, is known as the *vertical distance*. A second topology in S_D is defined such that an open ϵ -neighbourhood of $p \in S_D$ is now:

$$(3-20) \quad U_\epsilon(p) = \left\{ q \in S_D \mid \delta_1(p, q) < \epsilon \right\} \text{ for } 0 \leq \epsilon \leq 1.$$

The two truth spaces $\langle S_D, \delta_- \rangle$ and $\langle S_D, \delta_1 \rangle$ are *separable*, that is for any two propositions $p, q \in S_D$, there are open sets G and H in S_D such that p is in G and q is in H , and G and H are disjoint. Further:

$$(T1) \quad \text{If } p, q \in S_D, \text{ then } \delta_1(p, q) \geq \delta_-(p, q).$$

A number of consequences may be derived from these assumptions:

(1) for any $p, q \in S_D$, $V(p \vee q) \geq V(p \& q)$; (2) for any $p \in S_D$, $V(\neg p) = 1 - V(p)$; (3) for any $p, q \in S_D$, if $V(p \leftrightarrow q) = 1$, then $V(p) = V(q)$. Result (3) is not, Bunge claims, restricted to formally true biconditionals.

The following theorem is also of importance:

$$(T2) \quad \text{For any } p, q \in S_D, \text{ if } V(p \rightarrow q) = 1, \text{ then:}$$

$$(1) \quad V(p \& q) = V(p)$$

$$(2) \quad V(p \vee q) = V(q).$$

Let $p, q \in S_D$ with $V(p) \neq 0$. Then the truth-value of q relative to p is defined as:

$$(3-21) \quad V(p/q) = \frac{V(p \& q)}{V(p)}.$$

It is said that p is *alethically independent* of q if and only if $V(q/p) = V(p)$, and is *alethically dependent* upon q otherwise. Alethic dependence subsumes logical dependence. Note that since $V(p) \neq 0$, the truth-value of q relative to a contradiction cannot be made; this is inevitable given the mathematical form of Bunge's definition.

A further theorem is of importance:

(T3) If p and q are alethically independent statements in S_D then:

$$(1) V(p \ \& \ q) = V(p) \cdot V(q).$$

$$(2) V(p \ \vee \ q) = V(p) + V(q) - V(p) \cdot V(q).$$

With this logic machinery Bunge proceeds to define the degree of truth of a scientific theory: the degree of its initial assumptions, provided that these are independent. If T is a scientific theory with n independent assumptions A_i , then: (1) the degree of truth of the axiom base equals the product of the partial degrees of truth:

$$(3-22) \quad V \left(\bigwedge_{i=1}^n A_i \right) = \prod_{i=1}^n V(A_i);$$

(2) the degree of truth of an assumption conjoined with any of its logical consequences equals the former:

$$(3-23) \text{ If } A_i \vdash t, \text{ then } V(A_i \ \& \ t) = V(A_i).$$

On this basis, the concept of verisimilitude may be explicated as follows: theory T_2 has a greater verisimilitude than the theory T_1 if and only if the degree of truth of T_2 is greater than the degree of truth of T_1 .

I shall now outline some logical defects in Bunge's position. First however let us note that Bunge claims that $V(p) = 0$ for all $p \in \square$, such that p is a contradiction, and $V(p) = 1$ for all $p \in \square$, such that p is a tautology. However truth-values $V(p)$ are in the real interval $[0, 1]$. This leaves Bunge with the option of either stating that both truths and tautologies have value 1, and that both falsehoods and contradictions have value 0, or else to arbitrarily assign complete truth and complete falsity to some values in the interval $[0, 1]$. In either case, in what follows, let us agree that a contradiction has value 0, and a tautology has value 1.

Bunge claims that if $V(p \leftrightarrow q) = 1$, then $V(p) = V(q)$. Consider $V(p \leftrightarrow p) = 1$, and $V(p) = 1/2$ as p is a contingent partial truth. But also

$V(p \leftrightarrow (p \leftrightarrow p)) = 1$. Now $V(p) = 1/2$ from assumption and $V(p \leftrightarrow p) = 1$. But given that $V(p \leftrightarrow (p \leftrightarrow p)) = 1$, $V(p) \neq V(p \leftrightarrow p)$ as $1/2 \neq 1$. It is difficult to see how this result could be avoided since Bunge claims that his account is not merely restricted to formally true biconditionals.

A second defect is with T. Consider the claim:

$$(3-24) \text{ For any } p, q \in S_D \text{ if } V(p \rightarrow q) = 1, \text{ then } V(p \ \& \ q) = V(p).$$

For a counter-example consider the classical tautology, $p \ \& \ \sim p \rightarrow q$. Then $V(p \ \& \ \sim p \rightarrow q) = 1$. Consider $V(p) = 1/2$, so that $V(\sim p) = 1/2$. Now p and $\sim p$ are alethically independent statements if the logic of the system containing them is consistent. Hence $V(p \ \& \ \sim p) = V(p) \ \& \ V(\sim p) = 1/2 \cdot 1/2 = 1/4$. But since $p \ \& \ \sim p$ is a classical contradiction $V(p \ \& \ \sim p) = 0$. Hence $0 = 1/4$, which is absurd. Therefore Bunge's principle is absurd.

A parallel difficulty arises with the claim that for any $p, q \in S_D$ if $V(p \rightarrow q) = 1$, then $V(p \vee q) = V(q)$. Consider the classical tautology $q \rightarrow p \vee \sim p$, such that $V(q \rightarrow p \vee \sim p) = 1$. Then $V(q \rightarrow (p \vee \sim p)) = V(p \vee \sim p)$. We consider now $V(p \vee \sim p)$ where p and $\sim p$ are alethically independent and $V(p) = 1/2$ and $V(\sim p) = 1/2$. Then: $V(p \vee \sim p) = V(p) + V(\sim p) - V(p) \cdot V(\sim p) = 1/2 + 1/2 - 1/2 \cdot 1/2 = 1 - 1/4 = 3/4 \neq 1$. Once more we obtain a contradiction.

According to Bunge, if p and q are alethically independent statements in S_D , then:

$$(3-25) \quad V(p \ \& \ q) = V(p) \cdot V(q).$$

Substitute ' $\sim p$ ' for ' q ' in (25) to obtain:

$$(3-26) \quad V(p \ \& \ \sim p) = V(p) \cdot V(\sim p)$$

Since $V(\sim p) = 1 - V(p)$, we obtain:

$$(3-27) \quad V(p \ \& \ \sim p) = V(p) \cdot (1 - V(p)) = V(p) - V^2(p),$$

Now $V(p \ \& \ \sim p)$ we assume to be 0. However consider $V(p) = 1/2$. Then by

$$(3-27) \quad V(p \ \& \ \sim p) = 1/2 - (1/2)^2 = 1/4 \neq 0.$$

Finally, according to Bunge, if p and q are alethically independent statements in S_D , then:

$$(3-28) \quad V(p \vee q) = V(p) + V(q) - V(p) \cdot V(q).$$

Consider a tautology $V(p \vee \sim p) = 1$. Suppose $V(p) = 1/2$. Then as $V(\sim p) = 1 - V(p)$, $V(\sim p) = 1/2$. By (3-28), $V(p \vee \sim p) = V(p) + V(\sim p) - V(p) \cdot V(\sim p)$.
 $V(\sim p) = 1 - 1/4 = 3/4!$

These results indicate that Bunge's theory of partial truth is badly inconsistent. Therefore the position is severely defective.

8. KRAJEWSKI AND ROSENKRANTZ ON VERISIMILITUDE

Krajewski (1977; 1978) distinguishes between relative and absolute truths. Qualitative facts include event-facts (e.g. World War II occurred), facts about states of affairs (e.g. people die without food) and relational facts (e.g. a left hand is on the left side of a human body). Qualitative factual statements are not relatively true or false, but are either absolutely true, or absolutely false. Krajewski also recognizes other absolutely true statement types, including *existential statements* (e.g. there is more than one object in the world) and *qualitative law statements* (e.g. all metals are good electrical conductors). By contrast many other statements such as the statement of the gas laws and cosmological statements about recession velocities are only 'approximate', holding only within some margin of error. Krajewski attempts an explication of the notion of approximate or relative truth by means of the notion of relative errors. The mechanics of this account shall now be examined.

Let p be a quantitative fact-statement, and the truth-content of p be written ' $\text{Tr}(p)$ ', and let the relative error made by p be $E(p)$. Then:

$$(K1) \quad \text{Tr}(p) = 1 - E(p).$$

$E(p)$ is never known exactly, but the possible maximal error on the basis of a given measurement usually is. If a_1 is the result of such a measurement, and Δa is the maximal absolute error, then $E(p) = \Delta a/a_1$.

The case of defining $\text{Tr}(L)$ for a quantitative law L is more complex. The *degree of inadequateness* (DI) of a law L with respect to a parameter B contained in it, is equal to the Supremum of relative errors made by using L to predict values b_i of B . Let ' $\text{DI}_{B_i}(L)$ ' designate the DI of L with respect to B , and ' $E_{b_i}(L)$ ' designate the relative error made in the prediction of b_i of B , then:

$$(K2) \quad \text{DI}_B(L) = \text{Sup}_{b_i} [E_{b_i}(L)]$$

and the truth-content of L with respect to a parameter A , $\text{TrC}_A(L)$ is:

$$(K3) \quad \text{TrC}_A(L) = 1 - \text{DI}_A(L).$$

Let ' j ' designate any of the parameters contained in L , then:

$$(K4) \quad \begin{aligned} \text{TrC}(L) &= \text{Min}_j [\text{TrC}_j(L)] \\ &= 1 - \text{Max}_j \{ \text{Sup}_i [E_{j_i}(L)] \}. \end{aligned}$$

Finally, if a theory is held to be a conjunction of law-statements, then the truth-content of a theory T may be defined as the minimum of the truth-content of all laws L_i contained in it:

$$(K5) \quad \text{TrC}(L) = \text{Min}_i \text{TrC}(L_i).$$

Krajewski takes it for granted in his account, that all the inaccuracies of scientific theories are due to experimental errors. This project has some rather unacceptable counter-intuitive consequences if his proposals are taken to present a general theory of verisimilitude. These shall now be detailed.

Whilst Krajewski only defines the concept of truth-content of a single factual-statement p and law L , a natural extension of this idea

is to suppose that if some theory $T_1 = \{p_1, p_2, \dots, p_n\}$ where ' p_1 ', ' p_2 ', ... ' p_n ' denote the logical consequences of T_1 , then the truth-content of T_1 is as follows:

$$(K6)* \quad \text{TrC}(T_1) = \sum_{i=1}^n \text{TrC}(p_i) = \sum_{i=1}^n (1 - E(p_i)).$$

Now if $T_2 = \{q_1, q_2, \dots, q_r\}$, then $\text{TrC}(T_2) = \sum_{i=1}^r (1 - E(q_i))$. Suppose that $r \gg n$. Then it is possible for a theory T_1 , which had only absolutely true consequences, to nevertheless be of less truth-content than a theory T_2 . This would mean that the truth-content of T_2 could be much higher than that of T_1 , even if each of q_1, q_2, \dots, q_r had quite high relative errors. T_1 may say 'more' than T_2 , but what it says is quite inaccurate. Intuitively however, T_2 seems closer to the truth than T_1 .

To avoid the rejoinder that the previous definition puts words into Krajewski's mouth, consider now (K5). A theory T_1 is taken to be the conjunction of laws contained in it, i.e. $T_1 = L_1 \& L_2 \& \dots \& L_n$. The truth-content of T_1 is defined as the minimum of the truth-content of all laws of T_1 . If we interpret this statement to mean that the truth-content of T_1 is only as good as its weakest law, then a theory T_1 with only one completely false law is no better than a totally inadequate theory with all its laws false. Alternatively, we may interpret Krajewski's requirement to be this:

$$(K7)* \quad \text{TrC}(T_1) = \text{Min} \{ (1 - \text{Max}_j [DI_j (L_1)]) + (1 - \text{Max}_j [DI_m (L_2)]) \\ + \dots + (1 - \text{Max}_j [DI_j (L_n)]) \}.$$

Suppose $\text{TrC}(T_2)$ is such that n is quite small relative to r , but that its laws are highly accurate. Then $\text{TrC}(T_1) > \text{TrC}(T_2)$ even if the laws of T_1 are extremely inaccurate. For example $\text{TrC}(T_2) = \text{Min} \{ (1 - 0.1) + (1 - 0.1) + \dots + (1 - \text{Max}_j [DI_j (L_{10})] = 0.9) \} = 9$. On the other hand, $\text{TrC}(T_1) = \text{Min} \{ (1 - 0.9) + (1 - 0.9) + \dots + (1 - \text{Max}_i [DI_i (M_{1,000})] \} = 0.1$. But $100 > 9$.

R.D. Rosenkrantz (1980), has offered a probabilistic analysis of the notion of verisimilitude. We will say that the support which an observation E accords a hypothesis H is measured by the *likelihood* $P(E|H)$, i.e. by the probability that H accords E . For $K = K_1 \vee \dots \vee K_n$ and $P(K|E) = \sum_i P(K_i | E)$, we have by Bayes' Theorem:

$$(3-29) \quad P(K|E) = \frac{P(K)}{P(E)} \left\{ \sum_{i=1}^n \frac{P(E|K_i) \cdot P(K_i)}{P(K)} \right\}$$

where $\sum_{i=1}^n P(E|K_i) \cdot P(K_i) | P(K)$ is the average likelihood with $P(K_i) | P(K)$ as a weight factor. The expected weight of evidence of a true hypothesis H^* with respect to H is for outcome x of an experiment X is:

$$(3-30) \quad I(H^*, H) = \sum_{x \in X} P(x|H^*) \cdot \log_e \left\{ \frac{P(x|H^*)}{P(x|H)} \right\}$$

and verisimilitude is defined as follows:

$$(3-31) \quad \text{Ver}(H) < \text{Ver}(K) = \text{df } I(H^*, H) > I(H^*, K).$$

This account is open to a very basic objection. Likelihood is taken by Rosenkrantz to be the likelihood that a hypothesis is *true*. For a theory which is actually refuted, the weight factor in (3-30) may render $I(H^*, H)$ undefined as $P(x/H) = 0$ for $0 \leq P(x|H^*) \leq 1$. The very point however of a theory of verisimilitude is to be able to make truthlikeness comparisons between actually false theories, or actually false hypotheses. The model of Rosenkrantz is not designed to do this, and consequently it fails to be a satisfactorily general account of verisimilitude.

9. WOJCICKI'S ACCOUNT OF APPROXIMATE TRUTH

R. Wójcicki (1973), has produced a definition of approximate truth of a set A of sentences of a first-order language \mathcal{L} , as part of his project of developing a formal methodology of the empirical sciences. Before we can examine this definition, a number of other formal concepts must be discussed.

First, a set-theoretical model of an empirical theory is an ordered set:

$$(S) \langle \mathcal{L}, \vdash, A_0, K \rangle$$

such that K is a set of strictly similar operational empirical systems, \mathcal{L} is a language conformed with the set of all quantitative systems similar to idealizations of the systems in K , A_0 is a set of sentences of \mathcal{L} , and \vdash is a derivability relation defined on the set of sentences of \mathcal{L} . A number of further concepts now require explication, beginning with the concept of a quantitative system.

By ' $\langle t, a \rangle$ ' we denote that denoted by the sentence 'the object a taken at the time t ', and shall view every ordered pair of the form $\langle t, a \rangle$ as a thing-slice. The set $Ob(U)$ is the set of empirical objects and the set U is the set of thing-slices of objects in $Ob(U)$. We say that a exists at time t if and only if $\langle t, a \rangle \in U$. The interval $i_U(a) = \{t: \langle t, a \rangle \in U\}$, is called the *period of existence* of the object a in U and the union: $i(U) = \bigcup \{i_U(a): a \in Ob(U)\}$ shall be called the *period of existence* of U , the *universe*. If U and V are two universes, and if $V \subseteq U$, then V is a subuniverse of U . The symbol ' $U^{(n)}$ ', where n is a natural number ≥ 1 , denotes the set of n -th limited Cartesian powers of U :

$$(3-22) U^{(n)} = \{ \langle t, a_1, a_2, \dots, a_n \rangle \mid \langle t, a_1 \rangle, \dots, \langle t, a_n \rangle \in U \}$$

and the mapping: $F: U^{(n)} \rightarrow \text{Re}$ for real numbers, is a n -ary numerical parameter on U ; the number n is the *arity* of F . By the symbolization ' $F \langle t, a_1, \dots, a_n \rangle = x$ ' it is meant that the magnitude F measured on the objects a_1, a_2, \dots, a_n at time t , takes the value x . On this basis, a quantitative structure defined on U is a structure $\mathfrak{K} = \langle X, F_1, \dots, F_n \rangle$ if and only if U is a universe, X is a subuniverse of U and F_1, \dots, F_n are numerical parameters defined on U . Any two structures, \mathfrak{K} and \mathfrak{L} , such that $\mathfrak{K} = \langle X, F_1, \dots, F_n \rangle$ and $\mathfrak{L} = \langle Y, G_1, \dots, G_n \rangle$, not necessarily defined on the same universe, are similar if and only if for every i , $1 \leq i \leq n$, the parameters F_i and G_i are of the same arity. If F_1, \dots, F_n are S_1, \dots, S_n -ary parameters respectively, then the *similarity type* of the structure \mathfrak{K} is a $S = \langle S_1, S_2, \dots, S_n \rangle$. Two structures \mathfrak{K} and \mathfrak{L} are of the same similarity type if and only if the similarity type of both structures is $S = \langle S_1, S_2, \dots, S_n \rangle$. If for every i , $F_i = G_i$, then \mathfrak{K} and \mathfrak{L} are said to be *strictly similar*.

The second concept requiring explication is that of an *operational structure*. Let F be a k -ary quantity defined on a universe U . An operational measurement of F is an operation p which transforms F into a function pF such that:

$$(3-33) \quad (1) \quad pF: U^{(k)} \rightarrow \text{Re}, \text{ and} \\ (2) \quad (\forall \langle t, a_1, \dots, a_k \rangle) [(\langle t, a_1, \dots, a_k \rangle \in U^{(k)}) \rightarrow F \langle t, a_1, \dots, a_k \rangle \in pF \langle t, a_1, \dots, a_k \rangle].$$

Consider a quantitative structure $\mathfrak{K} = \langle X, F_1, \dots, F_n \rangle$ defined on a universe U with $p = \langle p_1, \dots, p_n \rangle$, where ' p_1 ', \dots , ' p_n ' denote operational measures of F_1, \dots, F_n respectively. Consider a set $\bar{\mathfrak{K}} = \langle X, \Phi_1, \dots, \Phi_n \rangle$. If there exists a $p = \langle p_1, \dots, p_n \rangle$ such that for every Φ_i , $\Phi_i = p_i F_i$, then $\bar{\mathfrak{K}}$ is said to be an *operational structure* defined on U and $p\mathfrak{K} = \langle X, p_1 F_1, \dots, p_n F_n \rangle$ is an *operational system* corresponding to \mathfrak{K} . If an operational

structure \bar{X} corresponds to a quantitative structure \mathfrak{K} (i.e. they are strictly similar), then \mathfrak{K} is an idealization of \bar{X} .

Consider now a language \mathcal{L} conformed to a set K of all quantitative systems of a given similarity type, and let \bar{X} be an operational system corresponding to a structure \mathfrak{X} in K . Then a sentence ϕ of the language \mathcal{L} is true if and only if it is true in every idealization of \bar{X} . A sentence ϕ of \mathcal{L} is false in \bar{X} if and only if it is false in every idealization of \bar{X} . A sentence ϕ of \mathcal{L} is indeterminate in \bar{X} if and only if it is neither true nor false for every idealization of \bar{X} . A set of sentences A of \mathcal{L} is *approximately true* in \bar{X} if and only if there is an idealization \mathfrak{X} of \bar{X} such that every sentence ϕ of A is true in \mathfrak{X} .

This account of approximate truth has little to offer as an account of verisimilitude. Note that if it is the case that not every sentence ϕ of A is true in some idealization \mathfrak{J} , then the set of sentences A of \mathfrak{J} is not approximately true. For the operational structure \bar{X} to have an idealization \mathfrak{J} , it is sufficient that \mathfrak{J} be a quantitative structure, and that \bar{X} and \mathfrak{J} be strictly similar. Let $\bar{X} = \langle X, \phi_1, \phi_2, \dots, \phi_n \rangle$ and $\mathfrak{J} = \langle Y, G_1, \dots, G_n \rangle$. Then if \bar{X} and \mathfrak{J} are strictly similar, then $\lceil (\phi_1 = G_1) \ \& \ (\phi_2 = G_2) \ \& \ \dots \ \& \ (\phi_n = G_n) \rceil$ is true. Now since strictly similar structures may differ only as to the sets of objects they involve, it may be assumed, given the condition that every sentence ϕ of A is true in \mathfrak{J} , that $X \subset Y$. Hence it is possible that there is a y such that $y \in Y$ but $y \notin X$. Thus $\lceil G_i y \rceil$ and $\lceil \phi_i y \rceil$ may differ in truth-value, in particular, that $\lceil G_i y \rceil$ may be true, whilst $\lceil \phi_i y \rceil$ is false. But if it is the case that $(\phi_i = G_i)$ is true, then if every sentence ϕ of A is true in \mathfrak{J} , then $\lceil G_i y \rceil$ and $\lceil \phi_i y \rceil$ cannot differ in truth-value. Hence $X = Y$. But if $X = Y$, then by the axiom of extensionality of sets, $\bar{X} = \mathfrak{J}$. This means that \bar{X} is approximately true if and only if every sentence of \mathfrak{J} is

true, that is, that \mathcal{F} is true. But this result is parallel to the Miller-Tichý-Harris Theorem and renders Wójcicki's concept of approximate truth, theoretically unworkable.

10. RELEVANCE LOGIC AND VERISIMILITUDE

Chris Mortensen in his paper entitled "A Theorem on Verisimilitude" (1978), argued that the Miller-Tichý-Harris Theorem is dependent upon the classical logical assumption \mathcal{Y} : if $\alpha \in A$ and $\sim\alpha \vee \beta \in A$, then $\beta \in A$. This principle however fails for large classes of theories based on logics other than classical logic. Mortensen argued that there exist two RM3-theories, A, B (RM3 being but one system discussed by Anderson and Belnap in *Entailment* (1975)) such that the verisimilitude of B is greater than the verisimilitude of A , i.e. $B >_v A$, and B has at least one false consequence i.e. $B_F \neq \{\emptyset\}$. A brief review of this result will be given, and its limitations in turn outlined.

Consider a language L_m which has a denumerable number of constants, $p_1, p_2, \dots, p_n, \dots$ closed under negation and conjunction. The RM3 matrices are as follows:

&	T	N	F	\sim
*T	T	N	F	F
*N	N	N	F	N
F	F	F	F	T

\rightarrow	T	N	F
*T	T	F	F
*N	T	N	F
F	T	T	T

We take $A = \{\alpha \mid V_A(\alpha) = T \text{ or } V_A(\alpha) = N\}$, where if α is of the form $\beta \& \mathcal{Y}$, then $V_A(\alpha)$ is determined from the RM3-matrix for '&' and if α is of the form $\sim^n(\beta \& \mathcal{Y})$ for some $n \geq 1$, then $V_A(\alpha)$ is also determined from the RM3 matrix for ' \sim '. Further, for all $n \geq 0$, $V_A(\sim^n p_1) = V_A(\sim^n p_2) = N$ and for all $n \geq 0$ and $m \geq 3$, $V_A(\sim^{2n} p_m) = T$ and

$V_A(\sim^{2n+1} p_m) = F$. We also take $B = \{\alpha \mid : V_B(\alpha) = T \text{ or } V_B(\alpha) = N\}$, where α is of the form $\beta \ \& \ \gamma$, then $V_B(\alpha)$ is determined from the RM3-matrix for '&', and if α is of the form $\sim^n(\beta \ \& \ \gamma)$ for some $n \geq 1$, then $V_B(\alpha)$ is also determined from the RM3-matrix for '~'. Further, for all $n \geq 0$, $V_B(\sim^n p_1) = N$ and for all $n \geq 0$ and $m \geq 2$, $V_B(\sim^{2n} p_m) = T$ and $V_B(\sim^{2n+1} p_m) = F$.

Now it is possible to construct a theory B^- , such that $B^- = \{\alpha \mid V_{B^-}(\alpha) = T \text{ or } V_{B^-}(\alpha) = N\}$ and for all $n \geq 0$ and $m \geq 1$, $V_{B^-}(\sim^{2n} p_m) = T$ and $V_{B^-}(\sim^{2n+1} p_m) = F$; if α is of the form $\beta \ \& \ \gamma$, then $V_{B^-}(\alpha)$ is determined from the RM3-matrix for '&' and if α is of the form $\sim^n(\beta \ \& \ \gamma)$ for some $n \geq 1$, then $V_{B^-}(\alpha)$ is also determined from the RM3-matrix for '~'. The principal theorem follows from the following five lemmas:

(3-34) Lemma 1. A, B, B^- are RM3 theories.

Lemma 2. $B^- \subset B \subset A$.

Lemma 3. A is nontrivial.

Lemma 4. B^- is a classical theory, which is negation consistent and complete in L_m .

Lemma 5. Let $T = B^-$, then $A \cap T = B \cap T = T$, and $B \cap F \neq \{\emptyset\}$.

Since a large class of relevant logics are weaker than RM3, E^- , R^- , EM^- , RM^- , etc. are also suitable logics for the establishment of the results of the principle theorem.

The rejection of the principle γ : if $\alpha \in A$ and $\sim \alpha \vee \beta \in A$, then $\beta \in A$, only serves to block one part of the Miller-Tichý-Harris Theorem, as Mortensen is well aware. Lemma 1 of the proof of (MTHT) given in section 2 of this chapter, does not depend upon γ , and the argument can only be blocked from a strictly logical point of view, by rejecting

either: (1) $f \in B$ and $b \in B$, then $f \& b \in B$; (2) $f \in F$, then $f \& b \in F$, or (3) $f \& b \in A$, then $b \in A$. None of these principles are rejected by any standard relevance logic. Hence Mortensen's hypothesis that the Miller-Tichý-Harris Theorem can be avoided by a change to relevance logic, is falsified. Relevance logic gives no general solution to this problem.

More recently Mortensen (1983(a)) has established that the Miller-Tichý-Harris Theorem cannot be avoided by using a relevance logic as a logic for scientific theories, since a severely limiting theorem can be proved for Popper's definition in even weak relevance logics. Consider a theory A . Then A is *prime* if and only if, whenever $a \vee b \in A$, then at least one of $a, b \in A$. Mortensen has shown that the Miller-Tichý-Harris Theorem holds for all consistent prime theories. In addition he has established the following limitation theorems:

- (3-35) (MT1) If A, B and L -theories, and L is prime, then
if $B_T \subseteq A_T$ and $A_F \subseteq B_F$, then $T \subseteq A$.
- (MT2) If A, B and L -theories, and L is prime, then
if $A >_{\vee} B$, then $A \subseteq T$ or $T \subseteq A$.
- (MT3) If $B_T \subseteq A_T$ and $A_F \subseteq B_F$ and A is complete, then
 $A = T$.
- (MT4) If A, B are classical theories and A is
consistent and complete, then if $A >_{\vee} B$
then $A = T$.

It is the case that if an L_{\circ} -theory is consistent and prime, then χ holds for it, for suppose $a \in A$ and $\sim a \vee b \in A$, then since A is consistent $\sim a \notin A$, but if A is prime at least one of $a, b \in A$, so $b \in A$. Thus most of Mortensen's limitation theorems are only directed against classical L_{\circ} -theories. However, (MT1) is applicable to theories which

may be paraconsistent (although non-trivial), as shall now be demonstrated. Assume that $B_T \subseteq A_T$ and $A_F \subset B_F$ and that $T \not\subseteq A$ for *reductio ad absurdum*. Let $t \in T$ and $t \notin A$ and $b \in B_F - A_F$ so that $b \in B$, $b \in F$ and $b \notin A$. Then since $b \in B$, $t \vee b \in B$ and since $t \in T$, then $t \vee b \in T$. Hence $t \vee b \in B_T$. But $t \notin A$ and $b \notin A$ and since A is prime, $t \vee b \notin A$. Hence $t \vee b \notin A_T$, thus contradicting $B_T \subseteq A_T$.

Mortensen has also considered the possibility of avoiding the Miller-Tichý-Harris Theorem by intensionalizing the metalanguage. He has shown however that if the intensional verisimilitude relation holds between sets, then so does Popper's extensional relation, since it is a theorem of all the standard relevance logics that: $(\forall x) (Fx \rightarrow Gx) \rightarrow (\forall x) (Fx \supset Gx)$.

These results establish that the relevance programme contributes nothing towards avoiding the Miller-Tichý-Harris Theorem. Indeed, as I pointed out earlier, Mortensen's programme was doomed from the outset, since one part of the limitation theorem went through making use only of $(a \ \& \ b) \rightarrow a$ and some plausible set-theoretical principles. It is of course true that $\vdash_{\text{L}} (X \ \& \ Y) \rightarrow X$ is rejected in connexive logic. To produce a unified solution to the verisimilitude limitation theorems by a change of logic will then require a more radical regimentation of logic than has yet been anticipated. The loss of provability power may prove to be too great a price to pay in restricting the logic of science, merely to save Popper's theory of verisimilitude. A solution should first be looked for elsewhere.

11. NEWTON-SMITH ON VERISIMILITUDE

Newton-Smith (1981) has recently attempted to defend the *thesis of verisimilitude* (TV) on grounds quite different from those already considered here. He takes the thesis TV to state that "the goal of the scientific

enterprise is to be understood in terms of progress towards increasing verisimilitude, and that we can have reasons (on occasion at least) for believing that we have indeed made progress" (ibid., p. 195). Newton-Smith proceeds in his argument, by making use of a style of argument frequently used in the natural sciences: an inference to the best explanation (cf. (Harman, 1973, pp. 130-135)). In his justification of TV Newton-Smith takes as an explanandum the fact that for mature sciences, contemporary theories provide us with better predictions about the world than their predecessors and have enabled us to have a more extensive degree of technological manipulation of the world than such predecessors. If TV was true, then we have an answer to the problem of explaining how it is that contemporary theories are more useful predictively and technologically than their predecessors. But, Newton-Smith asserts, we have at hand no better explanation than TV, therefore it is reasonable to believe that TV is true (ibid., p. 196).

This argument, apart from making use of an unanalyzed concept of verisimilitude, also requires the following crucial premises:

- (P1) In a typical mature science such as physics, there has been a significant improvement in the predictive power of theories;
- (P2) If a theory T_2 is a better approximation to the truth than a theory T_1 , then it is likely that T_2 will have greater predictive power than T_1 .

Of these premises (P₂) is the most controversial and is defended by Newton-Smith by developing an account of verisimilitude. In developing such an account, Newton-Smith must demonstrate that greater verisimilitude entails the likelihood of greater observational success, for TV fails if the premise (P₂) merely asserted a correlation between higher verisimilitude

and a greater observational success. TV fails because we cannot correlate an inductive degree of verisimilitude and a degree of observational success without a direct access to the relative verisimilitude of rival theories. This is precisely what we lack. On the other hand, Newton-Smith must guard against establishing that greater observational success *entails* greater verisimilitude, as this simply constitutes an uninteresting definition.

Newton-Smith correctly observes, that any materially adequate definition of verisimilitude must satisfy this constraint:

- (C1) If T_2 has greater verisimilitude than T_1 , then T_2 should have at least as much content as T_1 .

He restricts his attention to first order recursively axiomatized theories, whose deductive closure is recursively enumerable. Thus, the theoretical postulates or auxiliary hypothesis statements can be mechanically produced in a sequence, and in turn assigned a positive integer corresponding to their position in such an enumeration. The existence of such recursive enumerability is crucial for Newton-Smith's analysis of what it means for a theory T to answer a question '?p'. To answer a question '?p', T must contain as a deductive consequence either 'p' or ' \sim p'. T is thus said to *decide* '?p'.

Consider two theories T_1 and T_2 which either have the same vocabulary, or in which the vocabulary of one, includes the vocabulary of the other. Let t_1 and t_2 be enumerations of the deductive consequences of T_1 and T_2 respectively, such that all logically true and equivalent formulae have been deleted. For any positive integer n , there exists a ratio of the number of sentences among the first n of t_1 which are decided by T_2 . Consider R^1 , the infinite sequence of such ratios, and R^2 the infinite sequence of ratios generated by considering this time the random sequence of T_2 . The content of T_1 and T_2 is approximately equal if for a

sufficiently large n , the absolute value of the difference between the corresponding terms of the two sequences of ratios is small and constant, and if the terms of one sequence tend to be larger than the corresponding terms of the other sequences, then the theory from which this sequence is generated thus has a greater content than its rival.

This explication of relative content is as it stands merely qualitative. Further, as is well known from the theory of infinite sequences, whether an infinite sequence has a limit depends upon the order of terms in the sequence. If we attempt to attach a measure to the content of T_2 relative to the content of T_1 we run into the difficulty, of arbitrary changes in enumerative order altering the convergence or divergence of a sequence of ratios. To avoid this difficulty Newton-Smith restricts his definition of relative content to what he calls "respectable₁" theories for which the sequences of absolute differences of the corresponding terms in the ratios R^1 and R^2 has a limit insensitive to reasonable place selection on t_1 and t_2 (ibid., p. 202).

To explicate the notion of relative truth, Newton-Smith defines a new sequence of ratios called *truth-ratios*. The n -th term in the sequence gives the ratio of the number of truths in the first n terms of t_1 to the number of truths in the first n terms of t_2 . "Respective₂" theories are such that the truth ratio of T_1 and T_2 or of T_2 to T_1 has a limit, and any infinite sequence of the original sequences of sentences obtained by reasonable place selection, has the same limit.¹⁰ This limit is the truth-ratio of the poorer to the better theory.

Due to difficulties with the transcendental notion of truth (ibid., pp. 53-54), Newton-Smith suggests that the truth-ratio of T_1 and T_2 is determined relative to some theory T_3 , which might most plausibly be regarded as a current theory, or less plausibly as a total theory of nature in the Peircean sense. This results in the following two

definitions (ibid., p. 204):

(DGT) T_2 has a greater truth relative to T_3 than $T_1 =$ df. the infinite sequence of ratios giving the ratio of truths in T_1 to the truths in T_2 judged by reference to T_3 has a limit greater than $1/2$ which is unaffected by reasonable place-selection.

(DGV) T_2 has a greater verisimilitude than $T_1 =$ df. both conditions (1) and (2) hold:

- (1) the relative content of T_2 is equal to or greater than that of T_1 ;
- (2) T_2 has greater truth relative to T_3 than T_1 .

Now let us first ask how Newton-Smith's account of verisimilitude is a defense of premise (P_2) in his argument for TV. His argument here is well worth citing in full (ibid., p. 205):

(P_3) For one theory to be nearer to the truth than another it must have greater content and more of its content must be true. The definition of relative truth means that less of its content will be false. It follows from this definition that if one theory has greater verisimilitude than another it is likely to have greater observational success. For the greater relative truth of T_2 means that an arbitrary consequence of T_2 is more likely to be true than an arbitrary consequence of T_1 . Furthermore, this cannot be true of T_2 simply because T_1 is the weaker theory. For by the first clause in the definition T_2 has more content than T_1 . If one wants both to say more about the world and to say more true things in doing so, T_2 is the theory to adopt. The fact that an arbitrary consequence of T_2 is more likely to be true than an arbitrary consequence of T_1 means that an arbitrary observational condition in T_2 is more likely to be true than an arbitrary observational condition in T_1 .

This passage has been cited in full, to illustrate the fact that Newton-Smith's argument for (P_2) is quite obscure. What precisely is the



argument here which establishes the required entailment? One suggestion is that 'greater observational success' may be definitionally cashed in as follows: "Being observational just means being a sentence of the sort we feel we can test for truth and be confident in our results" (ibid., p. 205). This however seems to lead Newton-Smith into a violation of a condition which he set out earlier, namely that observational success is *not defined* by means of the concepts of truth and verisimilitude. But let us accept that T_2 has more verisimilitude than T_1 via (DGV): why *must* it follow that T_2 has therefore a *greater* observational success and predictive power than T_1 ? No argument for this conclusion seems to exist in passage (P₃).

We may be more successful in our search for such an argument if we turn to Newton-Smith's account of *observational nesting* (ibid., pp. 206-207). He states here that a more successful theory T_2 observationally nests a less successful theory T_1 if for some prediction \emptyset of T_1 for the value of a parameter V_1 , for some interval i representing the current limits of experimental accuracy, T_2 predicts a value of V_1 within the limits of i . Where T_2 and T_1 both have the same observational content, T_2 may make corroborated predictions on matters which T_1 remains silent. In this case, T_2 is said to have *content-increasing predictive success*. If T_2 is observationally more successful than T_1 then: (1) T_2 observationally nests T_1 , and (2) T_2 displays content-increasing predictive success over T_1 . Is it the case that the required entailment of (P₂) follows? Once more I do not believe that this is the case.

To establish that the entailment claimed by (P₂) fails, it is sufficient to establish that it is logically possible for T_2 to be of greater verisimilitude than T_1 and yet for T_2 not to be observationally more successful than T_1 . Let the antecedental condition be satisfied.

It is sufficient to establish that the entailment of (P_2) fails, to establish that condition (2) of content-increasing predictive success fails. That is to say, we must show that it is logically possible for T_2 to be of greater verisimilitude than T_1 and yet it is not the case that T_2 displays content-increasing predictive success over T_1 . If it is not the case that T_2 displays content-increasing predictive success over T_1 , then T_1 may make corroborated predictions on matters on which T_2 is silent. It is I maintain, logically possible for this to occur even though T_2 has a greater relative truth and relative content than T_1 , when T_1 is such that for some small range of phenomena it does make novel predictions which are in fact corroborated. Likewise, the condition of observational nesting may be violated by a series of local predictions which T_1 makes which T_2 does not, where T_1 is in a restricted range, more accurate. But in general T_2 may generate more accurate predictions than T_1 . Hence the required entailment of (P_2) fails. This is a crucial premise in Newton-Smith's defence of TV. It follows that his defense of TV also fails.

The second line of criticism will be concerned with Newton-Smith's account of verisimilitude. Note that according to (DGT) the limit of the required sequence of ratios must be greater than $1/2$. Consider however two theories T_1 and T_2 with associated sequences of consequences $t_1 = t_1^1, t_1^2, t_1^3, \dots, t_1^n, \dots$ and $t_2 = t_2^1, t_2^2, t_2^3, \dots, t_2^n, \dots$. The truth-ratio is, we recall, the number of truths in the first n terms of t_1 to the number of truths in the first n terms of t_2 . Consider also a theory T_3 , such that $t_3 = t_3^1, t_3^2, t_3^3, \dots, t_3^n, \dots$. Now let it be the case that none of the consequences of t_1 are true so we have a sequence $t_1 = 0, 0, 0, \dots, 0, \dots$. Thus T_1 is a strictly false scientific theory. Suppose that t_2 and t_3 are strictly true (and perhaps

extensionally identical) so that we have $t_2 = 1, 1, 1, \dots 1, \dots$ and $t_3 = 1, 1, 1, \dots 1, \dots$. We obtain a truth of 0 for each term of the truth-ratio sequence. The limit of the sequence is 0, and is totally unaffected by the ordering of sequence terms. Thus we have an intuitively clear case where T_2 is of greater truth relative to T_3 than T_1 , but the required limit fails to be greater than $1/2$. Hence (DGV) also fails.

The second major problem with (DGV) is that if T_2 happened to be a strictly false theory, then immediately our truth-ratio sequence contains undefined terms. Newton-Smith attempts to avoid divergencies by taking relative content to be only satisfactorily defined for respectable₁ theories, these being theories where the sequence of absolute differences of the corresponding terms in the sequence of ratios R^1 and R^2 actually is convergent. This immediately means that his account of verisimilitude fails, as we have seen, for cases of divergency. Also since Newton-Smith set out to defend TV we will require good reason to believe that at least some actual theories are respectable₁. He admits that he has no actual examples, but claims that he is in no worse boat than the frequency theory of probability (ibid., p. 202). Perhaps Newton-Smith and frequency theorists are shipwrecked together.

The third major defect with this account of verisimilitude is that we can no longer strictly speak of the greater truth of T_2 to T_1 . Rather we must introduce the idea of T_2 having a greater truth relative to T_3 than T_1 . If T_3 is a strictly false theory, then once again we will fail to establish convergence if $t_3 = 0, 0, 0, \dots 0, \dots$. So for this account to be of interest T_3 must be a theory which more closely approximates the truth than either T_2 or T_1 . If we have such a theory

already, to establish verisimilitude estimates we seem faced by the prospects of a vicious infinite regression, as both the verisimilitude and content of T_3 can only be decided by *first* making recourse to another 'truthlike' theory T_4 , and likewise in turn for T_4 before we can even ascertain the verisimilitude of T_3 . If on the other hand we already know that T_3 closely approximates the truth, as realists, our interest in two less successful theories T_1 and T_2 is minimal. Verisimilitude estimates in this situation seem merely redundant.

We are thus led to the conclusion that Newton-Smith's account of verisimilitude, whilst not subject to the Miller-Tichý-Harris limitation result, faces logical difficulties of its own which vitiate its use as an intuitively satisfactory account of verisimilitude.

12. AGASSI: VERISIMILITUDE UNSAVED¹¹

Joseph Agassi (1981) has attempted a reformulation of Popper's theory of verisimilitude. Popper developed this theory, Agassi points out, to overcome problems facing his theory of corroboration. Popper's earlier view in his classic *Logik der Forschung* was that scientific progress occurred when all extant crucial evidence favours the new theory T_2 , and none of which favours the old theory T_1 . The term 'crucial evidence' is taken by both Popper and Agassi to mean (and I paraphrase): the evidence refuting the older theory T_1 , which "follows from" (presumably in the sense of deductive consequence, although this is left unclear in Agassi's paper) the newer theory T_1 (ibid., p. 576). The difficulty with this account of scientific progress is that crucial evidence may exist unbeknown to us, pointing in the other direction. That is to say that it is not merely logically, but empirically possible that there may also exist crucial evidence which refutes T_2 but which

corroborates with T_1 . In such a situation the degree of corroboration of T_2 and T_1 is the lowest possible. Let us call such evidence 'mutually refuting evidence' (MRE).

It is precisely at this point that Agassi begins his repair job. As a criterion of verisimilitude increase it is maintained that there is no such MRE known or unknown, procurable by today's means. 'Empirical content' is defined as a class of evidence procurable today. Agassi then examines a definition of verisimilitude, which has it that verisimilitude increase is "the combination of an increase of true empirical content and a decrease of false empirical content" (ibid., p. 577). Whilst this definition ensures that there is no crucial evidence going the "wrong way", Agassi is right in rejecting it because this definition precludes the possibility of refuting the new theory T_2 with evidence not relevant to the old Theory T_1 , and any adequate theory of content increase of scientific theories requires this. This immediately leads us to Agassi's own account.

Here two definitions and two propositions taken to describe Popper's earlier and later views are advanced. The definitions are these:

- (A) a theory is more empirically successful than its predecessor if and only if all known crucial evidence concerning the two goes its way;
- (B) a theory is more verisimilar than its predecessor if and only if all crucial evidence concerning the two goes its way (ibid., p. 578).

The two propositions, taken by Agassi to describe Popper's earlier and later views respectively, are these:

- (E) Progress is empirical success;
- (L) Progress is verisimilitude increase.

It is alleged by Agassi that the following thesis, thesis (QI), follows from (A), (B), (E) and (L).

(QI) When crucial evidence repeatedly points one way it is unlikely that it also points the other way.

These definitions are theses which constitute Agassi's attempt to save the theory of verisimilitude. The remaining part of Agassi's discussion is concerned with interpreting and defending (QI). One of the difficulties in accepting (E), is that there may, as we have said, be crucial evidence existing unknown to us, yet procurable today which "points the other way". (QI) says that such evidence is not likely. Agassi offers various reasons in support of (QI) all of which, as I shall argue below, are inadequate.

I isolate three rather unclear and sketchy arguments for (QI) in Agassi's paper. First, he maintains, we take (QI) as true as a "matter of course" (*ibid.*, p. 579), and if we do find crucial evidence going the other way we simply reverse our judgements. The defence is unconvincing. To take (QI) to be true as a "matter of course" is to simply assume that it is true. It is not to offer any non-question begging good reasons for believing that (QI) is true. What is needed is precisely an argument which shows that it is unlikely that we will need to reverse our judgements *because it is unlikely that crucial evidence will point the other way*. This is not shown by Agassi's first argument.

The second argument is also unsatisfactory. He tells us that no one expects any crucial evidence to turn up which could support Galileo's mechanics. But why? The reason appears to be that such evidence would refute all later theories of gravitation and would therefore constitute

a major scientific upheaval. This is a strange argument for a neo-Popperian like Agassi to advance. We should recall, to put it clearly and simply, that for Popper *falsifiability* is the quality which distinguishes scientific theories from non-scientific theories. Major scientific upheavals in the field of the theory of gravity, evolution, the origin of society, may in fact be argued by a falsificationist, to be epistemically virtuous rather than epistemically damnable. In major scientific upheavals much is learnt about the defects of now questioned scientific theories. Many bold conjectures will be made, and there is generally a fast turnover of such conjectures, being falsified by other members of the scientific community. Agassi's argument is in my opinion inconsistent with some very basic insights which Popper has given us, and in any case does not constitute an argument for (QI) even if it is taken on an independent basis outside of a strictly Popperian viewpoint. To do this, Agassi needs some argument to show that it is unlikely that major scientific upheavals will not occur in the future. No argument is given and it is difficult to see what any such non-question begging argument would be like without a solution to the problem of induction, a problem which is in any case insoluble, or so Popperians tell us.

The final argument which Agassi gives in support of (QI) is that there may be metaphysical arguments which support (QI). He says (*ibid.*, p. 579):

... Popper's theory of verisimilitude does not judge things from the viewpoint of any specific scientific theory; rather it is both meta-scientific and ontic. This fact, I suggest, blocks the way to any answer to our question, why is (QI) true? For, meta-science with no metaphysics precludes all ontology .

After careful examination of this passage, I have concluded that if it is not simply nonsense, then it contains no intelligible arguments. Being

told that it is possible that there are good "metaphysical arguments" (perhaps in the form of *transcendental arguments*), will not convince any rational thinker, let alone the critics of verisimilitude. The arguments must be put before one to examine. It is evident that this has not been done. Hence Agassi has not saved verisimilitude.

13. CONCLUSION: STATE OF THE ARGUMENT

This chapter has outlined the problems facing both realist and non-realist accounts of philosophical progress in the light of the problem of perennial philosophical disputes. In section 1 of this chapter the main problems with non-realist views of philosophical progress, especially the theories of Lakatos and Laudan were considered. Whilst neither author has explicitly considered the applicability of their respective models of scientific progress to a philosophical subject matter, an extension in this direction is not illegitimate and I have indicated the most plausible line of development that I am aware. However both the Lakatosian and Laudian accounts of philosophical progress were found to be untenable, and we turned immediately back to a consideration of a realist accounts of cognitive progress. After all, if one wishes to argue that philosophy is a progressive enterprise, then one must explicate the notion of *progress*. The key notion of the realist theory of cognitive progress, verisimilitude, is subject to a trivializing result. Therefore no satisfactory account of philosophical progress can be based on this notion. After conducting a detailed examination of the major theories of verisimilitude, this negative conclusion has been reinforced. A realist theory of cognitive progress is thus in a state of epistemological crisis.

My research strategy will be to shelve this problem until Chapter 11,

where my own definition of verisimilitude will be given. The chapters to follow will present a critical examination of various responses to the problem of perennial philosophical disputes, beginning with sceptical, relativist, anarchist and nihilist responses in the next chapter. Let me note once more however, that unless the problem of verisimilitude can be satisfactorily dealt with, the principle thesis of this work can in no matter be considered to have been rationally supported. We do not know *what* we are talking about when we say that philosophy is a progressive enterprise giving us increasing quantities of truth-like or verisimilar information.

3. NOTES

1. We should be on guard though in regarding this proposition as something which Feyerabend believes to be *true*, for one of his favourite pastimes is inventing fairy tales to confuse gullible rationalists (Stove, 1982).
2. c.f. also (Suppe (ed.), 1977), (Dilworth, 1981).
3. For a discussion of realism and scientific progress c.f. (Smith, 1981).
4. The thesis of the progress of science through an increase in verisimilitude, is to be distinguished from the first thesis of *convergent realism* as stated by L. Laudan (1980, pp. 233-234):

(R₁) scientific theories (at least in the 'mature' sciences) are typically approximately true and more recent theories are closer to the truth than older theories in the same domain...

Thesis (R₁) is quite problematic, and this situation has not been improved even by C.L. Hardin and A. Rosenberg's (1982) response to Laudan. Thesis (R₁), as it originally occurs in R. Boyd's, "Scientific Realism and Naturalist Epistemology" (manuscript), makes use of the notion of a 'mature science' to rule out counter-examples of reference - failure made by recourse to any 'arbitrary chosen scientific theory'. Boyd takes 'mature scientific theories', to be those which have passed a take-off point. This concept in turn, insofar as it is explicated at all, is explicated by reference to the concept of truth, or approximate truth of certain background theories. This however leads us straight into a vicious infinite regress, as the concept of a 'mature science' was initially introduced to rule out counter-examples of reference-failure, and in turn defend the convergent realist's idea of approximate truth. Boyd's explication only serves to lead us back to the very idea of truth and approximate truth.

We have argued elsewhere (Goodwin, Webster and Smith, 198+), that (R₁) is empirically false: a 'mature science' such as the neo-Darwinist account of evolution is *not* closer to the truth than an older position such as Rational Morphology. The realist thesis to be defended later in this work is the *conditional claim*, that *if* cognitive progress occurs, then an increase in verisimilitude of the compared theories will occur.

5. The present chapter is primarily concerned with qualitative accounts of verisimilitude, and the difficulties in presenting a quantitative account of verisimilitude must be addressed elsewhere.
6. A source of possible confusion should be disarmed at this point. It might be argued, as is suggested by some remarks of R. Harré (1980, pp. 292-293), that the present "logician" conception of

the nature of scientific theories is responsible for the Miller-Tichý-Harris Theorem. This theorem might be taken to constitute a *reductio ad absurdum* of such a conception of scientific theories. (For this style of argument c.f. (Harré, 1970).) However, little of significance is at stake in our present use of the term 'theory': it may be replaced by another term such as 'set', and the Miller-Tichý-Harris Theorem will still stand, as long as Popper's original qualitative definition of verisimilitude is upheld and as long as we can still meaningfully form the set of logical consequences of theory.

7. References to the respective works of these authors are cited below.
8. The failure of verisimilitude orderings, with respect to the relation of logical equivalence, is a criticism which Graham Oddie (1978) has successfully advanced against Miller's own account (Miller, 1977(a); (b)).
9. It is strange that someone who accepts J. Hintikka's (1973) notion of depth and his account of distributive normal forms in first-order logic would choose to take this exit, since Hintikka is a sharp critic of the thesis that tautologies are "uninformative".
10. Newton-Smith (1981, p. 203), speaks of the ratio of T_1 to T_2 and of T_2 to T_1 . But we have not been presented by any logical machinery capable of explicating the notion of the ratio of theories. I assume that he is speaking here of truth-ratios.
11. Material in this section first appeared in (Smith, 1984 (a)).

4. SCEPTICISM AND RELATIVISM, ANARCHISM ANDNIHILISM IN METAPHILOSOPHY1. STATEMENT OF THE ARGUMENT

Sceptical, relativistic, anarchist and nihilist responses to the problem of perennial philosophical disputes will now be considered.¹ As the reader will recall, these positions were defined in chapter 1. Whilst I recognize that my categories are not discrete, this will not cause any major logical problems. All of these positions deny that philosophy is a cognitive progressive enterprise and that genuine philosophical truths are accessible to knowing subjects. I oppose all of these positions. If the borderlines between, say, metaphilosophical scepticism and metaphilosophical nihilism are very fuzzy (Richard Rorty's *Philosophy and the Mirror of Science* would seem to be classifiable as both according to my definitions), then it is not unreasonable to suppose that positions in the fuzzy area would also be refuted if successful critical arguments were advanced against both metaphilosophical nihilism and metaphilosophical scepticism. My principal concern here is with refuting these positions, rather than with complete taxonomic precision. For stylistic reasons I will not consider Benson Mates' (1981) work in this chapter, deferring its discussion until chapter 7. Mates' position of *solvability scepticism* is the view that whilst the traditional problems of philosophy are cognitively meaningful, and certainly are not pseudo-problems, they are *absolutely unsolvable*.² Whilst I believe that there are at least some unsolvable philosophical problems, and this accounts for a minor part of philosophical disagreements, I will reject Mates' claim that *all* philosophical problems are unsolvable. But to do so, will first require an independent discussion of the solvability of philosophical problems, which will be given later.

