Jaina and Post-Normal Perspectives on Uncertainty in Climate Change Mitigation

by

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B.A., University of Victoria, 2006
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Supervisory Committee

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I examine uncertainty in climate change mitigation using the Jaina theory of reality, *anekāntavāda*, and post-normal science, focussing on carbon dioxide capture and storage (CCS). *Anekāntavāda* reveals all conceptual knowledge, not just science, as reductionist. Traditional science is *ekānta*, failing to acknowledge its reductionism, and this has led to incautious and contextually inappropriate application of scientific results. However, through Jaina principles and post-normal methodology, we can avoid scientific and technological *ekāntavādism.* For example, we must ensure "continuous iterative control" is possible in any CCS project. Accordingly, geological and mineral carbonate storage are preferable to oceanic storage. The IPCC report on CCS is not a post-normal document, since only scientific and technical experts prepared it. This is not necessarily a criticism: the report could be an expert input preceding post-normal dialogue. However, since values enter scientific undertakings early on, a broader community of stakeholders should perhaps have prepared the *Special Report.*

Table of Contents

List of Figures

Dedication

This thesis is for my mother and father, Lyse and John Burgess. Without your love, it would not have been possible.

Chapter 1: Introduction

Upon hearing about my studies in Eastern and Western philosophy and climate science, many people have responded with confusion, scepticism, and even restrained dismissiveness. For instance, a layperson wondered aloud if my thesis would involve yogic flying, while an economist suggested, "Perhaps epistemology cannot give you the answer," and argued that a mathematical analysis would be more useful. Over and over again I have been asked, "What does philosophy have to do with the climate?" This thesis will, I hope, begin to answer that question.

There is growing awareness among scientists and other thinkers that modern environmental problems, including climate change, differ from most scientific problems of the past. Climate scientists commonly distinguish their discipline from traditional sciences like physics and chemistry, characterising it as "interdisciplinary", "not reductionist", and "effectively probabilistic", with precise predictions fundamentally impossible. Unlike physics and chemistry, climate science is heavily politicised: in climate labs, calls from reporters and local newspapers are a daily occurrence, and fielding questions from the media is an almost unavoidable part of climate scientists' jobs. In addition, many climate scientists work on policy-related initiatives, most notably the reports produced by the Intergovernmental Panel on Climate Change (IPCC).

Meanwhile, in the humanities and social sciences, there exists a large, complex and continually growing body of literature criticising science. Scientific reductionism has been accused of practically all the world's ills, perspectives such as social constructivism question the possibility of objectivity, and post-modernism has criticised scientific "grand narratives". Recently, philosophers Silvio Funtowicz and Jerome Ravetz have called for a "new science", which they dub "post-normal science", and which differs from traditional sciences in methodology.

This thesis is concerned, in large part, with exploring the difference between climate science and more traditional sciences. I believe we cannot hope to comprehend the origins of, or formulate solutions to, modern environmental problems without epistemological analysis. Specifically, I locate one origin of global warming in humanity's application of knowledge, and argue that coping with climate change requires us to understand and compensate for the limitations of knowledge.

In the spirit of interdisciplinarity, I use two philosophical perspectives – the Jaina philosophy *anekāntavāda* and post-normal science. This, I hope, will both enrich post-normal science and promote a deeper understanding of climate change science.

To demonstrate how the epistemological principles and insights of *anekāntavāda* and post-normal science might inform attempts to study global warming and solve its problems, I examine a specific climate change mitigation option, carbon dioxide capture and storage (CCS). I have selected this mitigation option for two reasons. Firstly, it belongs to a class of mitigation options involving further technological intervention into the climate system. I believe the epistemological insights I develop in this thesis are particularly pertinent to such mitigation options. Secondly, the Intergovernmental Panel on Climate Change (IPCC) has produced an entire report on CCS, the *IPCC Special Report on Carbon Dioxide Capture and Storage*, on which I comment with post-normal methodology in mind.

The goals of this thesis are threefold. Firstly, I hope to enrich post-normal science by comparing it with *anekāntavāda*, which I consider a closely related philosophy. Secondly, I hope to demonstrate how epistemology can contribute to solving the problems posed by global warming. Indeed, I hope to convince the reader that an understanding of knowledge and its limitations is a precondition to effectively addressing climate change. Finally, and most specifically, I want to offer some comments based in epistemology that may help us to evaluate the IPCC reports, as well as carbon dioxide capture and storage relative to other mitigation options.

Chapter 2: *Anekāntavāda*

Introduction:

I begin with an examination of the Jaina doctrine, *anekāntavāda*, which B. K. Matilal translates as "the theory of non-one-sidedness" or "the theory of the many-sided nature of reality" (1981, p. 1). Following N. J. Shah's characterisation, *anekāntavāda* entails an ontological and an epistemological position, as well as a logical method.

One can adopt *anekāntavāda* as a philosophical principle without necessarily subscribing to Jaina cosmology, as Matilal (1981) points out. I will thus be concerned with the logical structure of *anekāntavāda*.

I will not constrain myself to recounting Jaina doctrine as it is and has been understood. Instead, I will engage with *anekāntavāda* critically, trying to clarify some points that seem important in context of a possible contribution to post-normal science. I will argue for a slight re-interpretation of some key ideas, and I will show how this re-interpretation is consistent with certain arguments in the extant literature, and with the central tenets of the philosophy. In so doing, I take the risky step of critically engaging with an Indian philosophy as a Westerner – of not simply recounting doctrines developed in another cultural context and time, but of attempting to interpret and develop them myself. Because I have grounded my arguments in pre-existing Jaina thought, I think this step is justified.

Ontologically, *anekāntavāda* holds that reality has many facets, in fact, an infinite number of them. The well-known Indian parable of the elephant, as recounted by the

English poet John Godfrey Saxe (see appendix A), aptly captures this ontology: philosophers arguing about the nature of reality, so goes the metaphor, are like blind men grasping various parts of an elephant. One touches the side, and says, "The elephant is like a wall". Another feels the tusk and says, "The elephant is like a spear". Another grasps the trunk, and says, "The elephant is like a snake", and so on. They are all right, and this is what it means for reality to be many-faceted: each person, depending upon standpoint, sees a different, partial aspect of the whole. The philosophers err, however, insofar as they believe their incomplete views of reality to be comprehensive. In arguing with each other about whose view is correct, they fragment the truth. They might quarrel ad infinitum and not come to comprehend the elephant as a whole: their argument, rather than advancing knowledge, obstructs understanding.

Jaina epistemology follows from this ontological claim. The Jainas contend that in order to attain complete conceptual knowledge, one must hold in one's mind all facets of reality *simultaneously*, rather than sequentially. Such knowledge is called "omniscient", because it is accessible only to an omniscient being. In terms of the above metaphor, the blind men would have to touch all parts of the elephant at once in order to conceptualize it as a whole. However, it is not possible for them to touch all parts at once. Similarly, ordinary non-omniscient human beings can only assume one standpoint and attend to one aspect of reality at a time, and the aspect attended to depends on momentary purpose. Just as blind men cannot *see* a whole elephant, so we cannot conceptualize reality as a whole. Our knowledge is always partial and relative to our standpoint – to whatever facet we single out as most important at the time.

It is important to specify that the knowledge concerned here is *conceptual*, also called "ordinary" knowledge. It seems to me that, in the Jaina epistemology, standpoints and concepts are inextricable. A standpoint corresponds to a particular set of concepts or conceptual scheme that organizes one's knowledge of reality, highlighting certain features and not others. The relevance of this epistemology to Western critiques of supposed scientific objectivity is virtually self-evident: for the Jainas, an objective viewpoint is a contradiction in terms. All viewpoints, including scientific viewpoints, are inextricably bound up with interests and priorities. As we shall see, however, this does not mean that we should adopt a position of unqualified relativism. Nor does it imply that scientific viewpoints and knowledge are invalid, or that all viewpoints and knowledge claims are equally valid.

In general, Indian philosophies do admit one way in which it *is* possible to have complete knowledge of reality – through direct intuitive experience unmediated by concepts. Such understanding is not simply attained by piecing together all views of the elephant: analytic puzzle assemblage is not sufficient, since it is still a sequential tallying. Rather, a final gestalt shift is required, in which the parts gel into a whole experienced free of concepts. In the context of Indian philosophy, this whole is the "ultimate reality", or, in the above metaphor, the elephant.

Logically, *anekāntavāda* implies that the verbal expression of knowledge should reflect the fact that it is partial and relative. Unconditionally asserted propositions are called *ekānta*, or "one-sided". Such propositions do not acknowledge that they are made relative to a certain viewpoint. An example of an *ekānta* statement is that of the philosopher who, having touched only the elephant's side, asserts that an elephant is like a wall. Because they believe reality to be manifold, the Jainas claim that all *ekānta* propositions are false. Hence, if one wishes to avoid stating falsehoods, one should make only conditional claims.

Syāt:

One transforms *ekānta* statements into *anekānta* statements very simply – by adding the particle '*syāt*' at the beginning of the proposition. *Syāt* explicitly conditionalizes a statement that might otherwise be understood as unconditional. In so doing, it conveys the relative nature of knowledge. By adding *syāt*, one transforms a categorical proposition into a complete statement. Rather than singling out one attribute of the entity in question, as a categorical proposition does, the complete statement evokes all the infinite attributes of an entity.

Syāt has been translated in a variety of ways, some of which better reveal the meaning of *anekāntavāda* than others. In ordinary Sanskrit, *syāt* is one of three possible answers to a direct question – yes, no, and *syāt* – and, as such, means 'maybe' (Matilal, 1981, p. 52). However, the philosophical meaning of *syāt* differs crucially. As used by the Jainas, *syāt* does not imply probability or possibility, but is rather a conditional affirmation. Rather than 'maybe', in Jaina philosophical usage, *syāt* means, 'from a certain standpoint, yes'. Translating *syāt* as 'maybe' can lead one to falsely construe *anekāntavāda* as a doctrine of doubt or indetermination, in which all knowledge is uncertain. Rather, the Jainas see all knowledge as definitely correct, but only from a given standpoint.

Syāt can also be used to partly concede an opponent's thesis by stating that it is "in a certain sense" true. Matilal sees this use of *syāt* in philosophical debate as a carefully calculated move:

> It concedes the opponent's thesis in order to blunt the sharpness of his attack and disagreement, and at the same time it is calculated to persuade the opponent to see another point of view or carefully consider the other side of the case. Thus, the Jaina use of "syāt" has both; it has a disarming effect and contains (implicitly) a persuasive force. (p. 52)

As a technique in argumentation then, the use of *syāt* depolarizes philosophical debate. While *anekāntavāda* is a philosophy of conciliation, its intention is not to shut down argument, but to discourage dogmatism.

Non-contradiction, *Anekāntavāda*, and Mysticism:

Anekāntavāda has been characterized as challenging the law of non-contradiction. According to Jaina ontology, reality possesses an infinite number of facets, and these can include mutually contradictory attributes. Hence, Jaina philosophers are willing to assert that an entity possesses contradictory properties *at the same time.*

In fact, the Jainas contend that any entity possesses at least one set of contrary properties: existence and non-existence. The argument runs as follows: in claiming that an entity has a given property, one also indicates a contradictory property or set of properties that are not present. For example, in saying, "The pot is on the counter", I implicitly assert that it is not on the floor. That is, in making a positive claim about any given entity, one necessarily also makes a negative claim (Matilal, 2000, p. 11). Thus the

Jainas argue that the entity has both existence and non-existence. In the above example, the pot exists on the counter, but is non-existent on the floor.

The Jainas recognize three primary, non-compound truth values: 'true', 'false', and *avaktavya*, or 'inexpressible'. According to the Jaina philosophers Samantabhadra and Vidyānanda, one assigns the values 'true' and 'false' to a proposition "gradually" – that is, sequentially. In contrast, 'inexpressible' corresponds to assigning the values true and false to one predicate simultaneously or "in the same breath" (Matilal, 2000, p. 14). This third truth value is 'inexpressible' because it cannot be translated into language, which is sequential. 'True', 'false', and 'inexpressible' can also be assigned to one predication in various compound ways, such as 'both true and false', 'true and inexpressible', 'true, false, and inexpressible', and so on.

Matilal writes that the seeming challenge *anekāntavāda* poses to the law of non-contradiction "...should not be construed as an invitation to jump into the ocean of irrationality and unintelligibility" (2000, p. 4). In fact, it can be argued that *anekāntavāda* does not in fact challenge the law of non-contradiction at all:

> ...Mahavira...did not reject both of the seemingly contradictory predicates...but rather accepted both of them and avoided the seeming contradiction by showing (or exposing) the different senses in which the predicates could be used. Thus, it could hardly be regarded as an acceptance of a real contradiction. (Matilal, 1981, p. 20)

In other words, a given proposition that is both true and false does not have both of those truth values as seen from one standpoint, but from two different standpoints. Hence, assigning the compound value 'both true and false' to a proposition does not violate non-contradiction.

What about the third truth value, 'inexpressible'? It is not the same as 'both true and false', because the latter is a sequential assignment of truth values, while the former involves assigning both true and false *simultaneously*. If 'inexpressible' is understood to mean that one proposition is true and false at the same time, in the same sense, from the same standpoint, then the 'inexpressible' truth value does indeed challenge the law of non-contradiction.

However, Pradeep P. Gokhale has argued that it is not in keeping with the meaning of *syāt* for *anekāntavāda* to challenge non-contradiction (2000, p. 77). *Syāt* indicates that both a proposition and its converse can be true, but from *different* standpoints. The reference to different standpoints is encoded in the very meaning of *syāt*: "in a certain sense", or "from a certain point of view". As Gokhale writes, "…the role of the term *syāt* in *syāt*-statements is to dissolve the apparent contradiction between statements by pointing out that the truth of apparently contradictory statements is relative to respective standpoints". It does not make sense to have a *syāt*-statement in which two contrary propositions are asserted to be true from the *same* standpoint. If *anekāntavāda* challenged the law of non-contradiction, there would thus be no point in affixing *syāt* to propositions.

Gokhale argues, "The middle value designated by the term *avaktavya* is therefore better understood as epistemic middle rather than as the logical middle" (2000, p. 78). Based on Gokhale's characterization of 'inexpressible' as epistemic middle, I interpret this truth value as denoting an attempt to think simultaneously using two conceptual schemes – to *see simultaneously* how a given proposition is true and false *from two different standpoints*.

Such an interpretation of 'inexpressible' allows one to link each truth value with one of the two Jaina categories of knowledge: ordinary and omniscient. 'True', 'false', and 'both true and false' correspond to ordinary knowledge, in that these truth values are sequentially applied, and in that ordinary knowledge involves thinking different aspects of reality sequentially. 'Inexpressible' corresponds to omniscient knowledge, in that this truth value involves the attempt to attend to two aspects of reality at once. Of course, the Jainas hold that this is impossible for an ordinary mortal.

If 'inexpressible' is understood in this way, it does not challenge the law of non-contradiction, as the proposition is still true and false from different standpoints. Assigning the truth value 'inexpressible' then becomes an exercise in cognitive acrobatics that reveals an epistemic limit.

If the truth values 'true', 'false', and 'inexpressible', and the compound truth value 'both true and false' do not violate the law of non-contradiction, then no compound truth value composed of any combination of "true", "false", and "inexpressible" will violate that law. Interpreting 'inexpressible' as an epistemic middle therefore allows *anekāntavāda* to avoid violating non-contradiction.