With these qualifications made, I now outline the structure of the argument to follow. The largest part of the argument of this chapter will consist of a critique of cognitive and Protagorean relativism. If either of these positions were accepted, then it would be easy to account for the problem of perennial philosophical disputes. This is so because this problem presupposes an objectivist account of truth in its formulation. If this notion of truth is rejected, as the cognitive relativist and Protagorean relativist propose that it should, then philosophical positions which were thought by the objectivist to be in conflict, are not. The positions P_1 and P_2 may be said to be true, false or perhaps undecidable, from some perspective W , but neither position is *objectively* true, false or undecidable. Cognitive and Protagorean relativism propose a very economical solution to our principal problem. Unfortunately it is unacceptable, I will argue, because both cognitive and Protagorean relativism are self-referentially inconsistent.³

Richard Rorty (1982, p. 167) says that the relativist who can be refuted by such self-referential arguments "is just one of the Platonist or Kantian philosopher's imaginary playmates, inhabiting the same realm of fantasy as the solipsist, the skeptic, and the moral nihilist". If the argument of this chapter is correct, then Rorty himself will be shown to be an inmate of the Platonist's playpen. I will employ the same type of argument against Unger (1984) and Rescher (1978) as well as advancing specific criticisms of these works. The conclusion of this chapter is thus strongly negative: scepticism and relativism, anarchism and nihilism in metaphilosophy are rejected and cannot therefore provide a satisfactory answer to the problem of perennial philosophical disputes.

2. THE SELF-REFERENTIAL INCONSISTENCY OF COGNITIVE AND PROTAGOREAN
RELATIVISM

Jack W. Meiland (1980) has recently argued that the thesis that cognitive relativism ("the doctrine that truth is relative rather than absolute" (ibid., p. 115)) is self-refuting, and thus internally inconsistent, is a thesis lacking adequate justification. We will call this thesis, following Meiland's terminology, "The Paradox of Cognitive Relativism". However, I shall argue here in reply to Meiland, that he fails himself to make good this charge. Cognitive relativism remains a paradoxical doctrine.

First we need a statement of "The Paradox of Cognitive Relativism". The following statements of the paradox are cited by Meiland and will serve as our explication as well:

- (A) If someone declares that truth is not objective but only relative to societies, he may very well claim 'there is no such thing as "objective truth"' or 'truth is relative to societies'. Both assertions, however, clearly purport to be objectively true, and intended as truths about all societies. There would not be much point in the relativist uttering them if he did not wish to convince someone else of them. He thus has to accept that sentences which state his thesis are apparently inconsistent with it . (Trigg, 1973, pp. 2-3).
- (B) ... relativistic theories presuppose the very concept of objective validity which they allegedly destroy, and without such presupposition they lose all meaning. For if they do not themselves claim to be objectively valid and true, we have no reason for taking any of the statements in the theory seriously, but if they do make such claims, then it is evident that certain kinds of statements and theories (e.g. at least those of the relativists) must be exempt from determination by non-rational, non-logical, situational factors, and thus it is not true that all of man's knowledge and truth is relative (Kaufman, 1960, p. 9). [Kaufman himself defends a modified form of cognitive relativism.]

Meiland takes the self-referential argument of these authors to present the following destructive dilemma: (1) either the cognitive

relativist's thesis applies to itself, in which case it too is only "relative", (2) or if it does not apply to itself, then there is according to the relativist, something which is absolutely and objectively true.

(A) Meiland attempts to escape the second horn of this dilemma as follows. Cognitive relativism may be absolutely and objectively true, and yet not internally inconsistent if we recognize that cognitive relativism is a meta-philosophical thesis and not just an ordinary object-language philosophical thesis, such as a Platonistic account of abstract reference. Meiland to be sure, does not find this form of cognitive relativism "interesting", but he certainly believes that it is a consistent position.

Meiland we have seen attempts to escape the problem of the self-referential inconsistency of cognitive relativism by treating cognitive relativism as a *second-order* meta-philosophical thesis rather than as a *first-order* epistemological thesis. In doing so he has certainly presented to us a consistent position free from self-referential inconsistency, but this is done at the expense of making an *ad hoc* move largely to save cognitive relativism from refutation. I believe that Meiland's strategy in treating cognitive relativism as a second-order meta-philosophical thesis is *ad hoc* because he makes *no* attempt to show that treating cognitive relativism in this fashion is a correct, informative or interesting way to view cognitive relativism. No reasons are given for us to view cognitive relativism in this way independent of the need to save cognitive relativism from self-refutation. Given that virtually all treatments of the problem of cognitive relativism by philosophers and anthropologists involve viewing cognitive relativism as a first-order epistemological thesis, Meiland's strategy for defending cognitive relativism would be of little interest to

the "working cognitive relativist" in philosophy or anthropology. Meiland then can escape the charge of the self-refutation of cognitive relativism, but only by making cognitive relativism an uninteresting doctrine.

(B) Meiland believes that horn (1) can be escaped even more easily. It would be inconsistent for the cognitive relativist to say both that all doctrines are relatively true and that cognitive relativism is not relatively true but rather, absolutely and objectively true. However all the cognitive relativist need say is that all doctrines, including cognitive relativism are only relatively true. And this is consistent.

Let us take up point (B) first. If the cognitive relativist thesis is that *all doctrines* including cognitive relativism are only relatively true, then we may represent this doctrine as follows:

(P₁) No doctrines are absolutely or objectively true.

Now to generate a self-referential paradox, let us ask whether proposition (P₁) is relativistically true. If (P₁) is absolutely and objectively true, then (P₁) stands open to the immediate generation of a liar-style antinomy if the scope of the quantifier is really *universal*. But if (P₁) is relativistically true, then it follows that from the cognitive relativists' own standards, that there must be a position, absolutism, which the cognitive relativist can only claim is relatively true. By the standards of truth of absolutism however, (P₁) is absolutely and objectively false. But if (P₁) is absolutely and objectively false, then it is false *simpliciter*, false universally from anyone's perspective. Hence if cognitive relativism is relatively true, then it is absolutely and objectively false, and hence relatively false.

This point also applies if cognitive relativism is taken to be absolutely true. Then the liar style antinomy is generated as follows. If (P_1) is taken to be absolutely and objectively true, then (P_1) must include itself within the scope of its quantifier, because (P_1) is a doctrine. But if (P_1) is objectively true, then $(\exists x) OT(x)$. But (P_1) is the statement: $\sim (\exists x) OT(x)$, so that by the conjunction principle we have fallen into contradiction: $(\exists x) OT(x) \ \& \ \sim (\exists x) OT(x)$. The familiar Tarskian metalanguage/object-language distinction does not dissolve this paradox as Meiland seems to think. The problem is that cognitive relativism is *not* a meta-philosophical doctrine. It is not a position which seeks in any way to make comments about the nature of philosophical doctrines in general. Rather it is an epistemological theory of truth. It says that truth is relative to one's culture, historical era and/or perspective. This is a comment about the nature of truth, even if it is not a conceptual explication of the term 'true'. To be sure, the cognitive relativist may be a coherence, pragmatist or correspondence theorist when it comes to presenting a conceptual explication of the term 'true', but the fact remains that doctrines about the nature of truth are no more 'meta-theoretical' than doctrines about the nature of universals, abstract reference and value. Truth is one thing, among others, that philosophers discuss. This being so, Meiland fails to rebut the standard self-referential argument against cognitive relativism.

The paradox of cognitive relativism is generated in a way quite similar to that in which many logical and semantical paradoxes are generated: including too much within its scope. This excessive generality however is unavoidable if cognitive relativism is to stand

as a non-trivial doctrine at all. From the cognitive relativist's own standards of relative truth, absolutism/objectivism must be regarded as being relatively true. This is so because the only plausible account of cognitive relativism is that all doctrines, including cognitive relativism are only relatively true. Hence absolutism is true by its own standards and unique perspective. But absolutism is a position which takes cognitive relativism to be false *simpliciter*. If a doctrine is false *simpliciter*, then it cannot even be relatively true, because if it was, there would exist some perspective from which absolutism/objectivism was false *absolutely* (for it takes only one genuine counter-example to refute a universal generalization) and this is inconsistent with the thesis that by the absolutist's own standards, absolutism is absolutely/objectively true.

It may be objected here that my self-referential argument fails because the absolutist's standards of truth operate only for the absolutist and not for the cognitive relativist. Could it not be that there is some perspective from which absolutism/objectivism was false *absolutely* where this perspective is in fact that of the cognitive relativist's? I do not believe that this is the case for the following reason. If the cognitive relativist claimed that there was a perspective from which absolutism/objectivism was false *absolutely*, then (P₁) is once again contradicted as there is at least one thesis which is absolutely true: that there is a perspective from which absolutism/objectivism is absolutely false. To claim that even this is a relative truth is to say that from the perspective of cognitive relativism, absolutism/objectivism is absolutely false, and this the cognitive relativism cannot say because their claim is that absolutism/objectivism

is only *relatively true*. If the absolutist's standards of truth operate at all, then they operate absolutely, and if they operate absolutely, then they operate for all positions including the cognitive relativist's.

I turn now to a critique of Protagorean relativism. In criticizing Protagorean relativism I shall attempt to perform two tasks; first to defend James Jordan's (1971) self-referential arguments for the inconsistency of Protagorean relativism from the criticisms of Jack Meiland (1979), and second to contribute towards the cause of undermining Protagorean and conceptual relativism by criticizing Meiland's own explication of the notion of relative truth (Meiland, 1977).

Jordan gives the following explication of the position of Protagorean relativism: "... the truth of a proposition is a function of being believed, and that whatever seems true to anyone is true 'for him'" (Jordan, 1971, p. 7) and: "All propositions are true for those who believe them" (*ibid.*, p. 12). This doctrine is, Jordan alleges: (1) simply inconsistent, in implying contradictions and (2) self-referentially inconsistent, in saying contradictory things about itself.

Jordan begins his first argument by noting that the Protagorean relativist is committed to the following line of argument:

- (A) (a) What seems true to anyone is true for him to whom it seems so.
- (b) Proposition (a) is true for anyone to whom it seems so.
- (c) The denial of (a) is true for anyone to whom it seems so.

(d) Propositions (b) and (c) are true for anyone to whom they seem so.

(e) The denials of (b) and (c) are true for anyone to whom they seem so.

Now Protagoras' theory may, like any other allegedly cognitively meaningful theory, be affirmed or denied, and is true in a relativist sense for those who accept it, and false in a relativist sense for those who do not accept it. But if nothing is objectively true, but believing for the believer makes it true in a relativist sense, then the same must be said for claim that there is something to believe. However that there is *any claim* to affirm or deny is held to depend on affirming or believing that there is, involves a plainly impossible state of affairs, since such an affirmation or belief would require the very object which it is supposed to conjure up. The claim that 'There is something to affirm' is true in a relativist sense if and only if affirmed, but that object which is affirmed cannot on Protagoras' view be anything apart from my affirming. If 'There is something to affirm' was true in a relativist sense *independently of this*, then Protagorean relativism would be inconsistent. But on the other hand if the Protagorean relativist denied that there were even objects of beliefs that one may believe or disbelieve, there would seem to be nothing to believe at all, mere empty "believing" or nothing of the sort (*ibid.*, p. 25):

Again, if man is the measure of the being of things that are and of the not-being of things that are not, then there is something for one to affirm or believe if and only if one affirms or believes that there is. The consequent of this implication comes to: "there is something for one to

affirm (viz., the proposition 'there are propositions') and one affirms it". But it is apparent that, if this were true, there would be nothing to affirm. There being something to affirm is made to depend on a condition that could not possibly be fulfilled because it is self-contradictory. In affirming, there must already be something (logically distinguishable from affirming) to affirm, but on Protagoras' theory there is nothing to affirm - no proposition - until one affirms that there is, i.e., affirms the proposition "there are propositions" .

Meiland's criticism of Jordan's argument is that Jordan makes use of a quite ontologically problematic entity - a proposition - to carry his argument through. It is relevant to cite in full Meiland's rejoinder (Meiland, 1979, p. 65):

... the claim that propositions are the objects of belief (or are some ingredients in beliefs in some way or other) is a theory which has been much disputed in the literature. I grant that this theory has its attractions; that is why it remains alive today. But it has engendered fierce opposition too, the opponents holding that belief-contexts and other situations can be analyzed without referring to or postulating propositions. Jordan has not shown that the Protagorean must analyze belief-contexts in terms of propositions. At most, Jordan's first criticism proves that the Protagorean relativist must not embrace an analysis of beliefs in terms of propositions or 'meanings'. But this is a far cry from showing that such an analysis is required or entailed by his relativism .

Meiland is quite correct to point out that Jordan has made an uncritical use of the notion of a proposition. This however is not sufficient to refute his argument, for Meiland would need to show that the use of the notion of a proposition is *essential* to the argument, and that the argument would not succeed without it. This he hasn't done. The argument merely needs the notion that beliefs have an object, and this is quite unproblematic. We may call this object of a belief a *claim* to avoid making reference to propositions if they offend one's ontological tastes. A claim *p* in the belief sentence Bsp need not be an abstract entity as a proposition is standardly taken to be, for *p* may be taken to be some physically respectable entity such as a sentence-

token. Jordan's point then becomes, as I have outlined above, that on Protagoras' theory there is nothing to affirm, i.e., no claim, until one affirms that there is - but this one cannot do, for there could never be anything with respect to which one may consider whether and why one may affirm it, if the above argument of Jordan's is correct.

Meiland however may wish to make the more radical claim that an *intensional* account of belief is incorrect, not merely that propositions or meanings are problematic abstract entities. If relativism must reject any intensional account of belief, then if we are to retain belief talk, such talk must be treated in an appropriate extensional fashion. I do not believe that such an extensionalist position is satisfactory because the extensionalist programme is open to decisive objections (cf. (Routley, 1980)).

Jordan's second argument is as follows. Consider the following statements:

(J) $(p \rightarrow p) \ \& \ \sim (p \rightarrow \sim p)$ ⁴

(B) a believes that J.

Jordan argues that (J) must be true in an objectivist or relativist sense of (B) simply for (B) to be an affirmative claim at all: otherwise Protagoras' theory would not state anything true even for him without stating what was also false even for him. Thus (J) must be "true" prior to and independently of the relativist truth of (B); hence the "truth" of (J) cannot be dependent on the affirming of (J), that is, on (B). Since Protagorean relativism requires that the "truth" of (J) depend on the affirming of (J), that is on (B), Protagorean relativism is self-referentially inconsistent.

Meiland's reply to this argument is to point out that the claim that one cannot believe a statement which implies its own negation is

quite problematic. Meiland could make a quite powerful case for this by appealing to the well known semantical and logical paradoxes and various systems of dialectical and "paradoxical" logic which reject the various 'spread laws' such as $p \ \& \ \sim p \rightarrow q$.⁵ Nevertheless, there is a point to Jordan's criticism. The point is that Protagorean relativism is not an outrightly logical anarchist position where "anything goes", but rather certain basic logical principles must be presupposed if it is to be formulated coherently. For example, if a statement is in fact true only for those who believe it, it is true for those who believe it, and not false for them or of a paradoxical truth-value. The Protagorean relativist could accept this, and weaken his/her position accordingly. But to do so, is not to avoid Jordan's first criticism, a criticism which is sufficient to refute Protagorean relativism.

I shall now turn to the issue of understanding the relativist's idea of "relative truth". There is, apart from Jack Meiland's own work, surprisingly little discussion of this notion. This is surprising precisely because the tenability of Protagorean and conceptual relativism hinges upon the provision of an acceptable explication of this concept. I shall argue that Meiland's own attempt does nothing to eliminate the problematic nature of this concept.

For Meiland, the concept of "absolute truth" is a two place relation between statements or propositions on the one hand, and facts or states of affairs on the other. The concept of "relative truth" is a three placed relation between statements, the world and a third term which is either persons, world views, or historical and cultural situations (Meiland, 1977, p. 571). Thus 'P is true relative to W' is explicated by 'P corresponds to the facts from the point of view of W', where 'P' is a statement or proposition and 'W' a person, world view or historical or cultural situation.

Edmund Husserl in his *Logical Investigations* (1970) has given a number of criticisms of the notion of relative truth. Here two such criticisms will be discussed. One argument seems to be this: what does the term 'true' mean in the expression ' \emptyset is true for W'? It cannot without either circularity of definition or vicious infinite regression mean 'relative truth'. Thus it seems that if the term has a meaning at all, it means 'absolute truth' - and this commits the relativist to the notion of absolute truth, even though the non-triviality of the position requires its denial. Indeed Meiland's style of analysis as we have seen gives rise to a further self-referential inconsistency: let 'W' be the perspective of a set of ideal truth seekers who uphold an absolute concept of truth, then according to the relativist concept of truth, relativism is absolutely false even if it is only relativistically true. But if relativism is *absolutely false*, how could it even be relativistically true?

Meiland's response to Husserl's criticism is to point out that the relativist is not talking about 'truth', but rather 'truth-for-W' and one can no more ask what 'true' means in the expression 'true-for-W', than one can ask what 'cat' means in the word 'cattle' (Meiland, 1977, p. 574). This response however betrays a logical confusion. The word 'cattle' is a *word* and is thus a special type of expression, one in which particles of the word such as the sequence of signs 'cat' do not have a meaning which in any way contributes to the meaning of the word 'cattle'. The term 'true' in 'true-for-W' is not a meaningless sequence of signs. To show this, change the focus of this discussion and consider the expression 'true-in-logistic system L' or 'true_L'. Now according to Meiland's argument we cannot meaningfully ask what 'true' means in this expression. But surely we can, and according to Tarski's theory of truth we can be

given a definite answer. A sentence \emptyset in a logistic system L is true just in case it is satisfied by all sequences; more precisely if \emptyset is of the form ' $F(x_1, x_2, \dots, x_n)$ ' then \emptyset is satisfied by the sequence $\langle 0_1, 0_2, 0_3, \dots, 0_n, 0_{n+1} \rangle$ just in case it is satisfied by the first n members of the sequence. I have no wish to address the question of whether Tarski's theory is an "objective" or a "non-objective" account of truth, and nor is it necessary to do this to develop my objection to Meiland. Rather I have shown that the term 'true' in the expression 'true-in-logistic system L ' is not like the term 'cat' in 'cattle'. In my emphasized sentence the term 'true' is not a meaningless sequence of signs like 'mil' is in 'smile' in English. Likewise there is no reason for us to say that the term 'true' in the expression 'true-for- W ' is like the term 'cat' in 'cattle'. Husserl's objection to Meiland's type of analysis of the concept of relative truth, is in my opinion substantially correct.

Husserl has a second objection (1970, p. 142):

On a relativist view, the constitution of a species might yield the 'truth', valid for the species, that no such constitution existed. Must we then say that there is in reality no such constitution, or that it exists, but only for us? But what if all men, and all species of judging beings, were destroyed, with the exception of the species in question? We are obviously talking nonsense. The notion that the nonexistence of a certain constitution should be based on this very constitution, is a flat contradiction...

Meiland responds to this objection as follows: Husserl's objection shows at best that relativism is *falsifiable* in principle. This, Meiland believes is a virtue and not a vice of a theory. Quite so. But the question not addressed by Meiland is: 'What does falsifiable mean here?'. We have seen after all, that Meiland is attempting to analyse the *meaning* of the notion of relativist truth, rather than present those *criteria* by

which we judge a statement to be either relativistically true or false.

Thus 'falsifiability' here could only mean 'absolutely falsifiable'.

The relativist then thereby finds out that his/her own theory of truth is false for him/her, whilst at the same time expressing this fact in terms of this very theory by saying what is false for him/her. Thus to avoid Husserl's contradiction, the relativist must both assert and deny the theory at the same time.

Meiland alleges that a statement such as (P):

(P) There is no constitution of the human species

could conform to the criteria of truth used by the relativist, so that if the relativist retains the criteria of truth, then the theory of truth must be changed, so that (P) will come out to be absolutely true (Meiland, 1977, p. 576). But this is once more a contradiction. The criteria of truth for a relativist are criteria for *relative truth*. It is logically impossible for (P) to come out as absolutely true by a set of truth criteria tailor-made to say when a statement is relatively true: this is simply outside their domain of applicability. This being so, Meiland's rejoinder collapses.

I have attempted to defend James Jordan's recent self-referential arguments for the inconsistency of Protagorean relativism from Jack Meiland's criticisms. Second, I have argued that Meiland's own account of relative truth does not escape objections which have been presented by Edmund Husserl. *Therefore relativism in metaphilosophy cannot be reached, if my arguments are sound, from the roads of cognitive and Protagorean relativism.*

3. AGAINST ORIENTATIONAL PLURALISM IN METAPHILOSOPHY

I turn now to an examination of Nicholas Rescher's (1978) position of *Oriental Pluralism*, which Rescher explicitly advances as a solution to the problem of perennial philosophical disagreements.

In Chapter 1, three broad explanations of the existence of perennial philosophical disputes were stated: (1) *sociological explanations*, (2) *methodological explanations* and (3) *eliminative explanations*. Opposed to these three views, Rescher takes the source of philosophical discord to lie in the very *modus operandi* of philosophical inquiry itself. Perennial disagreement is a feature of the very nature of philosophy and is not a ground for cognitive despair or scepticism about the intellectual value of the discipline as a whole. The source in turn of this diversity and discord lies in both (1) the problems of philosophy, (2) the solutions to such problems and (3) the arguments used to defend these solutions. Rescher states (*ibid.*, p. 220):

(R₁) ... the root cause of diversity lies in a combination of these three factors in that *philosophical issues are always such that arguments of substantial prima facie cogency can be built up for a cluster of mutually incompatible theses*. Philosophical argumentation is accordingly *nonpreemptive*: the existence of one cogent resolution of an issue does not block the prospect of an equally cogent basis for its alternatives; by positive argumentation an excellent case can be built up in substantiation of each of several mutually incompatible theses. It is the virtually characteristic feature of philosophy that its problems are such that eminently plausible arguments, arguments that strike the doctrinally uncommitted ear as having more or less equal cogency, can be built up on mutually incompatible sides of the issue.

In philosophy, supportive argumentation is never alternative-precluding. Thus the fact that a good case can be made out for giving one particular answer to a philosophical question is never to be considered as constituting a valid reason for denying that an equally good case can be produced for some other incompatible answers to this question. The diversity of philosophical doctrine is rooted in the pervasiveness of such *aporetic clusters*, as one may call them. Every philosophical problem thus admits of a variety of mutually conflicting solutions on whose behalf an impressively cogent case can be made out .

The passage (R_1) might strongly suggest that for any philosophical thesis T_R , a substantial *prima facie* case can be built up for $\neg T_n$, a claim which does seem plausible. However other passages in Rescher's paper suggests that not merely a *prima facie* case can be built up for incompatible theses, but a *de facto* case. He states for example, that philosophical problems are much like the various solutions to the logical and semantic antinomies: each proposed solution must do violence to at least some of our fundamental intuitions concerning the subject matter at issue (ibid., p. 223).

An attempt to eliminate some of the inconsistent n-ad of propositions that comprise an aporetic cluster of philosophical theses by eliminative argumentation, does not improve matters Rescher believes. The acceptability of overall philosophical argumentation depends upon the acceptability of the conclusions reached. If however one cannot evaluate the strength of a philosophical argument independently of assessing the acceptability of its consequences, then we must already be in a position to assess the relative merits of the theses at debate in the controversy (ibid., pp. 224-225). Rescher proposes that an "essentially evaluative methodology" for the cost-benefit assessment of doctrinal positions eliminates the above circularity problem. The *modus operandi* of this method consists in a consideration and weighting of the various alternatives vis-à-vis parameters of cognitive merit and demerit - such as consistency, comprehensiveness, economy, explanatory adequacy and so on. However, whilst Rescher is quite right to point out that such methodological orientations are not theories and doctrines, insofar as they are taken to embody judgements of plausibility in the assessment of arguments, they must still be viewed as being arguments. This is so because judgements of plausibility made in the assessment of arguments

are arguments about arguments, they are sets of reasons evaluating other sets of reasons. To judge on the basis of cognitive parameters of merit or demerit is to argue. If this is not what one is doing in making judgements on the basis of cognitive parameters, then Rescher has left the notion of such judgements unacceptably vague. If so, then the evaluative argument is open to Rescher's above circularity objection, hence generating a vicious infinite regress. The regress is vicious because if Rescher's cost-benefit assessment of doctrinal positions by reference to various cognitive values involves argumentation, then the acceptability of *this very argument* must, he tells us, depend upon the acceptability of the conclusions reached. The acceptability of the evaluative conclusion here is clearly dependent upon the acceptability of the cost-benefit premises. This is so because if we are to make a conclusion about the cognitive value of any position, then we must argue from premises which must consist of cost-benefit facts about a position. Yet if Rescher's views are accepted we must already be in a position of being able to independently assess the acceptability of the conclusion (about the cognitive value of some position) before appealing to the premises of the argument (consisting of cost-benefit facts about a position). This is a contradiction. Hence the regress, which Rescher believes is harmless, is *vicious*. Therefore I find Rescher's view about the assessment of philosophical positions totally unacceptable.

The case which I wish to now build up here against Rescher, is that contrary to his denials, he is committed to Protagorean relativism in matters metaphilosophical. Before detailing his position of Orientational Pluralism, I will cite one further piece of evidence for my interpretation of (R_1): Rescher himself draws the same parallel between his own position and Protagoras' contention that every issue can be disputed

with equal validity on either side, including this issue itself (ibid., p. 251).

Oriental Pluralism Rescher defines as the metaphilosophical position "that there are different and ... equally eligible alternative evaluative orientations which underwrite different and mutually incompatible resolutions of philosophical issues" (ibid., p. 229). On this view there is no such thing as a uniquely correct answer to a philosophical problem, rather, the best that one can do is to establish optimal tenability against a pre-established probative-value orientation. Likewise for the concept of philosophical truth. One cannot occupy more than one of these "probative-value orientations" at once, and unless two debaters agree on such methodological first principles, rational argument and the possibility of ultimate consensus will not occur. This ensures, Rescher believes, that a variety of incompatible solutions to any philosophical problem will exist, and that pluralism in philosophy is inescapable.

With regard to metaphilosophical matters, Rescher accepts an inevitable pluralism. However from any orientational perspective there is only one "correct" solution to a philosophical problem. "Correctness" here is a term defined within the meta-language of the particular orientational perspective. This combination of doctrinal unique correctness, with metaphilosophical pluralism or relativism is taken by Rescher to constitute a quite attractive position (ibid., p. 241):

Oriental pluralism enables us to have it both ways, so to speak. For on its teaching we can and should work out our own answers to philosophical problems (in a way that is rationally sound and altogether cogent relative to our own methodological perspective of consideration), but nevertheless we need not thereby feel compelled to dismiss as mistaken and misguided the work of colleagues whose conscientious labors lead them to other solutions. We can be fervent in our attachment to our own position

without writing off as altogether worthless the work of our competitors in the field .

Despite Orientational Pluralism's happy liberalism, I shall argue that the position is demonstrably unsatisfactory as a view of metaphilosophy.

As one would expect, Rescher's own position rests upon various philosophical theses. First is Rescher's claim that whilst in the sciences, the acceptability of a conclusion depends ultimately on the merits of the presented argument, in philosophy the strength of a philosophical argument cannot be judged independently of an assessment of the acceptability of the conclusion of the argument. We saw previously the conclusion which this very argument led us to, a rejection of the very possibility of arguing for any philosophical thesis at all. The very point of advancing philosophical arguments is to argue for specific conclusions. If however we must *already* be in a position to assess the acceptability of such conclusions before advancing our premises, then such argumentation involves us in a vicious circle. This circle I argued previously, is not broken by Rescher's own methodological considerations. If philosophical arguments *cannot* be assessed independently of an assessment of the acceptability of the conclusion of the argument, then philosophical argumentation is otiose because no premise set could ever convince one of the truth of a conclusion that one judged to be false (recall that it is sufficient to show that an argument is valid and has true premises, to show that its conclusion must be true).

Rescher's own conclusion is extremely implausible, and we should in accordance with his position, reject his own argument. Fortunately we can do this for good reasons. In philosophy, as in science, we *do* assess arguments through the acceptability of the principles that constitute the *terminus a quo* of its argumentation. This is a sociological fact and the reader can convince him/herself of this from studying journal

articles in *The Journal of Critical Analysis*, *Mind*, *Analysis*, *The Review of Metaphysics* and so on. Rescher's descriptions of philosophical inquiry do not conform to philosophical practice. It is however true that frequently the consequences which afford the *terminus ad quem* of an argument may lead us to reject the argument - only however by leading us back to its premises, with which we must find faults, or to its logical form which may be seen to be invalid. Arguments are not rejected by honest and rational philosophers merely because they have unacceptable conclusions; they are rejected because they are unsound. Therefore Rescher's argument is unsound, and cannot be reasonably cited in defense of Orientational Pluralism.

The second argument in support of Orientational Pluralism is (R_1). Here we must claim that a substantial *de facto* case can be built up for both T_n and $\forall T_n$ if we are to support Rescher's own metaphilosophical position. Earlier I gave textual reasons for this claim, now I advance an argument. Merely to claim that *prima facie* reasons can be given for both theses T_n and $\forall T_n$ is a rather trivial and uncontroversial observation. This proposal is consistent with the position that there is a uniquely correct solution to each philosophical problem, for the *prima facie* reasons may after debate be shown to be quite illusory. It is only if the Protagorean claim that every issue can be disputed with equal validity on either side, including this issue itself, that we have anything approaching an argument for Orientational Pluralism. If we do claim to have such an argument, then we need good reason to believe that this thesis is true:

(PRT) Every issue can be disputed with equal validity on either side, including the issue as to whether (PRT) is true or false or indeterminate in truth value.

Now we require as I said good reason to believe that (PRT) is true. Thus suppose (A_1) is an argument for this claim. Then if (PRT) is true, there exists an argument (A_2) which establishes with equal validity the invalidity of (A_1). This one would take to establish that (PRT) cannot be believed with good reason to be true. But let us think more carefully. There will also exist another argument (A_3) against (A_2), an argument (A_4) against (A_3) and so on. Proceeding in this way we reach a point where we must throw up our hands in despair, and admit that we do not know how to classify (PRT) in truth-value. Surely to say that (PRT) is even indeterminate invites the rebuttal that there must be a *de facto* counter-argument against even that claim! If this is so, then we cannot claim that (PRT) is true, or false, or make any satisfactory affirmative claims about it at all. Not even that it supports Orientational Pluralism.

This then seems to me to rebut the two arguments which Rescher advances in defense of his position. Still, the question remains, are there good reasons to accept Orientational Pluralism? I will now argue that Orientational Pluralism is unsatisfactory because like all affirmations of cognitive relativism it is both trivial and self-referentially inconsistent.

Relativism in its cognitive form proposes that either "truth", "correctness", "rationality" or some other cognitive standard is framework-internal and contextually relative. In Rescher's Orientational Pluralism in metaphilosophy for example, orientational perspectives cannot be judged in an "objective" framework independent way. If this is the claim that in the very act of judging and arguing one must presuppose various things such as the coherence of reasoning, Rescher is quite right, but the thesis is then trivial. Even if one knew what the non-

relative truth is, one would at least need a language to communicate this. To make a substantial claim Rescher must rule out the validity and satisfactoriness of *cognitive objectivism*, the metaphilosophical position which according to its own probative-values claims that Orientational Pluralism is invalid and unsatisfactory.

Talk of "validity" and "satisfactoriness" here must not be understood in a question-begging objectivist fashion. Virtually all self-referential arguments against relativism have failed in the past because of their viciously circular implicit acceptance of objectivist accounts of truth, rationality and so on. So let us then speak entirely of validity and satisfactoriness in an orientational perspective. This is harmless neutral talk because validity and satisfactoriness for the objectivist *is* precisely the denial of the relativist's account, and we can readily conduct the appropriate translation when the time requires this.

According to the relativist, in this case the Orientational Pluralist, there is a plurality of perspectives with no one of them objectively right. One of these perspectives is objectivism. According to Orientational Pluralism, objectivism is a perspective to which its own claims of relative-validity apply. According however to objectivism, Orientational Pluralism is objectively false, invalid and unsatisfactory. But the Orientational Pluralist must accept this: "yes, according to that perspective, my position is unacceptable". This confession leads however to the conclusion that Orientational Pluralism must be *objectively* false, invalid and unsatisfactory. Let us spell out why this is so more clearly.

For Orientational Pluralism, the denial that there is a uniquely correct position involves treating all positions of equal epistemic worth.

To avoid obvious self-referential inconsistency, this claim must also be made of Orientational Pluralism itself. If it is made, then objectivism cannot without inconsistency be ruled out of equal epistemic worth to Orientational Pluralism. This in fact means that Orientational Pluralism has no good *context-dependent* arguments against objectivism. If it did, then objectivism would not be of equal cognitive worth to any other orientational perspective, and this would contradict the very definition of "Orientational Pluralism". However without such excluding arguments, we have equally good context-dependent reasons for accepting objectivism over Orientational Pluralism. Unfortunately objectivism entails the falsity, invalidity and unacceptability of Orientational Pluralism. Hence Orientational Pluralism allows objectivism to survive as a live option, and it is this very option which once alive strangles the very acceptability of Orientational Pluralism.

4. UNGER'S HYPOTHESIS OF PHILOSOPHICAL RELATIVITY

Peter Unger (1984) has attempted to cast doubt upon the thesis that the traditional problems of philosophy have definite objective answers. Perhaps there are really no objective answers to most philosophical problems, neither "commonsensical" nor "sceptical", Unger suggests. If this was so, then it would explain the lack of progress which has been made with respect to the solution of the principal problems of philosophy (*ibid.*, pp. 4-5). This position readily solves the problem of perennial philosophical disputes, because if it is "correct", then there have never really been any genuine philosophical disagreements at all. Nor is there any objective philosophical knowledge. This at least seems to be his position as stated in chapter 1 of *Philosophical Relativity*. However later in the book (*ibid.*, p. 115) Unger qualifies his position.

"Philosophical relativity" exists only in areas whose key terms generate disputes between *contextualist* and *invariantist* theories of semantics. To outline Unger's hypothesis of philosophical relativity and the principal thesis of his book, some basic terms must now be explicated.

According to the hypothesis or thesis of philosophical relativity "[o]ne position on a philosophical problem is to be preferred only *relative* to assumptions involved in arriving at its answer to the problem; an opposed position is to be preferred only relative to alternative assumptions; there is nothing to determine the choice between the diverse assumptions and, hence, between the opposed positions" (*ibid.*, p. 5). Situations of philosophical relativity typically arise because of *semantic relativity*: "[one] set of assumptions leads to one semantic interpretation, another set leads to another, and there is nothing to decide objectively in favor of either set" (*ibid.*, p. 5).

Unger argues for semantic relativity by arguing that two general semantical theories, contextualism and invariantism, conflict. Contextualism is a thesis about the interpretation of predicates and terms in a language: to say that x is F means that x is F according to *contextually relevant standards*. A surface x may be said to be *flat* if according to contextually relevant standards the surface is sufficiently close to being absolutely flat. The thesis of invariantism states that the interpretation of predicates and terms in a language only involves contextually relevant standards in the evaluation of demonstrative subject terms rather than predicates. The uttered sentence 'that surface is flat', means for the invariantist, that such a surface is sufficiently close to being absolutely flat, that nothing could be flatter, but it could be equally as flat. According to the hypothesis of semantic relativity, there is no objective fact of the matter about which general semantical

theory is true, so that neither position is correct to the exclusion of the other.

Philosophical relativity arises because "for each problem studied, an invariantist can assign a semantics to the philosophically important terms that is comfortable to a skeptical view on the problem, and a contextualist can, with equal propriety, assign a semantics that is comfortable to the commonsense position on the problem, antithetical to the skeptic's position" (ibid., p. 46). It is Unger's position that philosophical relativity exists in any area, whose key terms involve a dispute between the contextualist and invariantist (ibid., p. 115). As an example of philosophical relativity we consider the statement 'S knows that the drug is safe; a drug tester told S'. In another context someone else may say: 'S does not know that the drug is safe; the drug tester could be lying because she is after all employed by the company'. Are these two statements contradictory? For the contextualist they are not because the context of the statement of the denial of S's knowledge claim probably has a higher standard for what alternatives must be excluded for a knowledge claim to be warrantably asserted, whilst the context of the knowledge claim probably employed a lower standard. But for the invariantist there is a genuine contradiction here, for surely S has knowledge or S does not. On Unger's relativity hypothesis neither epistemological position is correct at the expense of the other.

It may be thought that there is indeed a decidable issue between scepticism and commonsense epistemology. A commonsense epistemology seems to most of us *intuitively* more satisfactory than scepticism. Intuitions seem to many to be a fact of the matter which gives one more reason to believe that commonsense epistemology is true than it does to believe that a sceptical epistemology is true. Unger in reply argues that for

a subject to have an intuition he/she must have a belief and that belief must be true or correct. If this is not so then there will be no genuine intuition to which the theory must conform. If one has an intuition that a surface is flat, then it must be true that the surface is in fact flat. But the question of whether or not the surface is flat is precisely the issue at debate here. This objection amounts to a mere denial of Unger's relativity hypothesis, not an argument against it and so commits the fallacy of reason, *petitio principii*.

The first argument to be given here against Unger is an argument from self-referential consistency. Let us ask whether or not there is an objectively right answer which may be given to the philosophical problem of philosophical relativity? Is there a fact of the matter about the truth of the thesis of philosophical relativity? There would not be if the thesis of semantic relativity was applicable to any of the key terms involved in this question. It seems to me that the debate between the contextualist and the invariantist is quite relevant here. The terms 'relativity' and 'fact' are open to be same relativity which *prima facie* faces the terms 'knowledge' and 'flat'. The contextualist will find the thesis of philosophical relativity quite counterintuitive and incorrect, involving unreasonably high standards of objectivity and evaluation. The invariantist will opt for a sceptical view of these standards of objectivity and evaluation. The consistent philosophical relativist must claim that there is no fact of the matter enabling us to not only rationally choose through considerations of verisimilitude, philosophical relativity or philosophical objectivity (the negation of the thesis of philosophical relativity), but as well, there is no fact of the matter about the truth of the thesis of philosophical relativity, and no fact of the matter by which the thesis of philosophical relativity

may be taken to be true.

If there is no fact of the matter about the truth of the thesis of philosophical relativity, and no fact of the matter by which the thesis of philosophical relativity may be taken to be true, then Unger's book is dealing then with a pseudo-problem. This no doubt undermines his position, for it makes nonsense of his attempt to criticize opposing semantical theories such as causal theories (*ibid.*, pp. 77-104) - Unger seems to slide back into the position of a traditional philosopher at this point. However it is clear that if the thesis of philosophical relativity is upheld, then there is also no fact of the matter about the truth or falsity of causal theories of meaning and reference. This is so because the concept of *causation* suffers from semantical and hence philosophical relativity (*ibid.*, pp. 58-60). Unger falls into inconsistency in presenting *objections* to causal theories of meaning and reference. Yet if he does not, his own position remains *unjustified*.

It would be a mistake to conclude that Unger's position is incorrect. First, it may well be that the thesis of philosophical objectivity is also inflicted by philosophical relativity as well. Unger in reflection upon the possibility that his thesis of philosophical relativity is infected by philosophical relativity has this to say (*ibid.*, p. 44):

As far as I can see, any lack of determinacy, or of objectivity, in our account of compatible elasticities will only mean *even more* relativity than we have so far articulated, not no relativity at all. Statements that our language has such and such a range of allowable semantic interpretations, with such and such a range of correlative pragmatic employments, with themselves be true only relative to certain higher-order explanatory posits, each (group) of which will exclude the others. From a relativistic perspective, we can then say this: There will be nothing objective to decide matters between any two such higher-order alternatives .

It thus seems that the thesis of philosophical relativity leads to "an *infinite* hierarchy of arbitrary assumptions", that must be made "in

order to resolve conventionally an infinity of compatible elasticities" (ibid., p. 45). Thus we cannot even say that the thesis of philosophical relativity is *indeterminate* in truth value, for there is no fact of the matter about which the thesis of philosophical relativity may be taken to be indeterminate in truth value. This I take as a *reductio ad absurdum* of the position itself and a decisive argument in favour of philosophical objectivity. Unger may object here that my argument, much like the appeal to intuitions, begs the question against the thesis of philosophical relativity. However this can hardly be the case. For someone to beg the question in a debate there must be a determinate question to beg. If the thesis of philosophical relativity is upheld, then this cannot be so, because the very thesis itself entails that there is no fact of the matter by which a truth value may be assigned to this thesis.

If the thesis of philosophical relativity is rejected, then how can the thesis of philosophical objectivity be upheld? How can the dispute between the contextualist and the invariantist be resolved? I do not believe that there is a genuine dispute between the contextualist and the invariantist. The contextualist theory will at most apply to certain expressions and the invariantist theory will apply to certain other expressions. The hypothesis or thesis of philosophical objectivity asserts that there is no situation where the two positions will apply with equal justification to a single expression. The support which can be given for this thesis can only be inductive because I know of no way of demonstrating any inconsistency in the thesis that there is (at least) some expression for which no rational choice can be made for either contextualism or invariantism. However neither can Unger supply such a demonstration. It is for this reason that I have used the terms 'hypothesis' and 'thesis'

interchangeably. The 'thesis' or position of philosophical objectivity is open to rational justification, it is not a conjecture in Popper's sense. It remains however *hypothetical*, a position which cannot be as rigorously supported as we would demand most positions in philosophy to be open to.

My refutation of semantic relativity ironically proceeds along the same lines as Unger sketches as a refutation of Quine's thesis of the *indeterminacy of translation* (as outlined in chapter 2 of Quine's *Word and Object* (1960)). Unger seems to accept Quine's point that there will be indeterminacy of translation not only for cases of *radical translation*, but also in our own attempts to state in English or some other natural language N, the semantics of English or N. Behaviour itself is an inadequate ground for the rational choice of alternative behaviourally equivalent translation manuals; indeed as this thesis has been stated this claim appears tautological. So let us grant this to Quine. However as Unger notes "[o]ur translations, in effect our semantic theories, must accord with the rest of what we hold true, not only psychology but neurology, information theory, sociology, and more" (Unger, 1984, p. 19). Semantics then is not an isolated field of research. Other scientific and metaphysical theories stand as aids in semantical research. But if this is so, then Unger's entire book can be undermined: the debate between contextualists and invariantists is not unsolvable or indeterminate, because the truth of these positions in their application to expressions will depend upon arguments and theoretical considerations made on the basis of our best scientific theories. The very same criticism which Unger has made against Quine can be made against his own position.

Let us illustrate this solution to Unger's problem of philosophical relativity by some examples. Invariantism is the correct semantical

position to take for many terms of science. A logistic system L is said to be *trivial* if an arbitrary wff W^* is provable. But a logistic system L^* even if it is a logical extension of L , cannot be *more* trivial than L , if L^* is trivial. This is so because in both L and L^* every wff is provable. (To avoid some technical objections we assume that L and L^* have the same syntax and semantics, the same signs and formation rules, but not necessarily the same axioms for non-natural deductive systems.) The same argument can be repeated for other terms such as 'complete', 'theorem', 'argument', 'justified' and so on - these are clearly *absolute terms*.

I cannot however agree with Unger when he takes the term 'flat' to be an absolute term that is equally open to an invariantist treatment as a contextualist treatment. It is worthwhile stating my reasons for believing this, as *Philosophical Relativity* consists in the best part of a discourse on the semantics of 'flat'. If 'flat' is an absolute term, without special context-sensitive semantic features, then it is inconsistent to say that something is flatter than something that is said to be flat. Yet we do say this. To preserve consistency of common usage, we must reject the claim that 'flat' is a semantical term without special context-sensitive semantic features. A surface can be flatter than a flat surface, just as an object can be larger than a large object, if 'flat' is analyzed contextually rather than invariantly. Such an analysis is preferable because it is intuitively correct and saves common usage from what would be a serious inconsistency. To adopt an analysis that leads to inconsistency, and which conflicts with intuition, seems to me to not only beg the question against commonsense semantics, but to be a perverse insistence to see tragedy and misery where none need be seen at all.

It is not necessary to debate with Unger the semantics of terms such as 'knows', 'truth' and others of relevance to philosophy. Here I have attempted to cast doubt upon his claim that no rational choice can be made between the semantical theories of contextualism and invariantism. I have argued that the very same criticisms which Unger has made of Quine's indeterminacy of translation thesis undermine his own position. Further, I believe that Unger's thesis of philosophical relativity leads to a trivializing indeterminacy that cannot be rationally viewed as other than a *reductio ad absurdum* of his position. I therefore reject his position.

5. RORTY'S METAPHILOSOPHICAL SCEPTICISM

Richard Rorty's book, *Philosophy and the Mirror of Nature* (1979) is an attempt to undermine our confidence in the traditional view of philosophy and its problems including: "the mind" as something about which one should have a "philosophical" view, in knowledge as something about which there ought to be a "theory" and which has "foundations", and in "philosophy" as it has been conceived since Kant" (ibid., p. 7). The key to understanding this book is Rorty's "metaphilosophy": the problems which philosophers have been concerned with throughout the history of philosophy are pseudo-problems, resting upon false assumptions. The point is not to provide new philosophical theories to solve these problems, but rather to reject the tacit claim that these problems are in fact coherent and capable of solution. In the case of the mind/body problem our difficulties are generated by an intuition about the nature of the mental which is in fact, "no more than the ability to command a certain technical vocabulary - one which has no use outside of philosophy books and links up with no issues in daily life, empirical science, morals

or religion" (ibid., p. 22).

In this section I will approach the task of criticizing Rorty's book through examining his "metaphilosophy", this being the material in part three of his book. If this material proves to be a fabric of illusions, as I shall argue is the case, at best, Rorty has only managed to criticize a limited area of philosophy. He will thus fail to undermine our confidence in the traditional view of philosophy and its problems.

Rorty rejects three central themes which he takes as representative of mainstream philosophy. First is the Platonic doctrine of truth and knowledge, according to which, truth is correspondence with "nature" or the "world", and knowledge is a matter of possessing accurate representations. Second is the Cartesian doctrine of the mind as a private inner mirror which "mentalizes" and represents outer reality. Third is a Kantian conception of epistemology, which takes the proper task of epistemology to be to set universal standards of rationality and objectivity for all actual and possible claims to knowledge. If one rejects these three central themes of mainstream philosophy what enterprise does one place in the intellectual vacuum now existing?

Strictly speaking Rorty does not wish for any enterprise to fill this space, and takes *hermeneutics* as "an expression of hope that the cultural space left by the demise of epistemology will not be filled" (ibid., p. 315). Epistemology must assume that all discourse is commensurable - that is, brought under a set of rules which will tell us how rational agreement can be reached. Hermeneutics is an explicit struggle against this assumption (ibid., p. 318):

Hermeneutics sees the relation between various discourses as those of strands in a possible conversation, a conversation which presupposes no disciplinary matrix which unites the speakers, but where the hope of agreement is never lost so long as the conversation lasts. This hope is not a hope for the discovery of antecedently existing common ground, but *simply*

hope for agreement, or, at least, exciting and fruitful disagreement. Epistemology sees the hope of agreement as a token of the existence of common ground, which perhaps, unbeknown to the speakers, unites them in a common rationality. For hermeneutics, to be rational is to be willing to refrain from epistemology - from thinking that there is a special set of terms in which all contributions to the conversation should be put - and to be willing to pick up the jargon of the interlocutor rather than translating it into one's own. For epistemology, to be rational is to find the proper set of terms into which all the contributions should be translated if agreement is to become possible. For epistemology, conversation is implicit inquiry. For hermeneutics, inquiry is routine conversation. Epistemology views the participants as united in what Oakeshott calls an *universitas* - a group united by mutual interests in achieving a common end. Hermeneutics views them as united in what he calls a *societas* - persons whose paths through life have fallen together, united by civility rather than by a common goal, much less by a common ground .

This position which views hermeneutics and epistemology as ideal opposites is supported by considerations of the "hermeneutic circle". According to the idea of the hermeneutic circle, understanding of a culture, language, theory or whatever is impossible unless we understand, already, if only vaguely, the totality or "whole" of the object of understanding. Understanding is a back-and-forward shuffle from the parts to the whole, where our conjectures are corrected at each stage of the shuffle. This is much like coming to know a person, or acquiring a new skill. Epistemology on the other hand, is taken by Rorty to propose that certain processes are "basic" or "foundational". The general holist line of counter-argument against foundationalism is evident: alleged basic elements cannot be isolated without already prior knowledge of the conceptual or theoretical framework in which these elements feature (ibid., pp. 318-319).

Rorty attempts to generalize Kuhn's well known (and well criticized) idea of a paradigm, to apply it to discourses in general, including philosophy. In rejecting totally the idea of knowledge involving accurate representation of reality, we are left to take philosophy as

a mode of interparadigmatic conversation, where the purpose of "edification", education or self-formation become key goals. Part of our edification comes from understanding the outright impossibility of "systematic philosophy". Edifying philosophy is not a new philosophical theory, it is reactive against normal philosophy, that is systematic philosophy. Edifying philosophy is a medicine which cures us of the disease of epistemology. Among its most notably general practitioners, Rorty includes Dewey, Wittgenstein and Heidegger (*ibid.*, pp. 367-368). All three thinkers have called into question the traditional Western philosophical notion of philosophical truth because all three thinkers have called into question the idea of knowledge as accurate representation.

The implications of such an orientation are radical indeed. Strictly speaking good and consistent edifying philosophers must reject the notion of being committed to, and defending a philosophical position at all. To do this, one may well propose that to say something is not necessarily to express a view about something, or to claim that some proposition is true. A conversation may no more represent external reality than a casual sexual affair may represent long term emotional satisfaction. For the edifying philosopher what is important is *communication*, not necessarily communicating the truth.

This completes my summary of Rorty's metaphilosophy. I believe that the book *Philosophy and the Mirror of Nature* can only be satisfactorily understood by first understanding Rorty's overall metaphilosophy. For the purposes of critique we may isolate the following propositions, which if they were demonstrably unreasonable, would demonstrate the unreasonableness of Rorty's overall metaphilosophical orientation:

(R1) Mainstream philosophy is fundamentally flawed.

(R2) Mainstream philosophy, and in particular epistemology, is irreconcilable with hermeneutics.

(R3) Rejecting the ideas of mainstream philosophy is in general a good thing, an edifying and satisfying thing to do.

Let us briefly outline why Rorty must accept (R1), (R2) and (R3).

(R1) is something which he has explicitly stated. (R2) is something which he has implicitly stated in contrasting epistemology and hermeneutics. (R3) is something which Rorty must accept under the pain of having his proposals made in his book dismissed as irrelevant. Yet in stating that Rorty is making "proposals" we seem to be engaged from the outset of our inquiry in a *petitio principii*. Let us first address this question.

If Rorty claimed to be actually arguing for the proposals which we have cited above in section I, then *Philosophy and the Mirror of Nature* would be outrightly incoherent. Even if one claimed to be able to perform a grand *reductio ad absurdum* of epistemology, one is still in fact arguing. But Rorty we have seen rejects the ideal of philosophy as a discipline which has as its chief activity the presentation of rational arguments. Thus it seems that Rorty's critics⁶ have grossly misunderstood *Philosophy and the Mirror of Nature* by presenting counter-arguments to what they in fact take to be Rorty's arguments. Rorty's text must, if it is to be consistent, be edifying rather than analytical, systematic and cognitive. This text then must not be concerned about metaphilosophical truths, such as the alleged problems of mainstream philosophy, but must itself be a conversation, a matter of hermeneutic sounds, which may or may not be aesthetically pleasing. Hence if Rorty is making truth-claims, and claims in some way to be accurately *representing* the state of mainstream philosophy, then he is inconsistent.

Suppose that Rorty is consistent. Then he must claim that *Philosophy and the Mirror of Nature* is a non-epistemological book. Rather it seeks to edify, educate and express in literary form certain values, ideas and dreams of its author. This in itself does not mean that Rorty's text is not without value. Many would agree that it is a fine piece of writing. But this does nothing to cure the systematic philosopher from the epistemic disease which Rorty believes that he suffers from. Thus if Rorty argues with the systematic philosopher - even to produce a *reductio ad absurdum* of systematic philosophy, he is inconsistent. If Rorty does not argue with the systematic philosopher, then no critique of systematic philosophy is presented, and his hermeneutic sounds are at best irrelevant to the epistemologist.

Does this style of argument from self-referential consistency beg the question against Rorty? I think that this is not the case. To speak of "begging a question", presupposes that there exists an argumentative framework from which questions may be begged. Thus if Pro begs the question against Con, Pro is advancing a thesis T which Con would reject and Pro has no satisfactory independent argument to demonstrate that Con's criticism or rejection of T is unreasonable. This is not a rigorous definition of the expression "begging the question", but serves to illustrate the view that such a definition would already presuppose the idea of an *argument*. If however the very basic act of the giving of arguments is abandoned, so too must go ascriptions such as *petitio principii* which presuppose an argumentative framework.

Rorty's position suffers from inconsistency in another dimension as well. Here the problem is that one would expect an edifying philosopher to have abandoned metaphysics: Rorty is however not strong enough in will to surrender his eliminative materialism and determinism. He states

for example (ibid., p. 354).

(P1) To sum up what I want to say about the "irreducibility" of the *Geisteswissenschaften*, then, let me offer the following theses: Physicalism is probably right in saying that we shall someday be able, "in principle", to predict every movement of a person's body (including those of his larynx and his writing hand) by reference to microstructures within his body .

This passage as far as the present author is concerned, can only be understood as a tacit acceptance of the notion of philosophical truth - otherwise Rorty's own physicalism must be understood non-cognitively and the above passage becomes incomprehensible. But if this passage is a tacit acceptance of the notion of philosophical truth, then *Philosophy and the Mirror of Nature* is inconsistent. Passage (P1) is not a mere isolated fragment of Rorty's text, but clearly sums up the structure of a very major argument of the text, so the inconsistency cannot be regarded as a local one, nor one which is trivial.

We turn now to a consideration of the propositions (R1), (R2) and (R3) which we shall assume are propositions which Rorty should have defended by reasoned argument. The argument for (R1) consists of three parts. The first part is an historical argument which catalogues the failures of systematic philosophy to solve its basic problems, and hence to arouse our scepticism about the cognitive validity of the perennial problems of philosophy. We should recall, that Rorty equates epistemology with the attempt to achieve universal commensurability, and the existence of perennial philosophy disputes does as such, threaten the rationality of philosophy.

If philosophy, and especially epistemology was committed to the idea of commensurability, then Rorty has by appeal to the history of philosophy, a strong argument for (R1). But the epistemologist need not accept that progress in a discipline consists of agreed solutions to

problems. For one thoroughly immersed in the Platonic ideal of knowledge, it may simply be the case that agreement is nothing more than agreed ignorance or the agreed acceptance of falsehoods, and extensive disagreement may mean that many parties at a dispute are simply wrong. Progress in philosophy is a much richer notion than that which Rorty would have us accept. Plausible solutions to philosophical problems may require more sophisticated answers than those which have been yet given. Philosophical questions are not easy and test the human mind to its cognitive limits. Progress must then occur when problems are defined more sharply, and irrelevant issues removed from debates. To seal off blind alleys and expose errors, if taken to be part of an account of philosophical progress, leaves philosophy in a much better state than Rorty would have us believe. After all, who defends Descartes' version of the ontological argument today? Defenders of the ontological argument today have the critical reaction to Descartes' argument as data from which they may depart in building a more challenging ontological argument. These proposals are developed in more detail in later chapters of this work. It is concluded that Rorty's historical argument is less than compelling.

A second argument advanced by Rorty generalizes upon Kuhn: philosophy cannot articulate and validate the universal standards of objectivity and rationality for all human discourses or paradigms as there are no such commensurating grounds for different paradigms. Rorty here has cited a major difficulty facing philosophy in the systematic tradition, and it is not immediately solved as Kim believes (Kim, 1980, p. 595) by viewing philosophy as *intraparadigmatic* inquiry into the foundational aspects of a given paradigm. Such foundational inquiry frequently involves asking whether a given paradigm is in fact satisfactory, regardless of whether it has a competitor or

not. Christopher Clarke, himself a physicist has asked this question of current high energy physics (Clarke (et al), 1980), and an increasingly large number of biologists have come to criticize the entire orientation of the neo-Darwinist synthesis (Ho and Saunders, 1979), (Webster and Goodwin, 1982); cf. (Smith, 1984) for a survey).

Two points may be now established. Intraparadigmatic inquiry does not escape the issue that the rationality and objectivity of paradigms themselves are in need of establishment. Second this is a need felt by practitioners of science; it is especially a need felt by social scientists (Sztompka, 1979), (Smith, 1984). Now how can Rorty as a consistent edifying philosopher tell scientists how to do science? To do so, is nothing more than to allow the Kantian conception of philosophy entrance from the back door. Thus Rorty faces a major dilemma: on the one hand he asks us to abandon metaphysics and epistemology, on the other hand after abandoning them and becoming either social or natural scientists, it is found that we do not escape philosophical problems, even if we wish to call them by some other name such as "foundational problems". This leaves Rorty only with the option of either claiming that the scientists are not really doing science at such times, or to accept inconsistency by both accepting and simultaneously rejecting the Kantian conception of philosophy. To opt for the former horn of the dilemma also leads to inconsistency, because if there is one thing which an edifying philosopher does not do, it is to criticize the "forms of life" such as the sciences. But to say that one is only concerned to criticize a philosophical component of the sciences is just as problematic: (1) it is still a "foundational" criticism of a form of life; (2) it presupposes that a sharp philosophy/science distinction can be drawn, and Rorty has done nothing to establish this.

Rorty's third argument against systematic philosophy stems from his rejection of Platonic realism and any account of knowledge as accurate representation. The argument here is simply that systematic philosophy accepts Platonic realism, Platonic realism is *wrong*, therefore systematic philosophy is *wrong*. What however does it mean to speak of Platonic realism being "wrong"? It cannot mean that it is false or irrational, as these are concepts which Rorty has little use for. It is unclear what this term could in fact mean, and this unclarity erodes the credibility of Rorty's position.

Suppose however that we did accept that Platonic realism is false, and the whole notion of accurate representation a myth. Does this establish *merely* the incoherence of systematic philosophy? I think not. Rorty's proposal is nothing short of an outright rejection of any correspondence/referential use of language, and to see sentences as cohering with other sentences rather than with the world. This must in fact mean that no discourse has a representational function. But how in fact could this conclusion be established on the basis of true premises? Any attempt to do this seems little more than self-defeating for the reason that one has to *refer* to at least one object of discourse, namely the non-representational nature of language itself. The very idea of criticism presupposes the assertorial function of language, and criticism of *Weltanschauungs* is not something peculiar to systematic philosophy. As I will argue below, it is a characteristic of hermeneutics as well.

Thus, to sum up, Rorty fails to support proposition (R1). His attempts to do this lead him into incoherence. Consequently, it is *Philosophy and the Mirror of Nature* which is fundamentally flawed, rather than systematic philosophy.

Let us now examine proposition (R2). Is it the case that epistemology is irreconcilable with hermeneutics? Explicating the term 'hermeneutics' is no easy task (Palmer, 1982), but we can appeal to some generally accepted views about what hermeneutics is in our criticism of Rorty. Any hermeneutical approach must be concerned with the reflection upon the interpretation of texts, or more generally of any object of meaning, such as human actions (Taylor, 1971), (Giddens, 1976). Thus we have 'hermeneutics', whenever we have rules and systems of explaining, understanding and clarifying. Hermeneutics is not simply textual criticism, but presupposes it. Further, whilst it is true that some such as Gadamer (whom Rorty discusses in detail)⁷ reject the idea of "a general method of hermeneutics", others such as Betti have attempted to formulate a universal method of understanding.⁸ What this method in fact is, is not of importance here: we merely note that "general hermeneutics" stands very close to epistemology and is a friend rather than a foe as Rorty would have us believe. Rorty has merely taken for his characterization of hermeneutics a very narrow and carefully selected number of authors as representatives of his orientation. In a more general sense, an hermeneutic circle underlies all acts of inquiry (Bhaskar, 1979, pp. 195-203). I can hardly detail here a programme for the reconciliation of epistemology and hermeneutics, as useful as this would be, and nor need I do this to effectively criticize (R2). It is sufficient to cite hermeneutical works which are not opposed to epistemology, to present an effective counter-example to (R2). Let us further note that Rorty's sole defense of (R2), apart from an appeal to Gadamer's work, is to define 'epistemology' and 'hermeneutics' in such a way that they are irreconcilable. Whilst he is free to use such language in any way he wishes, we are not bound to

accept his non-standard usage.

This leaves us finally with proposition (R3) to consider, that it is an edifying thing to do to reject mainstream philosophy. Once more I must disagree with Rorty, especially in the light of my criticisms of (R2). As I see systematic philosophy, the attempt to build comprehensive theories is a highly edifying thing to do. It educates one in a whole range of phenomena which Rorty's orientation only leads us to ignore - such as the relationship between disciplines and fields in both philosophy and the social and natural sciences, and the overall consistency, coherence and parsimony of our accepted World-Views (*Weltanschauungs*). Not only does systematic philosophy greatly educate one, but I think most systematic philosophers will also claim that the activity itself is both exciting, satisfying, important - and just good fun. Indeed, the idea of a group of human organisms pursuing questions about the very fabric of reality, carries with it a great sense of grandeur, and affirms the dignity of human beings. In a world where human beings are exploited, degraded, raped, murdered and humiliated, anything which affirms our worth and dignity, is in my opinion a morally good thing. Thus, even ignoring all questions of truth, from a purely edificational perspective, systematic philosophy has much of value as a medication against the pains of the human condition. Therefore rejecting systematic philosophy is not in general a good and edifying thing to do.

I have appealed in the above argument to intuitions about the value of philosophy which may be "pumped" from the reader. Perhaps Rorty would deny that he has any of my intuitions at all and thus would still uphold (R3). Now (R3) implies that hermeneutics is a good, edifying and worthwhile thing. If my criticisms of (R2) are successful, then (R3) can also be rejected. Rorty presupposes, and does not show, that epistemology

and hermeneutics are discrete and irreconcilable modes of inquiry. If the two fields have an important and close cognitive relationship with each other, then philosophical inquiry is an edifying and satisfying thing to do and it is unreasonable to "abandon ship".

A fundamental claim of Rorty is that agreed upon facts can be variously assessed and the justification of conflicting assessments cannot involve appeal to further facts because there are none. The problem of perennial disputes *also* presupposes that certain agreed upon facts can be variously assessed, often in a mutually contradictory fashion, and the justification of these conflicting assessments cannot be solved by an appeal to further facts. Here I have not tried to refute Rorty's view of interpretation, although I believe that he cannot show by reasoned argument that his view is correct without self-refutation. What I have tried to show is that Rorty's metaphilosophy itself is unacceptable and that his criticism of the fundamental project of modern Western philosophy must fail because his position is internally incoherent.

This completes my consideration of Rorty's *Philosophy and the Mirror of Nature*. The book's implicit metaphilosophy is, as I have argued, outrightly incoherent and the major claims of the book are either unjustified, false or totally unreasonable. If there is an end in sight for systematic philosophy and epistemology, *Philosophy and the Mirror of Nature* is not an armageddon.

6. METAPHILOSOPHICAL ANARCHISM

The position of metaphilosophical anarchism is modelled upon Paul Feyerabend's epistemological anarchism (Feyerabend, 1975; 1978). This position has been subjected to extensive misinterpretations as Feyerabend's *Science in a Free Society* (1978) documents. He is not proposing any new scientific methodology with 'Anything goes!' on its banner, but is seeking to perform a *reductio ad absurdum* of the view that there exist organons of rational criteria which may be used in theory appraisal. As he states in one section of the 85% 'serious' part (ibid., p. 125) of his work (1975, p. 32):

One might ... get the impression that I recommend a new methodology which replaces induction by counterinduction and uses a multiplicity of theories, metaphysical views, fairy-tales instead of the customary pair theory/observation. This impression would certainly be mistaken. My intention is not to replace one set of general rules by another such set: my intention is, rather, to convince the reader that *all methodologies, even the most obvious ones, have their limits*. The best way to show this is to demonstrate the limits, and even the irrationality of some rules which she, or he, is likely to regard as basic. In the case of induction (including induction by falsification) this means demonstrating how well the counter-inductive procedure can be supported by argument .

In particular Feyerabend has attempted to show by detailed historical studies, that adherence to basic organons of scientific methodology would have arrested progress in a series of historical episodes which all rationalists regard as intuitively true cases of scientific progress (e.g. the Copernican revolution, the success of the kinetic theory, the rise of special relativity and quantum theory). Thus Feyerabend's point is that it is reasonable (vis-à-vis the rationalist's own organons of scientific methodology, 'logic of science') to assert that a number of intuitively true cases of scientific progress will fail to be consistent with one's organons of methodology. The point is not, as Newton-Smith

(1981, pp. 128-129) maintains, that the organons of scientific methodology are taken to be unchanging, so that essentially no new discoveries are made in the area of methodology. The point is, that our best organons of scientific methodology, such as logic, may lead us to conclude that some of our best scientific theories are untenable. As an example of this, consult Richard Routley's (1980, p. 957) demonstration that quantum theory is classically inconsistent.

It is traditionally argued (e.g. (Newton-Smith, 1981, p. 128)) that the fact that inconsistent theories have brought progress in science is no reason for abandoning the classical form of the law of non-contradiction, because progress has come from developing inconsistent theories into consistent theories. This traditional object has recently faced strong opposition from paraconsistent logicians who believe that the world is actually (non-trivially) inconsistent (Routley, 1979(a)). If the world was inconsistent, then there would be at least one 'true contradiction' $A \ \& \ \sim A$ in some field of study F , so that no classically consistent theory could be adequate for F . Now the burden of proof rests upon the classicist who makes the above objection to show that the world is not inconsistent.

Feyerabend's problem does not, it seems to me, raise major epistemological problems for metaphilosophy, so that metaphilosophical anarchism is a weak position. The reason for this is as follows. The very existence of the problem of perennial philosophical disputes is *prima facie* reason for believing that there are no cases of philosophical progress. Hence metaphilosophical anarchism is a quite trivial position as a response to the problem of perennial philosophical disagreements, as the key argument for the position requires intuitively

true cases of philosophical progress which are ruled out by the very existence of this problem. Philosophy seems to lack even these.

But this response is unsatisfactory: what *if* philosophy was in precisely the same position as science? Thus, to give a more satisfactory treatment of these issues, I will now respond to Feyerabend's problem as it was formulated above with respect to scientific inquiry.

The choice which Feyerabend offers us is between our organons of scientific methodology and some intuitively true episodes of scientific progress, and the answer must be: opt for the organons of scientific methodology and accept the consequences that we may be quite wrong about the alleged progressiveness of some basic scientific theories. Elsewhere I have argued that this is the case with the neo-Darwinist synthesis (Smith, 1984) and have suggested that pre-Darwinian traditions dealt more satisfactorily with basic problems of theoretical biology, such as the emergence of complexity, morphology and the problem of typical form (Webster and Goodwin, 1982). Not to have organons of scientific methodology will mean that we could never establish that received scientific traditions are defective. As long as reason is a useful tool (and we need not claim that it is the only such tool) for theoretical change, attempts to bring about theoretical change will be weakened. This follows because even accepting that 'paradigm-changes' are primarily caused by arational factors, reason may still have an important part to play: in convincing the unconverted who are not easily bought off. So, making all of these concessions to Feyerabend, we still reach the conclusion that progress in science requires organons of scientific methodology.

Feyerabend may respond to this by claiming that the solution is superficial. It is not that a conflict occurs between 'good methodology'

and some 'good science', but that good methodology and good science are in inevitable conflict. He states (Feyerabend, 1978, p.14):

It is true that two cases do not all rules remove but as far as I can see they remove basic rules that form an essential part of the rationalists' prayer book. Only some of these basic rules have been discussed in connection with case studies but the reader can easily apply the assembled material to Bayesean procedures, conventionalism (whether Poincaré or Dingler) and 'conditional rationalism' where rules and standards are asserted to hold under certain well-specified conditions only .

The claim that the assembled material can be applied to the cited fields of study is quite problematic: Feyerabend has not produced a *general argument* by which this could be done, and not only would outrightly reject the suggestion, but if he accepted standards of consistency (if only to confuse rationalists), then this proposal would contradict his statement of the position of epistemological anarchism. Thus the claim that really gives epistemological anarchism "intellectual bite", that 'good (general) methodology' and 'good science' are in inevitable conflict is not established because only a handful of historical case studies does not permit us to accept deductively Feyerabend's universal generalization. To argue on the other hand that epistemological anarchism should be accepted on inductive grounds is self-referentially inconsistent, because the epistemological anarchist position enables us to advance plausible counter-inductive arguments against epistemological anarchism itself (under pains of it becoming itself another rationalist dogma (Feyerabend, 1975, p. 32)).

Suppose however that it was actually the case that our best theories of rationality and organons of scientific methodology *did* conflict with paradigm examples of scientific progress. What should be rejected? To reject both 'good methodology' and 'good science' will only maximize our problems. Hence one or the other must be rejected. Now one may argue

that it is a *petitio principii* against the epistemological sceptic to maintain that science itself could not be shown to be methodologically incoherent. But consider how any standard of 'good methodology' is itself justified: surely because it preserves a maximally, consistently large set of intuitive examples of 'good science' consonant with more general principles of rational choice (Nielsen, 1974). If the best theory of methodology conflicted with *all* instances of 'good science', then it would follow that our methodology is unjustified. But now, contrary to our initial assumption, both the methodology and the instances of 'good science' must be regarded as problematic. Fortunately for the rationalist, Feyerabend has not given any satisfactory systematic argument for this conclusion. Hence both epistemological and metaphilosophical anarchism are rejected. They may be true in some formulation but they have not as yet been supported by satisfactory argumentation to show in fact that they are true.

7. METAPHILOSOPHICAL NIHILISM

Nihilism is a cluster of positions which have as their general form: there are no \emptyset -type objects, or nothing \emptyset 's (Routley, 1983, p. 3). It is not the position of Kielkopf (1975) who takes it that $\diamond\diamond A$ (Routley, 1983). Metaphilosophical nihilism, as it is of relevance to the problem of perennial philosophical disputes, is the position that there are no correct philosophical positions:

$$(MN) \quad (\forall x) (Px \rightarrow \sim Cx).$$

The term 'correct' may be conceptually explicated in various ways giving rise to a number of distinct metaphilosophical nihilist positions. No philosophical position may be *knowable*, *rational*, *true* or of *value* to

human life, are some of the forms of metaphilosophical nihilism. I am not concerned with presenting a neat classification of these positions (cf. (Routley, 1983)), as I will argue that metaphilosophical nihilism is self-referentially inconsistent. I have criticized the view that philosophy contributes little of value to human life in my paper "Philosophy and the Meaning of Life" (Smith, 1984(c)).

The most famous argument for (MN) was given by the early logical positivists who used their verificationist theory of meaning to show that metaphysical, ethical and aesthetic positions were cognitively meaningless, and hence could not be 'correct'. As is well known, the verificationist theory of meaning fell victim to a *tu quoque* argument, which showed that this theory of meaning could not itself be correct. Further, as Routley (1983) has argued, many nihilist positions fall prey to self-referential arguments for their inconsistency. Is (MN) immuned from this charge?

Let us suppose that (MN) is a philosophical position. Then by instantiation and substitution we infer: $\sim C((MN))$, i.e. that (MN) is not correct. Now this conclusion only follows if (MN) can be shown to be a philosophical position. But what is a philosophical position? There are two broad types of answer to this question: (a) philosophy is really \emptyset 'where \emptyset is the answers' own position); (b) philosophy is what philosophers do in their social roles as philosophers. The first response, (a), will take philosophical positions to be characterized by ψ : for example ψ may be the property of containing unified and systematic arguments for a position, rather than mere poetic images of the human condition. But ψ is not sufficient to distinguish philosophy from any other argumentative discipline (e.g. law, logic, dialectics), so something must be added about the types of arguments. If we say that

such arguments are to be 'philosophical', then our explication is circular. If we now say that these arguments should have some property ψ^* , then we immediately beg the question against a position which (even if incorrectly) denies this; this alternative position may be incorrect, but it is still a 'philosophical position'. Now to turn to alternative (b), to generate a circularity, we need only ask: 'but what is *it* that philosophers do?'. This leads us back to (a).

The utter diversity of positions which have been put forward as philosophical positions, makes it conceptually impossible to find some common essence to all of them. There is none: some are advanced simply to eliminate other positions (consider logical empiricism and traditional metaphysics). Nevertheless, it can be said that if a position or thesis falls into the field of metaphilosophy, it is still a philosophical position or thesis. This is not so for metamathematical or meta-scientific statements. A metamathematical statement about the nature of mathematical proof is not itself a mathematical proof. Metaphilosophical statements are however philosophical statements, as we have seen.

Since (MN) is a metaphilosophical thesis about the correctness of philosophical positions, (MN) is open to immediate self-refutation. The familiar Tarskian object language/meta-language distinction, useful in addressing the semantical paradoxes, cannot be used here. Therefore (MN) is incorrect, and hence cannot be a satisfactory solution to the problem of perennial philosophical disagreements.

8. CONCLUSION: STATE OF THE ARGUMENT

In this chapter sceptical, relativistic, anarchist and nihilist responses to the problem of perennial philosophical disagreements were considered, and all in turn rejected. The majority of these positions are simply self-refuting, and all of the considered positions are open to specific criticisms. I conclude that neither of the positions of metaphilosophical scepticism, relativism, anarchism or nihilism present a satisfactory response to the problem of perennial philosophical disagreements.

It is organizationally convenient to consider Mates' position of *solvability scepticism* in chapter 7. In the following chapter I will discuss an alternative approach to the problem of perennial philosophical disagreements, objectivist responses, giving special reference to the relevance of naturalized epistemology to our target problem.

4. NOTES

1. Unless stated otherwise, the terms 'scepticism', 'relativism', 'anarchism' and 'nihilism' shall refer to the positions outlined in chapter 1.
2. Solvability scepticism is to be distinguished of course from general epistemological scepticism. For recent responses to epistemological scepticism cf. (Johnson, 1978), (Cavell, 1979), (Cornman, 1980), (Rescher, 1980(a)), (Klein, 1981), (Odegard, 1982), (White, 1982). These works however, with the exception of (Cornman, 1980), tend to ignore the scepticism arising from recent work in the philosophy of science (Russell, 1981).
3. The literature on the topic of cognitive relativism is vast and rapidly expanding. For surveys and references to further literature cf. (Meiland and Krausz, (eds), 1982), (Hollis and Lukes, (eds), 1982). Brief mention should be made here of the alleged cognitive relativist implications of the so-called strong programme of the sociology of knowledge, for in the next chapter the relevance of this programme to (PPPD) shall be discussed. Bloor and Barnes (1982) have argued for the following two theses:

(T1) The balance of argument favours relativism over rationalism;

(T2) A scientific understanding of the forms of knowledge ('knowledge' meaning any "collectively accepted system of belief") by anthropology, history and sociology, presupposes a relativist theory of knowledge.

The popularity of their cited paper demands a response from any author who criticizes relativism. I shall argue in this footnote that neither (T1) nor (T2) are justified. In particular Bloor and Barnes set out to defend a thesis of *causal relativism*, but the arguments in the body of their paper are directed towards supporting the thesis of cognitive relativism.

First, it is important to be clear about what precisely a "relativist theory of knowledge" in fact is. All forms of relativism, they claim, are committed to these theses: (1) the beliefs held by subjects on a specific topic vary in specific ways (i.e. historically, spatially, culturally) and (2) which of these beliefs is found in a specific context depends upon the circumstances of the users. Relativistic doctrines also involve a "symmetry postulate" which claims that all of these beliefs are alike in specific ways. For example it may be claimed that all physical theories from Aristotle's theory of motion, to Einstein's are alike in the respect that they are all true or all false, or even truth-valueless as some instrumentalists might claim. Barnes and Bloor do not wish to defend any of these stated views about the relativity of truth. In an insightful passage (the forthcoming citation will play a vital part in my own rejoinder) they claim (*ibid.*, p. 23):

(T3) Our equivalence postulate is that all beliefs are on par with one another with respect to the causes of their credibility. *It is not that all beliefs are equally true or equally false, but that regardless of truth and falsity the fact of their credibility is to be seen as equally problematic.* The position we shall defend is that the incidence of all beliefs without exception calls for empirical investigation and must be accounted for by finding the specific, local causes of this credibility. This means that regardless of whether the sociologist evaluates a belief as true or rational, or as false and irrational, he must search for the causes of its credibility. In all cases he will ask, for instance, if a belief is part of the routine cognitive and technical competences handed down from generation to generation. Is it enjoined by the authorities of the society? Is it transmitted by established institutions of socialization or supported by accepted agencies of social control? Is it bound up with patterns of vested interest? Does it have a role in furthering shared goals, whether political or technical, or both? What are the practical and immediate consequences of particular judgements that are made with respect to the belief? All of these questions can, and should, be answered without regard to the status of the belief as it is judged and evaluated by the sociologist's own standards .

This long passage makes it unequivocal that Barnes and Bloor's relativism is *not* a relativism about the standards of rationality and of truth. Rationalists are keenly interested in refuting the latter doctrine 'cognitive relativism'. It is *prima facie* unclear as to why rationalism and Barnes and Bloor's doctrine of "causal relativism" should be regarded as inconsistent theses. As the above citation indicates the causal relativist must seek the causes of the credibility of a belief regardless of whether that belief is true or false, rational or irrational. If this is so, then *why* is it inconsistent to propose that a belief might well simultaneously be socially caused and true and rational? No explicit argument has ever been given by either Barnes or Bloor for this claim (Smith, 1982), and this omission is not corrected in the target paper. Indeed this claim would seem to outrightly violate Barnes and Bloor's own symmetry postulate. Thus, it would be informative to propose a sociology of knowledge style explanation for this omission.

If we survey the argumentative structure of the target paper by Barnes and Bloor we find discussions of the following subject matters: (1) an argument for the claim that there is no context-free and trans-cultural account of rationality (Bloor and Barnes, 1982, pp. 25-28); (2) an argument for the cultural relativity of the claims of validity of belief (*ibid.*, pp. 28-29); (3) an argument as to why moderate empiricism does not refute "relativism" (*ibid.*, pp. 30-32); (4) a critique of proposed context-independent accounts of rationality, including an argument that deductive inference is incapable of "justification" (*ibid.*, pp. 35-47) and (5) a causal hypothesis about why rationalists object so vehemently

to "relativism" (ibid., pp. 46-47). This exegesis indicates that Barnes and Bloor really wish to defend *cognitive relativism*. No further mention of the position of *causal relativism* explicated by (T3) above is made. Hence from a logical point of view the argument in the body of Barnes and Bloor's paper is strictly at variance with their stated thesis. We should inquire into the possible causes of such behaviour. Here I conjecture that whilst academics may fear "relativism" (i.e. cognitive relativism) because it tips the bucket on their "moralizing", relativists fear rationalism because of a fear of criticism and argument: in short a frustration with the basic process of thinking! It is so much easier to curl up inside one's incommensurable little paradigm/form of life than to face the dark possibility that one's position may be inadequate.

It is concluded that thesis (T1) fails because regardless of what arguments exist for the position of cognitive relativism Barnes and Bloor have given no discussion of causal relativism which might indicate that this position causes any epistemological problems for the rationalist at all. The question then remains as to whether (T2) is justified.

Given the thesis of causal relativism, can it be shown that anthropology, sociology and history *must* presuppose it to be able to give a "scientific" understanding of the forms of knowledge at all? This claim is also highly implausible. Consider the possibility that the symmetry thesis is false. Then it follows, as Martin Hollis (1982) has recently claimed, that "true and rational beliefs need one sort of explanation, false and irrational beliefs another" (ibid., p. 75). This means that the sociology of knowledge could not be as explanatorily strong as both Barnes and Bloor hoped that it would be. It does not follow that if Hollis was right then anthropology, sociology and history are incapable of scientifically understanding the forms of knowledge. It simply means that their explanatory scope is limited. It is concluded that thesis (T2) is unjustified. This being so, my own thesis is established: both (T1) and (T2) are unjustified. (cf also (Vallicella, 1984).

4. The statement (J) in both Jordan and Meiland's accounts is:

(J) p implies p and not $\sim p$.

This statement is ambiguous between:

(J*) $(p \rightarrow p) \ \& \ \sim (p \rightarrow \sim p)$

and:

(J**) $p \rightarrow (p \ \& \ \sim \sim p)$.

Clearly (J*) is the statement which both Jordan and Meiland require.

5. Dialectical logic and (PPPD) will be discussed in chapter 8.
6. cf. (Hacking, 1980), (Kim, 1980), (Pompa, 1981).

7. cf (Gadamer, 1975; 1976). For a critique cf. (Trigg, 1982, pp. 55-62).
8. cf. (Betti, 1967). For other discussions of general heremeneutics cf. (Coreth, 1971), (Hirsch, 1967; 1976), (Wuchterl, 1977).

5. NATURALIZED EPISTEMOLOGY AND EXTERNALIST RESPONSES

1. STATEMENT OF THE ARGUMENT

In this chapter I shall investigate the satisfactoriness of various "externalist" responses to the problem of perennial philosophical disputes that either have or could be advanced from the perspective of the programme of "naturalized epistemology". Externalist responses to our principal problem propose that a satisfactory explanation of the phenomenon of perennial philosophical disputes can be given by "external" conditions to these debates, the conditions being perhaps historical, psychoanalytic, sociological, psychological or biological. A condition will be said to be "external" if it is extrinsic to philosophical debate. Now some feel that the lack of progress of philosophy, and its failure to solve virtually all of its main problems requires a replacement of philosophy in general, or epistemology in particular, by a *naturalized epistemology*. Indeed the programme of naturalized epistemology has been offered by a number of philosophers as a response to the challenge of the epistemological sceptic (Bieri (et al), 1979) and no doubt it could also be offered as a response to the problem of perennial philosophical disagreements.

It will be argued here that the principal form of externalist response to the problem of perennial philosophical disputes, naturalized epistemology, fail in all the considered versions to lead philosophy out of the difficulty created by the problem of perennial philosophical disagreements.¹ Section 2 of this chapter will isolate the principal types of naturalized epistemology. The

remaining sections will provide a systematic critique of some such relevant forms of naturalized epistemology continuing the critique of naturalism which I began in *Reductionism and Cultural Being* (Smith, 1984). The conclusion of this chapter will then be an exceedingly negative one: leading externalist responses to the problem of perennial philosophical disagreements through the programme of naturalized epistemology are unsatisfactory, so that we must look elsewhere for a solution to our problem.

2. (ENE) AND PSYCHOANALYTIC ELIMINATIVISM

The position of (ENE) is that the traditional epistemological task of seeking to rationally justify knowledge-claims and provide an answer to the sceptic who in turn proposes that we know nothing, is an untenable task. The sceptic cannot be answered. This shows that the traditional philosophical enterprise is radically unsound, and should be eliminated in favour of some other cognitive enterprise, especially some designated empirical science. In this section and the following two sections, I will consider some (ENE) responses to the problem of perennial philosophical disagreements.

Morris Lazerowitz in his books *The Structure of Metaphysics* (1955), *Studies in Metaphilosophy* (1964), and *Philosophy and Illusion* (1968), as well as his more recent article "On a Property of a Perfect Being" (1983) pursues a two part programme in metaphilosophical inquiry: the first part is to offer a hypothesis to explain the situation of perennial philosophical disagreements and the resultant non-existence of established results in philosophy, whilst the

second part of the programme is to apply this hypothesis to representative philosophical problems (Lazerowitz, 1968, p.13). In this section I will be concerned with refuting the first part of Lazerowitz's programme.

Lazerowitz's central metaphilosophical concern is to resolve the problem of perennial philosophical disagreements and explain the seeming lack of progress in the discipline of philosophy. The unfruitful history of philosophy indicates that the discipline as traditionally conceived is cognitively bankrupt and pseudo-scientific. Philosophical theories do not have a truth-value and philosophical arguments neither establish theses nor even refute positions.

According to Lazerowitz (1968, p.101):

(L0) It is possible that the greatest philosophers, from Plato through Descartes, Kant, Hume and Russell, to the contemporary linguistic analysts, have succeeded only in contributing chimeras to a chimerical subject, a subject which presents itself in the guise of a fundamental investigation of the world. Wittgenstein said that a philosophical problem arises when language goes on holiday and it is not unlikely that a philosophical theory is only the spurious imitation of a theory, that it is merely a piece of re-edited terminology intended, not for practical adoption, but only for inner contemplation. The reality of technical philosophy, its substance is, according to this iconoclastic hypothesis, concealed, artificially retailed language, the superposition of different will-o'-the-wisp uses of the familiar language of common discourse. The illusion of philosophy is that its pronouncements state theories about the nature of things and that its arguments are pieces of evidence for or against the claimed truth-values of the theories.

For Lazerowitz, the philosopher is not using language for the expression of conscious thoughts, but rather is unwittingly giving

expression to unconscious passions and deep fantasies. Philosophical theories are viewed through the metaphor of a bridge with three piers, each one in one of the three designated areas of the "psychoanalytic" view of the mind: the conscious, the pre-conscious and the unconscious. At the pre-conscious level, philosophical terminology is introduced, accepted or rejected, creating at the conscious level an illusion that a theory about the nature of the world is being advanced which may be either true or false. At the unconscious level lurks various obsessions and fantasies which are given expression (Lazerowitz, 1964, p.217).

Lazerowitz's position thus consists of two parts. First is a thesis about the nature of philosophical theories and arguments; second is a causal explanation of why philosophizing occurs. Let us elaborate on these points. To begin with, Lazerowitz disagrees with Wittgensteinians that philosophical problems arise from a mistaken use of language, and are resolved once one is clear about the correct use of language. Rather, what the philosopher does is to unconsciously *change* language where the "emendations he effects are presented in a form of speech which produces the vivid, if delusive, impression that he is announcing a theory about a feature of reality (Lazerowitz, 1968, p.109). Consequently recourse to linguistic facts of correct usage are irrelevant to the solution of philosophical problems, for such problems are not a matter of fact. What occurs in philosophical problems is a re-editing of language.

To take but one of Lazerowitz's many illustrations, consider his remarks on Hume's theory of causality (ibid , pp.110-113).

Hume's account of causality, which stated roughly is the claim that causation is nothing more than the constant conjunction of independent events, is not laboured under a verbal misapprehension, as Hume knows as well as any one that 'x is the cause of y' does not *mean* 'y regularly occurs with x'. Rather, Hume is unconsciously changing language. Further, this move is illegitimate, Lazerowitz believes, because if the expression 'independent occurrences' has a use in language, it is only because the expression 'dependent occurrences' describes some actual or conceivable state of affairs. Once Hume has re-edited language in this way it is evident how in fact he can proceed to criticize the proposition that one thing can by its action produce a change in another thing - he has defined his terminology so that this conclusion follows.

The style of argument employed by Lazerowitz constantly throughout his works is summarized in the following remarks (*ibid*, pp.110-111):

(L1) In general, anyone who says, 'x is not really \emptyset ; it only appears to be', implies that he knows what it would be like for x really to be \emptyset . Read literally, his words imply that he can say what it is that x lacks, which if possessed by x would make it \emptyset . And in saying that x only appears to have \emptyset he implies that the appearance pictures x as having what it in fact lacks. Further, he implies that he can *identify* what the appearance pictures that is not to be found in the reality. If he is unable to do this, then whatever it is that he wishes to convey by his words, he is not telling us that x only appears to our senses to be \emptyset . He is using an ordinary form of speech to say something else than what his words naturally suggest.

This argument is used against Hume, against the rationalist position that relations are unreal (ibid , pp.113-118) and against philosophers such as Zeno, McTaggart and Bradley who have denied that some common sensical aspect of the world, such as motion, exist.

Why should philosophers have argued for such counter-intuitive positions? Here Lazerowitz advances psychoanalytic explanations, which involve appeal in one way or another to the obsessions, neuroses and unconscious fears of individual philosophers. Thus for example, the claim that change is unreal, defended by philosophers from Parmenides to Bradley, is merely an expression of the need for security in a world of anxious change (Lazerowitz, 1955, p.70); the philosophy of Spinoza stands as a mere rationalization of his unconscious concerns with the mystery of his birth and the realization that his father may have played some role in this (Lazerowitz, 1964, pp.251-256), and Hume's problem of induction is generated by Hume's own insecurity and melancholy (Lazerowitz, 1968, p.257). The unconscious underlies all our behaviour and philosophers are not immune despite their games and play-acting of rationality and cold logic.

To rebut Lazerowitz's nihilistic view about the nature of philosophy requires a two part critique. First, comment upon his logical criticism of philosophical positions, as expressed in (L1), must be made. Second, an empirical and scientific rebuttal of his use of psychoanalysis will be given. Since Lazerowitz devotes significantly more space to his psychoanalytic inquiries than a consideration of possible critical rejoinders I shall address the

second question first. I choose as an example for debate, the claim made by rationalist metaphysicians from Parmenides to Bradley, that change is unreal.

Two general problems exist for Lazerowitz's position. The first is his use of psychoanalysis, and the second is the scientificity of psychoanalysis itself. The second criticism is more interesting and will require an appeal to the secondary literature to state its case. The first point can be established by examining Lazerowitz's own methodology.

Lazerowitz comes to the conclusion that the metaphysical denial of change is an attempt to ward off anxiety by denying that anxious causing changes in our lives will occur. If the reader turns to pages 69 and 70 of *The Structure of Metaphysics* and runs his/her finger under every line, no argument for this conclusion will be found, no supporting clinical observations cited and no consideration of alternative psychoanalytic hypotheses considered. The reader will find assertion, but no argument. Hence the advanced hypothesis is pseudo-scientific in the extreme lacking all form of rational justification and thus is no more than the product of an imaginative guessing game, where hypotheses which sound plausible are taken without evidence to be true.

The same remarks apply to Lazerowitz's more detailed study of Bradley's denial of change. Bradley's philosophy is a product of his own self-estrangement and feelings of inadequacy (Lazerowitz, 1964, pp.248-249). The fate of "the finite" is a purely autobiographical fact. This is seen by Bradley's use of dependent incomplete

adjectives which indicate his need to express his own incompleteness. Lazerowitz makes no appeal to clinical or biographical studies of Bradley, so we can only conclude that this is an expression on Lazerowitz's part of his own obsessions and fantasies. It is unclear why Lazerowitz has this preoccupation, but if we are to take his own philosophy seriously, he can hardly be excluded from his fair share of neuroses and fears. If this is taken to discredit philosophical work, it clearly discredits his own. But enough of this *ad hominem* guess work, the point is made that virtually all of Lazerowitz's psychoanalytic adventures are groundless speculations, in need of establishment rather than repetitive assertion, the product of imagination rather than science.

My second criticism takes up the issue of the scientificity of psychoanalysis. It is hardly possible in this work to argue through such an issue on an independent basis. But this is no major problem, for we have at hand an excellent and recent critical study by David Stannard: *Shrinking History: On Freud and the Failure of Psychohistory* (1980) which achieves such a purpose. The book attempts the examination of fairly orthodox Freudian theory and questions its philosophic, scientific and universalistic status and validity, and is concerned especially with the application of psychoanalytic theory to historical documents.

Of special relevance to our concerns here is Stannard's consideration of Freud's *Leonardo da Vinci and a Memory of his Childhood* (Freud, 1964) undertaken in chapter one of Stannard's book. Freud proposed that da Vinci's "cool repudiation of sexuality" and

"insatiable thirst for knowledge" of his adult life is a result of sublimation where infantile sexual expression has been terminated by sexual repression. To substantiate this Freud requires detailed knowledge of da Vinci's mental development in the first years of his childhood, information which is completely lacking. Freud then turns to a childhood memory of da Vinci, where a vulture came down to his cradle and opened his mouth with its tail. This Freud takes to be a "passive" homosexual experience, where the tail of the vulture is a "substitutive expression" for a penis. Freud also notes that the vulture was also regarded in classical writings as a symbol of motherhood (which da Vinci would certainly be aware - but as a child?) so the vulture-fantasy is da Vinci's expression of being a vulture-child, of having a mother but no father. The problem of a maternal image possessing male qualities is resolved by Freud through his theory of infantile sexuality: da Vinci must have believed his mother still to have a penis, which allegedly is a universal assumption of young male children.

This enables Freud to seek a causal connection between da Vinci's relationship with his mother in childhood to his later manifest but sublimated homosexuality. Indeed the vulture/mother fantasy is responsible for the greatness of da Vinci's works such as the *Mona Lisa* with its remarkable smile, the same smile which his mother once gave him, and the same lips that once passionately kissed his own. The painting is in short, nothing more than the history of da Vinci's childhood, the story of his repressions in paint and on canvas.

As the rational inquirer would well note, the logical and evidential leaps in Freud's work are painfully obvious - ignoring straight-forward biographical errors, such as the bird in question being a kite rather than a vulture (Stannard, 1980, pp.12-13). Since this fact was supposed to make up for the lack of evidence about da Vinci's childhood, the contingent hypotheses based upon this, collapse. There is, as Stannard points out, no evidence to believe that da Vinci's early childhood was as Freud surmised. Nor can the absence of evidence for a positive and active sex life of da Vinci suggest that he did not have one - it merely indicates that we don't know.

Stannard takes Freud's *Leonard da Vinci* as a sample of the sort of work which psychohistory produces. It may rightly be argued that the work of such early researchers does face defects which more advanced psychohistory could eliminate. But such defects, Stannard argues are a function of the underlying theoretical structure of psychohistory. The defects include (1) problems of fact; (2) problems of logic; (3) problems of theory and (4) problems of culture. With regard to (1), frequently "just-so" stories are told and fiction writing and acts of imagination used to fill evidential gaps. With regard to (2) psychohistory is riddled with *post hoc, ergo propter hoc* fallacies, where causality between childhood events and adult events is assumed, but not established. With regard to (3), frequently psychoanalytic hypotheses are either too vague to test or else they rest upon already falsified conjectures. Finally, the cultural context of the writer or thinker is frequently ignored

or down played, constituting the fourth major defect.

The evidence for psychoanalytic theory has seldom been given a detailed and rigorous examination. The reason for this is that following the views of K.R. Popper (1959), psychoanalytic theory has been held to be untestable in principle. Popper's arguments have been criticized by Grünbaum (1979), and recently philosophers have extensively examined both the clinical and experimental evidence for psychoanalytic theory. Grünbaum (1980; 1981) has argued that there are unsolved epistemological problems in demonstrating that a therapy is not merely placebogenic, but genuinely effective. (For a criticism cf (Flax, 1981) and counter-criticism (von Eckardt, 1981).) Erwin (1980(a), (b); 1981) has maintained that there is no acceptable clinical evidence to support the claim that psychoanalysis is therapeutically effective, nor is there any firm experimental evidence for any psychoanalytic hypothesis.

It is of course not possible, nor necessary to summarize Stannard's book and the other cited critical material. Enough has been said I hope to establish some earlier criticisms made of Lazerowitz's psychoanalytic adventures with considerable force. What Stannard has said about Freud's study of da Vinci may be said about Lazerowitz's own psychoanalytic hypotheses about outstanding philosophers. We know utterly nothing about the childhood of Parmenides and Zeno, and nothing much about their adult lives. The quantity of evidence about the childhood of even thinkers like Kierkegaard (who deals so long and painfully in his *Journals* with adulthood events such as his broken engagement) is hardly extensive, and so psychoanalysis faces major difficulties in this area. All

the major figures which Lazerowitz puts on the couch , have childhood histories which are virtually unknown to us. Was Spinoza fascinated by his birth? Did he have a vague *awareness* of the role which his father played? The facts of reproduction would have hardly have been a mystery to one of Spinoza's genius, and he may or may not have been fascinated by his birth - he may have been equally fascinated by all aspects of reality since he was a philosophical systematicist, as his *Ethics* well testifies. The astute reader can only remain sceptical of the validity and even intellectual worth of Lazerowitz's psychoanalytic adventures, and wonder about the nature of the repressions and fears which produced these hypotheses.

We turn now to a consideration of Lazerowitz's chief logical objection to doctrines such as scepticism, rational metaphysics, and generally any position which stands in conflict with commonsense. This is proposition (L1) cited earlier. Before doing so however, some comment is necessary about Lazerowitz's implicit assumption that conclusions which conflict with commonsense must be problematic.

If this claim was accepted, then Lazerowitz would have to call into question many of the claims made by physicists and even psychoanalysts. David Bohm for example has documented the respects in which special relativity must lead to a revision of commonsense concepts of space, time and motion (Bohm, 1965) and the psychoanalytic claims of Lazerowitz himself are hardly consonant with commonsense. Thus if claims which conflict with commonsense must be regarded as problematic, so must Lazerowitz's own psychoanalytical claims. This in itself does not constitute a self-referential inconsistency,

but it does indicate the illegitimacy of singling out the discipline of philosophy for preferential treatment as an object of criticism.

Let us consider proposition (L1). In response to Lazerowitz's criticism of Parmenides, Zeno and Bradley, Brand Blanshard (1966) has argued in reply to Lazerowitz, that Parmenides, Zeno and Bradley had attempted to demonstrate that change cannot be real because it is self-contradictory. It is true that these philosophers denied what their senses revealed to them. But as Blanshard points out (ibid , p.348):

I do not think Zeno and Bradley would have been much perturbed. They would no doubt reply that the choice between absurdities confronts us still. It is absurd to deny that we have even the illusion of motion; granted. It is also absurd to say that even in that illusion we are experiencing the ending of an endless series. Forced to choose, these men think it more credible that we should be deluded about sense experience than that it should really elude the law of contradiction. They may of course have been mistaken that motion does violate that law. But this is not at the moment the point. They thought they saw clearly that it did and if one does think this, what other course can one take as a philosopher? It is surely a hard fate that an uncompromising devotion to reason should incur the charge of psychopathology.

In reply to this Lazerowitz maintains, that phrases using temporal (and motive) terminology must have no descriptive use whatsoever. This however is precisely what the consistent rationalist can do. The reason for this, is simply that the framework of commonsense is rejected by them. A criticism based upon this therefore constitutes a *petitio principii* against Zeno and Bradley. It may be argued that they have no good reasons after all for rejecting the framework

of commonsense, but this is another matter, the merits of which require independent consideration.

It remains however to specify what precisely is wrong with (L1). (L1) is in fact a tacit form of the well-known *paradigm case argument* (Passmore, 1961, chp. 6). The response which is given here to that famous argument, is that it may well be that *prima facie* descriptive phrases involving motion-terms do not have existing reference. Whilst such phrases are used by people to make descriptions, the existence of moving things cannot be deduced from the proposition that the term 'motion' has a descriptive use. Such a term may be much like the term 'phlogiston', and would be, if Parmenides and Bradley were right, destined for the conceptual scrap-pile. The paradigm case argument proposes that we know how to use certain empirical statements, such as those statements containing motion-terms, because we have learnt to do so from being shown cases of motion, such as a moving car. So, the supporter of this argument argues, Zeno must be wrong in asserting that motion does not occur for we would not have learnt how to use the term 'motion' in the first place. My point here is that Zeno could and would "bite the bullet" and assert that contrary to appearances we really haven't learnt how to correctly use the term 'motion' at all, or that all of our supposedly true commonsense motion statements, are really *false*.

It may be said in reply to this, that if this was true, then it is impossible to see how motion-terms could be taught and learned. This famous reply is nothing more than a *petitio principii*. One could just as well argue that Zeno and Bradley must be wrong because they had to write their arguments down, and this implies activity and motion. But it is precisely the existence of motion that they are denying - rightly or wrongly. If Zeno and Bradley could deny this, they would hardly be impressed by sociological facts about language learning. They could simply deny that motive terms are taught and learned. This is no doubt paradoxical and unnerving, but hardly less so than their initial denial of motion. Hence (L1) and any plausible variation upon it will fail against the rationalist.

Scharfstein and Ostow (1970) have also developed an argument which very closely parallels Lazerowitz's own position. They distinguish philosophy from the sciences, by the alleged fact that the philosopher whilst seeking to make statements about the structure of reality has no technique by which he/she can convince his/her colleagues that the advanced position is correct. This puts philosophers in a very strange position (ibid, pp.261-262, emphasis added):

They consider their views of great importance, they expend much effort in trying to prove that they are right, and they remain frustrated in their attempt. The fact is that they have accidentally or deliberately chosen a profession that lacks the publicly recognized methods of proof which we have come to identify with those of science. Their use of formal logic does not change this absence of proof, because they subject the logic to interpretations that do not convince their colleagues.

If, as their language suggests, the ambition of philosophers is to prove their case, why have they chosen a profession which will frustrate them? *Are they somehow choosing to deceive themselves, choosing to fail, or choosing to deal with insoluble problems because endless dealing with them is exactly what they want? The answer to the last question is a qualified "yes".*

Perennial disagreement in philosophy is a function of a brand of curiosity which is specific to the philosophical enterprise. Scharfstein and Ostow claim that a heightened curiosity is the result of unfulfilled desires for sexual knowledge and for power. Young children are faced with the mystery of their birth with few clues to aid them in their inquiries. They do not see their parents' sexual organs as much as they want to, and even if they did, they do not in any case understand their function. This infantile frustration leaves a residue of thwarted infantile sexual curiosity which stands as the very basis for the search for knowledge (ibid, p.262). The search for knowledge is an attempt to come to grips with the unfathomable mysteries of our birth and "philosophical curiosity is sexual

curiosity, sublimated, of course" (ibid, p.264).

Scharfstein and Ostow point out that philosophers have in general had difficulties in gratifying their sexual curiosity because of their near pathological fear of sex. They document that a number of professionally influential philosophers were sexually fearful: they conjecture that neither Leibniz, Locke nor Kant had sexual intercourse with a woman (ibid, pp.264-265). Such a fear is, they claim, on the basis of uncited but "reliable clinical evidence," the result of one's love for one's mother. Philosophers are "frightened lovers of their mothers" (ibid, p.266), who being unable to sexually and emotionally express this love, build philosophical systems which are nothing more than representations of their mothers. However mother is sexually unobtainable and the philosopher stands in the ambivalent position of wanting to love his mother, but being simultaneously afraid to love her. This ambivalent position is responsible for sadomasochistic behaviour: "because he often cannot allow himself any sensual love play, he substitutes the pleasure of sadomasochistic reasoning for it. In the process, he may well choose to hurt others; he surely hurts and pleases himself. He prefers to continue to enjoy the pleasure than to end it by 'solving' the problems" (ibid, p.271).²

Now Scharfstein and Ostow propose that it is in the interests of sound mental health that we do not repress the facts as they have uncovered them. Further, it is reasonable to conclude that if their diagnosis is correct, then we have a medical solution to the problem of perennial philosophical disagreements. But if this medical solution is totally ineffectual, then we have strong inductive evidence against the assumption of the correctness of their diagnosis.

Their medical solution to the problem of perennial philosophical disagreements amounts to this:

(MS1) A philosopher should not suppress his sexual desires and should have a healthy sex life;

(MS2) A philosopher should not suppress his sexual and emotional feelings for his mother - perhaps he should have, if she is willing, regular sexual intercourse with her.

If (MS1) and (MS2) are met, then given that Scharfstein and Ostow's diagnosis is correct, there should be observed a convergence towards consensus within the philosophical community on issues in ethics, social philosophy, aesthetics, logic, epistemology and metaphysics, for example. But given the existence of incest taboos and laws against incest in all countries where there are a significant number of philosophers, it is empirically impossible to document the success of (MS2): what philosopher would publicly admit to sleeping with his mother? To do so would be to face social shame as well as possible legal ramifications.

Empirically matters are not so difficult with regard to proposal (MS1). There is a balance of reason against the claim that male philosophers today live a sexually ascetic life which Leibniz, Locke or Kant may have lived. Many leading philosophers today are married, or have been married, and it is incredible to suppose that most of these heterosexually orientated philosophers have not had sexual intercourse. However the problem of perennial philosophical disagreements is still with us, which strongly indicates that it is not sexual repression which is explicitly responsible for the lack of consensus

about basic issues in any selected field of philosophy.