The relationship between *anekāntavāda* and mysticism is also worthy of investigation. B. K. Matilal (1981) argues that *anekāntavāda* will be misunderstood if it is taken to consist primarily of the joint assertion of contradictory predicates. Such joint assertion in religious and philosophical writings is usually intended to evoke the ineffable ultimate reality. However, *anekāntavāda* and mysticism are not identical, for mysticism – belief in an ultimate, ineffable reality – does not imply acceptance of the "many-sided nature" of everyday reality. I would take this further and argue that, conversely, the *anekānta* doctrine need not rely on positing nor necessarily imply the existence of an ineffable ultimate reality. I believe this is an important point, because if *anekāntavāda* were a mystical doctrine about an ineffable reality, its relevance to modern science and philosophy of science could be seen as highly questionable. What *anekāntavāda* does certainly imply is that language and sequential thought, and, in turn, conceptual understanding *of the ordinary world*, have limits:

> There are so many determinants and indexicals for the successful application of any predicate that the proper and strict formulation of the ways by which this can be contradicted (or the contradictory predicate can be applied to the same subject) will always outrun the linguistic devices at our disposal" (Matilal, 2000, p. 15).

There is no need to invoke an 'ultimate reality' in order to demonstrate the limitations of human conceptual knowledge, including scientific knowledge.

It seems to me that the first understanding of 'inexpressible' – true and false at the same time, in the same sense, from the same standpoint – could be said to evoke an ultimate ineffable reality, inasmuch as it challenges the law of non-contradiction and so our empirical experience of reality. The second understanding – trying to think from two standpoints at once – demonstrates the limits of human conceptual knowledge. The first concerns what, in the Western tradition, would be considered metaphysics, while the second concerns epistemology. The cognitive interpretation of 'inexpressible' thus clearly distinguishes *anekāntavāda* from mysticism and establishes it as a doctrine concerned with the empirical world.

Also, directly experiencing everyday empirical reality free of concepts is not necessarily the same as experiencing ultimate reality. While emptiness of concepts may be required for both experiences, this does not mean that the entity experienced is necessarily the same. Herein, I will be concerned only with empirical, everyday reality, and not with mysticism.

***Nayas, Pramanas*, and Intuitive Insight:**

Standpoints are referred to in Sanskrit as *nayas*. The Jainas held that *nayas* reveal entities only partially. A *pramana*, or "means of knowledge", on the other hand, reveals a thing as a whole. The Jainas view both *nayas* and *pramanas* as necessary for philosophical understanding – while *nayas* reveal things only partially, *pramanas* may obscure the more detailed characteristics of entities, since everything is infinitely-faceted (Matilal, 1981, p. 41). The following metaphor captures the relationship between the two: a *pramana* is an ocean, while a *naya* is a cup of ocean-water. When one employs a *naya*, one conceives an object based on one of its characters. When one employs a *pramana*, one conceives an object in all its characters. "Human nature is eternal", is a statement made from a standpoint considering only the perpetual aspect of human nature. "Human nature is multifaceted, for it has many characteristics including eternity and transience", is a *pramana*, for it considers all the many and contradictory characteristics of human nature.

Pramanas are reached by collating all relevant standpoints. Though they attempt to capture the whole, *pramanas* are not identical to direct intuitive experience. Nonetheless, they bear a strong relationship with intuitive understanding – one might look upon a *pramana* as an approximation to direct intuitive experience of reality. In the limit as a *pramana* contains all possible standpoints, the knowledge obtained through it begins to resemble that obtained through non-conceptual intuitive experience.

I do not, however, think that a *pramana* can ever equal direct, non-conceptual intuitive knowledge. As noted above, the Jainas believed that *pramanas* had the potential to obscure the finer details of entities. This is because pramanas are analytic. Analytic knowledge proceeds linearly, by considering each aspect of an entity sequentially. We can see this in even the structure of the *pramana*: "Human nature is multifaceted, for it has many characteristics including eternity and transience". The attributes of eternity and transience, visible from two different standpoints or conceptual schemes, are listed sequentially, of course. A *pramana* cannot list sequentially every attribute of an entity, or it would be infinitely long. It seems to me that stating a *pramana* is somewhat like sweeping one's arms out as if to gesture to a vast landscape – one doesn't stop to point out every rock and tree, but rather tries to convey the character of the landscape in broad strokes. This is why the Jainas say *pramanas* obscure the finer details of entities, and it is also why knowledge obtained through a *pramana* can never equal non-conceptual knowledge.

What a *pramana* does make possible is for one to hold seemingly contradictory attributes in one's mind simultaneously or at least closely in sequence. This is one way to evoke intuitive understanding. The effort to simultaneously hold contradictory attributes stretches the mind to a limit where a spontaneous gestalt shift into intuitive insight takes place.

Assigning the "inexpressible" truth value to a proposition may also trigger such a gestalt shift. Beyond demarcating the limits of conceptual knowledge, "inexpressible" may spark intuitive processes. In the effort to see from two perspectives at once, perhaps the mind breaks through into non-conceptual understanding.

Falsehood and Falsifiability:

As noted above, based on their ontology, the Jainas claim that all *ekānta* propositions are false. In addition, B.K. Matilal claims that stating a proposition unconditionally makes it falsifiable (Matilal, 1981, p. 2). That is, *ekānta* propositions become open to refutation specifically by virtue of being one-sided. What is the relationship between falsifiability and falsehood? Does falsifiability imply falsehood? In order to decide this, I will examine *ekānta* propositions and determine how they open themselves to refutation. Are *ekānta* propositions falsifiable for the same reason they are false? Are all falsifiable propositions then false? Are *anekānta* propositions non-falsifiable? In other words, I will examine the following relations:

1. *Ekānta* $\Rightarrow$ false.
2. *Ekānta* $\Rightarrow$ falsifiable.
3. Falsifiable $\Rightarrow$ false.
3. *Anekānta* $\Rightarrow$ non-falsifiable.

I note that scientific statements, in order to qualify as scientific, must be falsifiable. The results of this inquiry may thus have important implications for the status of scientific knowledge. I also note that the worth of non-falsifiable propositions is, at least from a Western empiricist standpoint, dubious.

Firstly, as noted above, the Jaina claim that all *ekānta* propositions are false is ontologically based. When the philosopher in the Indian parable argues, "The elephant is like a wall", he is in actuality claiming that the elephant is like a wall and *only* like a wall. That is, he implicitly claims that all other descriptions of elephants are wrong, while his is true and complete. This hidden assumption is false, and so his statement is false. As long as one accepts that reality is infinitely faceted, all *ekānta* propositions will similarly be false.

If, on the other hand, the philosopher had said, "From a certain point of view (*syāt*), the elephant is like a wall", his statement would be correct, for while asserting that the elephant does seem like a wall from one viewpoint, he leaves open the possibility that the elephant appears differently from other standpoints. Hence, it is the *ekānta* nature of the philosopher's statement – his failure to acknowledge the partiality of his viewpoint – and his subsequent one-sided belief that his conceptual knowledge is complete that make his statement false.

However, is it this failure to recognize and acknowledge partiality that also renders the statement falsifiable? Does one-sidedness imply falsifiability? It seems to me that all *ekānta* statements are indeed falsifiable. To falsify the first philosopher's statement (that the elephant is *only* like a wall), it is sufficient to show that the elephant is like something other than a wall. Any unconditional knowledge claim can be similarly falsified by showing that the entity in question possesses attributes other than the one asserted.

It seems, however, harder at first glance to falsify *anekānta* statements. Any conditional proposition, *when the condition is specified*, can be either falsified or proven. For example, "The elephant is like a wall given that the part in question is the elephant's side", is falsifiable. For example, one can show the statement to be false if one demonstrates that the elephant's side is *not* like a wall, but more like a sun-warmed boulder. Specificity and one-sidedness are not identical.

However, Pradeep P. Gokhale argues that requiring the standpoint to be specified within the formulation of the conditional *syāt*-statement distorts the original logical function of *syāt*, which is to indicate the existence of *some* standpoint or condition with regard to which a given claim is true, without specifying the standpoint (Gokhale, 2000. p. 83). Similarly, A. Uno writes, "The function of the term '*syāt*' is to imply all possible stand-points and widen the scope of the discourse" (2000, p. 46-47).

When *syāt* is understood as not gesturing to any *specific* condition, but simply to the existence of some non-specified condition (or to all the infinite possible conditions), one might venture to claim that an *anekānta* proposition indeed cannot be falsified. It is possible to entertain this claim because, according to Jaina ontology, reality is infinite-fold in character, and there are an infinite number of standpoints from which any given statement could be true. In order to completely falsify a general *syāt*-statement, one would have to demonstrate that the claim in question could not be true from *any* of the infinite possible standpoints. This is not possible unless the infinite number of propositions can be grouped into a finite and exhaustive classification scheme, so that one only has to deal with one example from each class. Even then, one can argue that there are an infinite number of possible exhaustive classification schemes, depending on which commonalities the propositions are grouped by. This is so because each proposition is infinitely faceted. Therefore, I argue that it is not possible to completely falsify *anekānta* statements.

However, it is evident that it is not the one-sidedness of *ekānta* statements that renders them falsifiable, but their specificity. Conditional statements can also be falsified if the conditions are specified: the statement can then be shown false under those conditions. Stating a proposition unconditionally does indeed make it falsifiable, because unconditional propositions are by nature specific. However, it is entirely possible to state a proposition so that it acknowledges its partiality and allows for the truth of other standpoints, while remaining falsifiable. Such propositions seem to fall into a third class, neither *anekānta* nor *ekānta*, specific, but not necessarily false. Scientific propositions might fall into this class, if stated in a conditional manner, and thereby remain falsifiable while avoiding outright falsehood.

Does the fact that *anekānta* propositions are not falsifiable land *anekāntavāda* in subjectivism and relativism? While advancing knowledge by promoting a many-faceted and truer view of reality (at least according to the Jaina ontology), it seems the predicate *syāt* can also be construed as coming dangerously close to shutting down discourse, just as the phrase "that's my opinion, you cannot change it, and I have as much a right to it as you do to yours" shuts down argument. If all propositions are equally true, no ground is left for discussion or compromise.

The similarity between *anekāntavāda* and relativism is, however, superficial, for one must not confuse facile relativism with recognizing the validity of others' views. *Syāt* does not leave one afloat in a sea of completely relative truth and dubiety. It is not equivalent to the statement, "that's my opinion, you cannot change it, and I have as much a right to it as you do to yours". It is more akin to the statement, "That's my opinion, based on my standpoint, but I recognize that yours may differ and be valid from your standpoint". The purpose of *syāt* is not to shut down argument, but to depolarize it in service of the truth. *Anekāntavāda* is not intended primarily as a method for making non-falsifiable statements, but of avoiding statements that are immediately false, which are polarizing, and which do nothing to contribute to true understanding of reality.

Anekāntavāda and *Ahimsa*:

Anekāntavāda originated at a time in Indian history when rival philosophical schools had proliferated, and debate among them was rife and vigorous. Several writers have suggested that the doctrine has its origins in the Jaina principle of *ahimsa*, or non-violence. They suggest that, in *anekāntavāda,* the Jainas carried the principle of respect for life to an intellectual level: respect for others' lives became translated into respect for their viewpoints. According to this theory, *ekānta* statements are a form of non-physical violence – violent because they deny the validity of others' viewpoints and experience, that is, the validity of their lives. In attempting synthesis and conciliation among the rival philosophical schools, the Jaina philosophers were attempting a non-violent philosophical discourse, one that would discourage intolerant dogmatism. Kapadia writes:

> …this doctrine of *anekānta-vada* helps us in cultivating the attitude of toleration towards the views of adversaries. It does not stop there but takes us a step forward by making us investigate as to how and why they hold a different view and how the seeming contradictories can be reconciled to evolve harmony. It is thus an attempt towards syncretism. (Matilal, 1981, p. 6)

However, there are logical objections against the argument that *anekāntavāda* was intended primarily to promote intellectual non-violence. J.M. Koller (2002) contends that this cannot be the case because *anekāntavāda* is not a theory of epistemological relativism. Jaina philosophers believed their ontology and epistemology to be correct, and defended them from the attacks of other philosophers. As Samantabhadra wrote, "Since the doctrines of all 'non-Jaina' philosophers contradict each other, none of them is trustworthy. Who, then, could be the *guru* 'instructor'?" (Matilal, 1981, p. 24). The suggestion is that *anekāntavāda* alone is not contradicted by the doctrines of all the other philosophers. Since there is no argument against it, it alone has the possibility of being correct.

Koller argues that, because of this ultimate epistemological non-relativism, the purpose of *anekāntavāda* cannot be primarily to promote intellectual non-violence. However, *anekāntavāda* does lay the ontological and epistemological foundation necessary for intellectual non-violence: in order to practice non-violence, we must first recognize the validity of others' lives, including their viewpoints. That is, *anekāntavāda* does not imply intellectual non-violence, but intellectual non-violence does require an acknowledgment of the many-faceted nature of reality, and the multiplicity of perspectives from which it can be viewed.

I am not sure, however, that Koller's argument holds. The Jainas do deny the validity of other philosophical doctrines. However, this denial is, in keeping with the principles of *anekāntavāda*, a qualified one. Other doctrines are wrong insofar as their proponents fail to recognize their partiality, and to qualify them. On the other hand, they are correct relative to the standpoint from which they are asserted, so long as they acknowledge their partiality. *Anekāntavāda* is thus not a wholesale denial of the validity of alternative philosophical doctrines. Rather, it espouses what I will call qualified epistemological relativism. The fact that the Jainas ultimately do not espouse a position of epistemological relativism does not imply that they commit intellectual violence. To the contrary, if the Jainas refused to point out that all *ekānta* statements are partial, based on their ontology, they would do intellectual violence, or at least allow it to continue.

It seems Koller equates intellectual non-violence with facile relativism. I, on the other hand, would be inclined to equate it with qualified assertion. Otherwise, non-violent philosophical discussion does not seem possible – we are back again to *syāt* shutting down discourse. Facile relativism reduces one to agreeing with every proposition, without qualification, in which case one might as well keep silent.

I think that *anekāntavāda* can also be understood in terms of a somewhat distinct sort of non-violence. As Nagin J. Shaw writes, "…while making absolute statements one harms the truth" (2000, p. xii). *Anekāntavāda* is thus a philosophy of epistemological non-violence. However, if one equates intellectual non-violence with qualified assertion, non-violence to truth and non-violence to the views of others are equivalent.

Objections to *Anekāntavāda*:

Non-Jaina philosophers have raised a number of objections against *anekāntavāda*. Drawing mainly on the work of B.K. Matilal (1981 & 2000), I will review some here, as well as the responses given by Jaina philosophers. According to Matilal (1981), *anekāntavāda* faces three major problems: intermixture, dubiety, and contradiction, and most other charges against the doctrine are variations on these three. Dubiety I have already dealt with – *anekāntavāda* is not a philosophy of doubt because it conditionally affirms, rather than doubts every knowledge claim.

Nor does *anekāntavāda* suffer from contradiction or self-contradiction. Because contradictory predicates are asserted from different standpoints, *anekāntavāda* does not in fact violate the law of non-contradiction. To give a banal example, the proposition "It is sunny" can be said to be true and false – true on a sunny day and false on a cloudy day. It is not unconditionally true or false; its truth value depends on context, and this is a core insight of the Jaina doctrine.

One charge of self-contradiction, discussed by B.K. Matilal (2000), is that *anekāntavāda*, in espousing epistemological relativism, actually contradicts itself: relativism, in asserting that all truths are relative, in fact utilizes an absolute notion of truth. However, as argued above, *anekāntavāda* is a theory of qualified rather than total epistemological relativism. Since Jaina philosophers believed their ontology and epistemology to be correct, they openly espoused an ultimate, absolute truth. Their argument that all statements are correct only relative to a certain viewpoint says less about the existence of ultimate truth than about human ability to know it. Once more, since conceptual knowledge is sequential, human beings can only apprehend reality from one standpoint at a time, and so our knowledge of reality is always partial. We cannot know ultimate truth, at least not conceptually, and whatever truth we know conceptually is necessarily not absolute. Rather than claiming that all truth is relative, *anekāntavāda* claims that *all truth we can know conceptually* is relative.