There is another hypothesis which accounts for the sexual asceticism of Leibniz, Locke and Kant. It is not a fear of sex which prevented these philosophers from pursuing fleshly pleasures, but the fact that these men were preoccupied with their work, and they simply didn't have time to endure all the trials, pains and tortures of romantic love. I can no more give good empirical reasons for believing that my hypothesis is true than Scharfstein and Ostow can for their hypothesis. Nevertheless we can explain the alleged social fact that a number of professionally influential philosophers lived sexually ascetic lives without postulating some overriding fear of sex which engulfed them. Considerations of economy support my explanation rather than that of Scharfstein and Ostow.

Let us summarize our conclusions. Lazerowitz's eliminativist view of philosophy is itself premised upon both philosophical and scientific assumptions. In the course of the argument of this section, both Lazerowitz's appeal to psychoanalysis and his specific philosophical arguments have been criticized. This means, if my criticisms are cogent, that the claims embodied in (L0) cited earlier, must fail. In addition I have considered and rejected the arguments of Scharfstein and Ostow which parallel Lazerowitz's own arguments. These authors fail to establish that (PPPD) is due to psychoanalytic causes, and nor do their arguments show (even if Scharfstein and Ostow do not argue for this point) that philosophy is pseudo-scientific, an enterprise which should be abandoned in favour of an empirical science such as psychoanalysis.

3. (ENE) AND THE STRONG PROGRAMME OF THE SOCIOLOGY OF KNOWLEDGE

A contemporary trend towards (ENE) has appeared in the social sciences stemming from the tradition of the strong programme of the sociology of knowledge.³ Of this work, no more explicit statement of (ENE) has been given than that by John Law in his paper "Is Epistemology Redundant? A Sociological View" (1975). There Law proposes that the "logic of science" presented by contemporary philosophers of science is virtually useless as a description of what scientists do, and if taken as a prescription for scientific activity, remains far too vague to be of any practical use to any scientist in the process of research. Law thus views "epistemology" (specifically the philosophy of science) as redundant and open to replacement by the sociology of knowledge programme. Whilst Law's proposal is far from clear, such a replacement represents a virtual theoretical elimination of epistemology, parallel to the elimination of mentalistic languages and theories which eliminative materialists look forward to (Rorty, 1965), (Shope, 1979), (Churchland, 1981).

I have criticized Law's position elsewhere (Smith, 1983(b)). Here I will criticize proponents of the strong programme of the sociology of knowledge who would attempt to explain by reference to purely social causes, the existence of perennial philosophical disputes. It will be argued that the resulting sociological hypothesis is virtually incapable of confirmation by any standard social research methodology. The relevance of this conclusion for metaphilosophical inquiry is immediate: the existence of perennial philosophical disputes must be explained other than by recourse to social variables.

I shall select one significant work from the tradition of the strong programme of the sociology of knowledge: B. Barnes' *Interests and the Growth of Knowledge* (1977) and investigate how one might attempt to explain why perennial philosophical disputes have occurred. This book discusses four broad areas: knowledge, ideology, imputation and the power of knowledge. For our interests here, the first three areas are of importance.

Barnes opts for an instrumentalist account of knowledge: knowledge is primarily produced and evaluated in terms of an interest in prediction and control, and it is normative, being sustained by a communal consensus (ibid, p.18). The significance of this view is summarized in the following words (ibid, p.24):

The upshot of all this is that our current scientific models and mechanisms are likely to be seen at some future time as part of what is an endlessly unfolding chain of such mechanisms, constructed and eventually abandoned (or stripped of their ontological standing) as the activity of knowledge generation proceeds. Clearly then our present theories should stand symmetrically with earlier scientific theories, and for that matter with any other instrumentally oriented knowledge, in all sociologically relevant respects. The diverse real universals postulated at different times and in different cultures and contents, should be regarded alike as inventions of the mind, sustained to the extent that they are instrumentally valuable in the settings where they are found. There is no means of going further and ranking or evaluating them in a way which does not simply *assume* the priority of one or other of them.

We would then expect Barnes to hold that a significant social phenomenon such as perennial philosophical disagreements, must be explained by reference to interests in prediction and control. The difficulty however is that we do not find here communal consensus between philosophers over questions of how such disputes may be resolved. Hence we

will need further sociological machinery if this phenomenon is to be satisfactorily explained at all. A theory of ideology would be an interesting point of departure.

Knowledge is taken by Barnes to be ideologically determined, "in so far as it is created, accepted or sustained by concealed, unacknowledged, illegitimate interests" (ibid, p.33). Individuals may well be consciously unaware of these interests and of the cognitive processes involved. Such beliefs however, do not carry their ideological nature as an imprint, stamped as it were upon them. Nor is there any good reason why beliefs related to concealed interests in one context, may not serve legitimate interests in prediction and control in another. How then can the existence of concealed interests be demonstrated?

Barnes rejects two popular ways in which concealed interests might be demonstrated. The first is to show that the beliefs and theories in question could not be arrived at solely through a rational attempt to understand and predict. This residual element would be then taken to be due to ideological forces. The difficulty with this position, Barnes points out, is that we do not have a sufficiently powerful theory of natural rationality which could show that the cognitive propensities of agents in a given cultural context would theorize in a particular way (ibid, p.34). The second attempt involves finding isomorphisms between sets of beliefs, where the structure of one set of beliefs is mirrored in another, and the one is invoked to legitimate the other. An excellent example of this is seen in an early seventeenth century refutation of Galileo's discovery of the moons of Jupiter (citation from Taylor, 1982, p.94):

There are seven windows given to animals in the domicile of the head, through which the air is admitted to the tabernacle of the body, to enlighten, to warm and to nourish it. What are these parts of the microcosmos: Two nostrils, two eyes, two ears and a mouth. So in the heavens, as in a macrocosmos, there are two favourable stars, two unpropitious, two luminaries, and Mercury undecided and indifferent. From this and from many other similarities in nature, such as the seven metals, etc., which it were tedious to enumerate, we gather that the number of planets is necessarily seven.

Nature here is seen to mirror the God-constructed microcosmos.

The difficulty which Barnes finds with the isomorphism thesis is that the thesis is ultimately unrevealing in itself. Explanation and understanding in science, insofar as it makes use of metaphors and analogies, inevitably generates such isomorphisms. This leads Barnes to claim that at the present time, there are no objective procedures by which the influence of concealed interests upon thought may be uncovered. This leaves us with only a subjective experimental approach, whereby we adopt the cultural orientation of the actor, and assess what plausibility the adopted beliefs have for us (ibid, p.35). The assessment is made by the "virtual experience" of the investigator (ibid, p.35). Barnes says little about such experiences apart from the claim that in practical explanations, how the role of concealed interests is to be assessed in speculation is an outstanding difficulty (ibid, p.37).

Let us ask, nevertheless, what a plausible sociology of knowledge account of the social phenomena of perennial philosophical disputes might look like from the material which we have taken so far from Barnes. The most plausible hypothesis (H1) which I can come up

with is this: it is in the interests of philosophers to disagree whether they are aware of this or not. The reward system of philosophy in terms of the attribution of fame and honour, is such that the mere developers of frameworks established by others, are faced by the prospects of obscurity. Trail blazers however, do not labour under the shadow of some great philosopher, and indeed get their notoriety from tearing down the framework established by that great philosopher. These institutional conditions make the existence of perennial philosophical disputes quite likely.

(H1) has merits which alternative sociological hypotheses lack. One popular strategy, beloved by structuralist Marxists, is to explain superstructural phenomena such as the existence of perennial philosophical disputes, with respect to infrastructural variables especially economic variables. This style of explanation faces immediate difficulties here due to the frequent abstract and trans-practical nature of the explanandum. It may well be argued with plausibility that specific philosophical disputes are of immediate relevance to the interests of a dominant social class, but it cannot be argued with any plausibility, as far as I have ever observed, that *all* philosophical questions are like this. Examples of some that would not include: "Why is there something rather than nothing?", "the antinomy of the liar", "the paradox of the surprise examination", "the justification of deduction" and so on. Structuralism must somehow relate such questions to real material interests: the problem is, that such questions are regarded by virtually everybody but the structuralists, as precisely those sorts of questions which cannot be related in any

sociologically interesting way to real material interests.

Hypothesis (H1) fares somewhat better than the structuralist position, in that it seems to provide an explanation of perennial philosophical disputes even in quite abstract areas of metaphysics and the philosophy of mathematics. But on closer methodological examination (H1) is demonstrably unsatisfactory. Let us turn to such an examination.

Difficulties with (H1) not only lie in conceptual problems facing the notion of 'interest' and 'philosophical honour' but with the actual empirical process of testing (H1). Here my concern is with the latter set of difficulties rather than with the former. The question which I wish to ask is: could we ever have, within standardly acceptable ranges of accuracy, good reason to believe that (H1) is true?

Let us note immediately, that the mere existence of the historical phenomena of disagreements, and of the younger philosophers tearing down the established frameworks of the older philosophers is not in itself evidence for (H1). To argue in this fashion would surely be circular reasoning. This is the phenomenon which (H1) must account for.

Before considering how one would go about obtaining confirming evidence for (H1), we can note from the outset, a possible falsifiable consequence of (H1). If it is the case that philosophers are primarily motivated by the desire for honour and fame rather than a pursuit of the truth, we would expect that mere intellectual underlabourers *have* faced obscurity. To develop and test this implication is not immediately easy: we need some operationalizable idea of a "great philosopher" and a "mere underlabourer", as well as some

satisfactory account of what it means to be a "success" in philosophy and what it means to be "obscure". To explicate these notions would certainly be difficult, but such notions do have great intuitive appeal and are not obviously incoherent. If one could succeed in this task, it is expected that mere underlabourers would not be successes in philosophy, and if we found otherwise, then (H1) would be immediately brought into question. Suppose however that (H1) passes this hurdle: how would one go about obtaining confirming evidence for (H1)?

To obtain such evidence, we would need confirming evidence for all historical periods about the relationship between philosophers' interests in obtaining honour and the reward system of philosophy of the time. This is a task of virtual superhuman proportions, so we will restrict our attention more or less to present day philosophy, let us say philosophy done this year. Our sample is thus restricted to authors, most of whom are still alive and in principle at least, open to questions from a social inquirer. To make our tasks even easier, let us narrow our sample down to academic philosophers working in philosophy departments in the universities of one country. In what follows, I will assume that the reader has a basic understanding of standard methods of social inquiry (cf (Moser and Kalton, 1975)).

The least difficult part of our inquiry would be to understand the reward system of academic philosophy of today. One could do this virtually by an appeal to already existing data about awards, conditions for promotions and pay increases, and so on. The principal methodological difficulty in my opinion is attempting to support the claim that the interests which philosophers have in obtaining such rewards leads to situations of perennial philosophical disagreements.

Let us run through all the existing popular methods of social inquiry and see what help they are to us.

Any appeal to existing data on this topic is impossible: the issue at stake is how we in fact *get* such data in the first place, and without a satisfactory response to this question the proposal is utterly circular.

One might next try a questionnaire approach. We have stated the problem so that we do not have very much problem with sampling and returns. Our subjects, we assume can respond to our questionnaire. The difficulty however is that even if our philosophers answered truthfully, according to Barnes, a completely negative response to carefully designed interest-testing questions would not refute (H1). The interests are not something which a subject need even be consciously aware of. Another major problem is that *if* such philosophers were simply doing philosophy for egoistic reasons rather than for reasons of an altruistic pursuit of the truth, would they admit it to a social scientist? Hardly! Such a confession seems outrightly self-defeating. Even if an individual was protected by a veil of anonymity, disclosure of such information may have severe long term effects on philosophy which no doubt will threaten all philosopher-egoists. Why take a risk solely to help a nose-y sociologist? Therefore a questionnaire inquiry is highly implausible.

Perhaps one could resort to *verstehen* techniques as Barnes suggests. Yet these as well face severe limitations in areas where self-deception, lies, cheating and criminal activities are operating. Can my empathetic understanding of a group of philosophers be accepted, when for all I know, they may well, in accordance with (H1) be

unconsciously deceiving themselves and others? Once more, it seems that the data which I would need as a sociologist is barred from my observation.

Sociologists frequently make recourse to other techniques such as the observation of *traces*. The use of various facilities might for example be roughly gauged by wear and tear. What traces could possibly be of relevance to our inquiry here? Certainly confession notes might well be, as might diary entries and other autobiographical data. But most of this material is unavailable to any practicing sociologist who wishes to stay out of the law courts. Even if one sought to steal such items, if they existed, the support which they would give to (H1) is minimal. One would need not only an extensive array of data about any such individual philosopher, but one would need as well such an array of data about a significant number of philosophers in one's sample. The hope to obtain this sort of material is a hope which is utterly utopian, ignoring even its gross immorality.

This seems to exhaust the major methods by which (H1) might be tested, and confirmed. None of these are even close to being satisfactory. This then leaves us with the possibility of either accepting that (H1) cannot be evidently supported (even though it might well be true), or rejecting (H1) as a methodologically invalid hypothesis. Either horn of this dilemma is sharp enough for one to wish to avoid contact with: if (H1) cannot be empirically supported, then there is no satisfactory sociological account of perennial philosophical disagreements. But to reject (H1) as a methodologically invalid

hypothesis is to reject what seems to me to be the only plausible sociological account of perennial philosophical disagreements.

A more general moral in conclusion: proponents of the strong programme of the sociology of knowledge frequently take a Laplacian view of the explanatory scope of sociology. This is hardly reasonable, and we have made little progress here with our examination of (H1). Thus alternative explanations of the existence of perennial philosophical disputes must be sought.

4. (ENE) AND QUINE

The philosophy of Quine does resemble, as Hilary Putnam is fond of remarking, a large continent with sprawling deserts, lofty snow capped mountain ranges and Okefenokee swamps, and this is without doubt far too much territory to even attempt to travel even briefly here. Consequently I shall concentrate solely upon Quine's paper "Epistemology Naturalized" (1969(a)). This in itself is not arbitrary choice, since this paper contains Quine's clearest and most detailed statement of (ENE).

Quine's paper at face-value advocates the elimination of traditional epistemology. He states (ibid, pp.82-83):

Epistemology, or something like it, simply falls into place as a chapter of psychology and hence of natural science. It studies a natural phenomenon, viz., a physical human subject. This human subject is accorded a certain experimentally controlled input - certain patterns of irradiation in assorted frequencies, for instance - and in the fullness of time the subject delivers as output a description of the three dimensional external world and its history. The relation between the meager input and the torrential output is a relation that we are prompted to study for somewhat the same reasons that always prompted

epistemology; namely, in order to see how evidence relates to theory, and in what ways one's theory of nature transcends any available evidence.

He states further in another important passage (ibid, p.83):

The old epistemology aspired to contain, in a sense, natural science; it would construct it somehow from sense data. Epistemology in its new setting, conversely, is contained in natural science as a chapter of psychology. But the old containment remains valid too, in its way. We are studying how the human subject of our study posits bodies and projects his physics from his data, and we appreciate that our position in the world is just like his. Our very epistemological enterprise, therefore, and the psychology where in it is a component chapter, and the whole of natural science where - in psychology is a component book - all this is our own construction or projection from stimulations like those we were meting out to our epistemological subject.

There is for Quine then, no "prior" or "first philosophy".⁴ For Quine, there are insuperable problems facing any traditional epistemological response to the sceptic, with regard to the justification of our knowledge of truths about nature. Quine despairs of any satisfactory response to the sceptical dilemmas raised by Hume. As he puts it: "The Humean predicament is the human predicament" (ibid, p.72). No more is said on this issue, and no more need be said as Quine's position is clear. Like Hume he sees no satisfactory response to the sceptic, beyond abandoning the cultural practice of epistemology in favour of natural science. I shall argue that this in itself is no satisfactory response to the sceptic.

The second reason which Quine has for opting out of the cultural practice of traditional epistemology is the failure of conceptual reduction in the form of phenomenalism as represented by Carnap's programme in

Der Logische Aufbau der Welt. Not only did Carnap's programme prove incapable of completion, but Quine argues, in the light of the thesis of the indeterminacy of translation, statements about the world do not always have a distinct domain of empirical consequences unique to themselves. Thus conceptual reductions where every sentence is equated to a sentence in observational and logico-mathematical terms, fail (Quine, 1969(a), p.82).

It is pointed out by Hilary Putnam that Quine in conversation has repeatedly said that he has not proposed that the "normative" be eliminated (Putnam, 1982(b), p.19). Indeed as Putnam points out, Quine in a later paper (Quine, 1975) has stated criteria for a choice of "systems of the world". Putnam is right to find this puzzling: Quine is inconsistent. Let us however distinguish between "Quine₁" the naturalistic epistemologist, and "Quine₂" the later Quine. Our concerns are with Quine₁.

Regardless of this point, the arguments of Quine's paper "Epistemology Naturalized" require response. Here we can see that Quine simply has not made any satisfactory case for the claim that traditional epistemology is illegitimate. The second reason cited above, is that the programme of conceptual reduction advocated by the early logical empiricists is inadequate. This is true. However, we only would establish that *all epistemology* is inadequate, if there were no alternative epistemologies to that of reductive logical empiricism - that there is *is* obviously true. Therefore Quine's second argument is unsatisfactory.

The first argument is *prima facie* more substantial: I do not claim to have any systematic and fully detailed response to Hume's scepticism about induction. The problem is not satisfactorily solved by Popper either - for the reason that at crucial places, Popper exhales more than a mere "whiff" of inductivism (O'Hear, 1980) (cf also (Stove, 1982) for a substantial critique). This controversy need not be entered here, so let us accept for the sake of argument that there is as yet no satisfactory response to Hume's challenge. Quine's (ENE) will face immediately a major problem: the problem of induction threatens to demonstrate that we do not have any "naturalistic knowledge" as well! This is the reason in fact why this problem has received such extensive treatment by philosophers from many traditions since Hume first explicitly stated the problem. Quine does nothing to show (unlike Karl Popper) that Hume's problem does not destroy the very basis of natural scientific reasoning. If Quine proposes that the problem of induction renders epistemology illegitimate, then he is also committed to claim (quite against his wishes of course) that the problem of induction renders natural science cognitively illegitimate as well. This leaves Quine with the dragon of epistemological scepticism, and without any epistemic swords to fight it. I take this conclusion to be unreasonable and to constitute a *reductio ad absurdum* the tenability of Quine's (ENE).

Barry Stroud (1981) has argued that Quine's version of naturalized epistemology is committed to the coherence of the traditional epistemological question of the justification of empirical and conceptual knowledge claims. Since the "input" from the external world is always

in general open to isolation from everything which we believe about the world as a result of this "input", the general possibility that the objective world is different from the way we believe it to be, stands open to consideration, as does the question of how we know that the sceptic's scenario does not obtain. This Stroud argues is not answered by any naturalistic epistemology. Briefly, an argument for this proposition, but one not given by Stroud is as follows. Any naturalized epistemology will take certain "natural" phenomena to exist: sense data, physical particles ordinary material objects and so on. The epistemological sceptic now asks for a justification of any of these existential claims. Then the sceptic must show that for logical and conceptual reasons, the naturalistic epistemologist fails to solve this problem, or else that the solution which the naturalistic epistemologist gives is inconsistent with the position of naturalized epistemology. Quine has replied to Stroud's paper, so it will be informative to state his reply, assess its merits and anticipate, if possible, objections to the arguments advanced here.

Quine (1981) begins his reply to Stroud by asking us to consider an inclusive theory of the world formalized within the framework of predicate logic. Let 'Fxy' stand for some open sentence that determines x uniquely for each value of 'y' and vice versa, and let 'p' be a one-place predicate. Then reinterpret 'Pz' for each value of 'z' as ' $(\exists y)(Fzy \ \& \ Py)$ ' and reinterpret every primitive one-place predicate in this way, and every primitive many-place predicate in this fashion as well. Such proxy functions, preserve the structure of our theory, and even fail to change its links to the observational evidence.

With this we agree. From this however, Quine claims that all our inclusive theory of the world really claims regarding the nature of the world is "that it is somehow so structured as to assure the sequences of stimulation that our theory gives us to expect. More concrete demands are indifferent to our scientific theory itself, what with the freedom of proxy functions" (ibid, p.474).

Given this statement it is indeed difficult to see how Quine can avoid the descent into epistemological scepticism. One well known form of epistemological scepticism proposes that we can have no knowledge of the world based upon our own sensory experience or "stimulations". To this scepticism Quine claims that it is an "overreaction" (ibid, p.475). This claim is however unsatisfactory: perhaps this "overreaction" is *reasonable*? Quine's previous argument from the proxy-functions, is taken to establish that the only real claim which our inclusive theory of the world commits us to, is that sequences of stimulations are preserved. This the sceptic proposes, may occur even if the world was different from what it was, or in the case of solipsism, if there was no world at all. Quine then has no right to claim that "people, sticks, stones, electrons, and molecules are real" (ibid, p.474) - all he has access to as a consistent physicalist-empiricist are sequences of sensory stimulations. It is a mystery as to how one salvages a world from this phenomenon without making use of some epistemological principles. Yet to do this is to immediately abandon the enterprise of naturalized epistemology.

Quine believes that a "robust realism" where "sticks, stones, electrons and molecules" are real and not merely "dim proxies" (ibid,

p.474) can be secured by naturalism, "the recognition that it is within science itself, and not in some prior philosophy, that reality is properly to be identified and described" (ibid, p.474). Elsewhere (Smith, 1983(b)) I have argued that such a form of naturalism is committed to either dogmatism or relativism. What is this thing called "science"? Are the creationists engaging in science or not? If they are not then why not? Quine's answer would no doubt be that the creationists are not engaging in science and that this can be shown by a careful study of "sensory observations". But this would only show that creationism is false, science, not that it is pseudo-science. Quine's naturalism will place upon us a very great restriction in our range of critical methods and make us dogmatists in accepting received science rather than some fringe position. The "furniture of the world" dwindles to merely "manners of speaking" for the Quinean naturalist and "other purported objects would serve as well, and may as well be said already to be doing so" (ibid, p.474). Nothing prevents us from taking once more these consequences as a *reductio ad absurdum* of Quine's naturalism. Alternatively if science is not to become choked by metaphysically problematic yet seemingly empirically equivalent systems of the world, then recourse must be made to a prior epistemology or metascience which serves as an organon of criticism.

It is concluded that Quine's (ENE) is untenable. Rather than escaping from the need for a traditional justificationist epistemology, Quine's (ENE) seems to require it, and such a requirement is straight forwardly incoherent for a position that claims to have eschewed the traditional epistemological framework.⁵

5. ANALYTIC NATURALIZED EPISTEMOLOGY (ANE)

The project of (ANE) attempts to explicate epistemic notions such as 'justified', 'warranted', 'has (good) reasons', 'has reason (to believe)', 'knows that', 'is probable' and so on. To illustrate what is involved here, the reader may compare the project of (ANE) with Harty Field's contention (Field, 1972) that Tarski claimed that his work on truth made semantics physicalistically respectable by explicating a small number of primitive semantic notions in physical terms (Tarski, 1956, p.406). This Field argues, has not been established by Tarski; here I will argue that my selected specimens of (ANE) also fail.⁶

For William Lycan (198+), the place of epistemic notions in the "closed causal order we call nature" is unclear, and the task of "naturalizing" them is as difficult as the task of naturalizing moral goodness. Since Lycan as a naturalist holds that there is no real differences which are not at bottom natural differences, to naturalize epistemology, he must show that there is something in nature "that distinguishes reasonable belief from unreasonable belief" (ibid, p.1). In actual fact however, all that is established by this proposal is a naturalistic analysis of one epistemic notion - but this is by no means a trivial accomplishment if Lycan is successful in this task. I will argue, that on the contrary he is not.⁷

Lycan cites five principles which serve as selection procedures for distinguishing the best "theory" from a number of available theories. For reference these principles are as follows:

1. Other things being equal, prefer T1 to T2 if T1 is simpler than T2.
2. Other things being equal, prefer T1 to T2 if T1 explains more than T2.
3. Other things being equal, prefer T1 to T2 if T1 is more testable than T2.
4. Other things being equal, prefer T1 to T2 if T1 leaves fewer messy unanswered questions behind (especially if T2 itself *raises* messy unanswered questions).
5. Other things being equal, prefer T1 to T2 if T1 squares better with what you already have reason to believe.

The issue as to whether these principles are satisfactory will not be discussed here: all that is necessary for the present argument is for one to accept that such principles could be satisfactorily expanded and explicated. Further these principles are not intended by Lycan as a description of the reasons people in fact have for their choices of hypotheses, that is, as a causal explanation of actual theory choices. Rather, the set of rules is taken to constitute a normative theory of justification, characterizing the distinction between rational and irrational theory choices. It is such a distinction which Lycan must "naturalize". I shall argue that he fails.

To do this Lycan advances an "optimality argument" based upon the metaphor of a skillful and benevolent "Mother Nature".⁸ This metaphor is adopted by Lycan to simplify his argument, and I shall grant this simplification. Shortly however I shall investigate how Lycan's position might be defended by one with an eye more keen for questions of biological detail. The question then to investigate is how one can ground "naturalistically" the above principles of theory choice. Lycan treats this as the question of *why it is good or utile* to use these principles rather than others, such as the precise negation of these principles. Thus we must ask, what cognitive habits

would a skillful and benevolent Mother Nature have given us in order that we might go on to form maximally utile beliefs?

To take one example, Lycan proposes that Mother Nature would have built us to prefer simpler hypotheses to complex ones. He advances the following reasons for this (ibid, pp.16-17):

(a) Simpler hypotheses are more efficient to work with. A simple handbook of rules, such as the Boy Scout Manual, is easier to use than is the 1976 U.S. Tax Code. (b) As Russell observed in defense of his version of Occam's Razor, complexities incur greater risk of error. A simpler device has less that can go wrong with it (think of a simplified record turntable or auto engine). (c) Simplicity is itself a form of efficiency. The whole point of obtaining simple and unified hypotheses in science is to achieve plenitude of result (in the way of data explained and results predicted) with parsimony of means. If we were not able to mobilize a few simple hypotheses and thereby obtain maximally informative analysis of the news, especially in the way of experiential predictions, we would be far less competent in coping with environmental developments; the world would present us with too many surprises and they would overwhelm us.

This "utility" Lycan proposes must be understood on the basis of neo-Darwinian accounts of "inclusive fitness" (Wilson, 1975): the recipient is the gene-type and the utility is its potential for being passed on. Thus operating according to the rules of theory-preference, Lycan alleges, we do maximize our inclusive fitness. Alleged counter-examples which many philosophers have produced, such that our rationality and science may well lead to our thermo-nuclear-extinction (Tennessen, 1973), are dealt with, successfully I believe, by claiming that it is not rationality alone which produces such genetically undesirable effects.

Lycan seems to follow the sociobiological tradition of "just-so" Panglossian selectionism which has been satisfactorily demolished in my opinion by the biologists Gould and Lewontin (1979). Lycan believes that he can avoid the Panglossian fallacy by arguing that design is optimal only relative to various physiological constraints, such as general anatomy (Lycan, 198+, p.25). This whilst true is not sufficient to rebut the objection of the Panglossian fallacy and the spinning of "just-so" stories. Let us detail why.

As is illustrated by Lycan's case of the simplicity rule, what has been presented is a set of plausible assertions that a skillful optimizing Mother Nature would build us to prefer simpler hypotheses to complex ones. This however tells us nothing about the actual traits which people have - which Lycan told us, may differ from his normative theory. He claimed not to be presenting an actual causal account of human reasoning, which is as is well known riddled with vagueness, wishful thinking and so on. Therefore in the light not only of evidence of human "irrationality" (taken now to be the failure of conformity with Lycan's normative theory) in both our own culture and others, the claim that adherence to Lycan's principles does result in a maximization of inclusive fitness requires empirical support. It is only reasonable to ask a naturalistic epistemologist for this - but no such evidence has been presented. Consequently Lycan's position is unsupported. Indeed it is extremely difficult to see how one could ever test Lycan's hypothesis, since we would need a human population where (1) propensities to believe Lycan's principles and act in accordance with them by contrast to not doing this, are of an inheritable basis and (2) two subpopulations exist of believers

and non-believers, who can be compared in fitness terms. It is easier to spin myths.

This is a practical difficulty - there is however a major theoretical difficulty facing Lycan's position. Selectionist arguments tacitly presuppose the prior evolution of the traits under consideration. Natural selection only operates if variation exists. But for variation to exist, the relevant traits must already be present. Hence any adequate evolutionary theory must be concerned with ontogenetic aspects of organisms (Ho and Saunders, 1979). In this domain Lycan's thesis suffers badly. Not only does he assume that the brain is a limited-capacity information processor,⁹ but he also assumes that dispositions to specific beliefs such as his principles of theory choice could be wired in. The former assumption is not only challenged by the "frame problem" in cybernetics (Dreyfus, 1979), but lands one in the dilemma of having to postulate enormous quantities of causative genes, which is totally inconsistent with Lycan's own canon of parsimony.

Much of human intellectual inquiry is only vaguely, if at all, linked with survival value. Only the slightest glance at the history of ideas of human kind informs us that one generation or culture frequently rejects the basic theories of another - among the graveyards of human thought lie virtually every philosophical system of the past (as it was initially stated), countless cosmologies, religions and so on - and yet our ancestors survived to produce us! It is true that a number of human beliefs if acted upon, may have radical effects on inclusive fitness - leading ultimately to extinction. A critic of evolutionary epistemology need not deny this. It is sufficient to

point out that evolutionary modes of explanation have their limits, and rationality is, I conjecture, one of them.¹⁰

The problem of accounting for rationality within the framework of the neo-Darwinism has also been discussed in a paper by Elliot Sober (1981). The problems facing an evolutionary account of the origins of rationality are: (1) much of the "scientific method" appears to confer no practical benefit of survival to those who employ it; (2) even if it did, the "scientific method" is not more fit than competing irrational methods in specific environments; (3) the scientific method and its competitors may be "locally equivalent" within a specific environment, and hence selectionally blind. Here I shall address Sober's response to (1).

With respect to the first problem Sober gives two replies. The first is the claim that scientific reasoning may be a pleiotropic effect of a single gene combination (ibid, pp.99-100). This, like the other stories told by sociobiologists is logically possible but unlikely in view of what the social sciences have told us about the social genesis of the scientific method. In any case no such gene combination has been isolated and until it has this conjecture is rightly regarded as science-fiction. Sober's second argument is addressed to showing that the bifurcation between techniques (or principles) for processing information which have some practical consequences for survival, and those techniques which are central to pure science and have no impact on inclusive fitness. It is unclear to me what precisely Sober's argument is for the conclusion that this distinction is not represented in nature. Nothing however hinges on a distinction between theoretical and practical reason. The point

that much of scientific inquiry, theorem proving, metaphysical argumentation and so on, does nothing to influence inclusive fitness (and may even distract one from reproductive concerns) remains unanswered.

Before closing this section I shall discuss the alternative form of (ANE) which has been stated and defended by Alvin Goldman (1979). Goldman proposes a theory of justified belief to specify in non-epistemic terms when a belief is justified. Thus a criteria of material adequacy for his analysis, is that only non-epistemic terms appear in the explicatum of any proposed analysis. A theory of justified belief will be a set of principles that specify truth-conditions for the schema '[S's belief in p at time t is justified]', thus supplying conditions for the satisfaction of this schema in all possible cases. The definition which Goldman proposes is recursive.

Goldman proposes that the failure of most current attempts to explicate epistemic notions is due to the absence of causal requirements in these principles. Counter-examples frequently involve beliefs being caused in some epistemically unacceptable ways. Thus he maintains, the correct principles of justified belief must be principles that make causal requirements, not only for "base-clause" principles, but also for recursive principles as well. For Goldman the "justificational status of a belief is a function of the reliability of the process or processes that cause it, where (as a first approximation) reliability consists in the tendency of a process to produce beliefs that are true rather than false" (ibid, p.10). This is not to say that there can not be justified beliefs which are false on Goldman's account: how reliable a belief-forming process must be in order that

its resultant beliefs be justified, is left vague by Goldman because our ordinary conception of justification *is* vague in this respect. So also is the concept of a 'tendency' in this epistemic context, which may refer to either actual long-run frequencies of truth to error or to a 'propensity' to generate true-beliefs. Finally by a 'process', Goldman means a functional operation or procedure which generates a mapping from input-states into output-states (these being states of believing this or that proposition at a given moment).

We will say that a process is *conditionally reliable* when a sufficient proportion of its output-beliefs are true given that its input-beliefs are true. *Belief-dependent cognitive processes*, are processes *some* of whose inputs are belief-states, and *belief-independent cognitive processes* are processes *none* of whose inputs are belief-states. Goldman then proposes that the following two principles, with a standard closure clause gives us a complete theory of justified belief (ibid, pp.13-14):

- (T1) If S's belief in p at t results ("immediately") from a belief-independent process that is (unconditionally) reliable, then S's belief in p at t is justified.
- (T2) If S's belief in p at t results ("immediately") from a belief-dependent process that is (at least) conditionally reliable, and if the beliefs (if any) on which this process operates in producing S's belief in p at t are themselves justified, then S's belief in p at t is justified.

To deal with the difficulty that even though the causal ancestry of S's belief is fully reliable, S may have reason to believe that S's belief is caused by an unreliable process (perhaps on the basis of a currently accepted scientific theory which fails precisely in the domain about which S has some specific beliefs), Goldman advances

this modification:

- (T3) If S's belief in p at t results from a reliable cognitive process, and there is no reliable or conditionally reliable process available to S which, had it been used by S in addition to the process actually used, would have resulted in S's not believing p at t, then S's belief in p at t is justified.

The final position seems then to be a materially adequate definition.

But is it correct?

Hilary Putnam (1982(b), p.7) has suggested in passing, with little argument, that Goldman's (ANE) is vulnerable to the following style of counter-example: suppose Tibetan Buddhism is true and that the Dalai Lama is epistemically infallible. Then the method:

(T4) Believe that which Dalai Lama says results in a 100% reliability of assessment of those true beliefs in a sample of true and false beliefs, even though the only argument which a follower of Tibetan Buddhism might be able to give for his/her belief that Dalai Lama is never wrong is "the Dalai Lama says so". For this to be a counter-example it must be shown that this instance is not really ruled out by (T1), (T2) and (T3), which Putnam has failed to do. Let us examine this question now.

(T4) is conditionally reliable, since 100% of its output-beliefs generated in our Tibetan Buddhist are true given true input-beliefs from Dalai Lama. Further the processes in question are belief-dependent cognitive processes, since what our Tibetan Buddhist gets from Dalai Lama are "beliefs" (or less loosely, input-information for the formation of beliefs) rather than just pieces of matter and energy. According to (T2) the beliefs upon which this whole process operates, i.e. of Dalai Lama, must themselves be justified by a reliable method

and that there is no other competing method available to Dalai Lama which would have resulted in Dalai Lama not believing what is believed.

It would seem at first sight that Putnam's counter-example immediately fails: there could be no other competing method available which would result in Dalai Lama not believing what is believed because all of Dalai Lama's beliefs are true by hypothesis. Whilst this reply is true, it does not answer a major problem which lays at the root of Putnam's complaint. This is that the long-run frequency of the selection of true beliefs is inadequate precisely because the mystical methods of Dalai Lama lack in any intuitive sense, philosophical justification (and mystics might well agree with this pointing out that this is so much the worse for Western philosophy!). The appropriate mechanism which produces true beliefs, whilst "reliable" in Goldman's sense, may be "irrational in an intuitive sense. But alas any (ANE) must analyse epistemic notions by means of non-epistemic notions under pains of outright failure. This suggests to me that (ANE) *is* in fact, an outright failure. Putnam is therefore correct in his assessment.

6. JUSTIFICATIONIST NATURALIZED EPISTEMOLOGY (JNE)

The final version of naturalized epistemology which I will consider in this chapter is (JNE), and I will restrict my discussion to what I take to be its major defender: Nicholas Rescher.¹¹ Further my discussion will be much briefer than with other authors, since the argument which I will advance against Rescher's (JNE) has already been given against evolutionary epistemology. Thus it should be stressed that I am commenting upon only a selected portion of Rescher's

impressive epistemological project, although an important portion at that.

For Rescher, the seemingly infinite regression of justification (or alternatively the prospects of the *diallelus*) is halted by a pragmatist-Darwinian argument. Methodological and epistemological principles are evaluated in the same manner as we would evaluate any tool or instrument: does it work? Does it produce the desired results? Rationality is thus definable by the concept of knowledge-wed-to-practical activity in controlling nature. Such a Darwinian legitimation requires a standard of "fitness". Whilst Rescher does not take this to be inclusive fitness maximization, he does regard the pivotal issue here to be *survivability*. Indeed he admits that this Darwinian line of methodological justification can only be effective with respect to a culture which has a high density of interactions with the natural environment.

The question which immediately must be asked is: what if this condition is not met, as is the case with the alleged logic-violating beliefs of the Nuer (Kekes, 1979) or perhaps by a possible technological society of the future where cognitive agents live in elaborate *artificial* environments? The answer seems unavoidable: Darwinian survival only could constitute evidence for pragmatic efficacy within certain ecological settings. In others it is impotent. For epistemologists this limitation is quite intolerable, for we surely would like to believe that in an arm-chair society with ample time for pure speculative thought, epistemological theories could still be either justified or not. The fault then lies with Rescher's proposal and his (JNE) fails.

7. CONCLUSION: STATE OF THE ARGUMENT

This chapter has considered various accounts of naturalized epistemology. The final conclusion reached is that none of the above accounts (i.e. (ENE), (ANE), (JNE)) are tenable, and take us no distance towards resolving major outstanding problems in philosophy and especially epistemology. The difficulties which the problem of perennial philosophical disputes raises for epistemology with regard to both its rationality and progressiveness as a cognitive enterprise is not addressed by any of these position: if anything they compound our epistemological problems. Naturalized epistemologies then have little philosophical virtue. But in stating this, I am not claiming that scientific inquiry contributes nothing to epistemology: on the contrary, its contribution is significant. Naturalized epistemologies to be of any philosophical interest must assert more than this, and this extra assertion is, I have argued here, unsubstantiated. In the next chapter I shall consider "internalist responses" to the problem of perennial philosophical disagreements. This is the natural direction which our argument should now follow: if no satisfactory response to the problem of perennial philosophical disputes is forthcoming from "externalist" sources, then perhaps a satisfactory response may be obtained by examining factors "internal" to philosophical inquiry itself.

5. NOTES

1. This chapter draws upon (Smith, 198+(d)).
2. We ignore the strong sexist biases in Scharfstein and Ostow's explanation.
3. On the strong programme of the sociology of knowledge cf. (Bloor, 1973; 1976; 1978; 1981(a), (b); (1982), (Barnes, 1974; 1982(a), (b)), (Bloor and Barnes, 1982), (Law and French, 1974), (Restivo, 1983), (Hesse, 1980), (Laudan, 1981), (Trigg, 1980), (Smith, 1983(b)).
4. Elsewhere (Quine, 1969(b) p.26), Quine is quite explicit about this:

Philosophically I am bound to Dewey by the naturalism that dominated his last three decades. With Dewey I hold that knowledge, mind and meaning are part of the same world, that they have to do with, and that they are to be studied in the same empirical spirit that animates natural science. There is no place for a prior philosophy.
5. For further criticism of Quine's "naturalized epistemology" cf (Giedymin, 1972).
6. A physicalist foundation for semantics is untenable in my opinion precisely because semantical notions of representation are already presupposed to interpret the mathematical formalism of our allegedly best physical theories such as the Hilbert space of quantum mechanics. Hence such physicalist programmes involve a vitiating circularity (cf also (McDowell, 1978)).
7. Lycan devotes considerable space to a defense of his epistemic position of "explanationism" from sceptical challenge. This work is welcome, but will not be discussed here. For a discussion of the explanationist's basic mode of ampliative inference cf (Harman, 1968).
8. On the nature of optimality arguments cf (Maynard Smith, 1978), and for a critique cf (Smith, 1984).
9. For criticisms of this assumption cf (Neisser, 1980).
10. This attitude need not lead to mysticism, Platonism, or any other disliked doctrine. Despite my scepticism about the tenability of the foundations of the neo-Darwinist synthesis expressed in *Reductionism and Cultural Being* (1984), contrary to my critics I certainly accept that the human mind is a product of evolutionary processes. Indeed, even within the framework of neo-Darwinism, good "just-so" stories can be given as for why the skillful "Mother Nature" of Lycan, would

in her optimalistic wisdom, produce a human organism which is behaviourally plastic to a high degree and has a quite small supply of innate "knowledge" (if any): such a creature could only survive through extensive learning, cultural innovation and the resultant modification of its environment. It would be plastic and adaptable to changing environments. The accomplishments of cosmology, elementary particle physics, relevance logic and positivistic epistemology may be taken straight forwardly as the products of cultural evolution. Studying these accomplishments in this way saves one from multiplying adaptationist stories and scores of causative genes.

11. For Rescher's view on the "Darwinian" basis of epistemic legitimacy cf (Rescher, 1973(b); 1977(b); 1979(a); 1980(a), (b)). The argument which I consider here is repeated virtually word for word in most of Rescher's major books, so I shall not cite specific page references.

6. INTERNALIST RESPONSES

1. STATEMENT OF THE ARGUMENT

An *internalist response* to the problem of perennial philosophical disputes, attempts to account for this problem either through an appeal to some general aspect of philosophical inquiry which is taken to be incapable of modification without modifying the basic nature of the philosophical enterprise, or through some undesirable, deeply ingrained but modifiable aspect of the philosophical enterprise. Thus internalist responses diagnose the source of our principal problem without making substantial use of non-philosophical or externalist data. Among the most important internalist responses in the recent literature are (Kekes, 1980), (Moulton, 1983), (Dorter, 1977), (Barber, 1958), (Gallie, 1955-56), as well as a proposed argument based upon Collingwood's notion of absolute presuppositions (Collingwood, 1940).¹

All of these internalist approaches locate the perennial aspect of philosophical disputes in the *nature* of philosophical inquiry. For Kekes the perenniality of philosophical disputes arises because philosophy is concerned with the analysis and justification of *ideals*, which are in turn (as we shall soon see) intimately related to the solution of enduring human problems. Moulton by contrast, sees any style of philosophizing which intrinsically involves adversary, criticism and refutation as not only conceptually inadequate, but sexist as well. Whilst Moulton is not explicit about the matter, it seems plausible to suppose that she would view perennial philosophical disputes as a function of a misguided male-biased and aggressive way of philosophizing. These examples illustrate the two principal types of internalist response to the problem of perennial philosophical disagreements. I shall argue that none of the

positions discussed in this chapter are logically satisfactory, so that they can be immediately disqualified as solutions to the target problem. In each case the view of the nature of philosophy sketched by each author fails to do justice to the theoretical richness of philosophical inquiry, and some positions whilst seeming plausible at a glance, slide into contradiction upon more careful study. I begin my study with an examination of Kekes' excellent book, *The Nature of Philosophy*.

2. KEKES ON THE NATURE OF PHILOSOPHICAL INQUIRY

For Kekes, philosophy had traditionally attempted to provide a rationally justified world-view or *Weltanschauung*, giving an account of the general nature of reality, a view of the "human condition", and a system of ideals which give sense and point to human life. The hope of many philosophers was to obtain *wisdom*: to gain knowledge and make on the basis of such knowledge, reasoned and good judgements in the business of life.

This is only possible he believes if philosophy can provide knowledge. But can one find a single instance of philosophical knowledge in philosophy's 2,500 year history? Even in antiquity, the sceptics made use of the fact of philosophical disagreements to discredit philosophy as a cognitively fruitful enterprise. There is not a single claim about the nature of reality which is not repudiated by as many philosophers using *prima facie* convincing arguments, as there are philosophers championing it. Is there then any reason to believe that philosophical knowledge exists, and that *Weltanschauungs* are open to rational examination and their fundamental assumptions rationally justifiable? If there is no such reason, then as Kekes maintains (Kekes, 1980, p.12):

...all honestly held convictions would have an equal claim upon general acceptance. So science and pseudo-science, history and myth, medicine and quackery, considered judgement and rabid prejudice, would be equally

acceptable. The civilizing restraints of debate, criticism, and rational discussion would disappear and force and propaganda would take their place as the method for settling disputes. Life, then, as Hobbes said, would be "nasty, brutish, and short .

Whether these social consequences would follow from the death of rationality, is a proposition asserted, but not empirically demonstrated by Kekes. But the stakes involved in a reaffirmation of the traditional role of philosophy are consequently *theoretically* high even if our civilization would not collapse if rationality proved to be incapable of cogent justification.

So much then for scene-setting. Kekes' response to the problem of philosophical disagreements is an ingenious attempt to invert the question against those who maintain that philosophical debates are futile, endless discussions of the same kind of questions. Philosophical debates occur mainly through perennial arguments, and whilst these constitute a recurrent discussion of the same type of question, they are far from futile. The perenniality of philosophical problems is generated by basic facts about the "human condition", that is, by enduring human problems whose persistent presence is an inevitable feature of human life as we know it. Thus Kekes' response to the problem of perennial philosophical disagreements, is to deny that such disagreements indicate some inherent defect in philosophy itself. The perenniality of philosophical problems is therefore a virtue, not a vice.

The subject matter of perennial arguments are *ideals*. Ideals are goals which are valued by their possessors, and they direct their behaviour towards achieving them, or in accordance with such goals. Whether or not such ideals are objectively valuable is a question which is logically independent of the question of the definition of the term 'ideal'. It is sufficient that the goal be valued by their possessors. Ideals include

rationality, democracy, equality and sexual responsibility. They also (arguably) include alcoholism and drug abuse and perhaps other activities which Pascal rightly classified as diversions. It is undeniable, I believe, that the goals of some people, namely the alcoholics of the world, are little more than the seemingly endless consumption of alcohol. Such consumptive activities are highly valued by them as ways of coping with life's problems. Therefore chronic alcoholism and drug abuse are ideals. As we shall see later, such phenomena are a solution to various problems of life which people have. Then I will argue that Kekes' categories are far too general to do justice to the peculiar nature of perennial philosophical disputes. At present, the reader should merely note the conceptual obesity of Kekes' notice of ideals.

This objection has already been anticipated by Kekes on pages 46-53 of *The Nature of Philosophy* (Kekes, 1980), and we must now consider a possible rejoinder. There we find that ideals are "human goals whose concrete expressions may be more or less satisfactory" (ibid, p.47). Further, ideals must not be "trivial, personal, or idiosyncratic; they are important in that they represent available options among which people in a given intellectual epoch must choose" (ibid, p.47). In summary (ibid, p.52):

"Ideals, in my sense, are vague and general descriptions of desirable goals. These may be exemplified in societies, institutions, practices, mental states, or conditions of life. What makes them desirable is that they are seen by their champions as the goals to which available solutions of enduring problems should aim.

None of this material gives me cause to alter my sceptical attitude about the adequacy of Kekes' notion of an ideal, a notion which plays an important role in Kekes' epistemology and his solution to the problem of perennial philosophical disputes. What counts as a "desirable goal" changes with the tastes and cultures of social groups. For example,

alcoholism and drug taking come out to be ideals for depressed groups of people. My examples of alcoholism and drug abuse are realistic examples of the (perhaps sadly mistaken) ways in which many young people today deal with the enduring problem of the meaning of life in a secular age. Many teenagers (and not just a few) opt for a drug-induced flight from reality which we feel in no way *solves* this enduring problem. Yet these same teenagers would say that drug abuse is a desirable goal, and hence is an *ideal* for them. This is counter-intuitive. The insane may well view any arbitrary goal, however absurd, as an available solution to an enduring problem, for all Kekes requires for a goal to be desirable is that it be "seen by their champions as the goals to which available solutions of enduring problems should aim" (ibid, p.52). What, therefore, is *not* an ideal? My objection, I believe, must stand because no restriction is placed upon either the groups that have goals, or upon the type of goals that they may have. As the case of the insane shows, nothing which Kekes has said excludes trivial phenomena becoming ideals. This severely limits the usefulness of his explication of the notion of an idea as applied to philosophy.

Perennial arguments may be either external or internal; they are external if the debate is about the merits and acceptability of the ideal itself and internal if the debate is about the interpretation of the ideal and not about its merits and acceptability. External perennial arguments involve conflicting ideals; internal perennial arguments whilst involving acceptance of the same ideal, involve disputes about the necessity and importance of various components of the ideal. Such arguments are said to be perennial not necessarily because they have a long history (although most philosophical problems and resultant debates do), but because of "their lack of finality and recurrence" (Kekes, 1980, p. 20). Further, in such disputes the participants are well aware of the

arguments the other side advances in support of the ideal, and they consciously criticize and champion their respective ideal. This dialectic also occurs with internal perennial arguments.

Internal and external perennial arguments certainly occur; the interesting question is how to interpret them. Kekes rejects two misinterpretations of perennial arguments: (1) they are factual and are resolvable once the relevant facts are ascertained; (2) they are about meaning, and are resolvable once the respective debaters become clear about the meaning of key words. Perennial arguments are not factual because the point of (external) perennial arguments is to decide upon what ideal should be used for evaluating the facts. Nor is it the case that all internal and external perennial arguments are verbal, since in many debates, debates are explicitly clear about the definition and senses of key terms of opposing views. Their debate is a substantive one. It is, however, a special kind of factual debate: perennial arguments are *interpretive* factual arguments (ibid, p.28). Whilst they are not "scientifically" resolvable, they are nevertheless rationally resolvable. Let us consider in some detail how Kekes believes that this is possible.

Perennial arguments are conducted to determine the best solution for problems which face the participants, and a "best choice" is determined by those ideals which guide problem-solving. This is so for both external and internal perennial arguments. If the perennial argument is about which ideal in accordance with which the problem should be solved, then the disagreement is an external perennial argument. If the disagreement is however about the interpretation of the ideal, then their disagreement is an internal perennial argument. Philosophical theories are attempts to solve problems, and the test of their adequacy is whether in fact problems are solved. But this seems to constitute a *petitio principii*, as what counts as a problem depends upon the theory in question. The problem of the paradox of omnipotence for example is only a

problem for those who hold to a theory of God which takes His nature to involve omnipotence.

To avoid this *petitio principii* Kekes distinguishes a field of problems which human beings encounter by virtue of being in the midst of the world, and which face them regardless of what theories they hold. There are three main problem areas: problems concerning a person's response to his/her physical environment; problems concerning a person's response to humanity and finally problems concerning a person's response to him/herself. In each of these areas there occur two types of problems: *problems of life* and *problems of reflection*. The former problems "occur because the species has evolved in a particular way and because the environment is what it is" (ibid, p.33). Since the solutions of problems of life are extremely varied, problems of reflection arise in choosing the most suitable solutions to problems of life. Problems of reflection are theory-generated; problems of life are not. This distinction, Kekes believes, enables him to solve one of the most pressing problems of epistemology. He states (ibid, pp.35-36):

(K1) The difficulty that besets much of the contemporary discussion about the rational evaluation of theories is that various evaluative standards offered all presuppose an already established theoretical framework. Those who are dubious of the possibility of rational evaluation acknowledge that what is claimed to be rational is indeed rational in a given framework, in logic, science, religion, or morality, and then go on to question the rationality of the framework. And, of course, so long as the standards offered are internal to one framework or another, their doubt cannot be removed. The merit of the proposed standard of solving problems of life is that with its help the critics' challenge can be met. A framework is rational, *inter alia*, if it contributes a possible solution to problems of life. Since problems of life are independent of theoretical frameworks, so is the standard based upon them.

In challenging Kekes' solution to the problem of perennial philosophical disagreements, I shall in turn challenge the claim of (K1). Then I shall seek to establish that by Kekes' account of rationality, his own paradigm cases of irrational frameworks - such as that of the Flagellants

- come out to be quite rational practices. That is to say, his distinction is completely ineffectual. Second, even whilst agreeing that problems of life are independent of theoretical frameworks, the justification of the inference that the theoretical standards based upon them are independent of theoretical frameworks is completely lacking. There is at present however, further material which requires summary. of the inference that the theoretical standards based upon them are independent of theoretical frameworks is completely lacking. There is at present however, further material which requires summary.

Kekes distinguishes between *removal problems* and *enduring problems*. Removable problems arise from merely short-term obstacles. Enduring problems exist by virtue of human nature and the world, and require to be constantly dealt with. Resolving a particular romantic relationship may be a removable problem; one's general response to the opposite or same sex is a problem to be constantly dealt with afresh throughout one's life. In the latter case we have an example of a problem of life which is also an enduring problem. In general though, enduring problems tend to be problems of reflection, while problems of life tend to be removable. The reason for this is taken to be that removable problems are more frequently solvable without reference to a theory, whereas the solution of enduring problems usually requires a theory to enable the "best choice" of policies.

The connection between these concepts and that of our initial concern, perennial arguments is this: perennial arguments are prompted by enduring problems in all three problem areas. The attitudes which one takes in response to such a triad of problems is the object of perennial arguments. There are many solutions to enduring problems and philosophical theories are reasoned attempts to present policies of coping with enduring problems. The rationality of perennial arguments depends upon the solution of enduring problems: "insofar as philosophical arguments are perennial, they deal with problems that need to be solved again and again" (ibid, p.42). The rationality of philosophical theories involves

demonstrating that the theory in question "best" solves an enduring problem which prompted it. More precisely, Kekes distinguishes between two levels at which the justification of such theories occurs. The first level is the *context* of introduction of a theory, which distinguishes theories which could be successful solutions to enduring problems and theories which could not. The second level is the *context of acceptance* of a theory; one is justified in accepting a theory if and only if it is reasonable to suppose that it is a successful solution to an enduring problem.

Corresponding to these two levels are two standards of justification: *problem-solving* and *truth-directedness*. In the context of introduction problem-solving is primary and truth-directedness secondary (for what is of immediate concern is that the theory is a possible solution of an enduring problem). In the context of acceptance, truth-directedness is primary and problem-solving is secondary (for what is involved in the acceptance of a theory are the reasons for believing that it is a successful solution, in being the closest approximation of the truth amongst competing solutions).

I shall argue now for the following propositions:

- (S1) On Kekes' account of rationality, as expressed by (K1) paradigm cases of irrational frameworks turn out to be rational. Therefore Kekes' distinction is completely ineffectual.
- (S2) Even though problems of life are independent of theoretical frameworks, the justification of the inference that the theoretical standards based upon them are therefore independent of theoretical frameworks is completely lacking.

(S3) The existence of neither problems of life nor enduring problems supply a satisfactory general explanation of perennial philosophical disagreements. There are clear counter-examples to Kekes' position.

To establish (S1), (S2) and (S3) is to establish the general inadequacy of Kekes' metaphilosophy. This is so because (S1) and (S2) attack his theory of rationality, whereas (S3) attacks his solution to the problem of perennial philosophical disagreements. I shall now argue for these claims. These negative conclusions do not however indicate that I do not consider Kekes' work to be of considerable intellectual value and merit. On the contrary, Kekes' work is a refreshing contribution.

With regard to (S1) Kekes accepts by virtue of (K1) this definition of the rationality of frameworks:

(D1) A framework is rational =_{df.} it contributes a possible solution to problems of life.

The immediate difficulty is, that problem-solving is guided by what people believe are in their best interests, and this may well conflict with survival as Kekes recognises (*ibid*, p.35). One problem of life is surely what to do about one's sexual urges whilst living with complicated social relationships. One way of coping with this is to establish meaningful and satisfying relationships which channel sexuality into socially accepted avenues. Another solution is that of the repressive practices of the Flagellants. This is a possible solution to a problem of life - but one which Kekes regards as quite *irrational* (*ibid*, p.97). Consequently (D1) is not merely unsatisfactory - it involves one in accepting the explicit contradiction that at least some irrational frameworks are rational.

Perhaps it could be claimed that my criticism of this position fails because it is nowhere claimed that the world view of the Flagellants is *irrational*. This would be a surprising and desperate claim. On page 97 of *The Nature of Philosophy*, it is stated that a good example of competing world views is between "our" world view (whatever that means) and "such ghastly alternatives as . . . the religiosity of some fanatical sect like the Flagellants". Kekes in his book goes on to say that "philosophy ought to be able to offer some ground for justification and criticism in such situations" (ibid, p.98) which involve choices between such different world views. If it cannot do this then "the most unpalatable form of scepticism prevails" (ibid, p.98). Now if the world view of the Flagellants was regarded as *rational* by this author then the whole point of the comparison between "our" world view and the "ghastly", "fanatical" world view of the Flagellants is lost. This comparison is used in an argument for "the most unpalatable form of scepticism" and this argument only holds if, from an intuitive standpoint, either one of these world views is "irrational". If both are rational then the sceptical conclusion noted by Kekes does not follow as it is meaningless to ask for grounds of rational choice between rational and incommensurable alternatives (recall that world views determine what constitutes a fact and radically different world views may well differ on what is empirically real). Second, Kekes asserts in his book that the world view of the Flagellants is "ghastly" and "fanatical". Therefore he asserts that the world view of the Flagellants is irrational because although not all irrational people are "fanatical", all fanatical people are to some degree irrational. Let us also note in conclusion, that if Kekes did accept that the world view of the Flagellants was rational, then it would be totally unclear as to what an irrational world view looked like. If the world view of the Flagellants is *not* irrational, then what world view *is* irrational?

It might be argued in reply, that this argument utterly ignores Kekes' distinction between the two levels, the context of introduction and the context of acceptance, and the two standards of justification corresponding to these two levels, problem-solving and truth-directedness. This is quite correct and requires consideration. However, once we consider these distinctions, matters are not greatly improved. Before a theory can be accepted, it must first be introduced. In the context of introduction it is problem-solving rather than truth-directedness which is primary. This means that many irrational theories and frameworks satisfy the context of introduction.

It may also be argued that my criticism is based upon a confusion between solutions and frameworks. Flagellation is a solution to various problems of life, it may be argued, not a framework itself. Within some set of rational constraints to do with probability, say, I might nevertheless make irrational assignments of probability. Likewise Kekes can allow irrational solutions within rational frameworks. In response to this objection I deny that "Flagellation" is a solution to a problem of life rather than a "framework". The metaphor of the Flagellants is used by Kekes in both of his principal books as a paradigm case of an irrational framework or system of beliefs. To belong to the Flagellants is not merely to flog oneself - something which could be done accidentally or in acting - but to hold to a specific set of religious beliefs (I shall take up this point further in response to another objection to be discussed in the next paragraph, as even if I am wrong in my theology, my basic epistemological criticism seems correct to me). My objection to (D1) is simply this: Kekes' definition is defective because a number of intuitively regarded "irrational" frameworks contribute possible solutions to problems of life, and because Kekes wishes to preserve an intuitively plausible distinction between rational and irrational frameworks, his definition does not serve his self-assigned point.

It may be argued that the repressive practices of the Flagellants fail to qualify for the status of rationality once the standard of truth-directedness is considered at either the level of the context of introduction or the level of the context of acceptance. Kekes does explicitly speak of truth-directedness as involving verisimilitude comparisons (ibid, p.101). But he also speaks of acceptance with respect to the reasons for believing that some solution is successful. What is needed here is an argument which links the mainstream conception of truth with the successfulness of solving problems. The repressive and allegedly "irrational" practices of the Flagellants do present a satisfactory response to the problem of life of dealing with one's sexuality, insofar as 'success' means something like 'getting the job over and done with' or 'addressing some problem'. Whether in fact such practices lead to a fully flourishing human life is a matter which can only be decided by an independent ethical and psychological theory, and for our specific example, considerable theological debate. Thus if a general "Christian" *Weltanschauung* is accepted, are the Flagellants (and the modern day New Mexico sect of the Penitentes) right in claiming that self-inflicted suffering by public flogging for sins (especially sexual sins) can stand as a penance? But at this point the standards based upon the solution of such a problem of life are hardly independent of theoretical frameworks as Kekes believes.² This leads us directly to (S2).

It is granted in response to (S2) that problems of life exist independent of theoretical frameworks insofar as this means that for any individual any of the principal three types of problems of life may face him/her regardless of what theories this individual may hold. Thus Kekes is not claiming that theories may not generate specific problems of life themselves (against a background of other existing problems of life) and Kekes does not collapse the distinction between problems of life and problems of reflection as Nathanson (1979, p.231) claims in discussion of

Kekes' previous work *A Justification of Rationality* (Kekes, 1976). The real difficulty for Kekes' position as I see it, is simply that the standards based upon the solution of problems of life (even if the latter are theory-independent) are not independent of theoretical frameworks as Kekes' basic argument in both of his major works requires. This is so because given the distinction between problems of life and problems of reflection, the theoretical standards based upon solutions to problems of life must be *classified* within the domain of reflection. These standards are after all, evaluations of solutions to problems of life by reason and reflection. But as we have seen by our example of the Flagellants, the evaluation of solutions to problems of life may involve us in complex theoretical considerations. So since the domain of reflection is theory-dependent, it follows that Kekes' standards of evaluation must also be theory-dependent. Kekes may very well have made a plausible case for believing that perennial philosophical problems have a constant external reference, grounded in the human condition, but this would at best show that this external source motivates or generates interest in philosophical inquiry - it would not in itself justify philosophical inquiry or give us any good theoretical reason to believe that philosophical knowledge exists.

My criticism of Kekes then, is that standards of evaluation cannot be independent of theoretical frameworks given Kekes' own distinction between problems of life and problems of reflection. My criticism differs from that which Corbin Fowler (1978) has advanced against Kekes. Fowler's criticism was that the criterion of problem-solving fails to satisfy the sceptic of rationality in supplying an external standard of rationality because the existence of problems of life does not imply that such problems *ought* to be solved: only if one is rational must one hold that such problems are in need of resolution. The insane or the very ill may choose to

run or hide, or simply die. However, as far as I can see, on Kekes' account of problem-solving, this does not present a problem for him at all. Death may well be a dramatic solution to a wide range, if not all problems of life. My criticism of Kekes is considerably stronger than Fowler's: if my previous arguments are correct, then Kekes not only fails to justify rationality, but some intuitively judged instances of irrational beliefs (or theories) are classified as being irrational.

I shall argue now for proposition (S3), that Kekes fails to satisfactorily solve the problem of perennial philosophical disputes.

It was pointed out earlier in this chapter, that even excessive drug taking can be taken as an ideal, as Kekes has defined this term. The significance of this point requires explanation. Insofar as Kekes hopes to present a *general* solution to the problem of perennial philosophical disagreements, many perennial philosophical disagreements are only vaguely, if at all, related to problems of life and enduring problems. Their perennality cannot therefore be explained by the perennality of problems of life and enduring problems. I shall now cite various philosophical problems which shall serve as counter-examples to Kekes' position, which by contrast to drug abuse, do not solve any problems of life or enduring problems. Before doing so, allow me to point out that I am not thereby committed to accepting that a wide range of philosophical problems cannot be analysed as Kekes proposes. The relevance of an outcome of even quite abstract philosophical debates, such as freedom versus determinism has an impact upon our moral life, and lawyers such as Clarence Darrow (1957) have raised such matters even in the law courts. My claim is only that there are clear counter-examples to Kekes' position, and that he therefore fails to supply a general solution to our principal problem.

Let me now cite some counter-examples to Kekes' position: (1) the

problem of "Why is there something rather than nothing"; (2) the paradox of the Liar and other logical and semantical paradoxes; (3) virtually all major philosophical problems of mathematics (e.g. infinity, infinitesimals, the ontological status of numbers and sets); (4) a significant proportion of problems in the special sciences such as cybernetics and information theory (e.g. 'what is information?', 'what are programmes?'). The examples, to which the reader may add at will his/her own, are all difficult and abstract problems of *speculation*. Their connections with problems of life and enduring problems may not be totally non-existent, but such connections are certainly very remote indeed. Some of these problems may be by their very nature *insolubilia*. I have argued elsewhere that (1) is such a problem (Smith and Ward, 198+(e)) and will discuss this issue in more detail in chapter 7. But if problems of life *must* be solved by virtue of human nature and the world as it is, it follows that since there may exist problems of philosophy which have no solution, not all problems of philosophy are reflections upon problems of life and enduring problems. Further, all of the above cited problems are perennial - we know that (1) was discussed by Leibniz and virtually all major modern philosophers up to the present day.

Kekes has a response to this style of criticism. It is worth citing the relevant passage in full so as to avoid the possible error of misrepresenting his views (Kekes, 1980, pp.205-206):

What, it might be said, does the immense literature that has grown up, for instance, around the Gettier-problem, possible world semantics, the derivability of ought from is, the justification of induction, have to do with the construction of world views? The answer is: a great deal! For the Gettier-problem is about the ideal of knowledge, possible world semantics involves rethinking the ideal of logic, the derivability of ought from is is about the nature of value in general, and the justification of induction is about the possibility of science. Ideals such as these are the desired goals in accordance with which the enduring problems of a particular epoch

are solved. These technical philosophical questions are in fact questions that arise in the course of perennial arguments about some of the ideals of world views.

It is true that the ideal of metaphysics includes the problem of 'Why is there something rather than nothing?'. What, however, requires demonstration is that this problem is a reflection upon a problem of life or enduring problem. It does not appear to be so. Unlike problems such as how to deal with one's sexuality, a problem of metaphysics or cosmology such as the problem of 'Why is there something rather than nothing?' does not lead to the impoverishment of life. Surely human beings can live with mysteries. But even if this is the case, are my counter-examples really effectual against Kekes' position given qualifications which he makes later in his book? He says for example (ibid, 206):

At the same time, I must concede that my view of philosophy is unlikely to fit philosophical practice perfectly. It is possible to find philosophical preoccupations which cannot be readily accommodated by my account. I would handle such instances in one of two ways. There may be practices overlapping philosophy and other inquiries; there must also be borderline cases; and idiosyncratic unclassifiable works. To any general thesis there are such exceptions. Their mere occurrence is not an objection to the accuracy of the description. They would become objectionable only if they occurred in such large numbers that they ceased to be exceptions and had to be regarded as standard features requiring inclusion in the description. But I do not think that there are such exceptions to my thesis.

I disagree; the exceptions of "high-level" speculative questions of logic and metaphysics are not isolated singularities and are hardly borderline or idiosyncratic problems. They are live philosophical issues. To establish that such questions are standard features of philosophy would be a very difficult task indeed. Not only would I need to be fully explicit about what such questions are, but I would need a statistic to count such questions and establish "standardness". This cannot and need not be done here. It is sufficient to point out that high-level specu-

lative questions are *a more common occurrence* than Kekes believes. This is sufficient to cast strong doubt upon the satisfactoriness of his general solution of the problem of perennial philosophical disagreements.

3. PHILOSOPHICAL MACHISMO AND THE ADVERSARY METHOD

Janice Moulton (1983) has recently criticized a feature of contemporary philosophy which has strong relevance to the principal concerns of this work even if no explicit discussion of (PPPD) is given in her paper. Moulton is concerned to criticize the "adversary method" in philosophy. According to this position, the best way of evaluating philosophical work is to subject it to debate. One attempts to muster, on the one hand, all the evidence which one can to support one's pet thesis, whilst attempting to produce counter-examples against all opposing positions. Conceptual conflict therefore seems built into this style of philosophizing. Whilst the adversary method does not and cannot guarantee truth, it does, it is frequently claimed, subject a thesis to the most extreme challenges practically possible. Moulton believes that philosophers by this method attempt to be "value-free" (ibid, p.153) and to distinguish themselves from scientists. She accepts a view of scientific activity culled from Kuhn (1970(b)), where scientific knowledge is not certain and nor is it value-free (Moulton, 1983, p.152):

Science involves more than a set of independent generalizations about the world waiting to be falsified by a single counter-instance. It involves a system, or "paradigm" of not only generalizations and concepts, but beliefs about the methodology and evaluation of research: about what are good questions to ask, what are proper developments of the theory, what are acceptable research methods. One theory replaces another, not because it functions successfully as a major premise in a greater number of deductions, but because it answers some questions that the other theory does not - even though it may not answer some questions the other theory does. Theory changes occur because one theory is more *satisfying* than the other, because

the questions it answers are considered more *important*. Research under a paradigm is not done to falsify the theory but to fill in and develop the knowledge that the paradigm provides a framework for.

Philosophy itself should, Moulton maintains, adopt the methods of argument and evaluation employed in the sciences, and cease being an aggressive "macho" enterprise. In particular philosophy should attempt to assess theses in the light of discussions of larger systems of ideas which such theses may relate to. I am very sympathetic with at least this aspect of Moulton's position, as I have also outlined in *Reductionism and Cultural Being* (Smith, 1984, chapter 2). Her feminist criticism of philosophical inquiry is, I believe, seriously defective.

Before evaluating Moulton's proposal, some reasons need to be given as to why we are discussing this issue at all. The reason is this: philosophical disagreements constitute a *prima facie reductio ad absurdum* of the claim that philosophy is a cognitively rational enterprise only if we accept that the methodology of philosophical inquiry embraces the adversary method. As a paper by Peter Facione (1975-76) illustrates, the dialectic by which counter-examples are produced to theses is precisely the sort of phenomenon which results in perennial philosophical disagreements. But if this methodology is simply misplaced, then we have come a significant distance in solving the principal problem of this work.³ The problem of perennial philosophical disagreements may be taken as a *reductio ad absurdum* of a way of philosophizing, as the adversary method may be taken to lead invariably to perennial and unsolvable conflicts. Reject this method, replace it by a method of inquiry which seems to be used in the more progressive sciences such as physics, and our principal problem would seem to immediately dissolve.

Moulton's thesis is unsatisfactory. First, it is self-refuting. Moulton through *critical arguments* in her paper attempts to show the

inadequacy and at the very least the limitations of the adversary method. But to do this is nothing to engage in the adversary method once more for she presents critical arguments which she believes refutes a philosophical position. Hence her position is pragmatically self-refuting.

Second, Moulton claims that users of the adversary method attempt to be "value-free". Now it may be a sociological fact that many contemporary philosophers hold to the notion that cognitive inquiry can be "value-free". But they hold to this position by acceptance of philosophical theses which have no logical relationship to the adversary method. In general most of these philosophers will support their positions of "value-freedom" by an appeal to the is-ought distinction, that there is a logical gap between factual statements and evaluative statements. Moulton does not show that in value-laden fields of inquiry, such as moral philosophy, the practice of critically evaluating arguments and presenting counter-arguments to theses, is either non-existent or seriously misplaced. In fact it is empirically true that moral philosophers do critically evaluate arguments and present counter-arguments to theses, as the reader may confirm by examining the latest edition of *Ethics* for example.

Third, the adversary method as described by Moulton can be shown to accommodate her principal alternative mode of evaluative reasoning, namely the consideration of how the reasoning relates to a larger system of ideas. Coherence considerations are not alien to contemporary philosophy (Lehrer, 1974), (Rescher, 1973(b) - so why can't the contemporary philosopher who makes use of the adversary method appeal to coherence considerations at the level of supplying a justification of his/her own thesis? I know of no reason against this. Perhaps contemporary philosophers have lost a sense of adventure and courage to attempt grand scale intellectual syntheses and systems building, but there is to the best of my knowledge no reason why philosophers with more courage and less pride in matters

such as being proved wrong should not attempt to build *Weltanschauungs* (Kekes, 1980).⁴

It is concluded that not only does Moulton's position suffer from considerable difficulties, but that our conjectured attempt to deal with the problem of perennial philosophical disagreements from Moulton's position, is a failure.⁵

4. DORTER: TRUTH AND PHILOSOPHY

Dorter (1977) has questioned the assumption that philosophical positions are contradictories or contraries, and proposes instead that philosophical positions are fundamentally complementary. Dorter is not therefore adopting a paraconsistent metaphilosophy where mutually contradictory philosophical positions may be taken to be true. (On this topic see chapter 8 below). To support his claim Dorter offers three illustrations, only the first of which will be cited here. Consider the dispute between Spinoza and Leibniz on the nature of substance. For Spinoza there is a single substance, which is infinite and all embracing. But for Leibniz there are an infinite number of substances each of which is absolutely simple and infinitesimal. This contradiction is resolved, Dorter proposes, once we look carefully at each thinker's criterion of substance. The criterion of substance for Spinoza is *completeness* (cf. *Ethics* I, def. 3) and for Leibniz *indivisibility* (cf. *Monadology*, beginning). Thus Spinoza and Leibniz are not engaged in a philosophical dispute; Spinoza is viewing reality in terms of the concept of universality, whilst Leibniz is viewing reality in terms of individuality. Hence there is no contradiction because they are speaking about different things. Dorter offers the following general resolution of the problem of perennial philosophical disagreements (Dorter, 1977, p.11):

Most philosophical disputes are not direct contradictions, but alternative ways of representing, hence formulating and categorizing reality. Thus different philosophies cannot fairly be compared in what they say on particular issues until one has gone through (not merely analysed into lifeless and ambiguous categories) each position so as to discover the *fundamental* dispute that is at the heart of the particular disagreements. Philosophical debates are valuable not in order to convert one's opponents, a very rare occurrence indeed, but because they often eventuate in the discovery and appreciation of the previously obscure point of fundamental disagreement, and therefore of the irreducible difference of commitment between the two positions, whereupon there is generally nothing more to be said. One can ultimately do no more than defend or formulate a position which most does justice to one's own experience of reality, and here we must resign ourselves to the fact that there are irreducible differences.

Dorter's proposals stand in contrast to the view of philosophical disagreements taken by the German idealist J.G. Fichte in his *Wissenschaftslehre* of 1794 (Fichte, 1970). For Fichte, the possibility of a non-arbitrary choice between comprehensive metaphysical *Weltanschauungs* such as Idealism and Realism is non-existent, for these positions are incommensurable (ibid, p.12)⁶:

Neither of these two systems can directly refute its opposite, for their quarrel is about the first principle which admits of no derivation from anything beyond it; each of the two, if only its first principle is granted, refutes that of the other; each denies everything in its opposite, and they have no point at all in common from which they could arrive at mutual understanding and unity. Even if they appear to agree about the words in a sentence, each takes them in a different sense.

For Fichte, the acceptance of a philosophical system is not a matter of rational argument but is a matter of personal inclination and social prejudice. The Realist will never become an Idealist because Realism for the Realist is a doctrine which gives meaning and self esteem to the Realist; to reject this doctrine is to devalue his/her person.

Fichte is proposing that Idealism and Realism actually are conflicting irreconcilable positions. Now I cite Fichte's comments, not merely

to point out one counter-example to Dorter's position, but also to show that Dorter cannot do justice to differences in *metaphilosophical* opinion. Consider for example his own position contrasted with that of Fichte. For Fichte, Idealism and Realism are conflicting and irreconcilable. But for Dorter, Idealism and Realism, must be viewed as he viewed the difference between Spinoza and Leibniz on substance: not conflicting doctrines, but merely alternative ways of categorizing reality. Thus we have our required counter-example to Dorter's theory, for Dorter and Fichte's metaphilosophies *do* conflict.

Whilst Dorter's point about the differing criteria of substance accepted by Spinoza and Leibniz respectively is a valuable point, he is incorrect to claim that Spinoza and Leibniz are not therefore in conflict about the nature of substance. Spinoza claims that the ultimately *correct* criterion of substance is completeness, whilst Leibniz claims that it is indivisibility. Perhaps both are wrong, so that their positions are not contradictories. But the positions are still contrary to each other, because a single undivided substance is not an infinite number of absolutely simple substances. The metaphysical idea of an undivided substance differs quite considerably from the mathematical idea of a line; whilst the linear continuum may be conceived to have a non-denumerable number of points, an undivided substance is a whole, a totality which cannot be even in principle "divided". So the positions are in conflict: both cannot be true, although both may be false. Contrary to Dorter, Spinoza and Leibniz are involved in a substantial disagreement.

It is concluded that Dorter's attempt to dissolve the problem of perennial philosophical disagreements is also a failure. Note as well that Dorter seems quite prepared to accept that there may be irreducible differences of commitment between philosophical positions (Dorter, 1977, p.11). This claim is inconsistent with his general solution to the problem

of perennial philosophical disagreements. Therefore his work is contradictory, and in any case, unsatisfactory.

5. BARBER ON PHILOSOPHICAL DISAGREEMENTS

Barber (1958) responds to the problem of perennial philosophical disagreements, by claiming that the only response to this problem which does justice to the history of philosophy "is to accept the diversities at face value and learn to live with them" (ibid, p.28). The task of philosophy is, Barber alleges, to formulate and express statements of maximum import about the whole of knowable reality. Stated very generally, the methodology of philosophy is as follows. The philosopher begins with the "totality of experience" as this is present to him/her. The "totality of experience" is that which his/her philosophical system must make sense of. To explain the "totality of experience" is to propose this: if the whole-of-reality is in itself as the present system describes it to be, then the "totality of experience" would be just as it is. To justify a philosophical system is just to show in comprehensive detail that his/her system is the best available antecedent hypothesis for a hypothesis which has as its consequence the "totality of experience". Barber has this to say about philosophical disagreements (ibid, p.32):

If I have understood the subject matter of the history of philosophy, it is an irreducible pluralism of explanatory hypotheses, in which there can be discovered broad and profound disagreement about the nature of the totality of experience requiring explanation; about the criteria of adequacy, both intrinsic and inferential, which any account of the whole of reality must satisfy; and about the nature of the implicative bond or verificatory demonstration by which the explanatory powers of the philosophic system are made evident.

Moreover, if I have given an admissible account of the philosophic enterprise and its methodology, then this disagreement seems quite understandable.. Each of the

three major stages admits of indefinitely great variety of specification; each particular specification can be held as an absolute tenet by its philosophic partisans; by self-willed right the philosopher need appeal to no higher court, and unprincipled compromise is equal anathema. The future of philosophy will witness and record the exfoliation and multiplication of systems, if history is any guide.

This proposal is the weakest of any explanations for our principal problem yet considered in this chapter. Barber's proposal amounts to the claim that there can be and in fact are, philosophical disagreements about: (1) what the "totality of experience" is; (2) how to best explain the "totality of experience" and (3) whether particular philosophical systems are justified. This is nothing more than a restatement of our principal problem, not its solution.

There is a more plausible interpretation of Barber's position. The claim that we must accept philosophical diversity as a fact of life, suggests that we view such diversity as a "natural state", not in need of explanation. Once we do this we can then restrict metaphilosophical inquiry to a hermeneutic practice of understanding the diversity of positions which constitute philosophy. Patrick Hill (1972) calls such a mode of inquiry 'the dialogical method'. Investigations of philosophical disagreements should be concerned with the empathetic understanding of why the other philosopher is not convinced, and the researcher should eschew "all concern for the truth, validity and worth of the positions that would be examined" (ibid, p.7).

Now as I have argued earlier in this work in a consideration of Rorty's metaphilosophical scepticism,⁷ hermeneutic inquiry is not inconsistent with the goal of the critical evaluation of arguments and the pursuit of truth. This being so, there is no good reason to be limited in one's inquiry by the Barber-Hill methodological desideratum.

Second point: to flatly claim that perennial philosophical disagreements do not require explanation, is an alarmingly *ad hoc* move. To claim that something is a "natural state", not in need of explanation, is usually justified on the basis of some comprehensive explanatory schema capable of not only explaining a wide range of phenomena, but also *unifying* our knowledge in some domain of inquiry. In Newtonian mechanics for example, rest or uniform rectilinear motion is the natural state requiring no explanation, while all other motions are to be explained by unbalanced forces acting upon bodies. The Barber-Hill proposal fails to satisfy these basic conditions, so that philosophical disagreements cannot be regarded as natural states. "The dialogical method" does not result in any *unification* of knowledge, but stands only as a methodological desideratum to give aging metaphilosophers something to do once the pursuit of truth and the critical evaluation of arguments is abandoned. We do not obtain any unification of knowledge, because *ex hypothesi* we are asked to accept philosophical disagreements at face value. To do this, is to deprive oneself of a reason for taking philosophical disagreements as "natural states".

6. GALLIE'S ESSENTIALLY CONTESTED CONCEPTS

W.B. Gallie (1964) accepted a view of metaphysics which is basically a modification of Collingwood's view (*ibid*, pp.220-224). I shall consider the relevance of Collingwood's view of metaphysics to the problem of perennial philosophical disagreements in the next section; for the moment I shall discuss Gallie's notion of 'essentially contested concepts' (Gallie, 1955-56) outlining its relevance to the problem of perennial philosophical disputes.

Essentially contested concepts are concepts which have no clearly

definable general use, and where there is no standard definition which defines correct usage. The proper use of such concepts is subject to perennial debate. Such debates are taken by Gallie to be perfectly genuine, and although parties at the dispute are quick to offer arguments and evidence in support of their pet usage, such debates are "not resolvable by argument of any kind" (ibid, p.169).

The formal defining conditions of *essential contestedness* may now be stated: (1) the concept must be *appraisive* insofar as it accredits some type of valued achievement; (2) the valued achievement accredited by the concept "must be of an internally complex character, for all that its worth is attributed to it as a whole" (ibid, pp.171-172); (3) granted (2) though, any explanation of such worth must make reference to the respective contributions of the parts; (4) this achievement must be open to modification, even though the form which this modification takes cannot be predicted in advance and (5) each party recognizes that the employed concept is an "essentially contested concept" and has at least some idea of their opponent's criteria for use of the contested concepts. These conditions fail to distinguish essentially contested concepts from concepts seen in a situation where disagreement occurs by the confusion of two *different* concepts whose use is not normally debated apart from situations where subjects are mutually confused and may be aware of this mutual confusion (ibid, p.175). To deal with this problem Gallie adds two further conditions: (6) "the derivation of any such concept from an original exemplar whose authority is acknowledged by all the contestant users of the concept" (ibid, p.180) and (7) "the probability or plausibility, in appropriate senses of these terms, of the claim that the continuous competition for acknowledgement as between the contestant users of the concept, enables the original exemplar's achievement to be sustained and/or developed in optimum fashion" (ibid, p.180).

An example of an essentially contested concept (discussed by Gallie) is the concept of *democracy*. The concept is clearly an appraisive one, for the achievement of "true democracy" seems to be the goal of both the liberal and socialist tradition. The concept is also internally complex insofar as different aspects of democratic achievement, such as for example equality of opportunity and "self-government" may be graded in different orders of importance. Openness to modification is also a feature of this concept. For example, the democracy of ancient Greece did not extend any alleged equality of opportunity to slaves. Many users of the concept of democracy also claim the authority of an exemplar, as illustrated by the number of political movements which allegedly have drawn their inspiration from the French Revolution.

Gallie is hesitant to offer predictions about whether continuous debate about the usage of the concept of democracy will lead to an optimal development of the achievements of the democratic tradition. To meet condition (7) some positive forecast must be given about the conceptual development of the original exemplar. Yet by condition (4), the achievements of the democratic tradition, whilst open to modification, cannot be predicted in advance. Hence Gallie's conditions would appear to be inconsistent. Moreover conditions (6) and (7), which Gallie adds to distinguish essentially contested concepts from concepts seen in a situation where disagreement occurs by the confusion of two different concepts, involve a great implausibility when applied to philosophical topics. It is far from clear that there is in fact any original exemplar whose authority is acknowledged by all the contestants at the debate, for perennial debates exist about even the commonsense meanings of ordinary concepts such as *truth*, *knowledge* and *inference*. If the original exemplar is vague, or possibly inconsistent, then it cannot stand as a conceptual authority worthy of acceptance by all contestants at the debate. I

conclude that Gallie's notion of an essentially contested concept is extremely problematic. Perhaps this is to be expected: the concept of an essentially contested concept is itself "essentially contested".

Despite these criticisms, the notion of essentially contested concepts does not take us very far in giving any satisfactorily general answer to our principal problem. We may claim, following Kekes (1980, p.29) that the key terms in perennial arguments are essentially contested concepts. It does not follow, and nor does Gallie assert that it does, that all philosophical disagreements arise from the use of essentially contested concepts. For a counter-example consider any standard logical or semantical paradox. Disagreements in this field are about how to best solve the paradox in an intuitively satisfying and methodologically *non-ad hoc* fashion; the debate is not *restricted* solely to issues about the proper meaning of some term, although such considerations may play an important part. Indeed in the case of many standard philosophical paradoxes such as the famous paradox of the surprise examination, the concepts involved do not seriously have their meanings contested - there seems instead to be a conflict involved between some fundamental logical principles (Smith, 1984 (d)). If this is correct then perennial philosophical disputes cannot merely be a product of the fact that in many philosophical debates, essentially contested concepts are found.

A final ground of dissatisfaction with Gallie's work on essentially contested concepts applied to philosophical topic, is that he does not attempt to explain why it is that some concepts are essentially contested, whilst others are not. This is to say that our interpretation of Gallie's work fails to address the problem of perennial philosophical disagreements: we seem to require a solution to our principal problem before we can answer the former problem.

7. ABSOLUTE PRESUPPOSITIONS AND PHILOSOPHICAL DISAGREEMENTS

This is not the place to survey Collingwood's metaphilosophy (Collingwood, 1940),⁸ but by way of formulation of our next argument the following may be stated. For Collingwood every statement is made in answer to a question, including of course, statements made by someone in the course of solitary thinking. Every question asked in the course of a scientific, philosophical or historical inquiry for example, involves at least one immediate presupposition from which it immediately and directly arises (ibid, p.25). A presupposition is either relative or absolute. A relative presupposition is one which stands relatively to one question as its presupposition and relatively to another question as its answer. Relative presuppositions may be verified, that is, given an affirmative answer as the presupposition itself in propositional form. Absolute presuppositions stand relatively to all questions to which it is related as a presupposition, but not as an answer. Consequently absolute presuppositions are not propositions, so that the concept of truth (and falsity) does not apply to them.

Collingwood gives a number of examples of absolute presuppositions: 'God exists', 'Everything that happens has a cause' and the Kantian principle of continuity 'Between any two terms in a series, there is a third term'. If we take absolute presuppositions to be presuppositions relative to any mode of inquiry, then we obtain nonsense as Michael Krausz (1972) has argued. For example, the Kantian principle of continuity is challenged by a number of quantum mechanical properties. Collingwood would be committed to the position that the Kantian principle is not *inconsistent* with quantum mechanics (since the Kantian principle is not a proposition and hence the concept of truth does not apply to it) and this is utterly implausible. The classical principle of continuity formulated as a statement in English does conflict with mainstream quantum

theory. Consequently Collingwood's absolute presuppositions must be taken to be relative to some given and particular systematic inquiry. This also enables us to answer a criticism of Collingwood given by Jay Newman (1973, p.280). Newman claims that Collingwood's examples of absolute presuppositions are nothing more than answers to philosophical questions. For any absolute presupposition p , we may ask ' $p?$ '; 'Does God really exist?' Collingwood as I understand him, would hardly claim as Newman interprets him to claim, that such questions do not arise. Rather 'God exists' is an absolute presupposition of certain theological inquiries, but it is not an absolute presupposition of the general philosophy of religion.

Thus for example, that God exists is an absolute presupposition of reformed dogmatics and is not questioned or defended within this field of study. However, this presupposition can be examined within the philosophy of religion, where 'God exists' is not an absolute presupposition. This field however would have other absolute presuppositions such as a trust in the non-paradoxical nature of rational argument which would not be contested in the philosophy of religion but only in a more fundamental discipline such as epistemology.

Absolute presuppositions we have seen cannot be either verified or falsified *within* the field of inquiry to which they refer, but they may be open to justification and criticism within some other field of inquiry. This also answers Watkins' (1978) criticism of Collingwood, that the claim that every question $q?$ has a non-analytic presupposition p is false because "it is necessary that there be a non-analytic statement p such that q entails p and p does not entail q ; and this condition is not satisfied when we substitute p^* for q ", (ibid, p.204) where p^* is taken to be an absolute presupposition. The reply to Watkins is that his entailment relationship simply cannot hold because absolute presuppositions

are neither true nor false *relative* to some systematic inquiry. It is therefore a mistake to analyse the notion of presuppositions in terms of entailment and advance the criticism which Watkin advances against Collingwood. This is not to say that I believe that Collingwood's notion of an absolute presupposition is satisfactory; I do not for the precise relationship between the absolute presupposition and the field of study remains exceedingly vague. Nevertheless it is important to state any position in its strongest and clearest form, even if it is to be dismissed as ultimately irrelevant to one's concerns.

The result of our defense has however an unfortunate consequence for any use to which the theory of absolute presuppositions may be put in attempting to resolve the problem of perennial philosophical disagreements. One may have thought that the theory of absolute presuppositions would lead to a radical metaphilosophical relativism; philosophers disagree about *x*, and the source of this is differing absolute presuppositions. Since these absolute presuppositions are neither true nor false, disagreement is inevitable. But it has been proposed here, that if Collingwood's position is to be plausible at all, absolute presuppositions must be taken to be relative to some systematic inquiry. Now in a philosophical debate, say over the issue of freewill, the disputing parties necessarily are engaged in the *same* systematic inquiry - otherwise they will be talking past each other. Since this must be so, it is hardly necessary that disputing parties hold to different absolute presuppositions. Indeed in the freewill debate, both parties (say a compatibilist and hard determinist) may accept the "law of universal causation". The debate will focus upon the significance of this principle. Clearly two parties then can be committed to the same absolute presupposition, *but dispute the significance of such a commitment.*

Thus even if we accept the brief and possibly unsatisfactory defense

of Collingwood's theory given here, we will fail to have a satisfactory general explanation of the problem of perennial philosophical disputes. This limitation, combined with other objections which could be made to Collingwood's position (example: there are no good reasons advanced to show that absolute presuppositions lack a truth-value) make the relevance of our suggested reconstruction of Collingwood's position quite minimal to any satisfactory resolution of our principal problem.

8. CONCLUSION

The conclusion now reached is parallel to the conclusion reached in the previous chapter: none of the surveyed responses to the problem of perennial philosophical disagreements are satisfactory. This is not to say that I do not believe that any internalist response to our target problem could be judged to be adequate: if this was so, given my scepticism expressed in the previous chapter about the satisfactoriness of externalist responses, it would seem that our target problem is in fact unsolvable. My own "dissolutionist" response, to be outlined in chapter 10 is a form of internalism. In this chapter my claim is that only the surveyed internalist responses can be judged to be inadequate. In chapter 7 I shall consider another form of internalist response to the problem of perennial philosophical disagreement which makes use of various mathematical theories to show that a convergence to consensus of opinion in cognitive matters is an inevitable outcome of a rational process of debate. Hence if philosophy is a rational enterprise, a convergence to consensus is inevitable. I shall attempt to refute each of these positions, to establish that philosophy may well be a rational enterprise even if no convergence to consensus is observed in the philosophical community. This is the first major proposition which I need to establish to formulate my own solution to the target problem.

6. NOTES

1. The style of this chapter will consist merely of outlines and critiques of the cited texts. There is little to be gained from a critical comparison of these works because of their mutual diversity.
2. Does this lead us then to cognitive relativism? Not as far as I can see, by any sound argument. The point made is that the standards of evaluation of the satisfactoriness of problems of life are not theory independent. It does *not* follow that the notion of *objective truth* is incoherent, for this notion itself, is part of a very general metaphysical *Weltanschauung* - metaphysical realism.
3. Moulton makes a suggestion in this direction when she states (Moulton, 1983, p.156): "The aim of the Adversary method...is to show that the other party is wrong, challenging them on any possible point, disregardless of whether the other person agrees. In fact, many contemporary philosophers avoid considerations of how to convince, supposing it to be related to trickery and bad reasoning...the Adversary Method is not a good way to convince someone who does not agree with you".
4. Robert Nozick (1981) has also been critical of the practices of what he calls "coercive philosophy". Whilst this mode of philosophy is concerned with producing arguments and critically evaluating them, it is a quite violent activity. Its language is modelled by boxing and military metaphors (e.g. knockdown arguments, the punch of an argument, strategies etc.), and its aim is to force people to believe things against their will. It is thus he believes, an immoral activity. Nozick advocates the production of philosophical explanations rather than arguments. A philosophical theory explains various puzzles, rendering them coherent and better understood. Now if we wish to claim that some philosophical theories are *better* or more *satisfactory* explanations of philosophical puzzles than other such theories, as Nozick wishes to do, critical activity is logically inescapable. And surely, justifying one's viewpoint, and demonstrating inadequacies in competing positions is a way of obtaining knowledge and enlightenment?
5. Moulton's reliance upon Kuhn's conception of science also leaves her open to the extensive array of criticisms that have been advanced against Kuhn's position (Newton-Smith, 1981).
6. For a more detailed discussion cf. (Rabb, 1975; 1978). The citation from Fichte given in the text first came to my notice from (Rabb, 1978, p.202).
7. cf. Chapter 4.
8. For detailed discussions of Collingwood's metaphilosophy, as well as his philosophical system cf. (Krausz (ed.), 1972), (Rubinoff, 1970).

7. EROTETIC UNSOLVABILITY AND METAPHILOSOPHY

1. STATEMENT OF THE ARGUMENT

I am concerned now with an investigation of the significance of "unsolvable problems" to the issues of philosophical disagreements and philosophical progress. The questions which I wish to address are these:

(Q1) If there are unsolvable problems, what is the significance of this to the problem of perennial philosophical disagreements?

(Q2) If there are unsolvable problems does this put limits upon the extent of theoretically possible philosophical progress, and if so, what in fact are these limits?

To answer (Q1) and (Q2), further questions must be answered. Obviously the first of these is the semantical question of 'What is the meaning of the expression 'unsolvable problem'?'. Second, given an answer to the first question in the form of an explication of the expression 'unsolvable problem' we must ask, 'do we have good reason to believe that there are in fact unsolvable problems?'

As we have also seen, the problem of perennial philosophical disagreements raises an immediate problem for any account of the cognitive progressiveness of a discipline which relies upon the statistics of high frequency of success in problem-solving. Where is philosophy's list of solved problems? I shall argue here, on the basis of work to appear elsewhere (Smith and Ward, 198+(e)) that at least some of the problems of philosophy are unsolvable. Whilst this places limits upon what we can come to know in philosophy, in the sense of positive resolution of

philosophical problems, nevertheless we can obtain a form of knowledge which has been neglected from serious consideration by all excluding Karl Popper and his 'immediate followers'. This is knowledge of the limits and inadequancies of our argumentative methods, knowledge of our cognitive impotence and fallibility in some domain of inquiry. My position is, that even if there are unsolvable problems in philosophy, the metaphilosophical sceptic is quite wrong to claim that we then have no philosophical knowledge at all. I shall conclude in this chapter, the debate which I began with the metaphilosophical sceptic in chapter 4. Let us turn now to the issue of explicating the idea of an unsolvable problem.

2. THE IDEA OF AN UNSOLVABLE PROBLEM

Perhaps we could best approach the semantical question by examining explications of the expression 'solvable problem'. Then we could simply take an unsolvable problem to be a problem without a solution, at least as a first approximation. It also seems a good strategy to examine contemporary fields of research which have as their subject matter, problems, and the solvability of such problems. Two fields of study may prove to be of interest to us here: *metamathematics* and *erotetic logic*. Let us examine whether this is the case beginning with a very general discussion of 'solvability' in metamathematics.

Mathematics as a field of study is not merely concerned with establishing the truth or the falsity of mathematical propositions, but seeks as well *algorithms* or *decision procedures* for various mathematical questions. For example, consider an arbitrary polynomial equation $P(x_1, x_2, \dots, x_n) = 0$, with integer coefficients. Is there some mechanical procedure for determining whether or not, in a finite number

of steps of application of this method, $P(x_1, x_2, \dots, x_n) = 0$ has a solution in integers or not? This is Hilbert's famous "tenth problem", and it was solved in the negative by Yu Matijacevič [1970]. Matijacevič showed that no algorithm meeting the requirements for a solution to Hilbert's "tenth problem" existed, and this constituted a proof of the metamathematical 'unsolvability' of the problem. In recursive function theory, unsolvability results, or as they are more commonly called, undecidability results, may be reduced to questions of whether or not a set S is recursive.¹ The undecidability of the decision problem reduces then to the question of whether $(\exists x)((x \in N) \ \& \ (\neg(x \in S)))$ for natural numbers N , provided that we accept Church's Thesis, i.e. the identification of the class of effectively computable functions with the class of general recursive functions.²

Undecidability, as a concept of metamathematics does not take us very far in any bid to explicate the intuitive idea of 'unsolvable problems' which is used both in philosophy and daily discourse (especially in romantic life). For we do, in both an intuitive and mathematical sense have an answer to the question of Hilbert's "tenth problem": there is no algorithm which determines whether an arbitrary polynomial equation $P(x_1, x_2, \dots, x_n) = 0$ with integer coefficients has a solution in integers. We do in fact know, given that Matijacevič's proof is accepted, that there is no algorithm for Hilbert's "tenth problem". What however we are looking for, is some account which would, at the very least, make sense of claims made by metaphilosophical sceptics such as Benson Mates that "the traditional problems of philosophy . . . are intelligible enough, but . . . absolutely insoluble" (Mates, 1981, p. 3). Metamathematics does not aid us in explicating Mates' intuition, and nor should it. Thus we had best look for semantical insights elsewhere. But before doing so, we must recognize that if metamathematics does not aid us in answering

the semantical question, it may well still be of relevance once we have answered this question on an independent basis, in so far as it may aid us in presenting an argument for the existence of unsolvable problems. One such argument will be reviewed in section 4 below.

Erotetic logic is the "logic" of questions and answers. 'Logic' is not meant here in the sense of a deductive system with proofs and derivations, but as a loose term for any formal system which has as its object language a formal apparatus permitting the asking and answering of questions, and for its metalanguage, a set of concepts for relating and evaluating questions and answers. This is how Belnap and Steel understood the subject matter of erotetic logic in their comprehensive text: *The Logic of Questions and Answers* (1976, p. 1).³ Let us advance a thesis, in a similar spirit to that of Church's Thesis, that every problem can be expressed as either a what, whether or why-question. Then one would expect that for any problem p , and its respective question (what (p)?, why (p)?, whether (p)?), there is a solution to the problem p if there is an answer to the respective question of the problem of p . However this thesis differs from Church's Thesis, in that we cannot plausibly suppose that there is any reasonable identification of the notions of questioning and problem-statement. That not all questions are the formation of some problem is supported by simple counter-examples such as: "Will you shut the door (please)?", "Did you take out the garbage?".

There is no systematic discussion in the literature of problem-solving as to what the expression 'the solution to a problem' actually means. It is inevitable that such an expression will acquire considerable vagueness, if not outright obscurity, when one views, as Karl Popper for example does, anatomical particulars of plants and animals as solving problems and embodying theories.⁴ Belnap and Steel take a question to be identical to a set of admissible answers with a selection demand on the form of the answer.⁵ They state (Belnap and Steele, 1976, p.17):

A first approximation to the central idea is that each question is to be conceived as presenting a range of alternatives as its subject, from among which alternatives the respondent is to make a selection as from a tray of hors d'oeuvres.

Such a proposal suffers however from insuperable difficulties. Assuming that one does not commit a *petitio principii* by assuming that there are no unsolvable problems, Belnap and Steel's proposal leads us to identify any two unsolvable problems, since for each question, the set of admissible answers is the null set. The strategy of taking answers to questions as complete sentences which repeat the question, "so that from the question it is possible for the participants in an erotetic situation to tell what the direct answers are to be" (ibid, p. 35), does not improve matters. The *petitio principii* is committed once more in assuming that such "direct answers" exist. As we shall see in the course of the argument of this chapter, this is not necessarily the case.

An 'approximate answer' to our semantic question, should be, I propose, along the following lines. Excluding whether-questions, which call for a 'yes' or 'no' answer, the relationship between a question and the answer (or an answer) to the question, is a relationship of *explanatory relevance*: the answer should *explain* the question. Note that this condition excludes problems such as the following: "Does God exist?", "No" - where nothing is explained. Questions which are adequately answered by 'yes' or 'no' are not of interest to us here. A number of answers might explain some question; those that do so correctly, may be taken as *the* answers to the stated question, whilst those that do not are inadequate.⁶

It is hardly possible here to present a theory of explanation, and nor is it a defect in this work not to present such an account merely for the purposes to which this very basic notion will be used. But some remarks are in order to illustrate how the term operates on my position. Our sense of the term 'explanation' must be wide enough to include at one

end of the spectrum mathematical explanations, consisting of the presentation of mathematical proofs (Steiner, 1978, 1983) - to causal explanations which characterize a significant part of the explanatory domain of the natural and social sciences (Harré, 1970), (Bhaskar, 1978).

Nicholas Rescher (1979(b) in his recent discussion of the completeness of science, distinguishes between three types of cognitive limits:

(1) *insolubilia*, meaningfully posed questions which at no time, no human (or other knowing subject) can answer; (2) *ultimate questions*, which are insoluble from a *framework-internal* point of view and which serve to highlight the fundamental commitments of an explanatory framework, and (3) *improper questions*, questions which are either semantically meaningless or otherwise semantically ill-formed, or else violate the essential presuppositions of a considered explanatory framework. It is cognitive limits of types (1) and (2) which are of interest here.⁷ These types of questions may be further categorized to illustrate types of insolubilia. One suggestion is as follows:

- (A) *Ontological Insolubilia*: Q is a meaningful question which is semantically proper, but for which no correct or appropriate answer can be given, *for no solution exists*;
- (B) *Epistemic Insolubilia*: Q is a meaningful question which is semantically proper, but for which there is no correct or appropriate answer which we can know or which other knowing subjects can know.

For ultimate questions, the qualification 'from the framework-internal point of view of α ' needs to be added. Then degrees of 'ontological unsolvability' and 'epistemic unsolvability' may be readily distinguished. A question Q is *strongly ontologically unsolvable* if and only if there

could not exist any fact of the matter about what would constitute an answer to Q. A question Q is *weakly ontologically unsolvable* if and only if there *does not exist* any fact of the matter about what would constitute an answer to Q. Strong ontological insolubilia cannot have answers because, although they are proper questions, the proposal that they are answerable leads to demonstrable contradictions or incoherence. Weak ontological insolubilia are also proper questions, and it is not incoherent to suppose that they are answerable, but it is as a contingent matter of fact, that they are not answerable. Strong ontological insolubilia are necessarily unsolvable, weak ontological insolubilia are contingently unsolvable. The same distinction can also be drawn for the family of epistemic insolubilia. Just as there is still supposed by many logicians to be both necessary and contingent truths, so I propose that there are both necessary and contingent insolubilia.

With this rough working account of *insolubilia*, I shall now turn to the next question which can naturally enough be asked: "are there ontological insolubilia and/or epistemic insolubilia?" Before presenting my own answer to this question, I shall examine two very interesting arguments for the existence of *insolubilia*: Richard Routley's (1981) argument for unknowable truths and *strong epistemic insolubilia*, and A.H. Basson's (1956-57) argument for the existence of strong ontological insolubilia. I argue that both arguments fail.

3. ROUTLEY ON UNKNOWABLE TRUTHS AND UNSOLVABLE PROBLEMS

Richard Routley (1981) has recently argued for the position that there are unknowable truths, and hence that it is impossible to know "everything", that some truths are unknowable, and hence that there is *no method* by which anything whatsoever can be known. Routley's arguments are of direct relevance to the principal thesis of interest here, since if there

are unknowable truths, then it is easily shown that there are also *epistemic insolubilia* (either strong or weak), because for any p , such that p is an unknowable truth, $p?$ can be asked, but not definitively answered.

Routley gives two basic arguments in support of his thesis: the first is a direct argument in sentential logic, the second, and I think the philosophically stronger argument, consists of using the Finsler-Gödel argument for the generation of undecidable propositions, to generate by diagonalization, statements which assert of themselves that they are unknowable.

The first argument assumes as its underlying logic SO_5 , although SO_5^0 would also be satisfactory.⁸ SO_5 adds to classical sentential logic S , the one place primitive connective \Box and apart from the standard transformation rules of uniform substitution, material detachment and the rule:

(r) If α is a valid wff of SO_5 , then $\vdash \Box \alpha$

we have the following axioms:

(P₁) $\Box p \rightarrow p$

(P₂) $\Box(p \rightarrow q) \rightarrow (\Box p \rightarrow \Box q)$.

The system SO_5^0 results from SO_5 by deleting only (P₁). Routley adds to this logic a one place primitive epistemic connective K , where if A is any wff, KA is another. This system is called KSO_5 . K is a knowledge functor, interpreted as either 'someone knows that at some time' or as a creature-relative functor 'every creature knows that'. Other interpretations of K such as 'it is provable that' or 'a rationally believes that' are also considered. K must in addition satisfy these conditions:

- (A₀) $q \rightarrow \diamond Kq$ (i.e. whatever is true is possibly known)
 (A₁) $Kq \rightarrow q$ (i.e. necessarily, whatever is known is true).