A related possible charge is that, in asserting *anekāntavāda* to be true and other philosophies to be false, the Jainas contradict their own doctrine of conditional assertion. I have already dealt with this charge: the Jaina denial of other doctrines' validity is a qualified denial. *Under the condition* that other doctrines are asserted categorically, these doctrines are wrong. The Jainas thus adhere to their own principal of conditional assertion even in their criticism of other doctrines.

Summing up the response to the charge of self-contradiction, a*nekāntavāda* is not self-contradictory because it espouses a position of qualified epistemological relativism: all knowledge claims are in some way true and in some way false because conceptual knowledge itself is inherently partial. This is a nuanced claim about the range and limits of knowledge, rather than a blanket statement that all viewpoints are equally valid, a claim that corresponds to the facile relativism of postmodernism. Further, the Jainas recognize an absolute truth, but in order to have knowledge of it, one would have to simultaneously see reality from all infinite possible positions, which is not possible.

I will examine one other potential charge against *anekāntavāda*. Consider a proposition that is true from one standpoint and false from another. The Jainas would say that the proposition has both truth values: it is "both true and false". However, one can argue that, because the proposition is judged from different standpoints, it in actuality consists of two different propositions, one true and the other false. Consider the proposition, "Increasing atmospheric carbon dioxide content is good". Someone with a strong interest in promoting plant growth might regard the proposition as true, for adding more carbon dioxide will, as far as we know, increase production of plant biomass. However, someone for whom increased global temperatures would cause damaging water shortages might regard the proposition as false. Hence, the Jainas would say the proposition is both true and false. The counterargument is that, in the first case, the proposition judged true is not, "Increasing atmospheric carbon dioxide content is good," but "If one wants to increase plant growth, increasing atmospheric carbon dioxide content is good". In the second case, the proposition judged false is really, "If one wants to avoid a water shortage, increasing atmospheric carbon dioxide content is good". Hence, we do not in fact have one proposition with two truth values, but two *different* propositions with different truth values. The Jaina claim that truth and falsity can reside simultaneously in one proposition thus seems overturned.

Again, however, this argument depends upon a particular interpretation of *syāt*. If *syāt* gestures to *specific* conditions or specific viewpoints under which the proposition is true and false, then the argument holds. However, as noted earlier, Gokhale argues that *syāt* should be understood as gesturing to the existence of some unspecified standpoint or standpoints from which the proposition is true or false. Rather than indicating one or even a finite number of standpoints, *syāt* invokes all the infinite possible standpoints. We return again to the fact that it is not possible in a human lifetime to think sequentially through all the infinite possible standpoints. In such a case, it makes sense to think not in terms of an infinite number of different propositions with different truth values, but in terms of one proposition seen from an infinite number of standpoints. The Jaina claim that one proposition can be simultaneously true and false is then really a shorthand way of referring to all the infinite standpoints at once, a reference that is otherwise not possible using sequential language.

The only way to counter *anekāntavāda* is to find "…an absolute, unconditionally applicable, totally uambiguous and categorically assertible predicate" (Matilal, 2000, p. 10). According to Jaina ontology, this is not possible, since reality is infinitely faceted, and since conceptual understanding is sequential. The only proposition that can be unconditionally and categorically asserted is the ultimate one – reality as seen by an omniscient. However, according to Jaina epistemology, it is not possible to assert the ultimate proposition using ordinary language.

Conclusion:

I have argued for a slightly different interpretation of 'inexpressible' than that presented by Matilal and others. According to my understanding, which extends Pradeep Gokhale's interpretation of 'inexpressible' as an epistemic limit, assigning this truth value means attempting to see reality simultaneously from multiple standpoints. This is an exercise in cognitive acrobatics that demarcates an epistemic limit, and may trigger intuitive insight.

I have argued that this interpretation of 'inexpressible' is desirable based on the fact that it allows *anekāntavāda* to avoid violating the principle of non-contradiction. I have also argued that this interpretation is supported by the sense of *syāt*. Further, the interpretation of 'inexpressible' as epistemic middle allows a clear distinction to be drawn between mysticism and *anekāntavāda*, a distinction that seems in keeping with the realist bent of Jaina philosophy.

I have examined the relationship between falsehood and falsifiability, in context of Matilal's claim that stating a proposition unconditionally makes it falsifiable. I found that is it not one-sidedness that makes a proposition falsifiable, but rather specificity. Because *syāt* does not gesture to a specific condition, but rather broadens the discourse to include all possible non-specified conditions, *anekānta* statements are not falsifiable. It is, however, possible to state a proposition so that it both recognizes its partiality and remains falsifiable. Such propositions seem to fall into a third class, neither one-sided nor *anekānta*. Scientific knowledge could be stated in such a qualified way and remain falsifiable.

I have further argued that *anekāntavāda* is a theory of qualified epistemological relativism, and may have the prevention of intellectual violence as a primary purpose. The Jaina denial of other doctrines' truth is a qualified one: other doctrines are wrong insofar as they do not acknowledge their partiality. While Koller seems to equate intellectual non-violence with facile relativism, I equate it with qualified relativism. If the Jainas failed to point out the falsity of *ekānta* statements, they would perpetuate intellectual violence.

Finally, I answer what seems to me one of the less easily refutable criticisms of *anekāntavāda* – namely, the claim that a given proposition is not, in actuality, both true and false, for we are really dealing with two different propositions. Because it is not possible to think sequentially through all the infinite possible standpoints, it makes sense to think not in terms of an infinite number of different propositions with different truth values, but in terms of one proposition seen from an infinite number of standpoints. Thus the Jaina claim that truth and falsity can obtain simultaneously in one proposition is a shorthand reference to all the infinite standpoints at once, a reference not otherwise possible.

Chapter 3: Post-Normal Science

Introduction:

Post-normal science, developed by philosophers of science Silvio O. Funtowicz and Jerome R. Ravetz, is a model of how science-for-policy can be most effectively conducted. It is also a theory about the limits of scientific knowledge. As such, it combines methodology and epistemology. Funtowicz and Ravetz argue that we need a new scientific style, post-normal science, to solve some of the problems we now face, such as climate change.

Technological innovation combined with consumerism has disrupted and destabilized the complex natural system on which we depend. We have, up until now, assumed that innovations are safe until proven dangerous (Ravetz, 2004, p. 348). This assumption has caused environmental and other disasters, and we should now proceed according to a 'precautionary principle': innovations are dangerous until proven safe. Precautionary science, such as carbon dioxide capture and storage, intersects with policy and reacts "to the unintended harmful effects of progress" (Ravetz, 2004, p. 349).

The post-normal methodology is appropriate where "…facts are uncertain; stakes high; decisions urgent; and where no single one of these dimensions can be managed in isolation from the rest" (Ravetz, 1987, p. 422). Post-normal issues are large-scale compared to traditional scientific problems, often including the whole biosphere, and the phenomena involved are "novel, complex, and variable, and themselves not well-understood" (Funtowicz and Ravetz, 1993, p. 742). These problems include ethical, social, and ecological aspects, such as safety and sustainability, as well as ineradicable uncertainty. In post-normal situations, science cannot abstract from the real world, but must rather cope directly with its complexity, both natural and human. Risk assessment is now central, and – because proposed solutions often threaten vested interests – uncertainty is subject to much political manipulation. Hence, post-normal science is also a "political epistemology" (Funtowicz and Ravetz, 1992a, p. 269).

Climate change is a post-normal problem. Global warming will affect the entire biosphere. While the basic science is simple, it is difficult to specify the severity and distribution of impacts, though predictions continually improve. Uncertainty stems from two sources. Firstly, the climate system is complex, and some of its major processes are not well-understood. Secondly, it is difficult to predict the future trajectory of anthropogenic greenhouse gas (GHG) emissions. In spite of these uncertainties, we must take action imminently to curb GHG emissions if we wish to avert the most serious potential effects of climate change. However, our society's dependence on oil complicates mitigation efforts. Politicians and corporate interest groups, whose power and wealth depend on fossil fuels, have used the media to obfuscate science, manipulate uncertainty, and influence public opinion in order to delay mitigation efforts.

The main innovation of the post-normal approach is an 'extended peer community' composed of all stakeholders – for example, scientists, professionals, interest groups, policy makers, and other citizens – in a given issue. This community, which includes both scientific and technical experts and non-experts, ensures the quality of science and policy in post-normal situations. The quality assurance of the extended peer community and continuous, iterative control, argue Funtowicz and Ravetz, allow us to make our ignorance "usable".

Post-normal science responds to Kuhnian normal science, and offers an alternative to postmodern fragmentation and relativism. Funtowicz and Ravetz provide a philosophical foundation for post-normal science with their theory of "emergent complexity", a conceptual extension of systems theory designed to capture specifically "human" elements of the biosphere. In brief, according to Funtowicz and Ravetz, emergent complex systems contain elements that cannot be accounted for mechanistically, such as rational choice, foresight and morality, and display different structural and dynamic patterns than do ordinary, or "natural" complex systems.

I will first briefly explore the epistemic and ontological stance of traditional science, then overview Kuhn's model of science and relevant features of post-modernism. I will then examine the philosophical basis of post-normal science. Following this, I will overview Funtowicz's and Ravetz's classification of scientific problem solving into core science, applied science, professional consultancy, and post-normal science, and examine post-normal science in more depth. Wherever appropriate, I will point out the potential relevance of post-normal science to the climate change debate and to technical mitigation options.

Philosophical Background:

Realism, Atomism, and Reductionism:

According to Funtowicz and Ravetz, the epistemic stance of traditional science is naïve realism, which assumes that scientific concepts and theories correspond to the actual world. Epistemic realism implies ontological realism: to hold that theories correspond to the real world, one must also believe in a real world, existing apart from the human mind. Ontological and epistemic realism held together allow for the possibility of objective measurement (though I note they do not necessarily imply it).

Traditional scientific approaches also view nature as mathematically tractable – practitioners widely assume that all scientific problems, given time and the proper mathematical tools, can be solved satisfactorily. Satisfactory solution in traditional science requires certainty and predictability: given some initial knowledge, particular features of phenomenal development, such as trajectory or statistics, are knowable.

Traditional science, which Ravetz (2004) also calls "mainstream science" (p. 349), is also atomistic and reductionist. Under an atomistic, reductionist paradigm, investigators treat nature as if it were composed of isolated bits, and assume that complete knowledge and control can be gained by studying these bits separately and then reassembling them. According to Ravetz (2004), reductionist approaches tend to ignore systemic properties, which they view as unfit for scientific study (p. 349). Atomism is closely related to analysis, which means "the resolution or breaking up of anything complex into its various simple elements" (Oxford English Dictionary Online). However, I note that reductionism and analysis are not identical, for analytic approaches can be used to study systemic properties, such as in the scientific field of pattern formation.

In keeping with physical atomism, traditional science assumes an observer atomistically detached from his or her subject of study. Predicated on the detached observer are claims of rationality, neutrality, and a privileged status for scientific knowledge. Furthermore, in traditional scientific worldviews, science itself is treated as an atomic entity, detached from its social and cultural context, including power relations.

In traditional science, knowledge production is also atomized. Specialization predominates, with each sub-discipline striving for autonomy. Achieving autonomy usually requires that a sub-discipline define itself and its territory of inquiry in such a way as to be clearly distinct from the other sub-disciplines; thus we have turf wars (such as that between sociology and psychology over the field of social psychology) where disciplines overlap.

Scientific knowledge itself is treated as a compilation of atomic facts, and is decontextualized:

> With the atomism of the physical reality goes atomism of our knowledge of it. Thus it has been highly effective to teach science as a collection of simple hard facts. Any given fact will be related to prior ones whose mastery is necessary for the understanding of it; but to relate forward and outward to the meaning and functioning of a fact in its context, be it technical, environmental, or philosophical, is normally considered a luxury, regularly crowded out of the syllabus by the demands of more important material. (Ravetz, 1987, p. 419)

This characterisation of traditional science is, of course, a gross generalization and simplification on Funtowicz and Ravetz's part, and many scientists would disagree vigorously with it. I think it is certainly an accurate description of the way science is taught in some instances, but certainly not in others. This decontextualization of knowledge is intimately related to the organization of science into segregated sub-disciplines, and with its (often vigorously defended) distinction from other, "non-scientific" fields, such as the arts, humanities, and even the social sciences. Contextualizing facts would require scientists to examine how their sub-disciplines relate to each other and the broader context of science, and how science relates to knowledge in general.

Funtowicz and Ravetz argue that, though reductionism has its place in laboratory work, it is not adequate for remedying the large-scale pathologies our science-based industrial civilization has generated. Rather, "a systemic, synthetic and humanistic approach" must develop, and is developing, within science (Funtowicz and Ravetz, 1993, p. 739).

Climate science showcases the limits of atomism and reductionism. The climate can be thought of, in atomistic fashion, as composed of many constituent systems. However, these subsystems are not completely isolable – they interact with each other in nonlinear and non-negligible ways, and can completely change each other's qualitative behaviour. For example, El Niño is correlated with decreased rainfall over southeast Africa and Madagascar (Barry and Chorley, 2003, p. 306). In addition, physical science alone is not adequate to solve the problems of climate change, which has societal, economic, and ethical dimensions.

Funtowicz and Ravetz contend the Baconian notion that scientific knowledge can allow us to dominate and control nature must now be seriously questioned:

> The 'domination of nature', the driving vision of our science-based civilization, may turn out in retrospect to have been just a disenchanted variety of magic. The recently discovered fact that we cannot dominate, though we can destroy, may be the decisive challenge to our civilization. (Ravetz, 1987, p. 431)

Ecological damage is a legacy of our atomistic, reductionist science and its technological application. If unchecked, this damage could threaten the integrity of the biosphere and the survival of human civilization.

Normal Science:

With his publication of *The Structure of Scientific Revolutions*, Thomas S. Kuhn announced a new model of scientific progression. According to Kuhn, scientific knowledge is never final or definitive. Rather, science develops in cycles: immature, pre-paradigmatic fields develop into mature, paradigm-governed disciplines. In mature or "normal" science, practitioners make progress through "puzzle solving", which articulates the dominant paradigm in greater and greater detail. Normal science eventually encounters an unsolvable problem, which triggers crisis, disagreement in the scientific community, and eventually the enthronement of a new paradigm.

Kuhn describes normal science as "a strenuous and devoted attempt to force nature into the conceptual boxes provided by professional education" (1996, p. 5). The governing paradigm provides a conceptual framework for understanding phenomena, as well as criteria for choosing acceptable problems, or "puzzles". Specifically, normal scientists choose only problems that, under the governing paradigm, appear solvable:

> To a great extent these are the only problems that the community will admit as scientific or encourage its members to undertake. Other problems, including many that had previously been standard, are rejected as metaphysical, as the concern of another discipline, or sometimes as just too problematic to be worth the time. A paradigm can, for that matter, even insulate the community from those socially important problems that are not reducible to the puzzle form, because they cannot be stated in terms of the conceptual and instrumental tools the paradigm supplies. (Kuhn, 1996, p. 37)

Expected solvability is not the only criterion for a puzzle, however. For a problem to classify as a puzzle, a set of rules, which direct the solution process, must exist. "Rules" may include preconceptions and established viewpoints (Kuhn, 1996, p. 38-39).

Moreover, there must be criteria that identify solutions as acceptable. Kuhn draws an analogy with jigsaw puzzles, for which there are guaranteed solutions, rules for assembly, and methods of determining whether the solution is correct.

A set of commitments provides the rules that enable the puzzle-solving activity:

> The existence of this strong network of commitments – conceptual, theoretical, instrumental, and methodological – is a principle source of the metaphor that relates normal science to puzzle solving. Because it provides rules that tell the practitioners of a mature specialty what both the world and his science are like, he can concentrate with assurance upon the esoteric problems that these rules and existing knowledge define for him. (p. 42)

In terms of experimental methodology, the community may recognize certain preferred instruments, and accepted ways in which to use them. Metaphysical commitments, on the other hand, can tell scientists what the universe is like, what entities it contains, and so guide experimentation.

Kuhn notes, "…the really pressing problems, e.g., a cure for cancer or the design of a lasting peace, are often not puzzles at all, largely because they may not have any solution" (1996, p. 36-37). In Funtowicz's and Ravetz's scheme, these problems would classify as post-normal. Thus, one might characterise post-normal science as an approach to solving problems that do not, and cannot under *any* paradigm, classify as puzzles in the Kuhnian sense, but which are nonetheless pressing and require scientific input.

Postmodernism and Metanarratives:

Ritzer (1997) calls postmodern social theory an "often self-consciously unreadable and incoherent body of work" (xvii). Debates about the meaning of "postmodernism" remain unsettled, which is perhaps appropriate: postmodernism is a theory about fragmentation, and its definition is itself fragmented. Nonetheless, some identifiable features of postmodernism are relevant to the development of post-normal science.

I will concentrate here on Lyotard's definition of postmodernism as "incredulity toward metanarratives" (1984, p. xxiv). According to Lyotard, postmodernism involves an attack on totalizing narratives, specifically the emancipatory and scientific grand narratives associated with the Enlightenment (Ritzer, 1997, p. 128). A metanarrative, or grand narrative, "...is a global or totalizing cultural narrative schema which orders and explains knowledge and experience" (Stephens and McCallum, 1998, p. 6).

Lyotard does not view postmodernity as a new cultural condition separate from modernism; rather, he sees it as a phase within modernism, which occurs cyclically, preceding the periodic emergence of new modernisms: "A work can become modern only if it is first postmodern. Thus understood, postmodernism is not modernism at its end, but in a nascent state, and this state is recurrent" (Lyotard, 1993, p. 13).

There is an obvious resemblance between the cyclicality of postmodernism as envisioned by Lyotard and the cyclicality of science as envisioned by Kuhn. Moreover, postmodernism has a number of characteristics in common with periods of scientific crisis and pre-paradigmatic science. In these phases of science, there is no one dominant paradigm. Instead, the scientific community fragments into plural competing schools, and many different paradigms vie for dominance. Similarly, rejection of grand narratives and competition between many small narratives characterises the postmodern phase. A similarity is thus apparent between scientific progression and the progression of culture and knowledge, as envisioned in postmodernism. Based on this structural similarity between Kuhnian science and post-modernism, grand narratives and governing paradigms appear to be analogous.

Funtowicz and Ravetz see postmodern fragmentation as symptomatic of the fall of Kuhnian normal science as the scientific methodology for our society. Like postmodernism, post-normal science rejects metanarratives and governing paradigms. However, it also rejects fragmentation and relativism, and Funtowicz and Ravetz proffer it as an alternative to postmodern nihilism and despair.

The Philosophical Foundation of Post-Normal Science:

Emergent Complexity:

Funtowicz and Ravetz (1994) develop the notion of "emergent complexity" to provide a philosophical underpinning for post-normal science. They differentiate emergent complexity from ordinary complexity, which they claim to be the sort of complexity found in purely natural (that is, non-human) systems. Ordinary complexity displays structure and self-organization, and system behaviour can be explained in terms of mechanics combined with simple functional teleology. For example, in biological systems, the teleology consists of goals like growth and survival. Diversity characterizes ordinarily complex systems, and their elements both compete and cooperate. The systems are also stable to perturbations, unless strained beyond a threshold.

Emergent complexity, on the other hand, incorporates what Funtowicz and Ravetz call the "specifically human aspects" of our societies and inventions: those aspects, such as consciousness, morality, symbol usage, prudence, and intentionality that cannot be fully explained in terms of function or mechanism.

Funtowicz and Ravetz (1994) differentiate emergent from ordinary complexity based on "patterns of stability and change" (p. 570). Emergent complex systems display a structural and temporal oscillation between "hegemony" and "fragmentation". In hegemony, the opposite of diversity, one element's or subsystem's goals marginalize or annihilate those of other elements and subsystems. Fragmentation, which emerges upon the collapse of hegemony, is characterized by power struggles among many attempted hegemonies.

A system in the hegemonic state suppresses rather than resolves internal conflict, does not recognize problems, and fails to adapt to challenges and changes. As a result, rather than being dynamically stable, the system can collapse in response to small perturbations. An example of hegemony is monoculture agriculture, characterized by low biodiversity and vulnerability to disease outbreak and pests. Funtowicz and Ravetz (1994) speak of an *ancien régime* syndrome, characterized by underperformance, lack of novelty, and lack of diversity (p. 571-572). In ordinary complex systems, diversity appears naturally, while attaining diversity in emergent complex systems requires intentionality.

I am not convinced that hegemony and fragmentation are specific to human systems, so I would not distinguish emergent from ordinary complexity based on these spatiotemporal patterns without further investigation. However, Funtowicz and Ravetz specify that "technique" enables hegemony: through technology, humans can influence all other species and systems for our benefit (1994, p. 571). It seems to me that technologies, such as carbon dioxide capture and storage, are the fundamental difference between hegemony as it exists in emergent complex systems and as it might be argued to exist in natural systems. The sort of hegemony seen in emergent complex systems depends on technology, which keeps the system in a state it would not otherwise attain. Specifically, technology suppresses internal processes that would otherwise force the system into another condition. Hence, scientific knowledge, as expressed in technology, is the essential force shaping emergent hegemony. I believe this has implications for the form and outcomes of hegemony, and I will discuss these implications in chapter four.

Funtowicz and Ravetz outline several processes that can trigger the collapse of hegemony. As mentioned above, an external perturbation can bring down the system. However, internal processes can also destroy it. Emergent complex systems display an 'autolytic property', which distinguishes them from ordinarily complex systems (Funtowicz and Ravetz, 1994, p. 572). The *ancien régime* state can intensify, resulting in rigidification and paralysis, so that system processes come to a halt. Alternatively, *ancien régimes* can pass into autolysis through a so-called "revolt of the slaved variables", which involves concatenation of changes upward from lower to higher levels: oscillations beginning at lower levels add and amplify, so that the entire system oscillates more and more wildly until it disintegrates (Funtowicz and Ravetz, 1994, p. 572).

The analogous process in ordinary complexity is "creative destruction" (Funtowicz and Ravetz, 1997, p. 794). Ecosystems in the "climax phase" maximise efficiency at the expense of losing robustness. These systems are thus vulnerable to external shocks, which can trigger their destruction, whereupon a new or renewed ecological balance emerges. Catastrophes and cyclical destruction and creation are thus part of the natural cycle.

Notwithstanding Funtowicz's and Ravetz's identification of technology as the feature differentiating emergent from ordinary complexity, I am not completely convinced that fragmentation – the collapse of hegemony – differs fundamentally from creative destruction. It seems to me that the two may differ only in that technological intervention intensifies the climax culture, and hence the effects of catastrophe, when it occurs. That is, fragmentation is also "creative destruction", but the emphasis is on destruction. In natural systems, the balance is comparatively more toward creation.

Another feature of emergent complexity is continuous novelty. This at first might seem to contradict the suppression of novelty seen in the *ancien régime* state. This contradiction is easily resolved by associating continuous novelty with fragmentation, and not with hegemony. Funtowicz and Ravetz do not do this explicitly; however, the association seems quite logical. When various elements and subsystems vie for dominance, one can expect a heterogeneous system state characterized by flux and instability, since there is no stable, recognized regime. Continuous novelty is an obvious characteristic of post-modernism.

Of course, even if one accepts that ordinary complexity and emergent complexity are distinct, the distinction is often neither clear nor stable. Funtowicz and Ravetz associate emergent complexity with humanity, and ordinary complexity with natural systems (an association many would consider arrogant and anthropocentric). However, human systems can sometimes be treated as if they were ordinarily complex, for, "…since we are natural as well as social beings, the emergent aspects of our social and technical systems will always be, as it were, the tips of an iceberg of which the greater part is ordinarily complex" (1994, p. 570). For example, Funtowicz and Ravetz argue that when individual human decisions are significant only in the aggregate, the system can be considered ordinarily complex. I am not convinced that morality, rational choice, and the other attributes the authors characterize as "specifically human" can, in the aggregate, produces systems behaviour that differs from ordinary complexity; hence, my suspicion of the distinction between ordinary and emergent complexity. Conversely, Funtowicz and Ravetz note that because human activities have altered almost every biospheric process, in a sense there are no longer any purely ordinarily complex systems. All include emergent complexity to some degree, and the biosphere as a whole is emergent.

Contradiction:

Funtowicz and Ravetz stress contradiction as a central concept in the analysis of emergent complex systems. As a heuristic, contradiction promotes a mindset in which conflict and change are seen as normal and necessary, and allows integration of paradoxical notions into one theoretical framework. Funtowicz and Ravetz claim that taking contradiction as a heuristic discourages oversimplification, and helps one recognize the co-existence of opposed forces:

> Within this style, one cannot envisage a beneficial progress without looking for its costs; the growth of knowledge without its interaction with ignorance; or the achievement of good without some production of evil. (1994, p. 572)

Accepting contradiction also allows one to view differences of opinion as natural and healthy, and as opportunities for dialogue and advancement rather than inherently problematic. As such, the centrality of contradiction in the dialectics of emergent complexity encourages diversity.

Funtowicz and Ravetz identify three kinds of contradiction: complementarity, destructive conflict, and creative tension. Complementarity resembles Newton's law of action and reaction, as well as the Eastern notion of yin and yang. In complementarity, opposed elements keep each other in balance. Predator-prey relations in biological systems exemplify complementarity. This sort of contradiction can persist indefinitely. In destructive conflict, on the other hand, struggle between opposed forces causes system collapse. In the final type of contradiction, creative tension, a qualitative transformation resolves the contradiction. Creative tension corresponds to Hegel's model of how thought advances: by positing a thesis, and then an antithesis, and finally reconciling both views in synthesis.

Complementarity characterizes natural, ordinarily complex systems: competing subsystems or elements keep each other in balance and the system stable. In emergent complex systems, on the other hand, contradictions can evolve into destructive conflict. For example, clear cut logging promotes soil erosion and degrades stream quality, with potential long term effects on habitat and ecosystem coherence in the affected areas. The "contradictory", unbalanced elements in this case are trees and human need for lumber.

Funtowicz and Ravetz analyze two kinds of contradiction they identify as specific to emergent complex systems. The first is conflict between individuals and the broader aggregate; the second is continuous novelty, which involves destruction. It seems to me these both must fall under destructive conflict. In the first type of contradiction, individuals are either required to endure hardships for the greater good, or are indifferent to the harmful effects of their aggregated actions. Ever-climbing GHG emissions are clearly the result of individuals continuing behaviours even though they are destructive in the aggregate, and if policy-makers did instate GHG emission controls, individuals would have to bear hardships for the greater good.

The authors do not, as far as my reading reveals, elaborate adequately on how "continuous novelty" constitutes a "contradiction". I would guess the contradiction might lie in competition between the various factions that typify fragmentation.

Funtowicz and Ravetz also identify a "characteristic contradiction of the biosphere" (1994, p, 579): each individual desires material wealth and convenience, but providing a Western level of affluence to all of humanity would strain the biosphere beyond capacity and result in ecological collapse. Hence, Western consumerist living is only possible under staggering global inequity, which contradicts Western ideals. I would classify this ideological contradiction as a type of destructive conflict, and specifically, a conflict between the individual and the aggregate.

Disasters and Emergent Complexity:

According to Funtowicz and Ravetz, disasters occur and evolve differently in ordinary and emergent complex systems. In ordinary complex systems, autolysis is rare, *ancien régimes* uncommon, and catastrophes part of an ongoing cycle. Disaster initially causes fragmentation, but the system eventually regains stability. For example, removing a key species from an ecosystem will initially cause competition among other species, but the system will eventually regain a stable, complementary state.

In emergent complex systems, on the other hand, "gross pathologies" can persist on a long timescale, and destruction frequently seems "pointless" (Funtowicz and Ravetz, 1994, p. 575). Again, this seems to me a difference in degree: technology, which differs from other species' adaptations in that it is rooted in conceptual knowledge, intensifies the natural disaster cycle.

The conflict between individual and aggregate accounts for the *ancien régime* state and autolysis: a few powerful individuals with vested interests in the status quo can inhibit change, even when doing so is not good for the system. This is clearly the case with global warming: powerful individuals and corporations connected to the oil industry have resisted greenhouse gas emissions reductions in order to avoid loss of revenue and concomitant power loss.

Funtowicz and Ravetz (1994) propose that all industrial processes can be viewed as "accident-generating systems", in which the results are partly unforeseen (p. 576). Control systems can detect precursor incidents, which signal greater danger, but if these systems fail, disaster can ensue. Systems in an *ancien régime* state are "disasters waiting to happen" (p. 576).

Multidimensional Phase Space:

To help explain emergent complexity, Funtowicz and Ravetz borrow the notion of multidimensional phase space from chaos theory. For a system with N degrees of freedom, the phase space is N dimensional, and the instantaneous state corresponds to a point in the space.

The N dimensions of an emergent complex system include what Funtowicz and Ravetz call "higher" and "lower" dimensions. The dimensions are not physical, but metaphorical, and Funtowicz and Ravetz call them 'hierarchical' in the sense that they involve qualitative distinctions (1997, p. 801). The "lower" dimensions comprise mechanical dimensions, such as space and time, while the "higher" dimensions are those characterised by reflexive self-awareness. These dimensions specifically demarcate the system as emergent, and include technological, economic, societal, personal, and moral realms. Unlike higher spatial dimensions, Funtowicz's and Ravetz's "higher" dimensions do not include the lower ones. In addition, the lower dimensions have clearer metrics. Higher dimensions cannot be measured as precisely, and their metrics are fuzzy.

Funtowicz and Ravetz claim that the multidimensional phase space metaphor helps us identify when reductionist approaches are appropriate and when not. "Stable lower-dimensional manifolds" are "slices across reality" that exclude the higher dimensions (Funtowicz and Ravetz, 1997, p. 801). According to Funtowicz and Ravetz, reductionist science is appropriate for the study of these lower-dimensional manifolds.

Environmental problems such as climate change involve all dimensions of the phase space, including ecological complexity and reflexivity. For these problems, the traditional "unreflective" methods are inappropriate. Hence, according to Funtowicz and Ravetz, it is when reflexivity and ethics enter the picture that reductionism fails utterly, and we need post-normal science's emphasis on uncertainty, values, and multiple perspectives.

In order to study an N-dimensional object, one can take projections on lower dimensional subspaces. For example, one might study a sphere by projecting it onto a plane, or investigate a four-dimensional object by projecting it onto three dimensions. However, projections on lower-dimensional subspaces do not capture all properties of higher dimensional spatial objects. These objects have properties that cannot be conceived in lower dimensions, or appear counterintuitive. Analogously, emergent complex systems have features that cannot be captured by mathematical analysis. The essential point is that no single projection can encompass a higher-dimensional object:

> To take any particular perception, or projection onto a subspace, as the true, real or total picture, amounts to reductionism. (Funtowicz and Ravetz, 1994, p. 575)

The multidimensional phase space metaphor thus allows one to appreciate that a plurality of legitimate perspectives, as in the extended peer community, might together provide a more accurate representation of a post-normal issue.

Post-Normal Science and the Fragmentation of Knowledge:

According to Funtowicz and Ravetz, postmodernity is a state of fragmentation brought on by the collapse of hegemonic reductionist science. Continuous novelty characterizes postmodernity: there is no stable interpersonal reality, and no dominant paradigm. All viewpoints have equal claim to validity, and absolute relativism prevails. Novelty is not identical to diversity, which is a stable system state characterized by balance between competing elements. Hence, the fragmented postmodern condition does not represent real diversity; rather, it displays a "contradiction" between hegemonic reductionism and fragmented relativism. The epistemic project of post-normal science, as envisioned by Funtowicz and Ravetz, is to resolve this contradiction.