A weaker condition than (A₁) is:

- (A₁*) $Kq \rightarrow \sim K \sim Kq$ (i.e. necessarily, for whatever is known, it is epistemically possible that it is known)
 (A₂) $K(q \& r) \rightarrow Kq \& Kr$ (necessarily, each conjunct of a known compound proposition is known).

Routley shows that in KSO.5, $\vdash p \rightarrow Kp$, i.e. whatever is true is known. Let us qualify this claim by recalling that we are not merely considering human knowers here, but also Divine omniscient knowers such as God. But further comments on that matter later. Routley's proof of $\vdash p \rightarrow Kp$ is as follows:

- (7-1) $\sim \diamond (q \& \sim q)$ Necessitated Non-contradiction.
 (7-2) $\sim \diamond (Kp \& \sim Kp)$ Substitution in (7-1).
 (7-3) $K \sim Kq \rightarrow \sim Kq$ By (A₁*) or (A₁).
 (7-4) $Kq \& K \sim Kq \rightarrow Kq \& \sim Kq$ From (7-3) by Composition.
 (7-5) $\sim \diamond (Kq \& \sim Kq) \rightarrow \sim \diamond (Kq \& K \sim Kq)$ From (7-4) by standard definition of ' \rightarrow '.
 (7-6) $\sim \diamond (Kp \& K \sim Kp)$ By (7-2) and (7-5).
 (7-7) $K(p \& \sim Kp) \rightarrow Kp \& K \sim Kp$ Substitution in (A₂).
 (7-8) $\sim \diamond (Kp \& K \sim Kp) \rightarrow \sim \diamond K(p \& \sim Kp)$ From (7-7) by:
 $A \rightarrow B \rightarrow (\sim \diamond B \rightarrow \sim \diamond A)$.
 (7-9) $\sim \diamond K(p \& \sim Kp)$ By (7-6) and (7-8).
 (7-10) $\sim (p \& \sim Kp)$ From (7-9) by (A₀).
 (7-11) $\sim p \vee Kp$ From (7-10) by: $\sim (A \& B) \leftrightarrow \sim A \vee \sim B$ and $\sim \sim B \leftrightarrow B$.
 (7-12) $p \rightarrow Kp$ From (7-11) by: $\sim A \vee B$ then $(A \rightarrow B)$.

Proposition (7-12) is, Routley maintains, self-evidently false, and the mistaken assumption in the argument is (A₀) which leads from (7-9) and

and (7-10) from what is true, to what is false.

An extension K^+ of $KSO.5$ is obtained by adding the axiom:

(A₃) (Eq) (q & $\sim Kq$) (i.e. for some q, q is true but not known by any subject).

That there are in principle unknowable truths, and hence *strong epistemic insolubilia* is readily provable:

(7-13) $\sim \diamond K(p \text{ \& } \sim Kp)$ From (7-9).

(7-14) $(\forall r)\sim \diamond K(r \text{ \& } \sim Kr)$ Generalization of (7-9).

(7-15) $(\exists r)(r \text{ \& } \sim Kr)$ From (A₃).

(7-16) $(\exists r)[\sim \diamond K(r \text{ \& } \sim Kr) \text{ \& } (r \text{ \& } \sim Kr)]$ From (7-14) and (7-15) by: $(\forall r)A \text{ \& } (\exists r)B \rightarrow (\exists r)(A \text{ \& } B)$ for 'A' and 'B' containing a free 'r'.

(7-17) $(\text{Eq})(q \text{ \& } \sim \diamond Kq)$ From (7-16) by particularization.

Routley's argument can be criticized at a number of points (Williamson, 1982). First, is it in fact the case that $p \rightarrow Kp$ for any knower including God, is self-evidently false? God being omniscient by definition knows every truth so $p \rightarrow Kp$ certainly does hold for God. Routley has however an argument against this proposal, which we shall consider shortly. For the moment, let us consider the objection that could be made to accepting $\vdash p \rightarrow Kp$, based upon interpreting 'K' as a human knowledge functor. This it seems does have a strong counter-intuitive flavour. However, (A₀) is not the only principle in the argument (7-1) & (7-2) & ... & (7-11) \rightarrow (7-12), which could be plausibly rejected. The principle $\sim A \vee B \rightarrow A \rightarrow B$ also seems criticizable on grounds of relevance, and it is surprising that a relevance logician such as Routley did not counter such a reply. For if ' \rightarrow ' is taken as a sign of the ordinary language notion of 'follows from', then $\sim A \vee B$ may be true whilst $A \rightarrow B$ is false solely because B does not 'follow' from A.

Without the principle: $\sim A \vee B$ then $A \rightarrow B$, we have no good reason for accepting that $p \rightarrow Kp$ follows from $\sim p \vee Kp$. Indeed we could take Routley's argument as showing that the allegedly valid principle: $\sim A \vee B$ then $A \rightarrow B$ is *invalid* (truth preserving). The proposition $p \rightarrow Kp$ is surely false (Williamson, 1982, p. 203). But as $Kp \rightarrow p$, then $\sim p \vee Kp$ implies $\sim p \vee p$ which even if it is not a logical truth, can certainly be true whilst $p \rightarrow Kp$ is false. Hence the principle: $\sim A \vee B$ then $A \rightarrow B$ is invalid. These considerations, despite their brevity, show that (A_0) is not the only principle that can be questioned. But Routley's argument for the existence of *strong epistemic insolubilia* depends upon this being the case. Hence this argument fails.

A second point can be made in criticism. His argument for the demonstration of unknowable truths hinges upon the acceptability of (A_3) : $(Eq)(q \ \& \ \sim Kq)$. Routley gives no argument at all as to why (A_3) should be accepted rather than its negation, apart from of course, his rejection of $p \rightarrow Kp$. The plausibility of outrightly rejecting (A_0) thus depends upon the acceptability of his second basic argument for the existence of unknowable truths, which will now be considered.

The second argument given by Routley makes use of the Finsler-Gödel argument for producing undecidable propositions by diagonalization. It is required that one demonstrate the existence of a true p such that:

$$(F_1) \quad p \leftrightarrow \sim \diamond Kp.$$

One such statement is allegedly (F_2) :

$$(F_2) \quad \text{This very statement } (F_2) \text{ is not knowable.}$$

Routley argues that since (F_2) is the statement that (F_2) is not knowable, (F_2) satisfies (F_1) and any p conforming to (F_1) is true but unknowable. He concludes that since statements of the form (F_2) are exhibitable, no being including God is omniscient, and hence that there exist unknowable

truths. It is however arguable, that since God is by definition omniscient, hence it is statement (F₂) which is in fact problematic. To avoid a *petitio principii* some independent grounds other than that already cited about the nature of God (at least as an object of thought, even if (S)He does not exist) must be given. This I shall now attempt to do.

The assumption that is contestable in Routley's argument is that truth-teller statements such as:

(TT) This very statement, (TT) is true,

are in fact true.⁹ Mortensen and Priest (1981(a) have argued that whilst there does not exist any proof that (TT) is either true or false, there is a proof that (TT) must be either true or false. Suppose that (TT) is neither true nor false. Then (TT) is not true and (TT) is not false. *But since (TT) asserts of itself that it is true, it is false, thus contradicting the supposition that it is neither true nor false.* The proof that (TT) is either true or false may be stated more formally by letting '(TT)' be an abbreviation for the sentence (TT) and not the name of the sentence, and letting quasi quotes be used as name forming functors. The proof is as follows:

$$\text{Let } \ulcorner (TT) \urcorner = \text{Tr } \ulcorner (TT) \urcorner \quad (1)$$

$$\text{Now } (\sim \text{Tr } \ulcorner (TT) \urcorner \ \& \ \sim \text{F } \ulcorner (TT) \urcorner) \rightarrow \sim \text{Tr } \ulcorner (TT) \urcorner \quad (2)$$

$$\text{But } \ulcorner (TT) \urcorner = \text{Tr } \ulcorner (TT) \urcorner \rightarrow (\sim \text{Tr } \ulcorner (TT) \urcorner \rightarrow \text{F } \ulcorner (TT) \urcorner) \quad (3)$$

$$\text{Then } (\sim \text{Tr } \ulcorner (TT) \urcorner \ \& \ \sim \text{F } \ulcorner (TT) \urcorner) \rightarrow \text{F } \ulcorner (TT) \urcorner \quad (4)$$

(4) from (1), (2), (3).

$$\text{Then } (\sim \text{Tr } \ulcorner (TT) \urcorner \ \& \ \sim \text{F } \ulcorner (TT) \urcorner) \rightarrow (\text{Tr } \ulcorner (TT) \urcorner \vee \text{F } \ulcorner (TT) \urcorner) \quad (5)$$

$$\text{Since } (\sim \text{Tr } \ulcorner (TT) \urcorner \ \& \ \sim \text{F } \ulcorner (TT) \urcorner) \quad (6)$$

$$\text{Then } \text{Tr } \ulcorner (TT) \urcorner \vee \text{F } \ulcorner (TT) \urcorner \quad (7)$$

(7) from (5) and (6).

I have previously proposed that (TT) should be regarded as being true.¹⁰ The argument may be summarized as follows. We have a proof that (TT) must be either true or false, so if (TT) is to have a truth-value at all, our range of options are restricted to either the truth-value 'true' or the truth-value 'false'. There seems, I argued, to be more reason to take (TT) to be true rather than false, even if neither ascription *prima facie* leads to contradiction, since (TT) asserts of itself that it is true, not that it is false. But the ascription of falsity to (TT) leads to problems. Let us assume that 'Tr(p) \leftrightarrow p' and 'F(p) \leftrightarrow \sim p'. Then the truth-teller sentence (TT) may be represented by:

(7-18) Tr(p).

The claim that (TT) is false is the sentence:

(7-19) F(Tr(p)).

Further, to claim that (TT) is false entails that (TT) is not true:

(7-20) F(Tr(p)) \rightarrow \sim Tr(p).

But from (7-20) and (7-19) by *modus ponens* we infer:

(7-21) \sim Tr(p)

which contradicts (7-18).

Consider now the assumption that (TT) is true. This does *not* lead to contradiction by a parallel style of argument:

(7-22) Tr(Tr(p)) \rightarrow \sim F(p) Classical entailment.

(7-23) Tr(Tr(p)) Assumption.

(7-24) \sim F(p) From (7-22), (7-23), *modus ponens*.

(7-25) \sim F(p) \leftrightarrow $\sim\sim$ Tr(p) Classical equivalence.

(7-26) Tr(p) From (7-24), (7-25), *modus ponens* and double negation.

This I concluded gave us good reason to regard (TT) as being true.

Some points of self-criticism need to be made here. First, I implicitly assumed that given $\neg \text{Tr} \ulcorner (\text{TT}) \urcorner \vee \text{F} \ulcorner (\text{TT}) \urcorner$ and the problematic nature of the assumption that $\text{F} \ulcorner (\text{TT}) \urcorner$, that therefore $\text{Tr} \ulcorner (\text{TT}) \urcorner$. But merely showing that the assumption that (TT) is consistent, does not constitute a ground for believing that (TT) is as a matter of fact true. There does not then seem, contrary to my previous arguments, to be any fact of the matter about the truth-value of (TT). This being so, the most reasonable methodological policy to adopt is to regard (TT) as having no truth-value at all.

This proposal, it may seem, runs into difficulties allegedly presented by the Mortensen-Priest proof that (TT) is either true or false. This argument however, only succeeds, as do my own previous arguments, by tacitly accepting Tarski's truth criterion $\text{Tr}(p) \leftrightarrow p$. But this principle and the proposition that some propositions are neither true or false leads to contradiction as McCall (1970) has proved. The proof is as follows. Let x be a proposition which is neither true nor false. Then:

$$(7-27) \quad \neg \text{Tr}(x) \ \& \ \neg \text{F}(x).$$

But since $\text{F}(p) \leftrightarrow \text{T}(\neg p)$, then:

$$(7-28) \quad \neg \text{Tr}(x) \ \& \ \text{Tr} \neg(x)$$

by replacement in (7-27) above. But by the principle $\text{Tr}(p) \leftrightarrow p$ we obtain:

$$(7-29) \quad \neg x \ \& \ \neg \neg x \ \text{(i.e. } \neg x \ \& \ x \text{)}.$$

Since Mortensen and Priest assume Tarski's equivalence in their proof that (TT) is either true or false, their proof is ultimately a *petitio principii* against the position that (TT) is truth-valueless.

Do we have stronger reasons for taking (TT) to be truth-valueless rather than true? It is an unjustified dogma of formal logic to expect

a 'proof' that (TT) is truth-valueless.¹¹ Rather, as is frequently done in areas outside of the logico-mathematical sciences (especially in epistemology and metaphysics), a *methodological decision* needs to be made as to how we deal with the alleged truth-value or truth-valuelessness of the sentence (TT). That there is no fact of the matter about (TT)'s truth, contrary to what I once thought, is a good reason for taking (TT) to be truth-valueless.

The implications of these inquiries for Routley's argument that there are demonstrable unknowable truths, establishable by diagonalization, is immediate. He assumes, wrongly, that truth-teller statements are true. Without this assumption, which in any case he nowhere defends, he cannot reasonably claim that statements such as (F₂) are true. Therefore Routley's second argument for the thesis that there are unknowable truths, and hence *strong epistemic insolubilia*, collapses.

4. BASSON AND STRONG ONTOLOGICAL INSOLUBILIA

Basson attempts to show through an application of Church's Theorem, that there are problems which can be expressed in a standard natural language such as English, but whose solutions, if they exist at all, cannot be expressed in that language. Basson considers a particular class of meaningful English sentences *Instructions*, and a sub-class of this class, *Sequence-Instructions*, these being instructions to write down a sequence of numerals. Sequence-Instructions are of two kinds:

- (1) instructions for writing down a finite sequence of numerals;
- (2) instructions for writing down an infinite sequence of numerals.

Basson's unsolvable problem is to describe a method for deciding for any arbitrary given letter sequence, the class to which it belongs. Assign positive integers to each Sequence-Instruction, and let those positive

integers assigned to instructions for writing down an infinite sequence, be called S-numbers. Basson's allegedly unsolvable problem reduces now to the form: describe a method or algorithm for deciding for any arbitrarily given number, whether or not it is an S-number.

Basson attempts to show that for any sequence formed by an instruction G , there is a diagonal sequence which will not occur in any row of the matrix array of sequences of S-numbers generated by G . This Cantorian argument is the well-known claim that for any infinite list:

$$(7-30) \quad S_1, S_2, S_3, \dots$$

of sets of positive integers, we can define a Cantorian diagonal set as follows:

$$(7-31) \quad \text{For each positive integer } n, n \text{ is in } \overline{D}(L) \text{ if and only} \\ \text{if } n \text{ is not in } S_n.$$

To attempt to repair the gap in one's enumeration caused by $\overline{D}(L)$, by adding $\overline{D}(L)$ to the list as a new first member is generally taken to be a fruitless strategy, for then a different set $\overline{D}(L^*)$ can be shown not to be in the augmented list. To suppose that for some positive integer m such that $S_m = \overline{D}(L)$, enables the deduction of a contradiction for $m = n$. For by (7-31) m is in $\overline{D}(L)$ if and only if m is not in S_m . But if $S_m = \overline{D}(L)$, then m is in $\overline{D}(L)$ if and only if m is not in $\overline{D}(L)$. This contradiction is generally taken to show that there exists a set of positive integers not in the original list.

I have argued against the claim that the Cantorian argument is sound elsewhere (Smith, 198+(a)) and have also suggested that Richard's 'Paradox' is not in fact a paradox, but a *reductio ad absurdum* of diagonal arguments (Smith, 198+(h)) (cf. also (Broyles, 1977)). If I am correct in these arguments, then many classical undecidability, indefinability and

incompleteness results in metamathematics can be rejected. These claims are of course quite controversial and may prove to be ultimately fallacious. Even so, results in metamathematics should not be treated as established dogmas, unopen to challenge, and so I believe that such criticisms even if ultimately unsuccessful are of great intellectual value. But for the moment, let us cast some doubt upon diagonal arguments by showing that they lead to contradiction.

Consider the infinite list in (7-30), only let us suppose that there is a set S_n of positive integers which is defined as follows:

(7-32) $S_n = \text{df}$ The set of all sets of positive integers.

We allegedly show according to Cantor's argument, that there is a diagonal set which does not occur in the infinite list $S_1, S_2, S_3 \dots S_n$:

(7-33) For each positive integer n , n is in $\overline{D}(M)$ if and only if n is not in S_n .

Now from (7-33) we conclude that $\overline{D}(M)$ is a set of positive integers not in S_n . But S_n is *ex hypothesi* the set of all sets of positive integers and by definition contains all such sets of positive integers. Thus $\overline{D}(M)$ is a set of positive integers if and only if it is not a set of positive integers. But why take this argument to show that the problem lies with the notion of the set of all sets of positive integers? It is true that the notion of a 'set of all sets' can lead to paradoxes, the most famous being Russell's paradox. The contradiction here however is not obtained merely by a definition and logical principles; the contradiction here involves a conflict between a postulated set and Cantor's premise (7-33) which is justified by Cantor's diagonal argument. Why *shouldn't* we accept Cantor's diagonal argument as being invalid? What non-question begging reason can be given against the reasonableness of the postulation of a set of all sets of positive integers? None seem to be forthcoming.¹²

A second paradox can be produced by considering the infinite list in (7-30) once more, only this time letting the sets $S_1, S_2, S_3, \dots S_n, \dots$ be sets of positive integers produced by all *logically possible* applications of Cantor's diagonal argument. Then we can obtain the diagonalization of these diagonal sets as follows:

(7-34) For each positive integer n , n is in $\bar{D}(n)$ if and only if n is not in S_n .

But if $\bar{D}(N)$ is a diagonal set, then *ex hypothesi* it must occur in our infinite list. And by Cantor's diagonal argument we establish that $\bar{D}(N)$ is not in our infinite list. This is a contradiction.

These sorts of contradictions are also to be seen more dramatically in the 1926 paper of Paul Finsler "Formale Beweise und die Entscheidbarkeit" (Finsler, 1967). Finsler shows by means of Cantor's diagonal argument that there exist in any fixed system F of finitely many mathematical signs, binary sequences of signs which are not definable in F . Yet we can unambiguously specify the antidiagonal sequence itself as being one of the binary sequences not a finitely definable, so that it must be finitely definable after all (ibid, p. 442). Finsler claims that because a binary sequence cannot have the properties of both meeting Cantor's requirements as well as satisfying the condition that "every object that is unambiguously characterized by a definition consisting of the words of $[F]$ must be finitely definable" (ibid, p. 443), then it is wrong to say that the antidiagonal sequence has been finitely defined. Finsler does not solve the paradox in my opinion. He grants us that the antidiagonal sequences can be unambiguously characterized, as well as the second condition cited a moment ago. It follows by *modus ponens* that the antidiagonal sequence is thereby finitely defined after all.

The same criticism can be made of Finsler's construct of a formally

undecidable, but false proposition in F. There Finsler shows by diagonalization that the associated binary sequence defined on p. 443 of his paper cannot belong to the sequence defined on the same page. Yet he also proves in F that the antidiagonal sequence also contains infinitely many zeros (ibid, p. 444 lines 7-12). Finsler objects to this proof only by saying that since the proof makes reference to a binary sequence which cannot occur in the defined sequence on page 443, the proof itself cannot be valid. This presupposes the correctness of diagonalization, it does not show it. Clearly if one accepts the proof given on p. 444 lines 7-12, then we have a counter-example to Cantor's diagonal method.

Such radical and controversial modes of argument are not necessary to rebut Basson's argument. What Basson allegedly shows is that there cannot be any algorithm for deciding for any arbitrary letter sequence, the class to which it belongs. Hence the answer to Basson's question is, *in English*: the problem is undecidable. This however is a long way from showing that there are problems which can be expressed in English, but whose solutions, if they exist, cannot. As I pointed out earlier, undecidability results in recursive function theory *do* provide (negative) answers to decidability questions. We then do not have any satisfactory argument for the existence of *insolubilia* as defined in this chapter.

5. THE EXISTENCE OF *INSOLUBILIA*, PHILOSOPHICAL PROGRESS AND DISAGREEMENTS

Are there *insolubilia* in philosophy? The questions of philosophical discourse have often been taken by both philosophers and non-philosophers to be "unsolvable". The explicit argument for this, if one is cited at all, is the fact of perennial philosophical disagreements: philosophers have disagreed about the solutions to all major philosophical problems, such as the mind-body problem, universals and particulars, the nature of

mathematical truth, and also about the nature of philosophy itself. This has led Benson Mates (1981) to claim that all of the problems of philosophy are unsolvable, being incapable of even being dissolved as pseudo-problems (ibid, p. 3). Mates adopts the ancient maxim *Ou mallon*, proposing that the reasons given on both sides of any philosophical issue are equally good. Philosophy then, as a cognitive enterprise, seems to be riddled with Kantian style antinomies.

Whilst Mates' discussion of the problems of free will, and the existence of the external world has been subjected to critique (Feldman, 1983), Mates' arguments for his metaphilosophical maxim of *Ou mallon* are less than compelling, and in fact the argument of his text merely contradicts the thesis which he advanced in the preface of his book. In the text of *Skeptical Essays* Mates argues that there exist no satisfactory solution to any of his three considered philosophical problems. But at no point does Mates engage in a rigorous dialectic, as one might expect from one who upheld the maxim of *Ou mallon*, where both a thesis T and its antithesis $\sim T$ are both defended. Without this, the thesis that philosophy is riddled with Kantian style antinomies is not supported. Indeed, if the metaphilosophical sceptic is to be consistent, then an antithesis to this thesis itself must also be defended, and an argument from self-referential consistency can be developed against the position. For we might now suppose that the following meta-thesis is true: both metaphilosophical scepticism M and anti-metaphilosophical scepticism $\sim M$ are rationally defensible. But this meta-thesis MT should also be counter-balanced by a defense of $\sim MT$. The metaphilosophical sceptic then seems to be making no decidable claim at all.

Now it might be argued in reply that an appeal to the meta-thesis MT , need not be made by the metaphilosophical sceptic, for the position might be simply taken to refer to object-language statements. The use of this

familiar Tarskian strategy to avoid self-referential refutation will not succeed in this instance, whatever its merits are in solving the semantical paradoxes. This is so because even though metaphilosophy is "about philosophy", it is a *philosophical* investigation of philosophy, just as metascience is a philosophical investigation of science. Thus thesis MT is in fact a philosophical thesis, and should therefore fall within the scope of the concerns of the metaphilosophical sceptic.¹³

These general remarks do not support the claim that there are no *specific* philosophical *insolubilia*, although this claim is not put forward very frequently for *specific* philosophical problems. An interesting suggestion was recently made by Malcolm Acock (1983), that "Russel's hypothesis" that the world sprang into existence five minutes ago, is unsolved. But I have argued in reply to Acock, with Sharyn Ward (Smith and Ward, 198+(a)) that "Russell's hypothesis" can be criticized and rejected on methodological grounds. We have also argued (Smith and Ward, 198+(e)) that the question 'Why Is There Something Rather Than Nothing?', is weakly epistemically unsolvable and hence that the principle of sufficient reason is not in general true. All the general responses to this question are either theoretically defective or commit a *petitio principii* by failing even to address the real issue of the question. If this is so, then we have an *inductive argument* for the existence of a weak ontologically unsolvable problem, contingent of course upon both the present state of argument and the comprehensiveness of our initial survey. Other *insolubilia* may also exist.

Given that there are at least some reasons to believe that there are *insolubilia*, what is the significance of this result for the problem of perennial philosophical disputes, and the issue of philosophical progress? As we have seen the problem-solving in contemporary metascience takes the progressiveness of a discipline to consist in the solvability of

problems. Laudan (1977) for example, states that problem-solving effectiveness is determined "by assessing the number and importance of the empirical problems which the theory solves and deducting therefrom the number and importance of the anomalies and conceptual problems which the theory generates" (ibid, p. 68). But if we accept that there are genuine philosophical *insolubilia*, a number of outstanding metaphilosophical problems can be directly attacked.

First, philosophical progress need not consist in the solving of philosophical problems. If there are genuine philosophical *insolubilia*, then this view cannot be correct. Whilst we may come to accept some such problem PP as unsolvable, our *knowledge is advanced*. For now we have come to the conclusion that all the arguments of the inquirers in our field are in one way or another defective and so we shall not further allow our minds to be seduced by them. This in itself is a discovery of major importance, and it should therefore be counted as a progressive move in any reasonable theory of cognitive progress. In philosophy then, the discovery of *insolubilia* is not a cognitive catastrophe, but constitutes a growth in our knowledge. It is of course undeniable that *insolubilia* demonstrate the erotetic incompleteness of our knowledge (Rescher, 1979). What however we lose in knowledge of solutions, we gain in knowledge of *insolubilia*, of the limits of our cognitive methods and styles of argument. More on this in chapters 8, 9 and 10.

It may be argued in reply, and with some plausibility, that such a view of cognitive progress is so general as to be theoretically useless. What doesn't count as progress in philosophy? The answer to this objection is that if we cannot determine whether some problem is unsolvable or solvable, then no progress is occurring in this field. A field may become clogged by power-politics, by *ad hominem* arguments and

bitter personal attacks. The empirical documentation of this is legally difficult, but I am sure that the aware reader will recognize this ugly aspect of the discipline. When debate becomes a rabble, it is an intellectual sin to speak of cognitive progress occurring, not merely the stating of a false proposition.

If there exist *insolubilia* then we also have an immediate explanation of the occurrence of some philosophical disagreements. Such disagreements have existed because the philosophical problem in question is unsolvable. Both parties in the dispute are therefore incorrect. Matters however cannot be left at such a brief statement of our resolution of the problem of philosophical progress, and the remaining chapters of this work shall present what I believe is a more satisfactory response to this problem than any of the positions considered earlier in the work. The points which I make here are as follows: (1) philosophical disagreements may arise if the problem under debate is (unknown to the debaters) an *insolubilia*; such disputes shall be perennial simply because the problem under debate is unsolvable; (2) the existence of *insolubilia* does not constitute an evidential ground for the belief in metaphilosophical scepticism.

Is the proposal that perennial philosophical disagreements may exist because the debated philosophical problem is unsolvable, merely circular reasoning? One explains the occurrence of a particular philosophical disagreement as arising from the fact that the problem is an *insolubilia*. And yet, the method of argument employed in reaching this conclusion can itself only be by criticizing each solution-candidate in turn, and concluding that each solution-candidate fails, and this method merely leads us back into the domain of argumentative controversy. But in reply to this objection, it is maintained that this alleged circularity is not vicious. To establish that some philosophical problem is unsolvable will

be made by arguments $A_1, A_2, A_3, \dots, A_n$. The explanatory hypothesis as to why that particular philosophical disagreement has been perennial is not in any way deduced by valid deductive arguments from $A_1, A_2, A_3, \dots, A_n$. Rather, it is an explanatory hypothesis, which requires examination along with any other explanatory hypothesis about the existence of that perennial philosophical dispute.

6. CONCLUSION: THE STATE OF THE ARGUMENT

In this chapter I first asked the questions: (1) "what are unsolvable problems?" and (2) "do we have good reason to believe that there are unsolvable problems in philosophy?". After answering the first question, a detailed examination of the arguments of Routley (1981) and Basson (1956-57) for the existence of *insolubilia*, was undertaken. These arguments proved to be defective. Nevertheless whilst disagreeing with the metaphilosophical sceptic Benson Mates (1981), that all philosophical problems are unsolvable, we do in fact have reason to believe that there are at least some philosophical *insolubilia*. The implications of this proposal for the metaphilosophical problems of progress and perennial philosophical disputes were detailed. The existence of philosophical *insolubilia* whilst necessarily demonstrating the erotetic incompleteness of philosophical inquiry, does not mean that no growth of philosophical knowledge occurs at a 'meta-level'; further, that many long standing philosophical disputes may in fact be over *insolubilia* stands as an interesting explanatory hypothesis, well worthy of further inquiry. Furthermore, the existence of unsolvable problems refutes the standard convergence to rational consensus models of Lehrer and Wagner (1981). As I have argued, if a philosophical problem is unsolvable, and this is not known, then all philosophers who put forward positive solutions to be philosophical problem will be wrong, a consensus

about the truth will not be reached and yet these philosophers would still be rational, in an intuitive sense and also according to the theory of rationality to be sketched in chapter 8.

Why then is this inquiry not immediately furthered here? The reason is, that in my assessment there are few philosophical insolubilia, and so we cannot satisfactorily answer our principal thesis (P.T.) by such considerations. Thus in the next chapter I shall consider an alternative argument in a bid to satisfactorily answer our principal thesis (P.T.).

7. Notes

1. For surveys of recursive function theory and undecidability results cf. (Kleene, 1952), (Davis, 1958), (Tarski, Mostowski and Robinson, 1968).
2. For Church's statement of this thesis, cf. (Church, 1936). For criticisms, cf. (Bowie, 1973), (Ross, 1974), (Kalmár, 1959), (Smith, 198+(h)).
3. That there is an erotetic logic distinct from the standard logic of propositions, has been questioned by P. Tichý (1978). Tichý's proposal is that when a subject utters an interrogative sentence, the speaker refers to a function defined on possible worlds, roughly speaking, a proposition. The unnaturalness of this proposal is explained away by Tichý, by maintaining that the critic must then accept that beliefs are not propositions either. But rather than this latter claim being taken as a *reductio ad absurdum* argument, this conclusion may well be taken as a plausible thesis, which has been argued for quite strongly by various authors, e.g. (Routley, 1980, pp. 685-687).
4. Cf. (Popper, 1972, p. 261); for a discussion of more plausible ecological models, cf. (Smith, 1984) and for a discussion of Popper's evolutionary naturalism (Smith, 198+(d)). For discussion of problem-solving, cf. (Kleiner, 1970), (Laudan, 1977), (Hattiangadi, 1978), (Wettersten and Agassi, 1978), (Wettersten and Goode, 1982). In the problem-solving metascience of Laudan (1977), problem-solving is explicated in a fashion reminiscent of the deductive-nomological account of explanation: "any theory, T, can be regarded as having solved an empirical problem, if T functions (significantly) in any scheme or inference whose conclusion is a statement of the problem" (ibid, p. 25). The difficulty with this proposal, and it is one not addressed by Laudan, is that a question p? is not usually regarded as the sort of entity which can be (at least in principle) true or false, a condition which is necessary for p? to feature in an argument.
5. The identification of a question with the set of its logically possible answers has also been advanced by Stahl (1969) and Hamblin (1973).
6. An inverse of the view stated here is given by P. Achinstein (1977), where the notion of an explanation is explicated by means of the notion of "a correct answer to a question". This position is committed to the counter-intuitive consequence that if K is *not* a *correct* answer to a question Q?, then K does not explain the topic of puzzlement Q. Consequently, most of the theories in science, being strictly speaking incorrect, must on Achinstein's account be non-explanatory.
7. If this work was written during the 'hey day' of Logical Positivism, then the issue of whether all major philosophical problems offend against the verificationist theory of meaning, and hence are cognitively meaningless pseudo-problems, would require rebuttal. But this is (rightly) no longer a live issue today in metascience, to state the obvious.

8. On SO_5 , cf. (Lemmon, 1957; 1959), (Cresswell, 1966; 1970), (Rennie, 1971), (Routley, 1968(a); 1968(b)).
9. For a discussion of the truth-teller sentence, cf. (Mortensen and Priest, 1981(a)).
10. Cf. (Smith, 1984(f)). The argument of this paper is itself unsatisfactory and the textual discussion here is based upon a revised, but unfortunately unpublished version of the paper.
11. Mortensen has made this requirement in conversation. But the formalist demand that every thesis to be discussed, must be capable of a logically rigorous proof, leads us headlong into the trilemma of an infinite regress, circularity or an arbitrary stopping point (cf. (Lakatos, 1962)). So much the worse for the formalist's demands.
12. Hence a popular ground for the rejection of the Richard paradox as a *reductio ad absurdum* of diagonal arguments, namely that the infinite list $S_1, S_2, \dots S_n, \dots$ does not exist (Cargile, 1979, p. 301), cannot be made to rebut this counter-argument, for such arguments beg the question at issue of the validity of the diagonal argument.
13. For an alternative response to the metaphilosophical sceptic cf. (Rapaport, 1982). The argument of this chapter meets the demand made by Renford Bambrough (1966) for an example of an "unanswerable" question.

8. THE PROGRESS AND RATIONALITY OF PHILOSOPHY I:
A DISSOLUTION OF THE PRINCIPAL PROBLEM

1. STATEMENT OF THE ARGUMENT

The limitations of various existing solution candidates to the problem of perennial philosophical disagreements have been exposed in previous chapters. In this chapter¹ I shall argue that there is no existing satisfactory and cogent argument to a conclusion asserting the irrationality of philosophy as a cognitive enterprise, from premises describing the existence of perennial philosophical disputes. This aim will be achieved by a rejection of an assumption about the rationality of scientific discourse, which underlies the argument from perennial philosophical disputes. This assumption I shall call, *the assumption of the rational consensus of scientific knowledge*, is an assumption which we encountered in our consideration of mathematical responses to PPPD in chapter 2 of this work. According to this position, widespread controversy across a discipline is problematic because one criterion of truth is that of ideal consensus between rational thinkers.² The propositions which those who are regarded in a culture as being the "experts" accept and believe to be true, are probably true, and it is reasonable for any arbitrary individual to accept such propositions as being true. Consensus between ideal thinkers is a reason or ground for accepting a proposition as being true. Of course the subject of the ideal consensus is the truth status of a proposition, whether p is true, false, meaningless or undecidable. The *definition* of 'true' need not involve consensus considerations; a correspondence of "semantic" definition of truth may be accepted.

Even in taking rational consensus as a criterion of truth, the

rational consensus position is not committed to claiming that the experts cannot ultimately be shown to be wrong in any matter to which they once held rational consensus. However, in so far as they are justifiably regarded as being mistaken this must always be because of a consensus formed by (1) some larger group of experts; (2) some group of even more respected experts or (3) a new consensus formed by the very same members of the formerly discussed group, about the incorrectness of previously held or presently held beliefs and theories.³

It is of interest to note that the assumption of the rational consensus of scientific knowledge, is an assumption widely accepted by actually practicing scientists. It is not thereby advanced as a rational reconstruction by the present author of the theoretical and metatheoretical behaviour of scientists. W.O. Hagstrom (1965) has concluded from a study of scientific disputes, that scientists view disputes in science as counter-productive and incompatible with scientific rationality and progress.⁴ Secondly, in so far as Kuhn's (1970(b)) well known view of scientific progress is sociologically accurate, periods of normal science are basically nothing more than periods of consensus about the satisfactory nature of some "paradigm". It is true that many scientists may not be fully aware of the nature of the paradigm that controls their work, but this does not affect the point which I wish to make (which is not an exercise in Kuhnian exegesis). To consent to a set of propositions it is not necessary to be fully aware of the logical implications of each element of the set. It is sufficient that no proposition in the set is explicitly rejected by the subject and that the truth of some other propositions presuppose the truth of the propositions in S. In so far as a scientist works within a paradigm which controls his/her research, I shall say that the scientist consents to the paradigm.

These periods, Kuhn proposed, are characterized by both a virtual

absence of debate about the satisfactory nature of the paradigm itself and extensive research done within its confines. For Kuhn, statistically, there is a much more extensive portion of the history of science devoted to normal science than there is devoted to revolutionary science - if for no other reason than that the existence of some accepted theory must be presupposed before revolutionary science can occur and scientific theories, like Rome, are not built in a day.

It is also an important part of the rational consensus viewpoint, that disagreements and disputes which are not the result of an inadequate data base held by one or both of the parties at the controversy are incapable of being resolved by rational means. So-called "disagreements in principle" are taken to be unsolvable because for any argument to be rationally acceptable and of persuasive force, it must appeal to premises already accepted by the person to be persuaded, or else commit a *petitio principii*. If the premises of the argument include the principle which is being contested, then the argument will not be acceptable to the opponent; on the other hand if the argument consists of premises which do not include the principle in question, then one does not succeed in presenting an argument for the principle in question, as deductive arguments are generally taken to be non-creative in an information-theoretic sense (Dayton, 1981). In philosophical disputes however, what is precisely at issue is the acceptability of certain very general principles, and the existence of the dispute establishes that no common ground for the resolution of the dispute could exist. All that remains then, is to offer sociological or psychological explanations of the existence of philosophical disputes as we have already seen in earlier chapters.

Finally, according to the rational consensus viewpoint, mature sciences do not exhibit a state of internal discord among its leading

researchers. It is this which methodologically distinguishes sciences such as physics, mathematics and biology from the so-called underdeveloped social sciences and philosophy.

The first part of the argument of this chapter will be an attempt to undermine the rational consensus viewpoint. First I shall argue that the rational consensus view of rationality is far from compelling and the mere existence of perennial disputes does not support any negative assessment about the rationality of such disciplines characterized by such states of discord. For the supporter of the rational consensus viewpoint, the existence of consensus is not in need of explanation; it is to use Robert Nozick's term a "natural state", and it is thus states of disagreements which are problematic (Nozick, 1981, p. 122). However, once a successful argument has been produced for the conclusion that the mere existence of perennial disputes does not support any negative assessment about the rationality of philosophy, there is no good reason to take states of perennial disagreements as unnatural states in need of explanation any more than consensus states are in need of explanation.

An important segment of the first part of the argument of this chapter will be an attempt to undermine the explicit metascientific assumption of the rational consensus viewpoint, which sees the natural and mathematical sciences as essentially free from the state of internal discord which allegedly characterizes philosophy as a discipline. To do this, I shall use two basic arguments. First, it is an empirical fact that major disputes exist in many areas of the natural and mathematical sciences, which are by no means philosophically trivial. Second, the natural sciences do not fail to escape a variant of the problem of perennial philosophical disagreements: the history of science supports a sceptical meta-induction - this being that a wide range of natural scientific theories have been found to be false, hence, probably, all of

humankind's natural scientific theories are strictly speaking false. If Kuhn's metascience is approximately true, then whilst science may exhibit periods of consensus, in the long run, human natural scientific inquiry is a matter of discord and dispute. Lack of consensus will be found not only between the proponents of different theoretical frameworks throughout historical intervals, but also between the proponents of theories throughout such historical intervals.

2. PERENNIAL DISPUTES AND RATIONAL DISAGREEMENT

To begin my critique of the rational consensus viewpoint, I shall address one of the strongest arguments advanced in support of this view. According to this argument, disagreements which are about matters of principle, and not merely about matters of fact are incapable of resolution by rational means. For any argument to be rationally persuasive it must already appeal to premises already accepted by one's opponent, and in the case of a disagreement, if the principle at debate is included in the premises of the advanced argument, a *petitio principii* is committed. On the other hand, if the premises of the advanced argument do not contain the principle under debate, then the argument will not be satisfactory because the needed conclusion can only be validly inferred from premises which include the principle in question.

Eric Dayton (1981) has pointed out that the above argument entails that no one could rationally infer that his/her present beliefs are mistaken, and that the only change possible in one's belief-set, is an addition. However, Dayton argues, it is not always irrational to conclude that one is mistaken in one's beliefs; therefore the rational persuasiveness of an argument cannot depend upon one's prior acceptance of the premises of the argument. Dayton attempts to formulate what the

necessary conditions of an argument's rational persuasiveness are. On Dayton's theory of rational inference (Dayton, 1976), the rationality of inference is explained in terms of the explanatory power of the beliefs inferred and this can be understood as adding or subtracting from the belief-set S of a person P at time t . Dayton claims that for P there will "doubtlessly exist" for P a set Σ of beliefs such that $S \in \Sigma$, but where Σ includes sets of beliefs which P has *some* respect for. No argument which is incompatible with every member of Σ will have any tendency at all to persuade P . The following definitions are first (i.e. (WA)) of the weight of an argument A for P at t , and second (i.e. (WAM)) of "more weight":

(WA) $A = \{p_1, p_2, \dots, p_n, C\}$ has weight for P at t if and only if, (a) P understands A , (b) $(p_1 \ \& \ p_2 \ \& \ \dots \ \& \ p_n \ \rightarrow C)$, (c) A is the intersection of the subset of the members of some sub-set, S_A of Σ , and (d) A causes P to consider whether some member of S_A is true.

(WAM) A has more weight than A' for P at t if and only if (a) both A and A' have weight for P at t and (b) after considering A and A' , P concludes that A is more likely to be true than A' .

One is said to have a good practical argument for accepting a proposition if and only if the argument is *weakly persuasive* as defined by the following:

(WPA2) A is weakly persuasive for P at t if and only if, (a) A has weight for P at t and (b) for all A' , such that A' has weight for P at t , A has more weight for P at t than A' .

It follows that:

(SPAC) A ought to accept C at t on the strength of $(p_1 \& p_2 \& \dots \& p_n \rightarrow C)$ if and only if A is weakly persuasive at t for P.

Finally Dayton offers the following criterion of epistemic reasonableness:

(SPA2) A is strongly persuasive for P at t if and only if,
 (a) A is weakly persuasive for P at t and (b) P is persuaded by A to accept C.

Dayton's view of rational inference does not make the persuasiveness of an argument depend upon the prior acceptance of the premises of the argument, as is assumed by the rational consensus viewpoint. It remains to be stated, first, how in fact one could rationally infer that one's present beliefs are mistaken, and second how disagreements in principle can be resolved. Dayton answers only the second question and his presentation leaves much to be desired by way of clarity and completeness. He first suggests that disagreements may simply be resolved in the same way that other disagreements can be resolved. A less trivial answer is also proposed where it is claimed that we examine the unique role which such principles play in our cognitive enquiries. On the view that principles can be viewed as canons of inference which offer the best explanatory account of what one believes, disagreements are resolved by recognizing that the epistemic policy proposed by one's opponent at the debate, or in general that some other epistemic policy, has more weight for one than one's own policy. This alternative policy is seen to constitute a better explanation of one's belief-set than the policy which one already has in fact has.

This account leaves unexplicated the notion of a 'better explanation', but this is hardly a major cognitive crime, for this concept proves to be one of the most important, and yet most difficult concepts to explicate

in metascientific considerations. It is by far too much of a tangent at this point to canvass a philosophy of explanation, so the argument will proceed from the assumption that this notion is intuitively comprehensible and capable of explication.

If this crucial assumption is granted (and it is unreasonable not to), then we can also outline how it is possible that one may discover that one's present beliefs are mistaken. What is involved in such a discovery is a recognition of the defectiveness of one's present epistemic policies. There are a quite large number of ways in which this may be done, corresponding to the various canons used in formulating an account of explanatory rationality. In what follows, no more than a brief listing is possible.

(1) It may be discovered that one's previous epistemic policy is defective because it is not as *comprehensive* as some other epistemic policy. To discover this, it would need to be shown that the competing epistemic policy explains a much wider range of phenomena than one's present epistemic policy. To discover this is to discover the limitations of one's own epistemic policy.

(2) It may be discovered that one's epistemic policy gives rise to an array of *philosophical, semantic or logical paradoxes and antinomies*. That is to say, one discovers that one's epistemic policy is in some way 'internally incoherent'. There is a variety of ways in which this may be discovered. It may on the one hand be shown that certain crucial supporting arguments which one accepts (a) are mutually inconsistent; (b) are viciously circular; (c) lead to a vicious infinite regress; (d) support some philosophical position which can be shown by some other arguments not questioned at the present debate to be defective. On the other hand the position which one accepts may be shown to be outrightly inconsistent, or to lead to some clear cognitive absurdity. In short,

if one's epistemic policy is paradoxical, then it is defective and in need of modification.⁵

(3) It may be discovered that one's epistemic policy is inconsistent with a much wider set of epistemic policies which one either holds, or could in principle be rationally persuaded to hold. In such a situation an epistemic policy may be rejected and an alternative accepted, even if this alternative is less comprehensive in the sense of point (1) above. What is at stake here is the need to maximize the overall coherence, comprehensiveness and non-paradoxicalness of one's *Weltanschauung*.

Whilst this work does not attempt to present any systematic philosophy of explanation, nevertheless, the previous remarks establish the unsoundness of one of the arguments of the rational consensus viewpoint. If it was the case that for an argument to be rationally persuasive it must appeal to premises already subject to consensus, then it would readily be established that the extreme disagreements between philosophers renders the enterprise of philosophy less than rational. This conclusion has however been blocked by our criticism of the antecedent claim.

3. A SECOND ARGUMENT AGAINST THE RATIONAL CONSENSUS VIEWPOINT

It is an assumption of the rational consensus viewpoint, that if various "distorting forces" are removed, then the "natural state" of rational consensus will be inevitably reached. Failure to obtain consensus may be due to many "distorting forces". On the one hand there are sociological factors such as the presence of self-interests which may be served by failure of rational consent, the existence of political, academic and sexual oppression, and in general the existence of non-emancipatory societies with inegalitarian social structures and various communication distorting factors. This style of explanation characterizes

the so-called "critical theory" tradition, especially the work of Jürgen Habermas (cf. (Smith and Boey, 1982(c))). On the other hand various psychological forces are often cited as *causes* of disagreements. These include obsessions, logical errors due to neurophysiological defects, fears and madness. Disagreements may also be a function of the cognitive situation which one finds oneself in, and may include factors such as lack of evidence, the possession of different evidence or of different methods of evaluating the same evidence. Disagreements may also be a result of genuine misunderstandings and logical errors. According to the rational consensus viewpoint, once these sources of disagreement are eliminated, disagreement would be impossible. To refute this position it is sufficient to show how disagreements may arise between even "ideally rational thinkers" who are not subject to any of the previously mentioned forces of distortion.

Roy Sorensen (1981), has recently given an argument for the thesis that "ideally rational thinkers" may disagree. Sorensen's ingenious argument is based upon the asymmetric credibility of certain Moorean sentences. A putative statement is said to be doxastically indefensible if and only if the person in question cannot consistently believe it. Both $B_a(B_a(\sim p \ \& \ p))$ and $B_a(\sim B_a p \ \& \ p)$ are indefensible (where 'B' is a belief-predicate, 'a' and 'b' constants designating epistemic subjects, and 'p' a constant designating a proposition), but neither $B_b(B_a(\sim p \ \& \ p))$ nor $B_b(\sim B_a p \ \& \ p)$ are indefensible provided that $a \neq b$ and a and b know their identities. A disagreement between ideal thinkers might be engendered simply by virtue of their different identities. Consider two ideal thinkers, Art and Bob who both know that they are ideal thinkers. Both agree that Harry Higher and Larry Lower are authorities on the matter of the national song writing contest. Both judge that Higher is more reliable than Lower on this matter, in so far as whenever Higher and

Lower make conflicting claims, Higher is more likely to be correct than Lower.⁶ Higher tells Art and Bob that:

(8 -1) Winners of the last contest will not believe that they won until Thursday.

Letting 'a' designate Art and 'b' designate Bob, we have assuming that both Art and Bob are aware of each other's beliefs:

(8 -2) $Ba[(\forall x)(Wx \rightarrow Bx \sim Wx)] \ \& \ Bb[(\forall x)(Wx \rightarrow Bx \sim Wx)]$.

Lower also asserts that:

(8 -3) Art is the winner of the last contest.

Both Art and Bob correctly infer that:

(8 -4) Art is a winner of the contest but will not believe so until Thursday.

Given that Art believes (8 -1), he must disbelieve (8 -3), for if he believed (8 -3) he would have an indefensible belief: $Ba[Ba(\forall x)(Wx \rightarrow Bx \sim Wx) \ \& \ BaWa]$, and if he neither believed nor disbelieved (8 -3) then he would again have an indefensible belief: $Ba[Ba(\forall x)(Wx \rightarrow Bx \sim Wx) \ \& \ \sim Ba \sim Wa \ \& \ \sim BaWa]$. Hence as an ideally rational thinker, Art must disbelieve (8 -3). Bob however can believe (8 -4) since $Bb[Wa \ \& \ Ba \sim Wa]$, so he need not reject (8 -1) or (8 -3). Thus both Art and Bob have formed opposing beliefs $Ba \sim Wa \ \& \ BbWa$. Thus two ideal thinkers may disagree by virtue of their different identities. To argue that such a disagreement cannot occur between such ideal thinkers on the grounds that ideal thinkers cannot disagree if they have the same evidence and know that they are in disagreement is a *petitio principii*.⁷

Sorensen's counter-example to the rational consensus viewpoint may be readily given a restatement to be of more interest to a philosophical audience. One need only take the matter to which Higher and Lower are

authorities to be the rational plausibility of some philosophical theory or set of arguments, and appropriately perform the necessary semantical plastic surgery upon sentences (10-1), (10-3) and (10-4) above.⁸

There are at least two other ways in which ideal thinkers may disagree in philosophy. The first of these ways has been stated by W.T. Jones in a number of publications (Jones, 1961; 1965; 1969-70; 1972). The disagreements found not only in philosophy, but also in the natural and social sciences, art and literature, are accounted for by Jones by differences in world views or *Weltanschauungs*. Jones in his paper "Philosophical Disagreements and World Views" (1969-70) offers the following definition of the term 'world view' (ibid, pp. 24-25):

- (D₁) By a world view I mean a configuration of cognitive and evaluative sets, analogous to the perceptual sets that cause different aspects of the experiential field to "stand out" and become noticeable - analogous, that is, to the sort of set that causes my name to stand out (for me) from the noisy and otherwise undistinguishable babble of sound at a cocktail party.

In his later paper "World Views: Their Nature and Function", (1972), Jones offers an alternative definition (ibid, p. 83):

- (D₂) The world view of any individual is a set of very wide range vectors in that individual's belief space (a) that he learned early in life and that are not readily changed and (b) have a determinate influence on much of his observable behavior, both verbal and nonverbal but (c) that he seldom or never verbalizes in the referential model, though (d) they are constantly conveyed by him in the expressive mode and as latent meanings.

Neither of these definitions, despite some anthropological merit, have the same sense as Pepper's "world hypotheses", (Pepper, 1942), the "ultimate cosmology" of Benedict (1934) or the climate of opinion of Whitehead (1925; 1933). Alternatively *Weltanschauungs* may be viewed as the most general descriptions of the world and objects of thought possible.

This does not result, as Nicholas Rescher (1978, p. 222) has claimed, in a generation of an inconsistent n-ad of alternative positions. Rather the rejection of the common assumptions of both the disputants enables the initial debate to be resolved. It may however be the case that the third position is in turn found to be defective for independent reasons. This is however not a point against Ramsey's maxim but only indicates that no philosophical position is immune to revision.

Let us now summarize our explanation of the fact of the diversity of opinion in matters philosophical and metaphilosophical. Already I have discussed one source of this diversity: ideal thinkers may disagree in philosophical matters if the actual philosophical problem being debated is unsolvable. A second source of the diversity of philosophical opinion is due to the fact that the subject matter of philosophical inquiry is an utterly vast and complex reality of which only a fragment can be grasped by any philosophical system; thus all such systems are in some way incomplete. As Böhm (1961) has recognized, nature is "infinitely complex", so that just as no scientific theory can be recognized as final, so can no philosophical theory or philosophical system be regarded as final and unrevisable. I shall leave the issue of the 'infinity' and the super-complexity of the world undefended until the next section, giving further explication and argumentation there. Here, it is sufficient to point out that the problem of perennial philosophical disagreements, is a quite natural product of the Böhm-Vigier position and a view of the nature of philosophy which I have argued for elsewhere (Smith, 1984). Philosophy *should* attempt to provide a comprehensive and coherent theory of the universe drawing upon, criticizing and revising the entire spectrum of available scientific knowledge.⁹ It is a second-order activity. It is in the very nature of this activity that cognitive diversity arises, which I will now outline.

A *Weltanschauung* is a system of thought consisting of a cosmology, metaphysics, epistemology philosophical anthropology as well as a comprehensive socio-political-ethical account of the human animal. A *Weltanschauung* attempts to systematically answer questions of a very general nature such as 'what is there in the world?', 'how do we know what is there in the world?', 'what should one do?', 'what is the right and just way to live?' and so on. Obviously a *Weltanschauung* may be incomplete in various ways, lacking answers to at least some of these questions.