Emergent complexity, as a conceptual tool, elucidates the value of and need for an extended peer group, with its associated diversity of perspectives. An environmental issue is analogous to an object in multidimensional phase space. No one viewpoint, or projection on a subspace, will capture all the issue's salient features. Here, subspaces correspond to the reality and concerns of individuals, or groups of individuals. It is clear that different features of the issue will be salient for different persons, according to their interests. Hence, no quality criterion has natural priority over any other. According to Funtowicz and Ravetz, the new scientific problems reveal the claimed objectivity of science as limited to the lower dimensions of the phase space. Objectivity is impossible when higher dimensions are involved, for everyone will see a different feature of the issue depending on his or her concerns.

Ravetz considers reductionism to be a viewpoint. He writes, "All we need is to appreciate that this 'reductionism' is a partial view, and then to be open to the importance of perspectives that are complementary to it" (1997, p. 537). I will examine this understanding of reductionism further in chapter three.

It is also evident that, though no single perspective can grasp the entire issue, a more complete picture might be obtained by collating as many viewpoints as possible. In other words, to learn about a higher dimensional object, we should study as many projections on lower dimensional subspaces as feasible, and to capture the salient features of an environmental issue, we should involve as many viewpoints as possible.

Through respectful appreciation of diversity, according to Funtowicz and Ravetz, we can achieve complementarity – that healthy contradiction characteristic of ordinary complex systems:

> Complementarity involves an awareness by each stakeholder that their own perception is partial, and (in terms of the phase-space metaphor) a projection of the whole configuration onto their own particular partial subspace. In the case of attempted hegemony, such an awareness of legitimacy of others' perception (and with it, values and rights) is either discounted or (in the extreme case) denied altogether. (1994, p. 579)

The methodology of post-normal science and the extended peer community thus, in theory, should allow emergent complex systems to display the patterns of structure and relationships characteristic of ordinary complexity, and to avoid hegemony and fragmentation.

It is essential to stress that appreciation for diversity is not identical to relativism, for, if it were, post-normal science would have no hope of dispelling postmodern fragmentation. This fragmentation is a direct result of relativism: when every opinion is equally correct, the notion of a solid external reality dissolves, and knowledge fragments. In contrast, post-normal science espouses ontological realism:

> Post-normal science enables us to avert the nihilistic implications of post-modernism by observing that there really is a hillside there, even though no-one (including ourselves) can see it as a whole. (Funtowicz and Ravetz, 1994, p. 579)

Post-normal science is thus epistemologically relativist, and ontologically realist, whereas I would characterize postmodernism as epistemologically relativist and ontologically nihilist. The epistemological relativism of post-normal science is not identical to that of post-modernism: post-normal science's realist ontology moderates its relativism, just as in *anekāntavāda*. Hence, one might say that both philosophies adhere to what I called, in the first chapter, "qualified epistemological relativism". This is relativism qualified by realist ontology. The belief in a real external world allows for the possibility of debate: all viewpoints are worthy of consideration, but all are also responsible to "reality". There is an external arbiter, which the postmodern worldview lacks, and so all viewpoints are not equally correct under all conditions.

The phase space metaphor also motivates irreducible uncertainty, which is unavoidable in the study of higher dimensional objects. There are two sorts of uncertainty that need consideration in post-normal situations. The first is due to the partiality of any one view. No viewpoint, or projection on a subspace, can capture aspects of the issue lying in dimensions outside that subspace. Hence, there are always, for any given viewpoint, major hidden features of the object/issue. This sort of uncertainty can be alleviated, to some extent, by involving as many viewpoints as possible. However, some features of higher dimensional objects cannot be conceived in lower dimensions at all, or appear paradoxical. This corresponds to a form of irreducible uncertainty at the epistemic level: there are features of the object that, no matter how many viewpoints are involved, will not be captured. In other words, the higher dimensional object is more than the sum of its projections; it cannot be reduced to them.

Problem-Solving Strategies:

Funtowicz and Ravetz analyse scientific problem-solving using a diagram with "systems uncertainties" and "decision stakes" on the axes (1992, p. 253-254). The term "systems uncertainties" is meant to convey that the problem under consideration is not simply ascertaining a particular fact or value, but managing an entire complex system. Decision stakes are all the stakeholders' costs, benefits, and commitments attendant on resolution of the problem.

The diagram displays the epistemic and axiological aspects of scientific problems. Funtowicz and Ravetz choose uncertainties and values as dimensions to contrast post-normal science with traditional science, which assumes certainty and value neutrality.

Quantification along the diagram axes is limited to the categories "low", "medium", and "high". When both uncertainty and decision stakes are almost negligible, the problem-solving activity is "core science". When both dimensions are low, we have "applied science", which is equivalent to Kuhnian normal science in the policy context (Ravetz, 2004, p. 354). When one or the other dimension is moderate, the activity is "professional consultancy". Finally, when one or the other dimension is high, we have "post-normal science". The nesting indicates that the more complex include the less complex strategies. Professional consultancy, for instance, includes applied science.

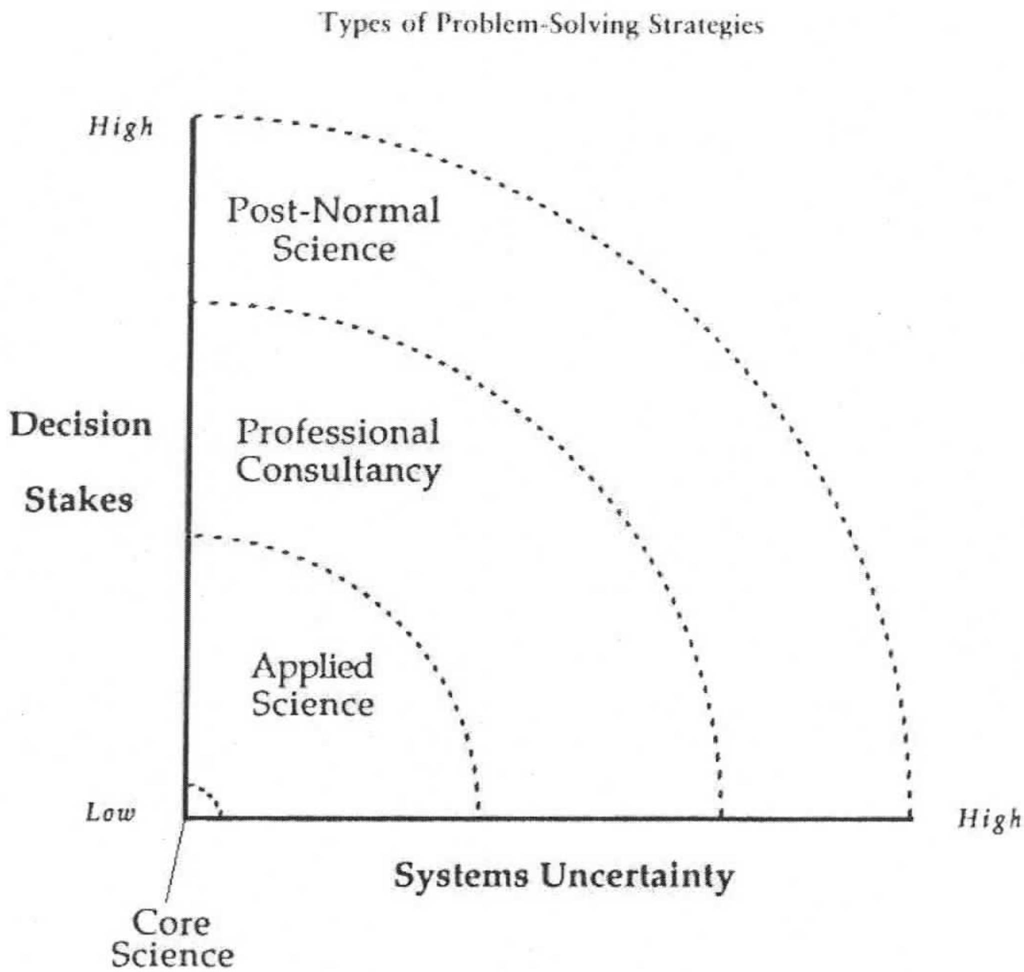

Figure 1 Adapted from Funtowicz and Ravetz (1992a), figure 11.1, p. 254, and Funtowicz and Ravetz 1993, figure 2, p. 746.

The diagram might seem to indicate an absolute separation between core and post-normal science, which is misleading, for Funtowicz and Ravetz note that there can be "continuity" between those two forms of problem solving.

Problems do not fall entirely in one category. They are multifaceted, and their different aspects may require different problem-solving strategies. Further, as an issue evolves, different problem-solving approaches may alternately be appropriate, and come to prominence.

Core Science:

Core science corresponds to pure research, which involves few (if any) external interests, and is curiosity-driven. The decision stakes are extremely low, and the systems uncertainties also tend to zero – the research is not revolutionary; it is not expected to threaten the paradigm within which it occurs. In Kuhnian terms, core science involves "puzzle solving". Quality assurance focuses on the scientific process itself.

Highly innovative research does not meet these criteria. Such research often involves guesswork and risk taking, and so has high systems uncertainties. When research threatens an established paradigm, the decision stakes are also high. Various stakeholders can find their life's work, their reputations, or their ideologies undermined by revolutionary undertakings. A good example is Galileo's astronomical research, which threatened the paradigm upheld by the Church of his time. Hence, as mentioned above, classic science is sometimes continuous with post-normal science. One does not have to go from core science through applied science and professional consultancy to get to post-normal science: surprises sometimes come up in the course of what was thought to be core science, and these can lead directly to post-normal situations. I will return to this continuity later in re-examining the relationship between normal and post-normal science.

Applied Science:

Applied science is what Funtowicz and Ravetz call "mission-oriented" (1993, p. 740). Their definition seems somewhat muddy: they state that applied scientists are typically employees, and that the problems they work on are chosen by supervisors, with technological application in mind. This suggests that applied science occurs outside academia, in arenas like industry and government research labs. However, the authors later state that an academic may work in either core or applied science. Academics are not restricted by employer wishes in selecting their research problems, so their work does not meet one criterion of the authors' definition. It seems to me that academic work in areas like engineering, and perhaps even climate science, falls in a category one might call "academic applied science", and so is spread between core and applied sciences, having aspects of both.

Applied science research is also "puzzle solving" within a paradigm. As with core science, the outcomes of the activity are largely predictable and reproducible. The decision stakes are low, even though there are costs and benefits, for these are minimal compared to those involved in professional consultancy and post-normal science.

The type of uncertainty involved in applied science is "technical uncertainty". Technical uncertainty might also be called "inexactness": it is random experimental error, adequately captured by statistical routines and procedures. Hence, quality assurance in applied science focuses on the problem-solving process and its technological products, and the criteria of quality are accepted and established.

Professional Consultancy:

Professional consultancy is scientific problem-solving geared toward serving a client. In this type of problem-solving, the decision stakes and/or uncertainties are moderate. Included in professional consultancy are fields like higher level engineering and medicine. The primary concern of such activities is the client's welfare, rather than contribution to scientific knowledge.

Unlike applied science, professional consultancy involves risk and responsibility, of which its practitioners have long been aware. Because skill and judgement are involved, professionals must undergo long training in "craftsmanship" – the ability to translate theory into everyday practice and troubleshoot unexpected situations. Professional consultants must be ready to improvise, as they face real-world situations that often do not match theory. Moreover, they must take responsibility for the outcomes of their improvisation.

Clients often seek out several consultations from different professionals. Such multiple consultations do not necessarily reflect on the first practitioner's competence, for the situations are complex and their evaluation involves subjective judgement. This is quite in contrast with core and applied science, where results are supposed to be reproducible. In these simpler types of problem-solving, disagreement signals trouble, and practitioners will work long and hard to obtain consensus.

The end product of professional consultancy also differs from those of core and applied science:

> The outcome of the process (which is conceived as continuously iterating) is not a general theory to be tested against particular facts, but rather a provisional assessment

> of the health of a particular system together with the relevant aspects of its environment. (Ravetz, 1987, p. 425)

The results are provisional and subjective, and, more importantly, recognized as such.

The uncertainty involved in professional consultancy is at what Funtowicz and Ravetz call the "methodological level". This sort of uncertainty stems from the individual professional consultant's decision-making methodology. It reflects the fact that different consultants make different assessments due to different weighting schemes.

Because judgement is involved, there are no clear criteria by which quality can be evaluated. Nor are there well-defined procedures by which it can be assured. In professional consultancy, the clients assess the quality of the work, though they have no technical training. Hence, in professional consultancy, there is already an extension of the peer community over that involved in core and applied science.

It is possible also for one individual to function in all four roles successively or simultaneously. This makes sense, given that a single individual will often deal with problems having aspects lying in all the problem-solving domains. For example, an academic might work mainly in core science, but also give advice on policy issues, and in so doing act as a professional consultant.

Post-Normal Science:

In post-normal problem-solving, decision stakes, systems uncertainties, or often both dimensions are high. The costs and benefits hinging on the decision may be enormous. For example, the decisions of the industrialized countries as to whether or how to mitigate sea level rise may determine whether or not the Bangladeshi delta, home to millions of people, will ultimately submerge. In the most extreme cases, the stakes may be the survival of human civilization, or of life on the planet.

Major technical hazards, such as poorly planned nuclear facilities and nuclear weapons, fit into the post-normal category. So do large-scale pollution problems, like acid rain, ozone depletion, and global warming. I also classify carbon dioxide capture and storage as a post-normal issue, because it is an immature climate change mitigation technology, with the potential for significant hazard.

Systems uncertainties need not be high or even moderate for an issue to classify as post-normal and become a policy flash point:

> If some institution sees its interests seriously threatened by an issue, then no matter how nearly conclusive the science, it will fight back with every means at its disposal, until such a time as further resistance would cause a serious loss of credibility in itself as a competent institution, and a damaging loss of power as a result. (Ravetz, 1987, p. 424)

This describes the evolution of the global warming debate very well. Scientists have known with high certainty for years that anthropogenic GHG emissions will increase global temperature, and that the effects will potentially be serious. Though there are still significant uncertainties about specific repercussions, temperature rise is virtually certain – indeed, it has already been observed and attributed scientifically to human activities. However, politicians, institutions, and large corporations with vested interests in the fossil fuel industry have successfully manipulated media coverage to make it appear that the scientific community is evenly split regarding the existence of anthropogenic climate change. They have delayed mitigation efforts by deliberately obfuscating the science, distorting uncertainty to their advantage.

Funtowicz and Ravetz make it very clear they do not intend post-normal science to displace the more traditional scientific problem-solving strategies:

> ...post-normal science functions as complementary to applied science and professional consultancy. It is not a challenge to the traditional practice of science, nor does it contest the claims to reliable knowledge or exclusive expertise that are made on behalf of science in its legitimate contexts. (Funtowicz and Ravetz, 1992a, p. 263)

Indeed, post-normal problem-solving includes applied science and professional consultancy, but is not limited to them, as post-normal issues subsume and surpass more narrowly defined problems for which these strategies are adequate.

Though Funtowicz and Ravetz do not question traditional science in its appropriate spheres, they do question the "quality" of traditional scientific problem-solving with regard to the environment, society, and ethics. In traditional science, these aspects are considered "externalities": any environmental, societal, or ethical difficulties are left to be managed by forces external to science. This criticism echoes Kuhn's comment on paradigm-driven problem selection:

> A paradigm can, for that matter, even insulate the community from those socially important problems that are not reducible to the puzzle form, because they cannot be stated in terms of the conceptual and instrumental tools the paradigm supplies. (p. 37)

According to Funtowicz and Ravetz, core and applied science lack quality with respect to ethics and societal problems because the paradigms that guide these activities prevent scientists from recognizing ethical and societal problems relevant to their work. Once again, many scientists engaged in or familiar with so-called "traditional science", including myself, would object to this claim as an over-simplified and substantially inaccurate generalization.

What about professional consultancy? This type of problem-solving frequently requires practitioners to deal with new and novel situations not covered by their formal training. Professional consultants are thus not insulated from certain problems in the same way as core and applied scientists. Evidently professional consultancy cannot be paradigm-driven to the same extent as core and applied science: any paradigm must at least be responsive and flexible enough to allow professional consultants to recognize those novel problems they must solve in order to serve clients.

It seems to me, however, that the extent of the paradigmatic failure is more severe – indeed, total – in post-normal science. This is because the complexity and extent of post-normal problems challenge the adequacy of any given paradigm, and therefore of normal science. This is a key point, to which I will return. Professional consultancy, in dealing with more well-defined, smaller scale problems, requires only acrobatics within and moderate extension of particular paradigms.

The uncertainties involved in post-normal situations are either epistemological or ethical:

> …decisions must be made involving the distribution of costs and benefits to different interests on the basis of future contingencies that are unknown and unknowable. (Funtowicz and Ravetz, 1992a, p. 258)

Funtowicz and Ravetz argue that both epistemological uncertainty and ethical uncertainty are "irreducible". I understand "epistemological uncertainty" to originate in the very nature of knowledge. Specifically, the irremediable uncertainty in post-normal problems indicates the failure of paradigmatic knowledge. Paradigms are conceptual schemes that organize knowledge – they lead us to see certain features of reality and neglect others. As such, they correspond to viewpoints, or projections on subspaces.

However, the complexity and scale of post-normal problems is such that any paradigm, viewpoint, or conceptual scheme is bound to neglect things that are, in reality, non-negligible. As a result, epistemological uncertainty cannot be reduced to the same extent as can technical and methodological uncertainty.

Funtowicz and Ravetz argue that in post-normal science "…the traditional domination of 'hard facts' over 'soft values' has been inverted" (1993, p. 750). On the problem-solving diagram, decision stakes correspond to values, because these are bound up with the interests of the various stakeholders, while systems uncertainties correspond, obviously, to uncertainty. In saying the domination of facts over values has been inverted, so that values now dominate facts, Funtowicz and Ravetz are suggesting that the axes of their diagram be inverted, so that values are the horizontal independent variable.

Facts are "soft" in the sense that they are extremely uncertain. The confidence interval associated with any value is huge, and the actual value correspondingly blurry. Because of this extreme uncertainty in facts, Funtowicz and Ravetz argue that appeals from policy-makers for sacrifice must be based on values:

> A new form of legitimation crisis could emerge; for if the authorities try to base their appeals for sacrifice on the traditional certainties of applied science…this will surely fail. Public agreement and participation, deriving essentially from value commitments, will be decisive for the assessment of risks and the setting of policy. (Funtowicz and Ravetz, 1993, p. 751)

Hence, facts and values are inverted in the sense that in the absence of hard facts, people will only sacrifice for the greater good due to value-based considerations.

In addition, Funtowicz and Ravetz argue that facts and values cannot be cleanly separated because irreducible epistemological uncertainty translates into irreducible ethical uncertainty:

> When any innovation…has the potential of producing evil in forms that cannot be predicted, it becomes impossible to apply simple ethical principles for the evaluation or control of the activities. (1992b, p. 964)

Ethical decision-making requires definite knowledge about how decisions will affect various stakeholders, but epistemological uncertainty precludes definite knowledge. This contrasts with the traditional model of science, in which facts and values are distinct, and the scientific endeavour value-neutral. It seems to me that ethical uncertainties are also irreducible because any decision will benefit some and harm others. Since harm cannot be avoided, the ethical tension associated with choosing between opposed interests cannot be dispelled.

Ravetz also seems to argue that, in a value-laden context, facts become negotiable:

> When science is involved in the policy process, particularly in the technical consultancy mode, then impersonal demonstrations give way to committed dialogue, and no facts are hard, massy, and impenetrable. They are used as evidence in arguments, necessarily inconclusive and debatable. (1987, p. 426)

I understand committed dialogue to mean dialogue between various parties committed to an issue because its resolution impacts their interests. Under this interpretation, when stakeholder values are involved, facts become debatable. I will further examine the relationship between facts and values, and their roles in policy-making, in chapter three.

Ravetz (1987) argues that the very source of extreme uncertainty, or "ignorance", paradoxically, is science:

> Now we face the paradox that while our knowledge continues to increase exponentially, our relevant ignorance does so, even more rapidly. And this is ignorance generated by science! (p. 423)

For example, when scientists created genetically modified organisms, they also generated new ignorance about how these organisms will interact with existent ecosystems. The invention of the internal combustion engine makes our ignorance of climate system dynamics relevant. Now, some proposed mitigation options, especially technical ones like carbon dioxide capture and storage, promise to generate new ignorance – for instance regarding the effect of oceanic sequestration on ocean ecosystems. This scientific creation of ignorance contradicts traditional notions of science as dispelling ignorance.

The epistemological and ethical uncertainties associated with post-normal issues alter the appropriate role for science in policy-making. Ravetz (1987) describes the traditional model of science in the policy process as "a meeting of two sides" (p. 422). Members of the public initiate the process, by communicating to decision-makers concern about a threat to some interest. In response to the complaint, decision-makers develop a strategy to address the perceived problem. In order to construct a workable solution, they need scientific input, and so scientists "provide the necessary facts", either by drawing on pre-existing knowledge, or by conducting "research to order" (Ravetz, 1987, p. 422). These scientific facts in turn determine the appropriate policy, or at least establish boundaries within which the policy-making process then occurs.

Decision-makers and the public have, in the past, seen scientific knowledge as an essential input, if not *the* essential input to policy formation. However, because of irreducible uncertainty, scientific facts alone are not adequate for policy in post-normal situations:

> When asked by policymakers: 'What will happen, and when?' the scientists must, in all honesty, reply in most cases: 'We don't know, and we won't know, certainly not in time for your next decisions.
>
> If this is the best that science can do…then the outlook for effective policy-making and for the credibility of science as a cornerstone of our civilization are not good. Yet, I believe, so long as scientists try to respond as if they face simple policy questions determined by simple factual inputs, the situation cannot improve.
>
> But what else can science do except provide facts for policy? (Ravetz, 1987, p. 417)

Further scientific research and technological innovation, though necessary, are not enough to solve post-normal problems. Nor, because of huge uncertainties and high decision stakes, can mathematical decision analysis provide determinate results and so guide policy (Funtowicz and Ravetz, 1992a, p. 270).

In order to meet post-normal challenges, argue Funtowicz and Ravetz, science needs to develop better procedures for self-criticism and quality control, and for integrating incomplete science into policy debates. The environmental problems created by science-based technology can only be solved through "the creation of a new, appropriate sort of science" (Ravetz, 1987, p. 417).

As evidenced in the above discussion of core science, applied science, and professional consultancy, there is already a precedent for differentiation in science. Hence, according to Funtowicz and Ravetz, it is not completely irregular to posit a new sort of science, emerging in response to the unprecedented ecological problems our civilization today faces. In the new science envisioned by Funtowicz and Ravetz, "…uncertainty is not banished but is managed, and values are not presupposed but are made explicit. The model for scientific argument is not a formalized deduction but an interactive dialogue" (1993, p. 740). Funtowicz and Ravetz (1993) argue that in order to evaluate the quality of scientific information, "the human face" of science must be taken into account, as well as the fact that scientific information can be of "variable quality". They envision decision-making in the new science as a dialectical process: iterative and interactive, rather than a one-time event. Awareness of the boundaries of scientific knowledge informs this decision-making process: there is "…a permeable boundary between knowledge and ignorance that allows us to review the antecedents and mode of gestation of a scientific fact" (Rayner, 1987, p. 433).

It seems to me that science has always involved interactive dialogue, as well as formalized deduction; these two forms of argumentation are complementary, not exclusive. Hence, Funtowicz's and Ravetz's portrayal of traditional science is not entirely accurate. It seems to me that the difference between post-normal science and traditional science is not that the former is interactive, and the latter deductive, but that in post-normal science, the interaction is between all members of the extended peer community, rather than just the expert members of the scientific peer community. In general, my impression is that Funtowicz and Ravetz's description of "traditional science" is an oversimplified straw man. The simplifications and generalizations they make are tailored to provide a starting point for their critique. This I see as a major weakness of post-normal science as Funtowicz and Ravetz have formulated it. The philosophy would rest on a firmer foundation, and find wider audience with scientists, if it incorporated a more nuanced and accurate description of "traditional science".

Funtowicz and Ravetz propose that we can accomplish quality assurance in post-normal science by extending the peer community to include all stakeholders, such as interest groups, concerned citizens, policy makers, scientists, engineers, technicians and others, whether or not they have scientific or technical qualifications. They write, "…the quality assessment of the scientific materials in such circumstances cannot be left to the experts themselves; in the face of such uncertainties, they too are amateurs" (Funtowicz and Ravetz, 1992a, p. 254). Citizens can engage in science through common-sense evaluations of issues in policy processes, through community-based research, by developing alternative technologies for local contexts, and through investigative journalism, for example. Ravetz does acknowledge that in the process of transitioning to post-normal science, there is a danger of losing quality, for it is possible that "in the turbulence of change, all standards will be lost, and then any charlatan or demagogue can successfully claim to be a scientist" (2004, p. 355).

The extended peer community, which Funtowicz and Ravetz identify as the distinguishing feature of post-normal science, brings along with it "extended facts", including local and indigenous knowledge, anecdotal evidence, and the results of informal research undertaken by citizens. These extended facts, argues Ravetz, can be as crucial to solution of the problem as "the selected sample of the research results which is permitted to enter the public domain" (1997, p. 534).

Policy-making would then involve a dialogue between all the stakeholders in the peer community. According to Funtowicz and Ravetz, extending the peer community democratizes science; however, this democratization is not for its own sake, but in the interests of assuring quality, for "…the official experts can be deficient on several fronts:

problem identification, traditional knowledge, evaluative understanding, and intuitive and particularized knowledge" (1992a, p. 270). The intent is not to turn over the whole scientific enterprise to laypersons. Rather, it is to enrich scientific problem-solving by including non-scientific perspectives:

> Knowledge of local conditions may not merely shape the policy problems; it can also determine which data are strong and relevant. Such knowledge cannot be the exclusive property of experts whose training and employment incline them to abstract, generalized conceptions. Those whose lives and livelihood depend on the solutions will have a keen awareness of how general principles are realized in their backyards. (Funtowicz and Ravetz, 1992a, p. 271)

Scientists, trained to think in generalities, are unlikely to possess local, context-specific knowledge. Hence, the extended peer community and extended facts enrich understanding by incorporating multiple viewpoints and attendant specialized knowledge.

Further, non-scientists in the extended peer community are often better qualified, and more highly motivated, to perform adequate quality control:

> ...persons directly affected by an environmental problem will have a keener awareness of its symptoms, and a more pressing concern with the quality of official reassurances, than those in any other role. They thus perform a function analogous to that of professional colleagues in the peer-review or refereeing process in traditional science. (Funtowicz and Ravetz, 1993, p. 752)

Funtowicz and Ravetz envision, in the end, a new form of applied science in which citizens research and monitor environmental problems. These citizens, though without scientific training, would have attained a requisite level of "competence", and derive their authority from community standing (Funtowicz and Ravetz, 1992a, p. 272). Through their participation in science and policy, expertise would be "democratized".

Funtowicz and Ravetz specify that post-normal science need not entail abandoning conflicting interpretations of facts and values, or unquestioning acceptance of others' views. What post-normal science does entail, however, is willingness to understand others' viewpoints, even in conflict:

> One must be able to assess productions from several points of view in succession, by means of an imaginative sympathy that involves seeing one's own role, one's own self, from a slight distance. (Ravetz, 1987, p. 428)

In other words, as mentioned before, post-normal science is not absolutely relativist; realist ontology preserves the possibility of debate. However, a certain epistemological relativism promotes respect for other viewpoints.

Ravetz (1987) argues that, in order to overcome ignorance, we need "control". Francis Bacon's maxim, "Knowledge and power meet in one" does not describe how knowledge functions in the post-normal context (Ravetz, 1987, p. 416). Knowledge and subsequent technological power are not sufficient; rather, we also need "control", which includes "goals, values, a historical element, and the unknowable future" (Ravetz, 1987, p. 416).

To illustrate the difference between power and control, Ravetz (1987) gives, as an example, a dam:

> It is relatively easy to build a dam to hold back river water; there is power. But to predict and eventually manage the manifold environmental changes *initiated* by that intrusion is another matter. The flows and cycles of energy and materials that are disrupted by the dam will, all unknown to us, take new patterns and then eventually present us with new, unexpected problems. (p. 419)

Similarly, fossil fuel technologies have given us great power, literally and figuratively, but we do not currently have control over their effects on the climate system and the consequences for us.

How can one achieve control when ignorance is irreducible? In applied and core science, ignorance has traditionally been tamed by examining its border with knowledge: scientists choose research problems based on educated guesses about where the border can be penetrated and new knowledge gained. Achieving control in post-normal situations involves a similar examination of the border between knowledge and ignorance. In this case, the examination takes the form of "continuous monitoring" to pick up signs of impending harmful events. The decision process must be "iterative, responding in a feedback loop to signals from the total environment of the operating system" (Ravetz, 1987, p. 429). In addition, the system and associated institutions must be constructed with ignorance in mind, so that they can respond to warning signals in a timely fashion.

Achieving control requires making values explicit. This is because, when ignorance is extreme, decision-making requires non-quantifiable "discretion". Costs and benefits, both probable and hypothetical, must be weighed, and this weighting is inevitably value-based. The task is complicated by the fact that many costs and benefits cannot begin to be foreseen when ignorance is severe.

Through continuous iterative control, discretion, and by making cost, benefits, and values explicit,

> ...we do not conquer ignorance directly, for that can be done only by replacing it with knowledge. But we cope with it and we ensure that by being aware of our ignorance we do not encounter disastrous pitfalls in our supposedly secure knowledge or supposedly effective technique. (Ravetz, 1987, p. 429)

Unfortunately, our record on achieving control is not good. Short term concerns, such as immediate affluence and welfare, tend to outweigh long-term concerns. This is certainly the case with climate change. In order to achieve control, we would need to approach issues from long-term perspectives.

According to my understanding, control includes technical monitoring systems on industrial facilities, but the concept can also be extended to social systems and knowledge-generation. The extended peer community is then a sort of monitoring system that warns of gaps in scientific and technical knowledge, and associated impending dangers.

Finally, in order to cope with ignorance, we must overcome our conception of science as "the facts". By "fact", Funtowicz and Ravetz seem to mean a piece of information known with a high degree of certainty. Science can be – indeed *must* be – useful for policy even when the information it provides is not completely certain. This is what Funtowicz and Ravetz mean by integrating incomplete science into policy formation.

Our image of science as "the facts" has caused widespread misunderstanding of climate science. Specifically, the general public misunderstands the extent and nature of the uncertainty in climate change predictions. Because of the notion that science solves problems exactly, climate science, which gives predictions in the forms of ranges in temperature rise, sea level rise, and severity of impacts, appears suspect. Climate science is arguably not fundamentally different from other sciences: there is no exact science. However, because global warming is such a charged issue, climate change deniers have exploited and misrepresented the uncertainty associated with climate predictions, using to advantage the common conception of science as producing exact "facts". Until recently, laypersons have widely believed it uncertain the climate will warm at all, while no such uncertainty exists in the serious scientific community.

Through the extended peer community, continuous, iterative control, and the use of scientific information, even if uncertain, we can make our ignorance "usable". The concept of "usable ignorance" renders post-normal science fundamentally different from traditional types of science.