An important role of *Weltanschauungs* is "world picture making". A *Weltanschauung* supplies us with very general images and metaphors of the world. It is easier to give examples of this than to analyse each and every one of these rather vague but suggestive terms. One metaphysical image of the world is that of *atomism* which sees the world as composed of discrete entities which are not connected to each other in any necessary way. Humean atomism has been argued to be responsible for not only a number of problems in metaphysics and metascience, such as the problem of induction, (Harré, 1970), (Harré and Madden, 1975), but atomism has also been seen as responsible for a number of outstanding theoretical problems in biology (Wester and Goodwin, 1981; 1982); (Smith, 1984), and physics (Böhm, 1980).

Disagreements arising from a "clash of metaphysical images" are at many times very difficult to localize. Frequently in philosophical disputes which involve a "clash of metaphysical images" which do not seem capable of decisive settlement, the advice of Ramsey is quite sound (Ramsey, 1931, pp. 115-116):

In such cases it is a heuristic maxim that the truth lies not in one of the two disputed views but in some third possibility which has not yet been thought of, which we can only discover by rejecting something assumed as obvious by both the disputants.

If the arguably correct aim of philosophy is to produce a comprehensive and coherent metaphysical *Weltanschauung*, obviously the philosopher cannot avoid controversies which exist within specific disciplines and/or about specific scientific theories. For example if the philosopher is to criticize and evaluate arguments, some theory of logic must be accepted. But is there a "correct logic"? What does it mean to say that say a many-valued relevant logic is "adequate" or "correct" whilst a classical logic in which $p \ \& \ \sim p \ \rightarrow \ q$ is a theorem is "inadequate" or "incorrect"? These matters are far from trivial since whether classical logic is accepted or not will affect our views about theory choice and rejection. A theory may have an isolated inconsistency: does this inconsistency, to use a popular metaphor, spread and infect the whole of the theory with triviality? Hence on my view of philosophy, the philosopher inherits most of the foundational problems of the analytic, physical and social sciences peculiar to specific disciplines and/or about specific scientific theories.

However, philosophy as a form of master science also faces the problem of dealing with inconsistencies which may arise between various disciplines and/or specific scientific theories. Examples of such conflicts include the alleged conflict between evolutionary biology and "fundamentalist" Christian theology, classical logic with its distributive rule $R \ \& \ (A_1 \vee A_2) \equiv R \ \& \ A_1 \vee R \ \& \ A_2$ and the non-distributive "logic" of the Hilbert space. Further examples of such inconsistencies will be documented in section 4 below. Reconciliation of conflicting fields of knowledge is a task not generally tackled by the specialized scientist, so the job is usually left to the philosopher. Diversity of opinion may readily arise here because there are a number of ways such conflicts can be resolved, and seldom does any one thinker systematically explore all of these opinions in the formulation of his/her solution of the antinomy.

The options which I have referred to are these: (1) both theories are false; (2) one or other theory is false; (3) the inconsistency is only apparent or (4) both theories are true and the inconsistency in question is a "true Hegelian contradiction" (and we recall here our previous discussion of this matter in chapter 4). I will now attempt to account for the diversity of philosophical opinion by a conjecture about the structure of philosophical arguments.

It is generally assumed that valid philosophical arguments consist of a number of premises $P_1, P_2, P_3, \dots, P_n$ and a conclusion C_n which may be validly deduced from the premises of the argument, where the premises and conclusion of this argument is given a philosophical interpretation. On a *multiple-conclusion* view of arguments (Shoemith and Smiley, 1978), a multiple conclusion argument can have a number of conclusions say $C_1, C_2, C_3, \dots, C_m$. Multiple conclusion arguments do not have conclusions which are merely bundles of conventional proofs with each of the C_j as the respective conclusions. Rather just as a proof from $P_1, P_2, P_3, \dots, P_n$ is different from a collection of proofs from P_1, P_2 and so on, so is a proof from $P_1, P_2, P_3, \dots, P_n$ to $C_1, C_2, C_3, \dots, C_m$ different from a mere collection of individual proofs. Multiple conclusions function collectively in a disjunctive way, just as the premises of a standard argument function collectively although in a conjunctive way. To say that C_n follows from $P_1, P_2, P_3, \dots, P_n$ is to say that any interpretation of $P_1, P_2, P_3, \dots, P_n$ which makes $P_1 \& P_2 \& P_3 \& \dots \& P_n$ true, will make C_n true. To say that $C_1, C_2, C_3, \dots, C_m$ follows from $P_1, P_2, P_3, \dots, P_n$ is to say that any interpretation which makes $P_1 \& P_2 \& P_3 \& \dots \& P_n$ true, makes $C_1 \vee C_2 \vee C_3 \vee \dots \vee C_m$ true. Multiple conclusions are not by the same token simply to be equated with the components of a single disjunction. Whilst any finite set of multiple conclusions is equivalent to a disjunctive single conclusion, to establish

such an equivalent we must appeal to the rule of inference 'from A, B, infer validly A V B' which clearly is two separate propositions, and not one disjunctive proposition. It would thus be circular to make use of the equivalence in question to reduce the rule of inference to a single proposition.

I conjecture that philosophical arguments are best analysed by means of a multiple conclusion logic than by standard single conclusion logics. The case of the inconsistency of two theories T_1 and T_2 discussed above may be represented by the following multiple conclusion argument:

(A*) (P₁) T_1 and T_2 appear to conflict,

Therefore,

(C₁) T_1 and T_2 are both false V(C₂) one theory is false V(C₃) the inconsistency is only apparent V(C₄) the contradiction is a "true Hegelian contradiction" and both T_1 and T_2 are true.

In this argument there is no conclusion which is "the" conclusion. Different philosophers opt for different conclusions depending upon their background knowledge and prior logical, semantical and metaphysical commitments. Thus for example conclusion (C₄) would be rejected by a philosopher accepting classical logic and semantics because $p \ \& \ \neg p \rightarrow q$, a theorem of classical logic would be counter-modelled, and classical logic shown to be *unsound*.

It is of course impossible here to show that my conjecture about the source of perennial disagreements is true. Nevertheless the position can be made plausible if it can be shown that a number of philosophical disputes conform to the pattern of argumentation of (A*) above. This I think is the case, and the truth of this claim will be evident upon consideration of a number of standard philosophical problems. Consider

the following problems:

- (i) freewill versus determinism,
- (ii) scepticism versus cognitivism,
- (iii) any paradoxes: the Liar, lottery, preface, and Newcomb paradoxes.

These problems can easily be represented in the form of (A*). For example in the freewill versus determinism debate T_1 may be taken to be a scientific theory, such as the special theory of relativity (which Rietdijk (1966) claims implies determinism¹⁰), whilst T_2 may be taken to be a commonsense theory of human action. The standard positions taken in response to this problem conform to (C₁), (C₂) and (C₃) of the argument (A*). For example the position of compatibilism conforms to conclusion (C₃). It is left as an exercise for the reader to interpret (ii) and (iii) as substitution instances of (A*), a somewhat mechanical exercise.

Is it true that all the theory of philosophical arguments being analysed by means of a multiple conclusion logic amounts to, once the formalism in which the conjecture is couched is removed, is that different people believe different things for different reasons? I do not make this claim, and I believe that this objection is incorrect. We are not interested in why different people believe different things for *different* reasons, but why different people believe different things for the *same* reasons. If philosophical arguments are multiple conclusion arguments, then it is easy to see how this is possible: different people *can* validly deduce different conclusions from the *same* premises, if in fact the "conclusion" was a multiple conclusion proposition. If this was so then it would account for many philosophical disagreements where philosophers seem to accept each other's premises but draw a different conclusion. I conclude then that this criticism of my position is unsatisfactory.

There is a problem though with taking *all* philosophical arguments to be multiple conclusion arguments. How can any philosophical position be rationally justified if any argumentative situation presents us with a maze of disjunctive propositions as our multiple conclusion, it seems that we could never establish by argument conclusion T, where T is a thesis which one wishes to defend. For if by another argument T V R was established, $\sim R$ would need to be established so that T could be inferred via the disjunctive syllogism. But to establish $\sim R$ some philosophical arguments must be best analyzable by means of single conclusion logics. Further discussion of this matter will be postponed until chapter 12.

Let us now sum up the state of the argument of this section. To undermine the rational consensus viewpoint, I have attempted to show that states of consensus are not as Lehrer and Wagner suppose them to be, natural states which are inevitably reached once all sociological and psychological distorting forces are removed. I attempted to show how disagreements may arise between even "ideally rational thinkers", who are not subjected to any of the previously mentioned forces of distortion. To conclude the argument of this chapter, I shall now examine the final outstanding dogma of the rational consensus viewpoint, that the natural sciences are quite free from the internal discord which is taken to characterize philosophy and the social sciences.

4. DISAGREEMENTS IN THE NATURAL SCIENCES

Despite a wide range of theoretical difficulties facing T.S. Kuhn's account of scientific practice, his view of the natural sciences as having periods of 'normal science', has met with some degree of acceptance. Nevertheless, it is an argument based upon the permanent possibility of scientific revolutions, which shows that in the long run the natural sciences are not free from the internal disputes which characterize philosophy.

Arguments against the possibility of a final state of scientific knowledge, based upon the 'infinite complexity of the world' have been presented by Popper (1963), Böhm (1961) and Vigier (1957). As Vigier has said in reflecting on the attempts which he and Böhm have made to present a 'satisfactory' interpretation of quantum theory (*ibid*, p. 77):

We would prefer to say that at all levels of Nature you have a mixture of causal and statistical laws (which come from deeper and external processes). As you progress from one level to another you get new qualitative laws. Causal laws at one level can result from averages of statistical behaviour at a deeper level, which in turn can be explained by deeper causal behaviour, and so on *ad infinitum*. If you then admit that Nature is infinitely complex and that in consequence, no final state of knowledge can be reached, you see that at any stage of scientific knowledge causal and probability laws are necessary to describe the behaviour of any phenomenon, and that any phenomenon is a combination of causal and random properties inextricably woven with one another. All things in Nature then appear as a dialectical synthesis of the infinitely complex motions of matter out of which they surge and grow and into which they finally are bound to disappear.

Even if it is the case that there is an historical convergence towards "one true theory", the existence of scientific revolutions in the history of natural science indicates that the rational consensus view of natural science is myopic. Disagreements will exist between successive "paradigms", and given the "infinite complexity of the world", even if convergence towards "one true theory" as a limit of scientific inquiry occurs, there is no good reason to assume that any theory is final. Thus in the *long run*, the natural sciences stand contrary to the metascientific assumptions of the rational consensus viewpoint.

William Kneale (1967) has argued that the principle of the perpetual revolution of scientific inquiry cannot be justified by an appeal to the cosmological hypothesis of the 'infinite complexity of the world'. This is to say that even if the world is infinitely complex, it still may be reasonable to believe that natural scientific inquiry could terminate in the acceptance of a single theoretical framework better than all

conceivable others. Kneale distinguishes between three forms of the "infinite complexity of the world" thesis: (1) the world contains an infinite multiplicity of particulars; (2) the world contains an infinite variety of natural phenomena and (3) the world contains an infinite number of distinguishable levels of fine structure. It is the third thesis which at least Böhm and Vigier accept, and I shall concentrate upon Kneale's criticism of this thesis.

Kneale's central criticism of the proposal that thesis (3) supports the principle of the perpetual revolution of scientific inquiry, is that thesis (3) means that there is no single theory of the whole world, but at best an infinite conjunction of explanatory theories. Kneale is right in stating this entailment of the Böhm-Vigier viewpoint. This is however precisely what one would expect if the world is composed of "layers": monistic forms of reductionism advocated by the unity of science movements fail (Smith, 1984). It does not show that the principle of the perpetual revolution of scientific inquiry is false (Niiniluoto, 1980, p. 436).

Robert Almeder (1973) has also claimed that the principle of the perpetual revolution of scientific inquiry is false. He maintains that if scientific inquiry was to proceed forever, then science would ultimately terminate in the acceptance of a single theoretical framework better than all other conceivable frameworks. Almeder's argument for this claim, is based as Niiniluoto (1980, pp. 437-438) has observed, on the proposal that the probable success of scientific theories in limit entails that the "true theory", will be found in a finite number of steps. This entailment does not hold as there is a difference between reaching a limit through a finite iteration and approaching a limit indefinitely.

It is concluded that the strongest, presently existing criticisms of the principle of perpetual revolution of scientific inquiry fail. It

remains now to respond to a strong criticism which may be made of my claim that perpetual scientific revolutions constitute cases of rational disagreement. It is true that the sequence of scientific theories ... $T_n, T_{n+1}, T_{n+2}, \dots$ is for the scientific realist, monotone convergent with regard to the degree of verisimilitude. Let us grant this assumption. Even so, it does not follow that in the case of some theory T_k which *replaces* a previous theory T_{k-1} , that T_k and T_{k-1} are *not* in conflict either in terms of the criteria of logical consistency and empirical adequacy. The positivist view of scientific progress, which took succeeding theories to be extensions of previously held theories (which in turn were viewed as true in limiting cases) has been widely criticized and is highly problematic, so T_{n-1} and T_n may well conflict.

I will develop a second argument now for my thesis of the inadequacy of the rational consensus view of the natural sciences. Let us refresh our memories to the fact that the natural sciences are at present permeated with many large scale disagreements of a quite radical nature. In the cases which I shall cite, these disagreements challenge the cogency of the science or theory in question. It is unreasonable to claim that such disagreements are due to non-rational factors such as the need to advance scientific careers, because the disputes to be cited are global, and if anything, the careers of the critics have been damaged by their challenge.

(1) *The Inconsistency of Classical Mathematics*. The inconsistency of classical mathematics has been proposed to be demonstrable by Eduard Wette (1971; 1974; 1976, 1977; 1979; 1981). He has claimed to have proved the inconsistency of elementary number theory, recursive function theory, the principle of mathematical induction and the classical propositional calculus. These claims have been met by the mathematical and logical community with considerable scepticism. Nevertheless it is interesting

to note that the respected mathematician and founding father of mathematical intuitionism, L.E.J. Brouwer has argued that classical mathematics is inconsistent and that a number of important theorems of classical mathematics are open to counter-examples (Heyting, 1966, pp. 121-124). For example, it is contradictory that for every real number a , $a \neq 0$ and $a \not< 0$ would imply $a > 0$. It is also contradictory that for every real number a , $a \neq 0$ would imply $a \not> 0$ or $a \not< 0$. As another example he established that in Euclidean plane geometry, it is contradictory that every two lines which can neither coincide nor be parallel, intersect. Thus it is difficult to propose that intuitionist mathematics is anything but a rival to classical mathematics, since one can prove intuitionistically, that classical mathematics is inconsistent.

(2) *Neo-Darwinism*. Difficulties facing the neo-Darwinist theory of evolution have been discussed elsewhere (Smith, 1984) and do not require immediate repetition.

(3) *Quantum Theory*. There exist a wide range of interpretations of the mathematical formalism of the quantum theory, from the mainstream "Copenhagen view", to the "realist" many world view (Jammer, 1974). This in itself is a situation which the rational consensus theorist should view with alarm, especially since it is very difficult to know what exactly the quantum theory is - quantum field theory certainly does not contain quantum mechanics as a limiting case (Strauss, 1972, pp. 240-241). The quantum theory is most likely one of the most controversial scientific theories ever advanced, and the existence of widespread debates within this field is undeniable.

(4) *Cancer Research*. A unified theory of cancer at present does not exist. Explanations range from the familiar viral theory to Simmons' psychogenic theory (Simmons, 1966), the view that cancer is a glandular malfunction caused by emotional stress.

(5) *The Theory of Special Relativity*. Herbert Dingle (1972) has for some years argued that the special theory of relativity is inconsistent, and hence false. Interestingly enough Dingle has presented evidence that his criticisms have been "ignored, evaded, suppressed and, indeed, treated in every possible way except that of answering it" (ibid, p. 15). If Dingle is right, then one of the basic theories of contemporary physics is inadequate, and should be rejected.

(6) Quantum theory presupposes a Minkowski space of infinite extent, general relativity a Riemannian space, so that the theories posit different space structures and are incompatible in regions of strong curvature (Stephani, 1982, p. 282).

One need not multiply examples (although this is very easily done) to show that the natural sciences are not free from wide ranging foundational disputes. To claim in reply that all of these disputes are simply philosophical debates, is a somewhat *ad hoc* and clearly trivial strategy. In doing this, one simply legislates that the disputes in the natural sciences which are contrary to the predictions of the rational consensus viewpoint, belong to philosophy. As a defense of the position, this strategy is clearly circular. Consequently I reject the view that the natural sciences are qualitatively different from philosophy in being in general free from a wide range of disputes. Note that the disputes cited are not trivial, but are basic disputes in many cases challenging the cogency of the theory in question. Debate, argument, counter-argument and controversy seem to be an omnipresent part of human intellectual life, and thus not peculiar to philosophy.

5. CONCLUSION: STATE OF THE ARGUMENT

The argument of this chapter has been directed towards *dissolving* the problem of perennial philosophical disputes. States of consensus are not natural states which are inevitably reached once distorting sociological and psychological forces are removed, as Lehrer and Wagner believe. Rather, disputes may exist between even ideally rational thinkers. Certainly the natural sciences are not, as the critics of philosophy would propose, generally free from wide ranging and basic disputes. It is not claimed that issues such as the inconsistency of number theory are of primary interest to all practising mathematicians, the "working mathematician". Of course it is not, and is only discussed within a small circle of researchers. But issues in metaphilosophy, such as the progressiveness of philosophy as a cognitive enterprise have also not received as much discussion recently, as say the problem of universals (as consultation of *The Philosopher's Index* will confirm). My point is that rational consensus between ideal thinkers cannot be taken as a criterion of the truth or probable truth of propositions. That Wette's work for example is widely rejected by mathematical logicians does not give me the slightest reason for believing that Wette's work is problematic. That philosophy as a discipline is subjected to wide ranging perennial disputes, gives me no good reason to believe that there is no philosophical knowledge and that philosophy is not a cognitively progressive enterprise. I have also argued in both this chapter and in earlier chapters, that there are a number of very different reasons *why* perennial philosophical debates may exist, *so that no unified theory of perennial philosophical debates can be given.*

This leads us to a major problem. How can we establish the progressiveness of a discipline or theory once rational consensus is rejected as a criterion of truth or probable truth? Recall that my aims

were to defend a view of cognitive progress which defined progress as an increase in verisimilitude. Not only must the logical antinomies facing the theory of verisimilitude be solved, but we must also solve the epistemological problem of justifying claims of the alleged degree of verisimilitude of a theory. These difficult questions will be discussed in chapter 9. Chapter 10 will outline a solution to a problem which has been left outstanding from section 3, which will call for a discussion of the methods of reasoning and the very point of philosophical inquiry itself.

8. Notes

1. The argument of this chapter appears in (Smith, 1984(g)).
2. By the expression 'criterion of truth' I mean what Nicholas Rescher means by this expression in (Rescher, 1973).
3. There is an argument against the rational consensus position which should be considered and rejected at this point: the position commits an elementary informal logical fallacy: *the fallacy of the appeal to authority* or *argumentum ad verecundiam*. Many logic texts have rejected an appeal to authority as a legitimate source of knowledge, with the seeming consequence that the bibliographic method of most research papers in the sciences would be unjustified, and the arguments of such papers fallacious. Woods and Walton (1982) have argued that an appeal to authority constitutes knowledge only if the following conditions are all met (*ibid*, pp. 86-109):
 - (i) The authority must be interpreted correctly.
 - (ii) The authority must have special competence in the field, and not simply glamour or popularity.
 - (iii) The authority's judgement must actually be within his/her special field of competence.
 - (iv) Direct evidence must be available in principle.
 - (v) A consensus technique is required for adjudicating disagreements among equally qualified authorities.

Here I will follow Woods' and Walton's position, with the difference that I rejected condition (v). If the experts disagree, then the *argumentum ad verecundiam* is genuinely fallacious because the ground upon which the premises of your argument is based, expert opinion on the matter, is inconsistent.

4. I have been led to Hagstrom's work by Andrew Lugg's citation in his (Lugg, 1978, pp. 276-277).
5. It may well be argued in reply to this proposal that the condition of the non-paradoxicalness of a viewpoint or epistemic policy, commits an outrageous *petitio principii* against paraconsistent positions (e.g. (Routley, 1980)). However, there seems to me to be good reason for accepting that even if there were "true Hegelian contradictions", that not *all* paradoxes are unproblematic. (Both Graham Priest and Richard Routley have stated to me in personal correspondence that the paraconsistent logician does not need to accept as true "any old contradiction".) Argumentation can be given for this position. If one was to propose that paradoxicalness was not at least a *prima facie* defect in a position, then it is difficult to see how any sort of rational criticism is possible. The argument in support of this conclusion was developed in (Smith, 1984(g)).

6. It is important to add a qualification to Sorensen's story which he unfortunately neglects to add. This is, that Higher and Lower are *not* ideal thinkers. If they were then it would be an utterly obvious circularity to claim that Higher and Lower make conflicting claims.
7. This example also refutes the Lehrer-Wagner rational consensus epistemology discussed previously, that under their specifically stated assumptions, the failure of two thinkers to obtain consensus indicates that one or other of these thinkers are irrational.
8. For an outline of some of the defects in standard attempts to dissolve "Moore's Paradox", cf. (Williams, 1982).
9. Apart from (Smith, 1984), I will discuss the matter of the nature and methods of philosophy further in chapter 10.
10. On this issue, cf. also (Stein, 1968).

9. THE PROGRESS AND RATIONALITY OF PHILOSOPHY II:
A THEORY OF COGNITIVE PROGRESS

1. STATEMENT OF THE ARGUMENT

In this chapter I shall defend a realist view of the cognitive progressiveness of philosophy, the view that philosophy attempts to "arrive at statements, in the form of hypotheses or theories, that claim to be true, and true explicitly with reference to the nature of the investigated phenomena as uncovered by the various investigative procedures" (Loewenberg, 1959, p. 31).¹ To do so, a solution must be given to the problem of verisimilitude discussed in chapter 3. There we saw that there is no logically cogent and non-paradoxical definition of 'verisimilitude'; in section 2 below I shall outline and defend a new definition of verisimilitude which avoids the Miller-Tichý-Harris result, yet is "structurally" similar to Popper's original definition of 'qualitative verisimilitude'. The second part of the argument of this chapter must establish the cogency and tenability of viewing philosophical progress as an increase in verisimilitude, including specific examples of philosophical fields where such an increase in verisimilitude has occurred. A discussion of the various "investigative procedures" whereby reason is established for the acceptance of the truth of philosophical hypotheses and theories will be given in chapter 10 below.

2. AN EXPLICATION OF 'QUALITATIVE VERISIMILITUDE'

It is a merit of any account of verisimilitude that it is not only intuitively plausible, but that it is as well, formally simple and hence easy to operate. Most of the existing accounts of verisimilitude violate this requirement of simplicity; they seem to add increasingly complex

epicycles rather than increasingly deep insights. I shall present, and defend, an intuitively plausible and formally simple account of verisimilitude, which also avoids the Miller-Tichý-Harris Theorem. Further, my account will meet David Miller's requirement that any adequate account of verisimilitude shall not permit any proposed ordering by the relation of verisimilitude, to be reversed by a simple linguistic reformulation. The section concludes with a response to the problems which Miller has raised in his paper "The Accuracy of Predictions" (1975).

The "truth" may be viewed as a superset Σ_T which contains all true statements, and the superset Σ_F contains every false statement. In my opinion truthlikeness or verisimilitude, is best viewed as the "closeness" in size of theories to Σ_T , and "smallness" in size to Σ_F . I take the metaphor of 'size' to be best explicated by the notion of the cardinality of a set. Rather than define cardinal numbers as certain types of ordinal numbers it is proposed that one simply adds to some 'logically regimented' set-theory, such as Zermelo-Fraenkel set-theory the following axiom for cardinal numbers:

$$(ACN) \quad \text{Card}_N(A) = \text{Card}_N(B) \leftrightarrow A \approx B$$

We will say that a set A has a greater cardinal number than a set B if and only if there is a 1-1 correspondence between B and a proper subset of A, but no 1-1 correspondence between B and the whole of A. The set A will be said to have a smaller cardinal number than a set B if and only if B has a greater cardinal number than A. It is hardly necessary to present a fully formal theory of cardinal numbers to supplement this gloss, since the reader can readily find this in the standard texts on set-theory (e.g. (Suppes, 1960)). Without further ado, our definition of verisimilitude is given:

$$\begin{aligned}
 (\text{SDV}) \quad A >_{\text{v}} B = \text{df} \{ & (\text{Card}_{\text{N}}(A_{\text{T}}) > \text{Card}_{\text{N}}(B_{\text{T}})) \\
 & \& (\text{Card}_{\text{N}}(A_{\text{F}}) \leq \text{Card}_{\text{N}}(B_{\text{F}})) \} \\
 \vee \{ & (\text{Card}_{\text{N}}(A_{\text{T}}) \geq \text{Card}_{\text{N}}(B_{\text{T}})) \\
 & \& (\text{Card}_{\text{N}}(A_{\text{F}}) < \text{Card}_{\text{N}}(B_{\text{F}})) \}
 \end{aligned}$$

This definition is similar to Popper's original qualitative definition of verisimilitude. It has however added advantages, ignoring for the moment the problem of the Miller-Tichý-Harris Theorem. First, on Popper's definition when dealing with infinite sets we get no satisfactory indication of the *size* of the respective sets of truths and falsehoods. The theory of cardinal numbers is especially devised to speak of the sizes of both finite and infinite sets, so any definition of verisimilitude based upon it will be quite general. Second, on Popper's definition, we are committed to the claim that if $A >_{\text{v}} B$, then if $B_{\text{T}} \subset A_{\text{T}}$, then if $\phi \in B_{\text{T}}$, $\phi \in A_{\text{T}}$. It is impossible to speak of verisimilitude relationships holding between theories where some sentence $\phi \in B_{\text{T}}$ is not also an element of A_{T} . That the more comprehensive theory must contain *all* of the truths of the less comprehensive theory, is an assumption which I have no use for, and further it is directly responsible for generating the various limitation results, as is obvious from my proofs given in chapter 3 above.

There is no doubt that it may be felt that some analogue of the Miller-Tichý-Harris Theorem might be provable for (SDV), so this issue should be addressed, and disarmed. The following limitation theorems might be suggested as suitable analogues:

- (1) (LT₁) $A >_{\text{v}} B \rightarrow \text{Card}_{\text{N}}(A) \leq \text{Card}_{\text{N}}(\Sigma_{\text{T}})$
- (LT₂) $A >_{\text{v}} B \rightarrow \text{Card}_{\text{N}}(A_{\text{T}}) > \text{Card}_{\text{N}}(\Sigma_{\text{T}})$
- (LT₃) $A >_{\text{v}} B \rightarrow \text{Card}_{\text{N}}(A_{\text{T}}) \leq \text{Card}_{\text{N}}(\Sigma_{\text{T}})$
- (LT₄) $A >_{\text{v}} B \rightarrow \text{Card}_{\text{N}}(A_{\text{T}}) \leq \text{Card}_{\text{N}}(B_{\text{T}})$.

Suppose \vdash (LT₁). All this would mean is that a verisimilitude relationship would not exist between A and B if $\text{Card}_{\text{N}}(A_{\text{T}}) > \text{Card}_{\text{N}}(\Sigma_{\text{T}})$ which

is quite true. Indeed the latter proposition is self-contradictory since there is no $\phi \in A_T$ which is not also in Σ_T , by definition of ' Σ_T '. If on the other hand $\vdash (LT_3)$, then one would not merely refute my account of verisimilitude, but one would have raised a major problem for the theory of cardinal numbers in general, for we may interpret T to be the set of truths of mathematics and rework $\vdash (LT_3)$ to produce a contradiction in the theory of cardinal numbers. In both cases however, it is difficult to imagine how such proofs could be forthcoming.

If $\vdash (LT_1)$ this would only mean that our theories must always fall far short of being absolutely comprehensive in their set of truths. Since no one can know that there are no unknowable truths (by definition) it would hardly be much of a limitation if $\vdash (LT_1)$.

Suppose on the other hand that $\vdash (LT_4)$. This would completely refute my account of verisimilitude. However I see no way of establishing this theorem without in the process raising once more, major problems for the theory of cardinal numbers.

It may be objected, as Tichý (1974) has suggested, that the verisimilitude of any false theory A would be increased by adding to A an arbitrary sentence which does not follow from it. Against this it may be replied that verisimilitude comparisons, if they are to have a point at all, must be strictly between the logical consequences of theories. Tichý's arbitrary sentence does not follow from A and we take this as a conclusive reason for disregarding it in our verisimilitude assessment. We should further note that this argument of Tichý is undermined by John Harris' proof that we cannot increase the truth-content of a false theory by conjoining a logically independent sentence b to it, without also increasing the falsity content and vice versa, whether b is true or not (Harris, 1974, p. 163).

It is also noted that (SDV) does not involve one in having to have an impossible comparison theory Σ_T to make verisimilitude estimates. Σ_T stands as a *metaphor*, and is not a part of the formal mechanics of (SDV). Verisimilitude comparisons are made without recourse to any third comparison theory. Hence John Harris' problems about verisimilitude comparisons between A and B with respect to a comparison theory C, do not arise.

Perhaps (SDV) might be taken to fail for theories which are inconsistent within the framework of classical logic. This however is not so. The possibility of a classically inconsistent (and hence trivial theory) being closer to the truth than a non-trivial theory is excluded by cardinal number comparisons of the respective sets of false statements of the two theories. Since $p \ \& \ \sim p \rightarrow q$ in classical logic, for a classically inconsistent theory A, $\text{Card}_N(A_T) = \text{Card}_N(\Sigma_T)$ and also $\text{Card}_N(A_F) = \text{Card}_N(\Sigma_F)$.

I shall also add here, that considerations of verisimilitude need not be made *solely* on the basis of the total number of truths and falsehoods of two theories. If the set of truths and the set of falsehoods of both theories is infinite, then some *filter* might be placed upon these sets to decrease their sizes. Thus the set A_T of truths of A may be deflated by considering the resultant truths relative to a set of problems or some set of interests. This will determine a set $SA_T \subseteq A_T$ of the *significant* or *relevant* truths of A. Criteria for deciding this can hardly be expected to be presented in a discussion of merely the explication of the concept of verisimilitude.

Tichý (1978(a), p.175) takes it as non-controversial that the person who maintains that there are exactly eight planets in the solar system is closer to the truth than his/her opponent who insists that there are only five. Agreed. This being granted, this situation does not constitute a counter-example to (SDV). It is one thing for a theory to be closer to

the truth than another, another thing for a statement to be closer to the truth than another. The account of verisimilitude advanced here is a purely qualitative one, which assumes that the truth valuations of statements are not 'degrees of truth'. If there existed a satisfactory account of partial truth, then the theory here would need to be modified to incorporate its insights.

This completes the defense of (SDV) which shall be offered here. Needless to say, there are still quite substantial epistemological and methodological questions about verisimilitude assessments which cannot be addressed here. The chapter will be concluded by a response to difficulties facing accounts of verisimilitude which David Miller (1975) has presented, and to which any tenable theory of verisimilitude must have a response.

David Miller has claimed to show, that if theories A and B answer the same quantitative questions and evaluate the same quantities, then the constants that A predicts truly cannot all be constants that B predicts truly unless B always predicts truly. Hence the constants that B predicts falsely cannot all be constants that A predicts falsely, unless B always predicts truly. Whilst Miller gives a general algebraic argument to this conclusion, it is more informative to review an example which he himself cites.

Let \textcircled{H} and ϕ be two physical constants, and suppose that A asserts that $\textcircled{H} = 8$ and $\phi = 0$ and B asserts that $\textcircled{H} = 7$ and $\phi = 2$. Suppose that \textcircled{H} 's true value is 0, whilst ϕ 's is 2. The theory A is then incorrect with respect to both constants, whilst B is right about one of them. Miller then asks us to consider the *physical constants* Ψ and X , defined as $\Psi = \textcircled{H} + \phi$ and $X = \textcircled{H} + 4\phi$. The predicted values and the true values of Ψ and X are now such that A is correct about X , but wrong about Ψ , whilst B is wrong about both. Indeed, as long as A is false for both \textcircled{H} and ϕ and B for only one, there will always be a linear combination of \textcircled{H} and ϕ for

which A predicts the true value whilst B is in error.

We can see that something is drastically wrong with Miller's argument if we allow $\Psi = f(\textcircled{H}, \Phi)$ and $X = G(\textcircled{H}, \Phi)$. Suppose we take Miller's own values for \textcircled{H} and Φ as given in the above example, and let $\Psi = \textcircled{H}.\Phi$ and $X = \textcircled{H}.\Phi$. Then for Miller's own values, A which was false for both \textcircled{H} and Φ is now true for both Ψ and X and B which was true about Φ is now false for both Ψ and X . As a second example, consider I.J. Good's (1975) proposal, that a switch between A and B can occur even when they refer to a one dimensional parameter. Suppose that the values of the parameters predicted by theories A and B are α and β respectively, such that $0 < \alpha < \beta$, then in *some cases*, by taking the decimal representation of the respective numbers and interchanging the first two digits, then the third and fourth digits, and so on, it will be found that the transformed values α' and β' are such that $0 < \beta' < \alpha'$. Thus if $\alpha = \alpha^*.a_1a_2 \dots$ and $\beta = \beta^*.b_1b_2 \dots$ then $\alpha' = \alpha^*.a_2a_1 \dots$ and $\beta' = \beta^*.b_2b_1 \dots$ and $a_2 > b_2$.

In reply, it is alleged that the strategies of Miller and Good are outrightly bogus. Miller hasn't shown how his result can be produced in any physical theory, and until he does, the claim that Ψ and X are 'physical constants' is unjustified. The physical significance of reversing decimal points is a game played by manipulating symbols, of no philosophical and scientific significance.

3. VERISIMILITUDE AND THE SCEPTICAL METAINDUCTION

A response has now been given to the principal semantic question of the theory of verisimilitude. In summary it has been shown how we can still hold to the intuitive notion of "closeness to the truth" without falling into the paradox of Miller-Tichý-Harris. The question which must in turn be considered is: 'is it tenable to view philosophical progress

as an increase in verisimilitude and if so give examples? There is an epistemological problem which must be dealt with before even this question can be answered, the problem of *the sceptical metainduction*.²

The problem of the *sceptical metainduction* is this: past theories have turned out to be false, and there is no good reason to believe that present theories are in any way exceptional. Hence it is reasonable to believe that all scientific theories are strictly speaking false. This argument destroys any applicability of my theory of verisimilitude: verisimilitude rankings presuppose on my account, reason to hold that some theoretical implications of scientific theories are true, and without this assumption, verisimilitude comparisons cannot be significantly made.

The Popperian tradition in the philosophy of science (Popper, 1972), (Miller, 1980), (Radnitzky, 1980(a); (b)) provides a concise and logically coherent response to this problem. Hume's general problem of induction shows that no empirical or non-logical theoretical claim is capable of *justification*, even if 'justification' is taken to mean 'to attain some degree of "reliability" or "probability"'. Even claims about the falsification of theoretical claims are *conjectural*. The radical challenge of the Popperian view of "knowledge" has not been appreciated because of Popper's own inability to abandon use of success-words such as "knowledge", "logic of discovery" (Stove, 1982). Miller (1980) has given a clear statement of the Popperian position which avoids many of Stove's objections: scientific knowledge in the classical sense of justified true beliefs/propositions does not exist (ibid, p. 129):

. . . in the normal run of things, scientific knowledge is everything that a classical epistemologist says it ought not to be: it is unjustified, untrue, unbelief. From this point of view a logic of induction would not be wanted, even if it were available - for no effort is made, or should be made, to justify even the tiniest fragment of our knowledge. This new epistemology is, obviously, one that philosophers find so hard to digest that most would rather commit themselves to the

absurdities of inductive logic and the search for justification. But, indeed, it need never be asked again whether science can do without induction. It does.

I have said that Miller avoids many, but not all, of Stove's objections, because Miller cannot face the consequence himself that scientific propositions are unjustified, untrue, unbelief. Miller still cannot resist speaking of "knowledge". If he was consistent, he would have no further use of such classical epistemological concepts and would willingly fling them upon the conceptual scrapheap along with phlogiston and vital spirits.

Popperian sceptical rationalism precludes any realistic view of scientific progress. If scientific propositions are unjustified, untrue, unbelief, then there is no point in advancing theories of verisimilitude at all (Stove, 1982). This is a desperate position inconsistent with scientific practice: whatever rationality gaps exist in science it is a fact that scientists do attempt, perhaps quite inadequately, to justify scientific theories by appeal to evidence and argument. Disputes about the coherence of the special theory of relativity, the neo-Darwinian theory of evolution, the adequacy of classical logic, cited in the previous chapter become meaningless if Popperian sceptical rationalism is accepted. This is not to reject important insights which Popper has given us, such as his view of scientific activity as a *critical enterprise*; rather it is to reject his view that the justificationist enterprise is bankrupt.

This itself can only be done by means of a solution to the problem of induction. Harré and Madden (1975) and Bhaskar (1978) have given what is in my opinion a solution to this problem and I have defended their position against criticisms elsewhere (Smith, 1982(b); 1984(b)), and shall not repeat such discussions here. Here I will attempt to undermine the argument of the sceptical meta-induction by rejecting the claim that we have good reason to believe that *all* scientific theories are false. By use of the method

of counter-examples,³ I will cite a scientific theory which was accepted very early by Western science and is still accepted today. When this is done, the sceptical meta-induction can be rejected.

Consider this problem: where do human babies come from? Most cultures recognize that human females give birth to the succeeding generation, and this fact is not in any way refuted by any technological innovation such as purely artificial wombs. But not all cultures have agreed that the general sexual theory of human reproduction is true, this theory being the view that the reproduction of the human species is a function of a male "contribution" and a female "contribution". I wish to beg no questions of genetic detail here and so I use the deliberately vague term 'contribution'. Not all cultures have believed that the male makes a "contribution" to human reproduction through coitus: many Aboriginal tribes believe that a woman conceives a child because she has stood upon a fertility rock. The "contribution" of the male to human reproduction is quite conjectural because intercourse leading to conception and the resultant childbirth are separated by a period of time which may at its maximum be equal to the gestation period of the human species of approximately nine months. Thus the general sexual theory of human reproduction is far from trivial. Yet we and most contemporary Aborigines believe that it is true. Medical evidence against the general sexual theory of human reproduction is nil, and further evidence will today do nothing to increase our confidence in the truth of the theory. The theory therefore is not merely *probable* or highly confirmed, it is definitively true.

The critical fallibilist, impressed by received opinion in the philosophy of science such as the Duhem-Quine thesis,⁴ may argue that the general sexual theory of human reproduction is mistaken. If necessary appeals to hallucinations and systematic delusions could be made. Indeed

they could. This strategy does not show in *particular* how the general sexual theory of human reproduction is mistaken, for we have here nothing more than the epistemological sceptic's argument against all knowledge claims based upon the possibility of being mistaken. No doubt it is logically possible that the general sexual theory of human reproduction is false, just as it is logically possible that all presently existing scientific theories are in fact true. This does nothing to establish that the theory is actually false.

Other examples of true scientific theories can be readily given: the blood of animals circulates throughout the body being pumped by the heart, exfoliation of rocks is a product of differential heating and cooling in the presence of moisture and that pulmonary tuberculosis is caused by a bacterial infection rather than by "bad air" and "gases". Nevertheless, consistent with the fallibilism of the previous chapter, it is likely that many of today's accepted theories are in fact false. It is one thing to accept such a cautious scepticism, quite another to accept the conclusion of the argument from sceptical meta-induction.

I have noted previously (Smith, 1984) that philosophy of science in this century has taken physics as the science *par excellence* and metascientific models almost exclusively are devised to deal with problems in physics. Popper was greatly impressed by the replacement of Newton's theory of gravitation by Einstein's theory of gravitation; Kuhn bases his view about scientific paradigms predominantly upon data taken from the history of physics; Feyerabend supports his epistemological anarchism on the basis of case studies taken from the history of physics and the Sneed-Stegmüller structuralist account of scientific theories takes mathematical physics as its model. As I have previously argued, nothing compels us to accept this received prejudice and there are good reasons for rejecting it.

Hence the argument from the sceptical meta-induction does not challenge our account of verisimilitude: not *all* scientific theories are strictly false, although very many are.

4. PHILOSOPHY AND THE NEW THEORY OF VERISIMILITUDE

Is it at all plausible to view philosophical progress as an increase in true or highly truthlike information about the objects of philosophical discourse? Indeed it is, and there are at least some examples of such philosophical progress. Recognizing that many current scientific theories may be false we conclude that philosophy cannot be reasonably viewed as a degenerating research programme.

A gain in truthlike information occurs when a solution to a philosophical problem is given. Whilst in the experimental sciences such as molecular biology, or the strongly mathematico-theoretical sciences such as quantum gravity, there are as yet unanswered research problems, in philosophy (excluding mathematical logic) there are no unanswered philosophical problems, except for the instance when a new paradox is first put forward and before published responses can be given or verbal responses in conferences or research seminars made. By 'answered' I mean 'given a response, although not necessary a correct response'. There is thus a great abundance of riches in philosophy, which must be culled, pruned and regimented. This suggests another area in which a gain in truthlike information can be obtained in philosophy - through uncovering defects in positions, unjustified assumptions and fallacious arguments (Blanshard, 1966), (Rapaport, 1982).

Philosophy is a critical enterprise. A significant part of research in philosophy consists in sealing off blind alleys, exposing errors and misinterpretations, exhibiting fallacious arguments and contradictions, and

establishing the limitation of various positions and principles. It might seem at first glance to an outsider that such a negative process may serve to clear away philosophical dead wood, but it could hardly serve to provide truthlike information. This however is not so. It is of interest in philosophy to establish negative results, to establish the limitations of various methodologies and philosophical orientations. Articles which do no more than discuss defects in some already advanced philosophical theory are quite readily published in leading philosophical journals. The journals serving the natural and behavioural sciences do not publish polemical and purely critical articles as frequently as those journals serving the philosophical community (Hagstrom, 1965, pp. 276-279), (Lugg, 1978). It therefore seems reasonable to suppose that the critical aspect of philosophy supplies another source of truthlike information: to uncover the fallaciousness of some argument is to uncover a philosophical truth. Even to argue that some problem is weakly epistemically unsolvable, as has already been discussed in chapter 9, is to make a significant philosophical claim. Such negative results advance our philosophical knowledge, just as Gödel's theorems and the various other limitation theorems in mathematical logic advance our logical knowledge in establishing that certain propositions are "unprovable".

Another area in which truths may be uncovered in philosophical inquiry is in the conceptual explication and clarification of problems, in outlining what one is committed to. This process does not merely involve clarifying basic terms by semantical analysis, as has been stressed by the linguistic turn in philosophy, but it also involves studying the interrelationship between the problem in question and a set of other philosophical problems. The reason for this is that in philosophy, as in other disciplines, overall systemic consistency is vital between the various answers given to a wide range of philosophical problems. It is

important then to produce a coherent, consistent and systematic system of philosophical thought, and this as any working philosopher knows, is no easy matter. In so far as contradictions and defects are uncovered in systems of philosophical thought, further philosophical truths are presented.

It may be objected at this point that my view of philosophy as a critical enterprise is incongruent with my theory of verisimilitude, as I have only defined verisimilitude comparisons for *theories*, not positions. The objection is superficial. A theory T can be regarded as a set theoretical entity $\{t_1, t_2, \dots, t_n, \dots\}$ where t_j is an arbitrarily chosen theoretical consequence. A philosophical proposition can be regarded as a unit theory $\{t_j\}$ and a philosophical position as a set of philosophical theories.

Let us now answer the request for examples of progress in philosophy. If I held to the rational consensus viewpoint, then one could cite the recent rejection of sense-data theories of perception, the collapse of the logical empiricist metascience (and hence the solution of many problems, just as the justification of the observational-theoretical distinction, which were a function of this specific metascience), as examples of philosophical progress. Unfortunately, to argue in this fashion, would result in a major inconsistency in this work. Perhaps most philosophers of science reject logical empiricism - perhaps they do so wrongly. All that one can do is examine at first hand the relevant arguments and state one's conclusions explicitly. A philosopher can do no more, and no less. Philosophical inquiry, whilst sociologically dependent for its very existence upon a community of thinkers, is from an epistemological point of view, an *individualistic enterprise*.⁵ Even as a collectivist Marxist, one can do no more than state the *reasons which one has* for believing that collectivist Marxism is true.

Hence, no systematic answer can be given to the question of whether philosophy as a whole in each of its fields is progressive. That would require a comprehensive philosophical system. I take it to be the role (or "calling" if you like) of the philosopher to have as a life research project, the construction of such a system (Smith, 1984), but this can hardly be canvassed here. However what can be offered at this point is an answer to the solvability sceptics' challenge. Applying the very criteria of this chapter self-reflexively it can be shown that philosophical progress occurs. If the arguments of this work hold, then the problem of perennial philosophical disputes has been solved and the solvability sceptic answered. Yet if my critics do uncover major defects in this work, then at least one claim will survive. Progress would have occurred in discovering my errors. As my case either holds or does not hold, it follows that philosophical progress occurs. Thus the major thesis of this work is established.

5. CONCLUSION: THE STATE OF THE ARGUMENT

The tasks set for myself in section 1 are now completed. An account of verisimilitude has been given which avoids the Miller-Tichý-Harris result. Further the cogency and tenability of viewing philosophical progress as an increase in verisimilitude has been argued for, and reason given to believe that philosophical progress occurs. This leaves us with one remaining task: to discuss the various "investigative procedures" whereby reason is established for the acceptance of the truth of philosophical hypotheses and theories. Chapter 10 will address this task, as well as addressing an outstanding problem from chapter 8 - whether there are single conclusion philosophical arguments that may be taken as the objects of philosophical knowledge.

9. NOTES

1. This however is not to embrace Loewenberg's metaphilosophy, which whilst having many cognitive virtues, cannot number comprehensiveness among them. Loewenberg (1959, pp. 83-102) attempts to avoid the problem of perennial philosophical disputes by questioning the identification of all knowledge with scientific knowledge. Philosophy is characterized as a cognitive enterprise by *reflexivity*; as he puts it (ibid, p. 88):

What distinguishes philosophic activity as reflexive is this: matters elsewhere viewed as introductory or heuristic become here independent subjects of investigation, inchoate questions of method, for example, culminating in methodology and initial problems of definition in semasiology (or semantics). To speak of philosophy as a discipline chiefly intent upon method of method or meaning of meaning or theory of theory or knowledge of knowledge is to convey in terse fashion the reflexive or self-conscious aspect of the philosophical enterprise.

Such a view of philosophy has immediate difficulty in accounting for first-order philosophical research in areas which are not derived from any natural or social science - questions in ethics such as 'what is goodness?', 'what is justice?', 'what is value?'; questions in aesthetics such as 'what is beauty' and metaphysical and epistemological questions such as 'is epistemological scepticism refutable?', 'why is there something rather than nothing?', 'what reason have we to believe that the world was not created five minutes ago, complete with all possible traces of an old world, by an evil demon?'. Philosophy is thus not *merely* metascience, although metascience is one of the most important fields of philosophy.

2. For a further discussion of the problem of the *sceptical metainduction*, cf. (Newton-Smith, 1981, chp. 8).
3. The method of counter-examples will be discussed in some detail in chapter 10.
4. On the Duhem-Quine thesis, cf. (Harding (ed.), 1976).
5. Nothing of course precludes such "individuals" from being teams or groups. Joint papers, in so far as they are written at all in philosophy, are written so as to be a "collective voice".

10. THE PROGRESS AND RATIONALITY OF PHILOSOPHY III:
A THEORY OF PHILOSOPHICAL RATIONALITY

1. STATEMENT OF THE ARGUMENT

One of the most important problems discussed in twentieth century metaphilosophy was precisely how philosophical inquiry could result in *philosophical knowledge*. If philosophy could produce no *new* knowledge, and if it is questionable as to whether there is any philosophical knowledge at all, then philosophy can no more claim to be a rational cognitive enterprise than any other pseudo-science one might care to cite. Ross Harrison (1974, p. 1) gives an accurate description of this problem:

Philosophy is often dismissed by non-philosophers as being merely a misguided competitor to science. This is because it seems both to share the scientific aim of attempting to understand the nature of the world yet also to use inappropriate methods for making such an attempt. Unlike scientists, philosophers neither go into laboratories nor seem to be particularly well informed about what is known by those who do. Instead they feel themselves able to dispense with the services of observation and experiment and so able to work with pure reason alone. Yet a common assumption of both philosophers and non-philosophers is that someone operating with pure reason alone cannot discover the nature of the actual world. Armchair science is not a respectable activity; and it is not clear to the non-philosopher, nor sometimes to the philosopher himself, how philosophy differs from armchair science.¹

In this chapter I shall outline a solution to the problem of the rationality of philosophy, by showing how philosophical inquiry results in philosophical knowledge. Further, I shall take up an outstanding issue, of showing that there is good reason to believe that there are single conclusion arguments in philosophy. In short, I shall state what I believe philosophical rationality to consist in, and why philosophy done as I conceive it to be is a rational cognitive enterprise generating rationally appraisable, knowledge claims.

2. ON PHILOSOPHICAL METHOD

I wish to contrast my own views about philosophical method with Hector-Neri Castañeda's (1980) discussion, this contrast being one of basic orientation rather than of matters of detail. Castañeda states that his concern is with *phenomenological ontology* or *primary ontology* (ibid, p. 13) but discussions in his book range over the whole of philosophy including issues in epistemology (ibid, p. 46) and ethics (ibid, pp. 79-80). It is not unreasonable then to propose that Castañeda's views, if valuable, should be extended to produce a comprehensive metaphilosophy. It will be argued here however, that there are fundamental problems facing Castaneda's approach even as a metaphilosophy of phenomenological ontology.

Castañeda distinguishes between four main types of philosophical activity: (i) proto-philosophical, (ii) sym-philosophical, (iii) dia-philosophical and (iv) meta-philosophical. Proto-philosophical activity consists in the collection of both empirical and linguistic data from which criteria of adequacy for philosophical theories are formulated. Sym-philosophical activity consists in the development of philosophical theories based upon such data. Dia-philosophical activity involves the comparison of such philosophical theories, to establish through isomorphisms among them, a system of invariances which gives us insight into the general structure of the world and experience. I concentrate my attention upon proto-philosophical activity.²

For Castañeda, any experience or any aspect of reality can be a source of philosophical questions. To illustrate this, he considers the example of a comma on his page.³ The comma is an object (of ink) rather than a property, it has a history and is a subject of change, it is a linguistic token in a sentence of English word tokens. Philosophy begins with the initial empirico-existential assumption that the world contains

certain particular entities. It is an empirical matter that objects in the world have particular structures of individuation, differentiation, causation and predication, rather than different structures, and philosophical theories must be constantly tested against this basic data. This data includes both ordinary daily experience and scientific experience (ibid, pp. 31-32). Yet every experience reveals only a small fragment of the infinitely complex world. To obtain a rational conception of underlying patterns Castañeda suggests that we engage in the following process (ibid, p. 102):

- (1) gather a large collection of data;
- (2) make a careful exegesis of the gathered data so as to secure *points* through which the pattern sought after must pass; such points serve also as a criteria of adequacy for the testing of theories at step (4) below;
- (3) hypothesize the connections between the secured points, connections that taken all together constitute the proposal of a theory of the pattern sought after;
- (4) test the theorized pattern by deduction of the points distilled from the exegized data: this is the *ad hoc* adequacy of the theory;
- (5) test the theorized pattern against new data of different types and sorts: this establishes the fruitfulness of the theory;
- (6) repeat the steps (1)-(5) by embedding the theory in a more comprehensive one.

Kekes (1983, p. 223) has recently pointed out that philosophical theories cannot be compared solely on the basis of their capacity to explain the same data because theories disagree about what counts as data. Data could be multiplied infinitely without some prior constraint upon what data is to be explained. What data are to be explained depends upon theoretical considerations: different data are relevant to different philosophical theories. Keke cites the example of the datum of the existence of genuinely evil acts and natural evil, which is of high significance to Christian theism (which postulates the existence of a perfectly good, omniscient and omnipotent God), but which is commonplace to a Darwinian

ontology. Kekes' observation that philosophical data is not *theory-neutral* prevents Castañeda's method from proceeding from sym-philosophical activity to dia-philosophical activity. It also prevents Castaneda's method from even proceeding from first base, proto-philosophical activity. This is because the criteria of adequacy for philosophical theories, which are formulated from proto-philosophical data, in actual fact belong to stage 4: meta-philosophy. Castañeda's metaphilosophical principles (C.P.1) (Castañeda, 1980, p. 49), (C.P.2)-(C.P.5) (ibid, p.52) and (C.P.6)-(C.P.16) (ibid, pp. 112-114) are imposed upon the data as regulative principles and are in no way formulated on the basis of data.