One might argue that post-normal science is not really a distinct type of science, but rather a form of public participation in the policy-making process. However, in policy-related science, and particularly high-stake post-normal situations, scientists do not have the same freedom to choose research questions as they do in core science:

> The problems are set and the solutions evaluated by the criteria of the broader communities. Thus post-normal science is indeed a type of science and not merely politics or public participation. (Funtowicz and Ravetz, 1993, p. 750)

Funtowicz and Ravetz do not simply envision both scientists and other members of the extended peer community providing inputs to policy. Rather, the very problems science investigates in post-normal situations are defined by the broader community, so that the broader community provides inputs to science as well.

It seems to me, however, that the problems investigated by applied science have always been influenced, to some extent, by the broader community, so that this is not really an innovative feature of post-normal science. What does define post-normal science is, I think, the intensely value-laden context in which such science is done, a context that informs research questions, and influences the disposal of research results, and which scientists cannot ignore.

Post-normal environmental problems are "total environmental assessments", total because they involve facts, interests, values, and lifestyles, and there is no accepted methodology for solution (Ravetz, 1987, p. 425). Funtowicz and Ravetz (1992a, p. 262-263) describe the evolution of a post-normal problem. The initial phase of public discussion resembles political debate. Because the decision stakes are high, even in the absence of conclusive evidence, stakeholders present their opinions as if they are fact, and manipulate evidence and uncertainties to discredit opponents. This positioning takes place, of course, with the aid of the media. In the case of a controversial new technology, proponents will frame it as applied science, which connotes surety and low risk levels, while opponents will stress uncertainty and ethical problems.

According to Funtowicz and Ravetz, though issues are never de-politicized, open dialogue ensures that political concerns are not hidden. I do not think such an open dialogue has occurred with global warming, or that it is likely to occur in the near future. It would indeed be surprising to hear Exxon executives, for instance, admit that discrediting climate change science is in the interest of their continued wealth and power.

Funtowicz and Ravetz state that in form, post-normal debates are political, while in substance they are supposedly scientific (Ravetz, 1987, p. 425?). This is a good description of the climate change debate, specifically the arguments put forth by climate change deniers. I do not, however, see how attempts to fake an appearance of scientific rationality are compatible with open debate.

Debate over a post-normal issue stimulates new research, which introduces new facts. In addition, positions and values are clarified. In this way, argue Funtowicz and Ravetz, the issue is tamed, and professional consultancy becomes adequate, though there is no guarantee the problem will become tractable quickly enough to prevent irreparable harm. However, it seems to me that new research and the clarification of positions only reduce systems uncertainties, and do not necessarily lower decision stakes. Hence, according to the problem-solving strategy diagram, the issue would still count as post-normal, even if uncertainties were managed down to a moderate level. The authors thus seem to contradict their own classification scheme.

Normal and Post-Normal Science Revisited:

There is an obvious parallel between hegemony and normal science. In normal science one paradigm dominates to the exclusion of all others, and novelty is suppressed: only problems recognizable under the paradigm are admitted. Paradigms are thus hegemonies in knowledge.

Further, fragmentation, as described by Funtowicz and Ravetz, characterizes pre-paradigmatic science and scientific crises. In both such periods, rival paradigms vie for supremacy, and there is, as a result, continuous novelty.

The analogy goes further: "creative tension" as defined by Funtowicz and Ravetz plays an obvious role in scientific revolutions. Synthesis corresponds to a paradigm shift, which resolves previously existent contradictions.

Funtowicz and Ravetz speak of continuity between post-normal science and classical philosophy of science. Specifically, in both post-normal science and scientific revolutions, decision stakes and uncertainties can be high. They also argue that post-normal science will supplant normal science as the mode of doing policy-related science, because puzzle-solving is a grossly inadequate strategy when it comes to total environmental assessments. Is the shift from normal to post-normal science then simply another scientific revolution in the Kuhnian sense? Ravetz indeed classifies this shift as a Kuhnian paradigm shift (2004, p. 353).

However, upon further consideration, I do not think this characterisation accurate: post-normal science cannot be completely accounted for within Kuhn's framework. Funtowicz and Ravetz are really positing limits on the usefulness of single-paradigm science, rather than simply seeking to replace one paradigm with another. In post-normal situations, any given conceptual scheme, taken alone, is inadequate.

The authors note that post-normal situations can be managed down through dialogue and new research, so that they become amenable to professional consultancy and applied science approaches. Does this managing down correspond to a return to normal scientific practice? I do not think so, because when highly complex systems are involved, there must remain an element of interdisciplinarity not present in normal scientific practice. For example, the various chemists, biologists, physicists and other scientists whose combined efforts make up the discipline "climate science" all arguably work under different paradigms, but are engaged in a common scientific endeavour.

Furthermore, I doubt that post-normal problems can, as a class and using general methods be managed down once and for all to the professional consultancy and applied science levels. Because the systems involved are complex in both their natural and human aspects, it is impossible to develop general rules and reduce the solution of post-normal problems to an algorithmic activity. The uncertainty involved is epistemological – it stems from the nature of knowledge itself. As such, it cannot be done away with by substituting a new conceptual scheme or paradigm; all knowledge systems will be plagued by uncertainties when applied to a post-normal situation. As such, every problem must be approached on an individual basis, using holistic strategies to capture as many relevant features of the problem as possible. Accepting holism as a heuristic is not the same as adopting a conceptual scheme or paradigm. Rather, it involves recognizing the inadequacy of any one paradigm.

Conclusion:

One of the most salient features of post-normal science for this thesis is its philosophical basis, specifically the multidimensional phase space metaphor. Funtowicz and Ravetz compare total environmental assessments with objects in this phase space. These objects can be studied by taking projections on subspaces, but no single projection can capture all the object's features. Similarly, no one viewpoint regarding a post-normal issue can capture all its complexity. Essentially, Funtowicz and Ravetz challenge the adequacy of paradigmatic science, and paradigms in general, for dealing with post-normal issues.

The inadequacy of single viewpoints motivates Funtowicz's and Ravetz's main recommendation for quality assurance in post-normal science: the extended peer community. By including as many viewpoints as possible, we have a hope of capturing the complexity of post-normal issues. In addition, because of extreme ignorance regarding the effects of new technologies, we need "continuous, iterative control" to pick up danger signals. The need for iterative control has important implications for technical mitigation options like carbon capturing and sequestration.

The greatest lacuna in Funtowicz's and Ravetz's guidelines for post-normal science, as I see it, is how one decides who is a legitimate member of the extended peer community.

In the case of climate change, powerful groups with vested interests in the fossil fuel industry have obfuscated science in order to delay mitigation and maximize their own wealth, and the environmental repercussions of climate change will, as a result, be worse. Hence, one might argue that the extended peer community has so far worsened, rather than improved the quality of climate policy.

Chapter 4: Post-Normal Science and *Anekāntavāda:*

Introduction:

As the reader may already have noted, the ontological and epistemological stances of post-normal science are very similar to those taken by the Jaina philosophers. Other striking resemblances exist between post-normal science and the Jaina theory of reality, *anekāntavāda*. In the present chapter, I will identify parallels between the two philosophies, as well as points of divergence.

Post-normal science and *anekāntavāda* involve very similar epistemologies and ontologies, and deal with the same epistemological problem, namely the failure of conceptual knowledge. We make sense of reality using conceptual schemes, which organize and render coherent our experience. The Jainas call these conceptual schemes standpoints, while in Kuhn's terminology they are paradigms, and in post-normal science, they are projections on subspaces of a multidimensional phase space. For the Jainas, Kuhn, and Funtowicz and Ravetz, these conceptual schemes are all incomplete; any given scheme cannot capture all the features of reality pertinent in different situations and to different people.

My goal in comparing *anekāntavāda* and post-normal science is to facilitate a deeper understanding, and perhaps a refinement of Funtowicz's and Ravetz's theory. The comparison is very much in the spirit of post-normal science: by juxtaposing two philosophical perspectives, and bringing them to bear on the same problem, I may gain insight into the philosophical perspectives under comparison. I will also see the problem itself from two slightly different angles, and may thereby gain deeper understanding.

I have chosen to compare post-normal science with a similar rather than divergent philosophical position for several reasons. Firstly, my aim is to flesh out post-normal science. By facilitating a constructive dialogue between the two philosophies, I hope to illuminate Funtowicz's and Ravetz's theory, to elucidate some of its points, and to suggest refinement where indicated. I believe that the Jaina tradition is well-suited to this task, both because of its similar view of reality, and its degree of development. *Anekāntavāda* is thousands of years old, and presumably more refined and explicated than post-normal science. Hence, it may provide insight into how Funtowicz's and Ravetz's theory can be developed and clarified.

I have deliberately chosen not to assume an opposed philosophical position and then argue against post-normal science. Such an approach would be *ekānta*, or one-sided. It is exactly such polarized debate that the Jaina philosophers contend obscures the true nature of empirical reality. I acknowledge that this contradicts the received wisdom in Western philosophy and science, where debate between opposed factions is thought to clarify the positions of each faction, and often ultimately to reveal one position as superior. According to Jainism, however, such a style of argument entrenches beliefs and encourages dogmatic intolerance. Hence, such an approach would run the risk of hampering rather than enhancing understanding.

I point out that it is through polarized debate that science "advances". The outcome is a switch from one conceptual scheme or paradigm to another, rather than the realization that all conceptual schemes are incomplete, and that each, when properly qualified, may have something to offer. Such a mode of investigation and debate is not only *ekānta*, but also characteristic of Kuhnian normal science. It is perhaps not appropriate for an examination of post-normal science, which does not seek to replace one conceptual scheme with another, but to demonstrate that the usefulness of conceptual schemes – that is, viewpoints, or projections on subspaces – is context-dependent. As Funtowicz and Ravetz emphasize, post-normal science is not intended to replace traditional forms of science, which are still valid within their appropriate domains, but to provide a framework for tackling a new set of problems for which traditional science is inadequate.

I could alternatively have selected an opposed philosophical tradition, and then tried to reconcile it with post-normal science, showing under which circumstances each philosophy is valid. However, this approach does not bring to bear the insight of an ancient philosophical tradition, developed in a different context. Moreover, Funtowicz and Ravetz have already discussed extensively the differences between and appropriate contexts of post-normal science, core science, applied science, and professional consultancy. Through this discussion, they have also implicitly compared and reconciled post-normal philosophy with its most important alternatives. Hence, if I were to compare post-normal science with an opposed philosophical position, I would simply repeat work already done by Funtowicz and Ravetz.

Elephants, Phase Spaces, and the Extended Peer Community:

The *Ekānta* Nature of Traditional Science:

Funtowicz and Ravetz write the following about traditional science:

> The classic scientific perspective, involving a reductionist quantification, attempted to legitimize itself in a logically

> closed way. It claimed to be both 'rational' and 'neutral', thereby claiming a privileged status, while denying that it was doing so. (Funtowicz and Ravetz, 1994, p. 579)

Claims to neutrality – that is, objectivity – are one-sided, for they assume that knowledge is not perspectival, and that scientific observation and theorizing are unaffected by interests and values. If knowledge is not perspectival, there can be one "correct" conceptual scheme, or paradigm, which captures reality completely.

As I noted in my examination of *anekāntavāda*, the Jainas believe that what features of reality one attends to at a given time depend on one's interests at that time. In light of the Jaina theory, conceptual schemes are inherently value-based: what is important to us determines what we take note of in the world around us. Since scientific knowledge is conceptual, it cannot avoid being informed by values, even though its object of study, the natural world, is value-neutral. In failing to acknowledge its dependence on viewpoints and interests, traditional science, as described by Funtowicz and Ravetz, is *ekānta*.

Reductionism, *Ekāntavādism*, and the Possibility of Prediction:

Commentators on traditional science have often claimed that it is reductionist, and that this reductionism is the source of many problems – such as environmental degradation – associated with the application of scientific knowledge. I will now examine the validity of this claim, and the relationship between *ekāntavādism* and reductionism.

Firstly, I note that Funtowicz's and Ravetz's multidimensional phase space metaphor is highly analogous to the elephant metaphor that elucidates the epistemology and ontology of many Indian philosophical systems, including Jainism. Just as no single tactile viewpoint can reveal the elephant in its entirety, for a system in multidimensional phase space, "…no single perspective from within a subsystem of fewer dimensions can fully encompass the reality of the whole system" (Funtowicz and Ravetz, 1994, p. 575).

Funtowicz and Ravetz argue the following about emergent complexity, modelled using the multidimensional phase space metaphor:

> This concept helps us to appreciate that there is no single perception providing a comprehensive or adequate vision of the whole issue, nor any particular criteria of quality that can hegemonically exclude all others…. In our heuristic phase space for emergent complexity, the analogous property of dimensionality is that no particular partial view can encompass the whole. (Funtowicz and Ravetz, 1994, p. 578)

Each of the blind men in the Indian parable argues that his partial perception of the elephant is true and complete, and that the perceptions of the other men are wrong. In doing so, he makes an *ekānta* claim and fragments the truth. Similarly, Funtowicz and Ravetz write, "To take any particular perception, or projection onto a subspace, as the true, real or total picture, amounts to reductionism" (Funtowicz and Ravetz, 1994, p. 575). Hence, reductionism, as defined by Funtowicz and Ravetz, is analogous to *ekāntavādism.*

However, I do not think that *ekāntavādism* and reductionism are identical. I take as my definition of reductionism the act of explaining a phenomenon by simpler phenomena, entities and processes that underlie it (Oxford English Dictionary Online). These simpler phenomena, entities and processes are the concepts used to make sense of the initially infinitely faceted object under study. In other words, they *reduce* its infinite number of facets to a set that can be grasped and organized within one conceptual scheme. According to this understanding, all conceptual statements are reductionist. Since *anekānta* statements are conceptual, they are also reductionist. Hence, reductionism does not necessarily imply *ekāntavādism*. What differentiates *anekānta* from *ekānta* statements is not that they are non-reductionist, but that they explicitly acknowledge their reductionism.

In chapter one, I identified a third class of propositions, which are neither *ekānta* nor *anekānta*, non-one-sided but also falsifiable. These propositions are qualified by specific conditions under which they are purported true. For example, the claim that the elephant is like a wall given that the part in question is the side can be shown false if the side is demonstrably more like a sun-warmed boulder. This claim is conditional in that it applies to only part of the elephant. Analogously, in multidimensional phase space, one might make a claim that applies only to a subsystem, or part of the whole system. The set of conditions must specify what part of the system the claim applies to, as well as the purpose of the conceptual scheme applied. One can then verify whether or not the conceptual scheme adequately describes the subsystem relative to the purpose at hand. In such a case, the claim is arguably not *ekānta*, because it is qualified, but it is reductionist, because it purports to definitively capture a subsystem *for a given purpose* using one conceptual scheme. Note that I am assuming here that the system in its entirety is so complex (like the elephant) that it cannot be described adequately by one conceptual scheme, for any purpose. Hence, one can only make claims about subsystems, which are relatively simpler.

I make these points specifically with scientific knowledge in mind. Reductionism is not a feature solely of science, but of all conceptual knowledge. It is not reductionism, but rather *ekāntavādism* that has been the problem with traditional science, for one-sidedness has led to incautious and contextually inappropriate application of scientific knowledge.

Conceptual statements can either be asserted as absolute truth, or in such a way as to acknowledge their incompleteness, and make room for features of reality that they do not, and cannot capture. That is, conceptual statements can either be asserted so that they ignore or acknowledge their reductionism. *Anekānta* statements, because they are non-falsifiable, are of questionable use in science. However, by the assertion and then refutation or confirmation of qualified propositions, relative to *specified* subsystems and purposes, scientific investigation can proceed in a non-one-sided fashion.

Note that according to this argument, post-normal science is reductionist, because it is conceptual. I thus do not claim non-reductionism as a possibility for post-normal science, or for any other kind of science. I also note that, while post-normal problems may absolutely require non-one-sided statements, it is also possible (even if not imperative) to make non-one-sided statements in core science, applied science, and professional consultancy.