Castañeda's metaphilosophy is also criticized by Kekes on the ground that he finds no resemblance between what philosophers who have made important contributions to ontology were doing and what Castaneda recommends should be done. This failure lies, in my opinion, in Castañeda's neglect in outlining what is unique about philosophical inquiry: its style of argument.

Philosophy, like any science, is a problem-solving activity controlled by rational criticism; in this respect I am in agreement with the metaphilosophy of Kekes. The source however of philosophical problems is not merely problems of life, it is rather as Castañeda has noted, pregnant in any aspect of our experience. To illustrate this consider a philosophical problem posed by K. Lehrer and R. Taylor (1965). A person, Smith, is presently at the country airport where a plane will depart at 3.30 p.m. for the city. It is now shortly before 3.30 p.m. and Smith has business obligations which require him to get to the city by 4.00 p.m. No alternative form of transport could get Smith to the city to meet the 4.00 p.m. deadline, so if Smith does not take this plane at 3.30 p.m. then he will not meet his deadline. Suppose that although there is nothing to stop Smith leaving on that plane at 3.30 p.m., Smith decides not to go.

Thus the following four statements are all true of Smith:

(10-1) If Smith does not leave at 3.30 p.m., then he cannot arrive at 4.00 p.m.

(10-2) If Smith does leave at 3.30 p.m., then he will arrive at 4.00 p.m.

(10-3) Smith can leave at 3.30 p.m.

(10-4) Smith does not leave at 3.30 p.m.

The set of statements {(10-1), (10-2), (10-3), (10-4)} are despite appearances, inconsistent. For:

(10-2) If Smith does leave at 3.30 p.m., then he will arrive at 4.00 p.m.

and

(10-3) Smith can leave at 3.30 p.m.,

entail

(10-5) Smith can arrive at 4.00 p.m.

However

(10-1) If Smith does not leave at 3.30 p.m., then he cannot arrive at 4.00 p.m.

and

(10-4) Smith does not leave at 3.30 p.m.

entail

(10-6) Smith cannot arrive at 4.00 p.m.

Statements (10-5) and (10-6) seem to be contradictions. We have fallen into a seeming paradox since statements (10-5) and (10-6) both cannot be true, contrary to the views of some paraconsistent logicians (Routley, 1980).

To solve the Lehrer-Taylor problem one or more of premises (10-1)-(10-4) must be faulted and/or it must be shown that one or both of the two entailments does not hold.

The Lehrer-Taylor problem is a standard type of philosophical problem. A paradox, or a strongly counter-intuitive result is obtained from seemingly innocuous premises. If the argument in question is formally valid and the premises were true, then the conclusion is also true. But *if* we have independent reasons for regarding the conclusion as being false, then one or more of the premises must be shown to be false. A *philosophical theory* gives a solution to a philosophical problem. Like a scientific theory, philosophical theories involve a non-demonstrative *reasoning to the best explanation* (Harman, 1973) (Thagard, 1978). A philosopher, after reflection upon a certain problem for some time, obtains rational insight into the nature of the philosophical problem at hand. This "insight" or "intuition" does not itself constitute knowledge, it is nothing more than a hypothesis about the source of the problem at hand and how it may be solved (Passmore, 1966, p. 358). It must be explained how the hypothesis at hand solves the problem and evidence cited in support of the position must be given.⁴ For example, the Lehrer-Taylor problem would be resolved if it was shown that premise (10-2) of the argument was false (although I do not support this solution) and this claim was rigorously defended against all *presently* conceivable non-trivial objections. Further, opposing solutions to this problem must be criticized.

It is instructive to give an example of how a philosophical position can be defended; I choose as an example the position of panpsychism, a position almost universally regarded as absurd by contemporary philosophers. Panpsychism is the thesis that there is a mental aspect to all, or most things that exist. In particular panpsychists have held that matter such as that to be found in non-living particulars such as rocks, has a mental or proto-mental aspect. Thus to defend the thesis of panpsychism it is a sufficient condition to show that at least some mental properties - perception, thought, rationality etc. are possessed by non-living material

objects such as rocks or electrons. As a philosophical thesis attempting to solve the mind-body problem, panpsychism has only a handful of recent defenders (Globus, 1976), (Butler, 1978) and many critics (for a survey of objections, cf. (Edwards, 1967)). Here, I will show that panpsychism, even if it is false, is capable of rational defense in accordance with metaphilosophical position.

One of the best known and most concise attempts to refute the doctrine of *metaphysical realism* (the doctrine that unobserved and imperceptible objects exist) was given by W.T. Stace (1934). How could the metaphysical realist know anything about objects existing unobserved? There are only two ways Stace replied: by observation and perception or by inference. But it is self-contradictory to claim to have observational or perceptual knowledge about unobserved or imperceptible objects. This leaves the metaphysical realist the avenue of inference. Inference is either enumerative inductive or deductive. To know that unobserved objects exist by induction requires a solution to what is essentially Hume's unsolved problem of induction, so this avenue is blocked, Stace believes. However we cannot know that unobserved or imperceptible objects exist by deductive means because it is not self-contradictory to suppose that unobserved and imperceptible objects do not exist. Hence we have no knowledge of unobserved and imperceptible objects, and although Stace does not draw this conclusion, we can conclude that there is no good reason to postulate the existence of those mysterious unparsimonious entities which the metaphysical realist entertains as being existents.

The weakness in Stace's argument is that there are only two modes of inference: (enumerative) induction and deduction. But there are other modes of inference, such as explanatory inference, or reasoning to the best explanation. The metaphysical realist would point out that metaphysical realism offers a much better metaphysical framework for the

understanding of the success of science; indeed it is the one framework where the success of science is not an outright miracle - or so the argument goes. Whether this is so or not requires independent argument.

Berkeley himself had hoped to avoid this implication of the position of subjective idealism by the postulation of a universal perceiver, God. But there is another avenue which might be taken, which involves the subjective idealist becoming a panpsychist. This is as follows: *postulate that the so-called unobserved and imperceptible objects of the metaphysical realist have observational and perceptual capacities.* This is precisely what the panpsychist claims. There is thus no need to postulate the existence of a Berkeleyian God, and no need to become a metaphysical realist to explain the success of science. Electrons are postulated by our new panpsychist to have perceptual capacities and exist solely by virtue of this property (through apperception), in addition to all the other properties which physics says that they have. Further, electrons exist independently of human minds, but they do not exist independently of perceivers. So on the one hand the subjective idealist can hold to the doctrine that *to be is to be perceived*, whilst on the other, not having to reduce the fact of the success of science to the level of cosmic coincidence.

This sketch of a defense of a doctrine which most philosophers regard as absurd, stands as a good test case for my metaphilosophical position. The position developed here is in any case nothing more than the style of argument employed throughout both this work and others, so that this work and its implicit metaphilosophy stand or fall together. Philosophy as so far described, seems to be no more than a very general science. It is however in philosophers' methods for criticizing arguments and philosophical theories that philosophy can be methodologically distinguished from the natural and social sciences. These are single conclusion arguments,

primarily designed to show the inadequacy of a philosophical position (Passmore, 1966, 1970). In the sections to follow I will discuss: (1) self-referential arguments, (2) arguments to inconsistencies, (3) the method of counterexamples and (4) *Petitio principii*, vicious infinite regresses and circularities, as examples of such critical arguments. This exposition should serve as a "reminder" (cf. (Passmore, 1970, pp. 8-13) to the metaphilosophical sceptic that philosophical inquiry is far from an empty procedure.

3. SELF-REFERENTIAL ARGUMENTS

J.M. Boyle (1969) in his Ph.D thesis on the argument from self-referential consistency, takes such arguments to be "uniquely philosophical" because of "the need of philosophy to include itself in its subject matter" (ibid, p. 1). Further, philosophers "have claimed that this type of argument can be used to terminate philosophical disagreements and that it may be used to establish certain metaphysical claims" (ibid, p. 2). Grisez (1975) uses this argument quite extensively in developing his philosophical theology, in criticizing empiricism (ibid, pp. 114-121), Kant's epistemology (ibid, pp. 152-180) and Hegelian absolute idealism (ibid, pp. 195-204). Boyle, Grisez and Tollefsen (1976) have used a self-referential argument to defend "freedom of will"; I have criticized this position elsewhere (Smith, 1982(d)). Self-referential arguments have also been used quite recently in attacks upon the strong programme of the sociology of knowledge; I have also criticized this position (Smith, 1983(b)). I have however made extensive use of this argument in this work; first in attacking metaphilosophical relativism, and second in the previous chapter in demonstrating that philosophical progress occurs. This method of argumentation requires both exposition and defense.⁵

Following Boyle, Grisez and Tollefsen (1976, p. 125) I take a statement to be self-referential "if and only if the proposition which is affirmed refers to some aspect of the statement - that is, either to the sentence, or to the performance of affirming or uttering, or to the proposition itself". Self-referential arguments for Boyle, Grisez and Tollefsen, show that certain statements are *performatively false*, the inconsistency arising between a statement and some aspects of its utterance or usage. Such arguments produce necessary statements which have a distinctive necessity which is neither physical necessity nor logical necessity (Grisez, 1975, p. 113). This view, as I have argued elsewhere is not sufficiently strong enough to refute doctrines such as hard determinism (Smith, 1982(d)). This type of argument has not been employed in this work.

A stronger form of self-referential argument is absolute self-refutation. A proposition p is absolutely self-refuting if and only if p entails that p is false. Thus if p is true, it is false, and if p is false, then it is false (Passmore, 1970, p. 60), (Mackie, 1964). The statement 'There are no true statements' is necessarily false because if it is assumed to be true, it implies that it is false and if it is assumed to be false, then it is false. The statement 'There are some truths' is necessarily true. Absolute self-refutation is a special form of philosophical argument which enables us to show that certain positions refute themselves, and that the contrary positions are necessary truths. Since the statements describing these contrary positions need not refer to logical or mathematical data, contrary to Mackie (*ibid*, p. 203) we do present a problem to empiricism. It seems that there are at least some necessarily true statements which are not only synthetic, but *a priori*.

Statements which obtain their truth-value by means of absolute self-refutation must be distinguished from semantically paradoxical statements.

This distinction enables us to reject the view of Russell and Whitehead in *Principia Mathematica* (1927, vol. 1, pp. 37-38, 60-65) that all self-referential statements are nonsensical since they violate the "vicious circle principle". The Liar sentence 'This very statement is false' is paradoxical because the sentence entails its truth on the assumption of its falsity, and its falsity on the assumption of its truth. It is easy to show in classical logic that the Liar sentence is both true and false, that is paradoxical. But a statement which is necessarily false is not paradoxical. To ban all self-reference to solve the semantical paradoxes is illegitimate: it is too extensive a reform which does not precisely diagnose why some self-referential statements lead to paradox, whilst others do not (Priest, 1979).

It is concluded that there are no good arguments for the illegitimacy of the use of self-referential arguments in philosophy. Consequently the use of such arguments, made in support of crucial theses in the present work is justified, and the conclusions of such arguments are objects of philosophical knowledge.

4. ARGUMENTS TO INCONSISTENCIES

Another method of philosophical argumentation, which supplies a virtually conclusive refutation of an opponent's philosophical theory is to demonstrate that the theory is inconsistent. The problem with an inconsistent theory is, as I have argued elsewhere (Smith, 198+(f)), that it contains a manifest falsehood. There are no good reasons for supposing that there are "true contradictions" (Smith, 198+(f)). Hence the paraconsistent logician's strategy to undermine the famous Lewis argument for *Ex Falso Quodlibet* fails: $p \ \& \ \sim p \rightarrow q$ and $(\sim p \vee q) \ \& \ p \rightarrow q$ cannot presently be regarded as countermodelled.⁶ In any case, even though disjunctive syllogism is rejected by both relevant and

paraconsistent logicians, as a *universally* valid rule of inference, it is upheld as satisfactory in "normal reasoning situations (Mortensen, 1983). Normal reasoning situations are situations where we do not encounter interpretations such that $V|p| = \text{True}$ and $V|\neg p| = \text{True}$; that is, we do not encounter true contradictions. Thus consider a "normal reasoning" situation where $V|p| \neq V|\neg p|$ (unless $V|p| = \text{False}$). In such a situation disjunctive syllogism is valid. But now the Lewis argument goes through: in normal reasoning situations not involving true contradictions, the principle of *Ex Falso Quodlibet* holds. The paraconsistent logician may be able to show that not any arbitrary statement is implied by a *true* contradiction, but he cannot show by countermodelling techniques that *Ex Falso Quodlibet* fails for $V|p| \neq V|\neg p|$ (unless $V|p| = \text{False}$). We have good reason then to be fearful of contradictions - a theory containing a contradiction is either false or trivial.⁷

To illustrate the use of the argument to inconsistencies, I will once more outline a very radical thesis which I shall defend: from the perspective of classical logic, formal elementary number theory is inconsistent. By the expression 'formal elementary number theory' I understand a formal theory such as \bar{N} given in Kleene's *Mathematical Logic* (1967), having as its underlying logic, classical logic. Let us argue that if a natural number is even (symbolized as 'Ex'), then it is not odd (symbolized as 'Ox'). Hence the following proposition is true:

$$(10-7) \quad (\forall x) (Ex \rightarrow \neg Ox).$$

But we may also symbolize the statement 'if a natural number is even then it is not odd' as:

$$(10-8) \quad (\forall x) \neg (Ex \rightarrow Ox).$$

Both of statements (10-7) and (10-8) are correct formalizations of the natural language statement 'if a natural number is even then it is not

odd', and no contradiction can be produced by their mutual affirmation.

Consider now the following argument:

- (10-8) $(\forall x)\neg(\text{Ex} \rightarrow \text{Ox})$ Mathematical Truth
 (10-9) $(\forall x)\neg(\neg\text{Ex} \vee \text{Ox})$ (10-8), Transformation of the Conditional
 (10-10) $(\forall x)(\text{Ex} \ \& \ \neg\text{Ox})$ (10-9), Duality Law

Proposition (10-10) is the claim that all natural numbers are even and no natural number is odd; it implies that all natural numbers are even. We can readily demonstrate that all natural numbers are odd. But this is impossible if \bar{N} is consistent. It is the most certain and elementary result of elementary number theory that there is at least one odd number, i.e. $(\exists x)\text{Ox}$, and there is at least one even number, i.e. $(\exists x)\text{Ex}$. The number 3 for example is odd, whilst the number 4 is even, as we would all agree. Yet if we accept the above argument, we must be led to deny such facts.

Perhaps the most reasonable response to make to this argument is to deny that (10-8) is a "correct" formalization of the statement 'if a natural number is even then it is not odd', and to claim that in the light of my paradoxical conclusion, that (10-7) must be the correct formalization. This counter-argument however commits a *petitio principii*. The production of a paradox is not a good reason for rejecting (10-8) as a formalization of a natural language statement. The paradox may be the result of the inconsistency of number theory. The critic presupposes that elementary number theory is consistent: *if* we accept Gödel's second theorem, we have no logical guarantee that this is the case, as the formula *Consis* expressing in \bar{N} the consistency of \bar{N} , is unprovable in \bar{N} . In any case, proposition (10-8) is an accurate formalization of the natural language statement 'it is not the case that if a natural number is even then it is odd'. The burden of disproof is upon my critics.

Thus we must conclude that from the perspective of classical logic, formal elementary number theory is inconsistent. This inconsistency is not merely local phenomenon, since we could interpret 'Ex' to mean 'the natural number x is prime' and 'Ox' to mean 'the natural number x is composite'. Consequently every mathematical problem is "solved": Fermat's theorem is both true and false! The price which we pay for this however is the destruction of mathematics as a cognitive enterprise. Surely then the paradox must be resolved by admitting that the classical propositional and predicate calculi are unsound. If we do not reject standard soundness proofs, then we must accept that the metalanguage of these classical calculi are inconsistent.

I cite another example of the argument to inconsistencies. Patrick Grim in his paper "There Is No Set Of All Truths" (1984) offers a proof that there is no set of all truths. Grim's argument is simply this: the assumption that there is a set of all truths conflicts with Cantor's power set theorem, *therefore* there is no set of all truths. Grim uses this result to undermine a popular approach to possible world semantics, which takes possible worlds as proposition-saturated sets. Here I shall criticize Grim's argument that there is no set of all truths, undermining in the process his criticism of possible worlds viewed as proposition-saturated sets and establishing the inconsistency of standard set theory.

Grim *assumes* that because the set of all truths has a greater cardinality than its power set, that such a set cannot *exist*; he gives no argument against the existence of the set of all truths apart from showing that the assumption of the existence of this set is precluded by the power set theorem. He does not show that the set of all truths is an inconsistent set as the Russell set has been shown to be. The Russell set, the set of all sets which are not elements of themselves can be shown in a naive set theory supplemented by classical logic to be an element of

itself if and only if it is not an element of itself. In the axiom of abstraction: $(\exists y)(\forall x)(x \in y \leftrightarrow \phi(x))$ (where 'y' is not free), consider $\phi(x) = \sim(x \in x)$, which gives us as an instance of the axiom of abstraction: $(\exists y)(\forall x)(x \in y \leftrightarrow \sim(x \in x))$. Let $x = y$ to infer $y \in y \leftrightarrow \sim(y \in y)$, from which $y \in y \ \& \ \sim(y \in y)$ is provable by means of classical logic. Thus the Russell set is a 'contradictory set' or 'inconsistent object' to use the terms of paraconsistent logicians. The set of all truths is not *itself* an inconsistent object: it is problematic only because its cardinality violates the dictates of the power set theorem.

Why then *must* we conclude that there is no set of all truths? Why not accept that there is such a set and that the power set theorem is false? If this was so then set theory, both naive and axiomatic, would be shown to be inconsistent, for the power set theorem, a basic theorem of any mathematically useful set theory, is both provable on the basis of the axioms of standard set theory, and refutable by a counter-example.

It may be thought that the set of all truths is a set which offends against the vicious circle principle. However this principle cannot exclude the set of all truths by any non-arbitrary means. Unlike the Russell set, the elements of the set of all truths are non-set theoretical entities. If set theory must be so severely regimented that restrictions must be placed on what sorts of non-set theoretical entities can be members of sets, then set theory has a limited application in philosophy and science.

Some readers will reject such set-theoretical nihilism immediately: the price to pay for retaining the set of all truths is high even by British standards of inflation. Indeed it is but this is hardly a good philosophical argument against the alternative interpretation of Grim's result given above. My critics may be right in recognizing my disrespect

for matters of economy, but in doing so they do not show that my set-theoretical nihilism is untenable. The set of all truths is not an inconsistent set and its guilt must not be assumed without argument. But what argument have we against the claim of the existence of the set of all truths? Only Grim's argument, but to cite this against me is to commit a *petitio principii*, for the issue which we now debate concerns the *correct* interpretation of Grim's result and my critic presupposes without argument the correctness of *one* view of Grim's result. There seems to me to be no non-ad hoc or non-question begging argument against the claim that there is a set of all truths and thus I accept that naive and axiomatic set theories which have the power set theorem as one of their theorems, are inconsistent.

These are certainly very important results if correct (and like all such controversial results, they may be simply the result of a fallacy of reason). If classical logic was *the correct* logic, then mathematics would be refuted! But if not, then classical logic is unsatisfactory. Also reason has been given to believe that standard set theory is inconsistent. These examples illustrate the power of the argument to inconsistencies. Further, the method provides a definitive refutation of a position. If the arguments are sound, then the criticized positions, if not outrightly trivial, are certainly false. There is little need to further document the use of this method in philosophy: virtually every philosophical work has attempted to show that opposing positions are contradictory, or that some philosopher is inconsistent. If such arguments are taken to be a vital part of philosophy, and I have given first hand arguments in this work that they are, then philosophical knowledge exists. Therefore philosophical knowledge exists.

Does it follow that because there are legitimate philosophical methods, there must therefore be philosophical knowledge? Of course this

conclusion does not follow, and it is not in any case my position. What I have outlined in this work is a dialectical mode of argumentation which uses error elimination to reach philosophical truth. Unlike scientific questions there are usually only a limited number of answers to a philosophical problem: we are either free, determined or both are compatible; we either have knowledge or we do not; motion either exists or it does not as Zeno believes. The importance of certain philosophical methods is that they give a decisive refutation of alternative positions *if* it can be shown that these methods have been correctly applied. This requirement does not in itself show that none of the alternative solutions to a philosophical problem can be eliminated. On the contrary it may well be easier to establish the legitimacy of the use of some argument, such as the argument from self-referential consistency or the argument to inconsistency, then it would be to give first hand argument for a position. If either position p or $\sim p$ is true, as they are suitably exhaustive of cognitive alternatives, and p is self-referentially inconsistent, then $\sim p$ is acceptable as being true.

5. THE METHOD OF COUNTER-EXAMPLES

The method of counter-examples to demonstrate a flaw in a thesis is one of the most widespread, yet infrequently discussed methods in metaphilosophy. Peter Facione (1976) has however given an accurate description of this method. Professor McBrain wishes to explicate the notion of "being an F" and proposes the following explication:

$$(10-11) \quad (\forall x) (Fx \equiv (Ax \ \& \ Bx \ \& \ Cx))$$

Professor Discard however argues that (12-11) is faulty and should be rejected, in one of two ways. First he/she argues that:

(10-12) $(\exists x)(Fx \ \& \ (\sim Ax \vee \sim Bx \vee \sim Cx))$

is true, so that (10-11) is too strong in that it rules out genuine F's.

Alternatively Professor Discard argues that:

(10-13) $(\exists x)(Ax \ \& \ Bx \ \& \ Cx \ \& \ \sim Fx)$

is true, so that (10-11) is too weak, allowing things to be F's which are not F's.

Facione and Schlesinger (1983) view the method of counter-examples as providing anything but a philosophical "knock down" argument. After all, when an alleged counter-example is supplied to a position, either (a) the position is shown to be untenable, (b) the position is reformulated to avoid the counter-example, or (c) it is argued that the alleged counter-example does not really refute the position in question - which is anything but a single conclusion. However, upon closer reflection we see that two of these alternatives can be dismissed. As Facione (1976, p. 524) recognizes, it is usually very difficult to show that the alleged counter-examples are not genuine - as the extensive literature surrounding the Gettier problem illustrates (Pappas and Swain (eds.), 1978). There can be no guard against mistakes in philosophy. Thus possibility (c) can be dismissed. This leaves possibilities (a) and (b), and I need only deal with (b). Now any position can be saved from refutation by refinements; this is willingly granted. Nevertheless, the mere fact that a refinement is necessary indicates that the original position is untenable and has been abandoned. If the new position is not to fall prey to the same counter-example, then at least it cannot be strictly identical to the old position. This is to change the problem. Thus to provide a genuine counter-example to a position is to provide a definitive refutation of it.

Facione recognizes that the process of counter-example and re-explication may reach an impasse. The disagreement may shift its focus to the question of the adequacy of the two protagonists' own conceptions of "being an F". This may have the following result (Facione, 1976, p. 529):

When the issue becomes . . . whether the proposed counter-examples are "genuine" or not, then it is fair to say that one philosopher's idea about what is being explicated is just not the same as the other philosopher's idea. A fundamental intuitive disagreement separates the two philosophers. It is the kind of disagreement that counter-examples cannot resolve for it is a disagreement that has resulted from offering counterexamples and disputing their authenticity. Each new counterexample, at this level, unless it is accompanied by some fundamental philosophical attack . . . must, if it is not deliberately tailored to suit the other protagonists apparent conception of what "being an F" means, seem irrelevant at worst and question-begging at best.

Meyn (1977, p. 46) argues that Facione's argument leads to a *reductio ad absurdum* of analytic philosophy. The method of counter-examples is the most popular method of criticism of analytic philosophy, and since it fails to resolve disputes involving fundamental intuitive disagreements, the method itself is bankrupt.

Meyn's attack upon the method of counter-examples is quite misguided. We can agree that this method will fail in a grave situation of fundamental intuitive disagreement. It does not follow from this that the method cannot succeed where there is at least some common ground between protagonists. But suppose that there is no common conceptual ground between protagonists, that the situation is of radical incommensurability. If this is so, then the assumption that there is actually a disagreement between the two parties becomes problematic. If there is no cognitively significant dispute to dissolve, then Meyn's criticism of the method of counter-examples collapses. In either case then, Meyn's criticism is unfounded.

The method of counter-examples is thus defended against recent criticisms. Whether this method has or has not "always produced happy results" (Schlesinger, 1983, p. 123) is of no interest for discussion here. No doubt there have been, and will be many errors made in the use of this method. This does nothing to establish that the method is not of significant and widespread use, and Schlesinger certainly does not view this method with the same scepticism which Meyn views it. To produce then a genuine counter-example to a position is to provide a definitive refutation of it and hence to provide philosophical knowledge.

6. PETITIO PRINCIPII, VICIOUS INFINITE REGRESSES AND CIRCULARITIES

I will consider now three methods of criticism of philosophical positions which if soundly used provide a definitive and virtually knock down refutation of a position: *petitio principii*, vicious infinite regression and circularities. These three methods of critique are united in the famous "criterion argument" or the *diallelus*. The classical formulation of the argument given by Sextus Empiricus in his *Outlines of Pyrrhonism* (1939, pp. 163-165) is as follows:

. . . in order to decide the dispute which has arisen about the criterion, we must possess an accepted criterion by which we shall be able to judge the dispute; and in order to possess an accepted criterion, the dispute about the criterion must first be decided. And when the argument thus reduces itself to a form of circular reasoning the discovery of the criterion becomes impracticable, since we do not allow them to adopt a criterion by assumption, while if they offer to judge the criterion by a criterion we force them into a regress ad infinitum. And furthermore, since demonstration requires a demonstrated criterion, whilst the criterion requires an approved demonstration, they are forced into circular reasoning.

The problem of the criterion is a recurrent problem in the philosophical work of Nicholas Rescher (1973(a), (b); 1977(b); 1979(a); 1980(a), (b)). Perhaps the clearest formulation of the problem of the criterion in

Rescher's work occurs in his *The Coherence Theory of Truth* (1973(b), pp. 12-17). A criterion of truth is of the form: whenever a proposition p meets requirement R , then p is true:

$$(C) \quad (\forall p)(R(p) \rightarrow T(p)).$$

To establish the truth of p is to give a deductively sound argument: $C \ \& \ R(p) \rightarrow T(p)$. Now if this argument is sound, then it must be both formally valid, and have true premises. Consequently, if $C \ \& \ R(p)$ is to be established as true, we must establish $T(C)$. To give a deductively sound argument for $T(C)$ by taking C to be self-applicable, is to give an argument of the form: $C \ \& \ R(C) \rightarrow T(C)$. To establish $T(C)$, it is necessary that the truth of C is established, i.e. $T(C)$, and we thereby fall into vicious circularity. If C is not self-applicable, then $T(C)$ is established by use of another criterion C_1 by an argument $C_1 \ \& \ R_1(C) \rightarrow T(C)$. For this argument to be accepted, it must be established to be deductively sound. To do this, we must establish that $T(C_1)$. To appeal to another criterion C_2 leads us into an infinite regress.⁸

There is a considerable literature diagnosing the problem with infinite regresses, especially distinguishing virtuous from vicious infinite regresses (Nathan, 1977), (Foley, 1978), (Post, 1980). Authors seem to be universally united in believing that vicious infinite regresses are problematic in some way dependent upon the infinite series of acts that such a regress allegedly commits one to.⁹ This however is not so (Smith, 1982(b)). Consider for example the infinite regress of reasons (Deutscher, 1973): no proposition is justified unless justified by another justified proposition. Suppose we cited P_2 to justify P_1 , then P_1 is not justified, since P_2 is not justified. If we attempt to justify P_2 by P_3 then nothing is achieved unless P_3 is justified. But to justify P_3 only repeats our problem. Any step in the series of the regress leaves us

equally as badly off as the initial problem is not solved, it is merely restated. The problem raised by an allegedly vicious "infinite" regress is an epistemological problem, the problem of failure of explanation.

Regress arguments are often confused with circularity arguments, and circularity arguments in turn with *petitio principii*. Circularity in an argument is an epistemic fallacy; the *petitio principii* is a dialectical fallacy, arising in contentious debate. In both cases the argument advanced may be semantically and logically valid.

I follow Hamblin (1970, p. 73) in taking *petitio principii* to be a fallacy arising in debate when one participant asks the other, or simply takes it for granted, that a premise will be mutually accepted but which contains the substance of what is in dispute. To commit a *petitio principii* is to fail in some way to meet the burden of proof, *onus probandi*. *Onus* - assignment does not depend upon the acceptance of certain onus - assigning propositions, because there are none (Brown, 1970). The analysis of *onus probandi* given by Rescher (1977(a)) is accepted here. There are two distinct but related conceptions of *onus probandi*. The first, the probative burden of an initiating assertion, is the basic rule that whichever side initiates the assertion of a thesis has the burden of proof of supporting it in argument. The second, the evidential burden of proof of further reply in the face of contrary considerations, is the basic rule that when strong arguments have been given in support of a thesis, the thesis stands until a sufficiently strong rebuttal has been given to it. The burden of *agenti incumbit probatio* remains constant throughout the argument, whilst the evidential burden of proof may shift from side to side as the debate progresses.

Johnson (1968) illustrates the importance of the *petitio principii* argument in philosophy.¹⁰ Many philosophical positions are question-

begging; empiricism with its verificationist theory of meaning begs the question against alternative positions which reject this theory of meaning (ibid, p. 138). Johnson believes that a *prima facie* plausible case can be made that *all* philosophical arguments are question-begging (excluding those concerned simply with logical implications). This is so because frequently philosophers accept certain principles as ultimate principles without in anyway arguing *for* them rather than *from* them. He comments on this (ibid, p. 135):

Unless one can demonstrate that some arguments are not question-begging, philosophy, as a discipline leading to knowledge, becomes impossible. But the damage does not end there. For failure to solve this problem precludes finally the possibility of any knowledge whatsoever. Although contemporary epistemologists have devoted a good deal of attention to the issue, the results they have achieved have not been notably successful. In fact most of their theories seem to fall into the very trap they have been designed to avoid; somewhere they beg a vital question. Is this because the trap is unavoidable? Or is it because the philosophers in question have failed to find a way of avoiding it?

This question will be addressed in section 7 below; but let us note here that Johnson's problem establishes the undeniable importance of *petitio principii* to philosophy. As a method of criticism it is as Johnson recognizes one of the most successful ways to win a debate: "If you are sufficiently skilful in asking him leading questions and have a good sense of timing you can usually succeed in stripping him to his bare principles, with no ascertainable means for their support." (ibid, p. 135).

The classical notion of a circular argument depends upon two conditions: the *dependency condition* and the *equivalence condition* (Woods and Walton, 1975). An argument is circular if either some premise actually depends on the conclusion as part of the evidential backing of the premise-conjunct or where the conclusion is equivalent to, or identical to, some premise-conjunct.¹¹ A more detailed breakdown of these conditions is paraphrased as follows (ibid, p. 109):

(a) Dependency Conditions

- (CD) The conclusion entails some premise-conjunct.
- (CDE) In order to know that some premise-conjunct is true, a must know that the conclusion is true.
- (CM) There is some premise-conjunct that can be known to be true only by inference from the conclusion.

(b) Equivalence Conditions

- (CQ) The conclusion is equivalent to some premise-conjunct.
- (CI) The conclusion is identical to some premise-conjunct.
- (CQE) To know that a premise-conjunct is true is to know that the conclusion is true, and vice versa.
- (CP) One has to state the conclusion in order to state some premise-conjunct, and vice versa.

The classical account of circularity, making use of the equivalence condition is taken by Woods and Walton to be too wide a criterion, attributing circularity to single-premised arguments such as:

$(\forall x)(Rx \rightarrow Ax)$, $\therefore \sim(\exists x)(Rx \ \& \ \sim Ax)$, and certainly to A , $\therefore A$. I do not view this as a problem. Suppose someone questioned the conclusions A or $\sim(\exists x)(Rx \ \& \ \sim Ax)$, believing them to be false. Citing the premises A and $(\forall x)(Rx \rightarrow Ax)$ would do nothing to advance the debate, because if the critic is correct, then the premises are false. But the argument P , $P \rightarrow Q$, $\therefore Q$ is not a circular argument if valid. Accepting the validity of *modus ponens*, to establish soundness all one need do is to establish the truth of P and $P \rightarrow Q$. If the critic agreed that *modus ponens* was valid, that P was true and $P \rightarrow Q$ was true, but that Q was *false*, then we would have at hand a semantic paradox more challenging than any yet explored. We would not however have a circular argument.

I conclude that the classical account of circular arguments survives Woods and Walton's criticism. Before discussing an example of the use of the three arguments discussed in this section, it remains to point out why

regress, circularity arguments and *petitio principii* supply virtually conclusive refutations of a position, at some stage of a debate. The reason simply is that a position which is circular, regressive, or question-begging fails to convince ideally rational opponents, or rational but neutral truth-seekers. An argument is only epistemically satisfactory, if it is formally sound and has persuasive force. The discussed fallacies all show that the convicted argument, if it is circular, regressive or commits the *petitio principii* gives an ideally rational thinker no reason at all to accept the position which the argument is advanced to defend. No increase in knowledge occurs and debate is not progressed.

7. AN OBJECTION: RATIONALITY SCEPTICISM

As Johnson (1968, p. 135) recognizes, the problem of the criterion or the *diallelus* raises grave problems for the claim that there is philosophical knowledge, and this difficult problem can hardly be passed by unaddressed. A more detailed discussion of this problem has been given elsewhere (Smith, 1982(a); 198+(c)) so I shall canvass my solution without an exposition and criticism of opposing positions.

I interpret the criterion argument to show that there can be no presuppositionless knowledge, that our cognitive inquiries must begin on the one hand with both instances of knowledge as well as criteria of knowledge, to avoid the vicious chicken-and-egg regress which the criterion problem seems to present us with. But on the other hand, I see no way in which we can exclude the possibility that all of the instances of our knowledge are systematic delusions, and that our criteria and principles of knowledge self-destruct in mutual inconsistency. For the sceptic to conclude that no knowledge is possible unless the rationalist meet this impossible burden of proof, is a *petitio principii*. There is an escape

from the *diallelus* which has seldom been taken; this is to deny that if justification stops, it must stop at an arbitrary point which begs the question against the sceptic.

Suppose in a debate against the sceptic one is pushed back to some ultimate principle which one claims is self-justifying, or self-evident. The sceptic denies this, stating that this defense is arbitrary and a *petitio principii* since no principle is self-evident to him/her. What is wrong with being arbitrary at the *last* point of the debate? The problem seems to be that arbitrariness leads to *triviality*. If knowledge inquiries could begin just as well with counter-deductive criteria and opposing knowledge claims, then *any* proposition could be justified. The rationalist cannot accept this state of affairs, the sceptic only too willingly points out, because if any proposition is justified, then so is the proposition 'no proposition is justified'. Apart from being absolutely inconsistent, the rationalist is seen to be a sceptic after all.

Scepticism would be refuted if it was shown that at least one cognitive principle was not arbitrary. The sceptics' objections to rationalism, voiced in the paragraph gives us one principle which is not arbitrary: what Putnam (1979) calls the minimal principle of non-contradiction, not every proposition is true. The *presupposition* of the truth of the minimal principle of non-contradiction is *a priori* (Thompson, 1981, p. 463). Consider the sceptic's claim that this principle is arbitrary. Then the counter-deductive principle of triviality: 'every proposition is true', is just as satisfactory. Then because the proposition 'every proposition is true' is *ex hypothesi* true, it follows that the proposition 'not every proposition is true' is true. If this is so, then the entire motivation for the counter-deductive argument collapses, because we have reaffirmed the very principle which we sought to criticize. There are of course many other principles of rationality (Nielson, 1974), (Kekes, 1976). They too

are justified by demonstrating that they are presuppositions of theoretical practice, necessary conditions for the processes of explaining, theorizing and criticizing to occur. Reject these principles and one is led into silence. Silence requires no refutation.

Does this style of argument beg the question against scepticism? I do not believe that this is so. The claim being made is that there are non-arbitrary cessation points in justification, not that there could not be any sound *reductio ad absurdum* argument which demonstrated the incoherence of even our best theory of rationality. The evidential burden of proof is upon the sceptic to show otherwise, and to ceaselessly demand justification for knowledge claims, not only makes scepticism an epistemically superficial position: it begs the question. Hence rationality scepticism does not refute my thesis that philosophical knowledge exists (Smith, 198+(c)).

8. METAPHILOSOPHY AND MONISTIC-SYSTEMIC PERSPECTIVISM

It remains to state how the theory of philosophical rationality, sketched in a tentative form in this chapter coheres with the position of monistic-systemic perspectivism discussed in *Reductionism and Cultural Being* (Smith, 1984). Now it seems that a major inconsistency exists in the present work. I have argued that virtually all of the leading approaches to the problem of perennial disputes are inadequate. This seems to be anything but the process of the combination and synthesis of different points of view into a unified whole.

This criticism fails to meet the mark. (MSP) does not involve recognizing that every position contains some significant truths; it is quite consistent with the fact that virtually all the leading approaches to the problem of perennial philosophical disputes are inadequate. The solution which I have given to this problem, that consensus states are not

necessarily "natural states", and that philosophical discord is to be accounted for by a multiplicity of causes does seem to be quite consistent with my previous writings. This work thus illustrates the (MSP) response to (PPPD).

In this chapter I have argued that philosophy is a rational cognitive enterprise because philosophical knowledge exists, and philosophical methods exist by which knowledge may be obtained. One of the most salient features of this discussion is the *incompleteness* of the theory of rationality sketched here. I have done no more here than scratch the surface of a very fertile terrain, but even this surface soil has proved surprisingly rich. Yet each of the methods of philosophical argumentation discussed here requires book length investigation. This is recommended as a research programme. No mention has been made of philosophical arguments making use of parameters of systemicity such as wholeness, consonance, architectonicity, functional unity, regularity and simplicity (Rescher, 1979(a)). This is yet another research topic.

It is also worthwhile at this point to reconsider in more detail how the master science view of philosophy is capable of providing answers to evaluative philosophical questions, and how this point of view can be correctly seen to be compatible with the "socio-political-ethical" account of human beings which I believe to be necessary to any adequate understanding of human nature. How can my position suggest answers to questions such as 'what shall we do?', 'what is the right and just way to live?' and 'what is the nature of reality?'. Note that it is unreasonable to expect a metaphilosophy to generate mechanically solutions to specific philosophical problems; unreasonable because any such *general* theory cannot deal adequately with details peculiar to a particular philosophical problem. Nevertheless a metaphilosophy can give a general

methodology which may be of great use in solving particular philosophical problems.

I believe that philosophy does, and should, provide answers to philosophical problems in the following fashion. In philosophy, unlike the sciences, there *usually* exists a finite number of answers to any specific philosophical problem. By contrast, for all we know, there may be an indefinitely large number of conceivable theories of gravitation. The nature of the question 'what is gravity?' by no means suggests any limits to the number of theories of gravity which could be outlined. In the case of a philosophical problem, such as the criterion of truth problem examined in chapter 1, we are not faced with an indefinite series of options. Either the sceptic is right - in which case no standard of truth, rationality or knowledge is justified - or the sceptic is wrong. These are the only two *real* options. Faced then with two answers to a philosophical question, *both of which have intuitive plausibility*, one then attempts to examine arguments for and against each position according to the dialectical model of argumentation outlined in chapter 2. The correct philosophical position is the one which answers satisfactorily all serious objections to it, as well as advancing telling objections to an opposing position. Thus the dogmatist in our previous example must show some defect in the sceptic's criterion argument and show how his/her own epistemology avoids this problem. If this is done, then until further criticism is given, we have good reason to believe that the advanced philosophical theory is *true*. This state of affairs is consistent with us rejecting this theory in the light of further argument: all that this means is that we were mistaken in taking that philosophical theory to be true.

This metaphilosophical position is advanced as both a description of what actually occurs in philosophical practice, as well as a prescription,

of what should occur in philosophical practice. Virtually any philosophical article or book which you may select at random, if it is concerned with arguing for a thesis, begins by criticizing alternatives to the author's position, eliminating them, and then giving reasons for accepting the author's position which remains as the only position surviving criticism. This is not a "bleak view of philosophy", but is what I think a social scientist with no "philosophical axe to grind" would see when looking at philosophical practice.

I also see no reason to give evaluative philosophical questions such as 'does life have a meaning' any special treatment that any other philosophical problem does not receive. Again to answer the question 'does life have a meaning?' we need to consider arguments advanced by the rational pessimist seeking to show that life has no meaning at all. These arguments are tested for soundness, and if found to be unsound may be rejected. Given a comprehensive survey of all known and serious arguments seeking to show that life has no meaning, we can conclude from a successful critical survey that *life has a meaning*. This is a *positive* philosophical proposition, established by my proposed metaphilosophical methodology. We do not of course establish what precisely this meaning is, but nor should we hope to be able to do this so simply. That would require further argument. So one would be mistaken in claiming that my method of philosophical criticism is basically "negative": to do so would be to forget that if $\sim p$ is shown to be false, then given the truth of $p \vee \sim p$, p must be true.

My view of philosophical progress must be distinguished from a position which sees philosophical progress not as the acquisition of new knowledge, but as the elimination of mistakes. It could be said that no cognitive activity "progresses" by eliminating poor answers (or errors or

false conjectures) and since this is what I am saying, my view is defective and empty. In saying this one seems to forget the existence of the metascience of Karl Popper which does in fact claim (stated roughly) that the basic elements of the scientific process are bold conjectures and refutations. This view, if false, is far from empty. But my position must be distinguished from Popper's. I have sketched a dialectical model of argument in chapter 2 and have said above how the elimination of errors can lead to philosophical knowledge, not merely "negative" statements. If we can show that epistemological scepticism for example is self-contradictory and that either epistemological scepticism or dogmatism are true as they exhaust all the real cognitive alternatives, then we do in fact *know* that dogmatism is *true*. Hence we have philosophical knowledge, and thus philosophical progress occurs. The situation in philosophy is quite unlike the situation in science. Progress in science cannot occur merely by the elimination of errors: it is always possible that the existing theory is wrong and that some undiscovered alternative is right. In philosophy this is fortunately not always possible. For example we either have knowledge or we do not. The alternatives are limited.

Finally the material in this chapter has a "rationalistic" bias. Obviously (MSP) must in addition accept principles of rationality and empirical methods of argument exploited by the empiricist tradition. In metaphilosophy research in this area has been done by Pepper (1961) and Lee (1983), and I am sympathetic with their position. It is not necessary to present a more complete theory of rationality in philosophy to support (PT); "scepticism" denies that philosophy is a rational enterprise at all, and scientism denies that philosophy is a *sui generis* enterprise with methods distinct from the empirical sciences. Here I have opposed both of these positions, arguing that philosophical knowledge exists, and philosophical methods exist by which further knowledge may be obtained.

9. CONCLUSION: STATE OF THE ARGUMENT

This chapter completes the argument of this work. On the basis of these arguments I present my response to the problem of perennial philosophical disagreements. I have given reason to believe that philosophy is both a progressive and a rational cognitive enterprise. Thus I conclude that philosophy is not a degenerating research programme. If the arguments of this chapter and that of my other papers are correct, then philosophy is not merely an under labourer of the sciences; it is "dangerous stuff", capable of showing the inadequacies of scientific theories. The principal thesis of this work is now established.

10. NOTES

1. Whilst citing Harrison's description as an apt description of the problem of the genesis of philosophical knowledge, I strongly disagree with Harrison's view that the results of philosophy "are immune from subsequent discoveries based on science and observation" (Harrison, 1974, p. 9). This disagreement stems from a difference about the nature of *transcendental arguments* in philosophy. (On transcendental arguments cf. (Bieri, Horstmann and Krüger (eds.), 1979), (Bhaskar, 1982). Wilkerson (1976, pp. 200-202) has given good reason for rejecting the view that transcendental arguments are analytically necessary. This leaves as the only serious contender about the nature of transcendental arguments, that they are synthetically necessary - that is that the relation between the premise and the conclusion of a transcendental argument is synthetic *a priori*. Wilkerson (ibid, pp. 205-206) points out that this view faces the objection of historical parochialism, that it makes a necessary truth out of the conceptual idiosyncrasies of contemporary thought. After all, wasn't Kant wrong in granting a finality to Euclidean geometry and Newtonian mechanics? This criticism, whilst quite damaging to Harrison, only establishes that it is always empirically possible that we could be wrong in taking certain propositions to be synthetic necessary truths. It does not establish that there are no synthetic necessary truths.
2. For more comprehensive criticisms of Castañeda's metaphilosophy cf. (Sontag, 1983, pp. 199-201), (Kekes, 1983, pp. 222-224).
3. Blanshard (1966, pp. 331-332) takes the example of a pebble to illustrate the ancient problems of substance, the nature of matter, the status of sense-data and others. As he puts it: "Metaphysical problems lie round us on all sides, and usually just a few steps away. There is nothing artificial about these problems; they rise naturally in the reflection of any thoughtful man; indeed many of them are questions that a child could ask, though the most determined of metaphysicians might find it hard to answer them" (ibid, p. 332).
4. I will not beg any questions here about how a satisfactory theory of explanation shall explicate the notion of an "explanation", as I foresee no difficulties in applying the notion of "explaining" to philosophical matters which would not raise difficulties for the metascientific application of this notion.
5. Self-referential arguments have been used to refute the positions of scepticism, behaviourism, pragmatism, intuitionism and the coherence theory (Boyle, 1969, p. 5), and have featured quite prominently in the work of the "transcendental Thomists" such as Bernard Lonergan (1963).
6. The "Lewis argument" is a proof that $p \ \& \ \sim p \rightarrow q$ is a theorem of classical logic. The proof is as follows:

(L1) $p \ \& \ \sim p$	Assumption
(L2) p	(L1), Conjunction
(L3) $\sim p$	(L1), Conjunction

- (L4) $p \vee q$ (L2), Addition
 (L5) q (L4), (L3), Disjunctive Syllogism

Paraconsistent logicians have argued that the Lewis argument is invalid (Routley, 1980), (Priest, 198+(b)). Given that some interpretation of p & $\sim p$ can be given such that $\forall |p| = \text{True}$ and $\forall |\sim p| = \text{True}$, then $\forall |p \& \sim p| = \text{True}$. But $\forall |q| = \text{False}$ for at least some interpretations of the wff q . Hence the theorem $p \& \sim p \rightarrow q$ can be countermodelled and the classical propositional calculus is shown to be unsound. The fault here has been placed upon the shoulders of the disjunctive syllogism by all paraconsistent logicians. Given that $\forall |p| = \text{True}$ and $\forall |\sim p| = \text{True}$, then $\forall |p \vee q| = \text{True}$ and an interpretation of q can readily be given such that $\forall |q| = \text{False}$. Hence, concludes the paraconsistent logician, the disjunctive syllogism fails to invariably transmit truth from premises to conclusion.

7. This modified form of the Lewis argument is sufficient to trivialize any paraconsistent logic which does not outrightly reject the disjunctive syllogism, and John Burgess (1981) has given strong arguments against the radical relevantism that *completely* outlaws the disjunctive syllogism.
8. This argument is not affected by the strength of the logical connective ' \rightarrow ', it could just as easily be taken as inductive entailment and the same problem would be generated.
9. Schlesinger (1983, pp. 218-219) remarks that the infinite regress argument has never been subjected to a thorough analysis beyond Passmore's (1970) pioneering work. Schlesinger himself gives no clear statements as to what constitutes a vicious infinite regress, although he is quick to judge that this method has been misused. Despite this point Schlesinger gives a brilliant defense of our belief in the efficacy of induction by means of the infinite regress argument. This demonstrates that the infinite regress argument is not solely, as Passmore (1970, p. 17) has asserted, a principally destructive argument. I do not agree however that Schlesinger has succeeded in solving the problem of induction. He defends the rationality of the choice of the rule:

(R1) Assume that the regularity described by the equation $y = f(x)$ which has been obeyed by the instances which we have checked in the past is also obeyed by future instances,

over the counter-inductive rule:

(R2) Assume that the regularity in (R1) will not hold in the future,

on the grounds that (R2) does not yield a unique hypothesis. This defect can be easily corrected. We specify quite arbitrarily a unique function $g(x)$, which is of the same degree of simplicity as the function $f(x)$ over the domain of past events and future events, but which gives different values for future events: call this rule (R2*). Thus if $f(x) = ax + b$, $g(x) = ax + b$ for past events, but $g(x) = a^*x + b^*$ for future events where if $a^* = a$, $b^* \neq b$ and if $b^* = b$, then $a^* \neq a$. Schlesinger no doubt feels that counter-inductive rules are arbitrary - but he ignores the rejoinder which could be made by the inductive sceptic, that no reason has been given as to why one should believe that it is (R1) which is arbitrary and not (R2*).

10. Johnson takes an argument to be a *petitio principii* if the argument arbitrarily rules out some competing theory advanced by a dialectical opponent, presupposing the truth of the proponent's theory in the process of criticism. This conception of *petitio principii* would correspond on Rescher's position to a failure to meet the evidential *onus probandi*.
11. The dependency condition and equivalence condition can be formulated so as to rely upon each other, cf. (Woods and Walton, 1982, p. 81).

11. CONCLUSION: GENERAL STATE OF THE ARGUMENT

The central argument of this work is now complete and the principal thesis (PT) stated in chapter 1, rationally supported. The bulk of this work has criticized various metaphilosophical responses to the problem of perennial philosophical disagreements (PPPD). Nevertheless, consistent with the position of monistic-systemic perspectivism (MSP), I have recognized that many of these metaphilosophical responses offer valuable insights into the genesis of (PPPD). I reject the view that there is one underlying cause of the perennial discord which we find in philosophy; the causes are rather multifarious. Nevertheless the most important innovation added to this field of discussion by the present work is the rejection of the thesis that consensus between rational thinkers is a criterion of truth and rationality, and indeed that a failure of debate to converge to consensus is a clear symptom of irrationality, as Lehrer and Wagner believe (cf. chapter 7). In *Essays on Ultimate Questions: Against Received Opinion in Science and Philosophy* (198+(a)) I will "empirically" supplement the arguments of this work by a critique and proposed rejection of a number of leading theories in science and philosophy; (Smith, 1984) in its critique of leading paradigms in the allegedly secure domain of theoretical biology, is another empirical supplement to the view of the nature of philosophy which I have given here.

The picture painted of cognitive inquiry then, is both a critically fallibilistic one, and a strongly individualistic one. It stands in rigid opposition to the over-socialized, over-biologized and over-relativized conceptions of inquiry which are quite widely accepted today. Nevertheless, despite this strong fallibilism and epistemological individualism, I have attempted to show that philosophy is both a progressive and rational

enterprise; philosophical progress occurs and genuine philosophical knowledge exists.

So much then for the proposed achievements of this work. It would be self-referentially inconsistent to conclude at this point without expressing some severe limitations of the present work, and recommendations for future research. As a field of philosophical research, metaphilosophy is very undeveloped compared to fields such as metascience. Much of the time of metaphilosophers in this century has been spent defending the tenability of philosophy as a cognitive enterprise, against attacks from positivists, Wittgensteinians, relativists and sceptics. Little attention has been given to the systematic study of the methods of argumentation in philosophy, as Schlesinger (1983) repeatedly observes. Certainly chapter 13 of this work does little more than outline some of these methods (and my argument does not require more than this), although in another work entitled *Metaphilosophical Methodology*, a fuller description is hoped to be given. Questions such as "what is the relationship between confirmation theory and philosophical inquiry?", "what are philosophical theories, systems and world views?", "what are the dynamics of the interaction between philosophical systems and scientific theories?" have hardly been addressed. But they are important and they should be addressed in the future.

It can only be something of a scandal that an ultra-critical enterprise such as philosophy, an enterprise which asks such basic questions and is so critical of the rest of culture (Rorty, 1979), devotes so little of its community time and resources to a critical examination of itself. If this work does prove to be wrong in defense of all its major theses, then it will still be of value if it delivers a prick to the philosophical community's metaphilosophical conscience.

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