One might argue that, from the Jaina viewpoint, subsystems, like the larger systems of which they are part, can be described by an infinite number of viewpoints. Hence, it should not be possible to describe even subsystems for a given purpose using one conceptual scheme. However, from mathematics, we know that there exist different sizes of infinity, so that a subsystem may have strictly fewer dimensions than the whole system, even if both are infinitely faceted. Furthermore, from dynamical systems theory, we know that it is sometimes possible to describe a system with infinite dimensions using a finite, and even a very small number of dimensions. Specifically, such descriptions are possible near equilibrium solutions (Gollub and Langer, 1999).

Two questions then seem essential to answer. Firstly, under what conditions is it impossible to describe a system adequately (relative to a given purpose) using a single conceptual scheme? Secondly, under what conditions is it impossible to describe a system adequately relative to a given purpose using a finite number of conceptual schemes? It seems to me that the answers to these questions would define the subset of problems to which adequate solutions, for given purposes, can be known. The complement of this subset would include those problems in whose solutions there will always be an element of irremediable uncertainty. That is, uncertainty is not irreducible simply by virtue of a system being nonlinear and complex, even infinite-dimensional. This suggests a correction to the following statement by Funtowicz and Ravetz:

> With the phase space metaphor, we can appreciate irreducible uncertainty as a systemic property of emergent complexity. For any particular perception (or projection on a subspace) will be incapable of describing what goes on in the dimensions lying outside its scope. But there can be effects on the total system, including the subspaces under study, deriving from those ever-present 'hidden variables'. Should there be a change of perception to encompass them, then other subspaces will be lost to view, so that some irremediable uncertainty will always be present.
> (Funtowicz and Ravetz, 1994, p. 578)

The argument depends on the assumption that hidden variables affect the subspaces under study. If the climate system were an elephant, Funtowicz and Ravetz might say that the behaviour of the ear cannot be predicted without taking into account the tail, but that switching to a viewpoint that reveals the tail obscures the ear. Note, however, that whether or not the ear flaps may have nothing to do with whether or not the tail swishes, and everything to do with whether or not a fly lands on the ear. Of course, if there are many flies in the vicinity, one expects much tail swishing *and* ear flapping, but this does not imply a causal relationship between the two actions.

In light of the above-discussed fact from dynamical systems theory, it seems to me that further analysis, specifically examination of the relevant mathematics, is needed to determine when uncertainty is irreducible and when it is not. Nonetheless, in absence of definite knowledge, when dealing with complex environmental systems, the precautionary principle dictates that we should assume the presence of non-negligible hidden variables unless evidence demonstrates otherwise.

Reductionism and Climate Science:

Climate scientists sometimes claim that their science is not reductionist (see, for example, Monahan, 2006, p. 14). Because the climate is a complex, nonlinear system, its study calls on many different sciences, from biology to physics, and involves the examination of multiple "simpler" systems, as well as interactions between these systems. Different scientists, for example chemists and palaeontologists, identify and study the phenomena of interest in their disciplines using different conceptual schemes, so this is genuinely a multi-paradigm undertaking. Since the scientists who collaborate on climate-related problems recognize each others' paradigms as valid, these scientists are *anekāntavadins*, at least when it comes to other scientists. To drive this home, compare the interdisciplinary attitude prevalent in climatology with Ernest Rutherford's famous quote: "In science there is only physics; all the rest is stamp collecting".

However, as I argued in the last section, all conceptual knowledge is reductionist. Climate science, because it is conceptual, is also reductionist. When climate scientists claim that their science is non-reductionist, what they really mean is that it does not purport to describe the climate using a single conceptual scheme, but instead approaches the problem from as many different angles as possible.

It seems useful to introduce here the concept of degree of reductionism. In order for conceptual knowledge to be non-reductionist, one would have to see reality simultaneously using an infinite number of conceptual schemes, capable of capturing all its features. However, because human beings can only assume one conceptual scheme at a time, this is impossible. By successively assuming different conceptual schemes though, one can, in a way, *reduce reductionism.* Such an approach can never capture all features of the object under study, but it certainly will capture more than if one paradigm were used exclusively. Thus, I would argue that climate science is *less* reductionist than physics, chemistry, or biology taken alone.

At this point, someone might claim that we should, if we wish to avoid reductionism, instead abandon science altogether and rely on some more "holistic" form of knowledge. The faultiness of this reasoning should, by now, be self-evident. Jaina philosophers would respond that *all* conceptual schemes, taken alone, are *ekānta.* We cannot, by avoiding science and substituting other, "holistic" forms of knowledge, solve the problem of scientific reductionism, for *all conceptual schemes, including those that purport to be "holistic", are context-dependent, and all knowledge gained from any one conceptual scheme is incomplete, and reductionist.* The problem with some science is not that is has been reductionist, but that, like many other conceptual schemes, it can be *ekānta*, failing to acknowledge its partiality. Excluding scientific knowledge when forming an understanding of reality does not increase holism, but rather decreases it, for scientific knowledge has as many unique insights to offer as does any other form of knowledge.

The Extended Peer Community and Reducing Reductionism:

The extended peer community is the tool recommended by Funtowicz and Ravetz to democratise science and assure the quality of science and policy. The recommendation is based on the insight that no one viewpoint, or projection on a subspace, can reveal all features of reality, and that different viewpoints are needed in order to see the features relevant to a given problem.

The recommendation for the extended peer community is positively Jainist:

> Now, any one of the actors in such a process must, if she or he is to really effective in a cooperative endeavour, undertake a task that is not traditionally associated with science: to appreciate another person's point of view….each participant must appreciate what it is that another is invoking, explicitly or implicitly, when making points about the quality of contested materials. (Ravetz, 1987, p. 427)

Along similar lines, Ravetz writes:

> One must be able to assess productions from several points of view in succession, by means of an imaginative sympathy that involves seeing one's own role, one's own self from a slight distance. (1987, p. 428)

Compare these statements with Kapadia's description of the *anekānta* doctrine's utility:

> …this doctrine of *anekānta-vada* helps us in cultivating the attitude of toleration towards the views of adversaries. It does not stop there but takes us a step forward by making us investigate as to how and why they hold a different view and how the seeming contradictories can be reconciled to

> evolve harmony. It is thus an attempt towards syncretism. (Matilal, 1981, p. 6)

Hence, the Jainas and Funtowicz and Ravetz similarly call for toleration of and an attempt to understand other viewpoints. Funtowicz and Ravetz specifically call for awareness of both the explicit and implicit claims made by others; that is, they call for awareness of the contexts of different claims. This "imaginative sympathy" makes it possible to appreciate differing criteria of quality, and thus to carry out adequate quality assessment of science and policy (Ravetz, 1987, p. 430). I note that Funtowicz and Ravetz call for awareness of implicit claims, while the Jainas advise that all implicit claims should be made explicit in order to avoid polarized dialogue.

There is, of course, a problem: how far should toleration go? Should a climate scientist tolerate commentary from an oil-funded geographer or economist denying climate change, and masquerading as a qualified scientist? That is, who qualifies as a legitimate member of the peer community? I will revisit this question later.

The extended peer community produces a more holistic picture of reality by collating as many conceptual schemes as possible. Thus, post-normal science, like *anekāntavāda* is a philosophy of synthesis. In viewing reality using multiple conceptual schemes, the extended peer community reduces reductionism. Though the primary purpose of post-normal science is to produce as accurate a picture of reality as possible, as a by-product of this goal, it also promotes intellectual non-violence.

As I mentioned in chapter one, the Jainas admit one way to have complete knowledge of reality – through direct intuitive experience unmediated by concepts. Such understanding is not simply attained by collating all possible viewpoints, as in the extended peer community, but through a final gestalt shift, in which the parts gel into a whole experienced free of concepts. However, it is not clear to me that non-conceptual direct experience would be particularly useful in post-normal science, and I will not deal with this type of experience here.

The climate science peer community itself is "extended" as compared to normal scientific peer communities in that it is truly interdisciplinary. This peer community already recognizes the multifaceted nature of the global climate system, and the peer community continues to expand. Integrated assessment, which takes into account how human systems interact with the climate system, is a burgeoning field, and brings economists, financial mathematicians, and social scientists together with natural scientists. Hence, integrated assessment is *less* reductionist than the natural climate science that has prevailed until recently.

In addition, there has been a broad debate about global warming, involving politicians, interest groups, media, scientists, and ordinary citizens. I wish to examine the role this broader peer community has played in shaping the global warming debate, but first it is necessary to scrutinize the relationship between facts and values in post-normal science.

Facts and Values:

Funtowicz and Ravetz argue that, in post-normal situations, facts and values can be inverted, so that values are the independent and facts the dependent variable. One possible interpretation of the claimed "inversion" is simply that in post-normal situations, we are more certain of our values than we are of the facts. The following statement suggests this interpretation:

> Whereas formerly we had the contrast between *hard* science and *soft* values, now we must take hard decisions

> between discrete alternatives, with only soft scientific inputs to them. (Funtowicz and Ravetz, 1992a, p. 258-259)

Funtowicz and Ravetz do not simply suggest an inversion in levels of certainty, however, for the uses of the words "hard" and "soft" also suggest an inversion in negotiability. Namely, in traditional science, values were seen as negotiable, whereas facts were not. Do Funtowicz and Ravetz mean to argue, then, that facts are negotiable in post-normal science, more negotiable than values? The following statement by Ravetz suggests that they do mean this:

> When science is involved in the policy process, particularly in the technical consultancy mode, then impersonal demonstrations give way to committed dialogue, and no facts are hard, massy, and impenetrable. They are used as evidence in arguments, necessarily inconclusive and debatable. (Ravetz, 1987, p. 426)

What does it mean for facts to be debatable? What does it mean for them to depend on the independent variable, values?

A Jaina would argue that facts are negotiable insofar as the facts one attends to depend on one's values. For the Jainas, values truly are the independent variable, for they determine what facets of reality we see, and hence which facts are salient to us. However, according to the Jaina perspective, facts and values have not been "inverted" in post-normal science. They maintain the same relationship across all kinds of science. Namely, values are *always* the independent variable in that they determine to which facts we attend. It is just that, in post-normal situations, the value-dependence of facts can no longer be ignored.

Funtowicz and Ravetz also state that, in post-normal science, the traditional fact/value dichotomy is being transcended. It seems to me that this could be mistaken for a relativist claim, and I do not think this is what Funtowicz and Ravetz intend. Before I examine their claim, I should define what I mean by "fact", and what I mean by "value", so that the conceptual scheme with regard to which I make my arguments is clear. A "fact", to me, means an empirically observable and verifiable property of the world, as viewed from a particular conceptual scheme. This is an epistemological sense of the word, in which "fact" means what appears to be the case about reality based on empirical evidence. Fact claims can be confirmed or refuted *only* by others sharing the same conceptual scheme as the person who asserts the claim. Consider a fact claim, which I will label (i), made with regard to a given conceptual scheme. Someone who has a different conceptual scheme may be unequipped to observe (i); in other words, the facts might appear differently to her. However, she would commit an epistemological error in arguing (i) to be false, because she would be unable to see the requisite facet of reality. (She could, however, argue that a different conceptual scheme and fact claim would better serve the other person in achieving his or her purposes.) *Anekāntavāda* is intended to avert exactly such fruitless and destructive argument between people with different conceptual schemes, who would do much better to listen to each other and try to understand each other's viewpoints, than to suppress the truth in each others' claims.

Our standpoint and values shape the epistemological "facts" we perceive, but this does not make them any less "true"; as the Jainas would argue, standpoints reveal *partial* truths about *facets* of reality, but they are *truths* nonetheless (as they must be under ontological realism). When I say "true", I mean that a claim is true if the world, relative to a given standpoint, appears consistently to have the claimed property. This is the only sense, I believe, in which any claim can be asserted "true". Note that a group of people, scientists, for example, can share a standpoint to a sufficient extent that they are able to agree on certain "facts".

I say that, under ontological realism, epistemological facts must be taken to reveal partial "truths" about reality, because otherwise we might as well be facile relativists. I do not see a difference between asserting that "facts" cannot be "true" because they are coloured by viewpoints and asserting that all viewpoints are equally invalid (or valid). If facts cannot be true because they are coloured by viewpoints, then we cannot make any claims that actually reveal anything about external reality, and we might as well not be responsible to external reality when making our claims, since we can say nothing about it. In other words, we have no access to the external arbiter, and might as well be facile relativists. My argument rests, of course, on distinguishing between partial truth and falsity. I think both post-normal science and *anekāntavāda* require this distinction, for labelling someone's truth claim false because it is partial amounts to suppressing the truth in that claim, and makes dialogue impossible, since all anyone can ever make is partial truth claims.

Another sense of "fact" is ontological, what is "really" out there, unfiltered by our conceptual schemes. I think Kuhn's distinction between stimuli and sensations corresponds very closely to my distinction between ontological and epistemological facts:

> If two people stand in the same place and gaze in the same direction, we must, under pain of solipsism, conclude that they receive closely similar stimuli. (If they could put their eyes at the same place, the stimuli would be identical.) But people do not see stimuli....Instead they have sensations, and we are under no compulsion to suppose that the sensations of our two viewers are the same....On the contrary, much neural processing takes place between the receipt of a stimulus and the awareness of a sensation....the route from stimulus to sensation is in part conditioned by

> education. Individuals raised in different societies behave on some occasions as though they saw different things. If we were not tempted to identify stimuli one-to-one with sensations, we might recognize that they do so. (1996, p. 193)

Stimuli correspond to ontological facts, and these are immutable. To take them as immutable is to assume an ontologically realist position, which we do to avoid solipsism and relativism. Taking them as immutable also allows us to make fact claims (relative to conceptual schemes, of course), and to account for the observation that people sharing conceptual schemes will see the same things when confronted with the same stimuli. Given a conceptual scheme, there is correspondence between stimuli and sensations, that is, epistemological facts reflect ontological facts. Note that ontological truth differs from the Jaina ultimate truth, apprehension of which would involve seeing reality from all possible conceptual schemes at once. Seeing reality from all conceptual schemes at once does not necessarily amount to seeing ontological truth (stimuli *as they are*), since perception is still filtered by conceptual schemes, albeit infinitely many of them. We cannot see ontological facts – stimuli as they are – and the only fact claims we can make are epistemological.

A "value", in contrast to a "fact", is a judgement about what is desirable. Thus, facts and values might, at first, seem to be distinct, though they are by no means independent. Our educations, cultural backgrounds, personal physiologies and other factors shape our values, and our values in turn shape the epistemological facts we perceive.

I have already discussed how values determine which epistemological facts we apprehend, that is, which properties of the world we observe. However, facts can also shape values, just as values shape facts. For example, I may be an exceptionally tall person – my height would be a "fact". In such a case, I might value high ceilings.

Alternatively, my metabolism and body may be such that hot days make me extremely uncomfortable, and perhaps even threaten my health. In such a case, I might not want to alter the climate so as to increase the frequency of very hot days. In both these cases, facts about my body lead me to have certain values. Because facts shape values, values are not entirely subjective – they are responsible to an external arbiter, immutable ontological reality, just as are epistemological facts.

I note that there are potentially two forms of relativism at issue here – relativism in terms of what factual claims are correct under what circumstances, and relativism with regard to values, that is, with regard to what we should see as desirable and appropriate. We can argue that only certain fact claims are correct with regard to a given standpoint, but if we maintain that values are unconstrained by ontological fact, we will still be caught in a form of relativism. Namely, if we simultaneously respect all other viewpoints and assert that there is no external arbiter by which to evaluate *values*, all outcomes and behaviours will be equally desirable and acceptable. Hence, to completely avert relativism, we must claim that values, as well as epistemological facts, are subject to an external arbiter.

According to my view of reality, ontological facts are immutable, as are epistemological facts *relative to a given conceptual scheme.* If I look at the world using a given conceptual scheme, because the stimuli are immutable, I will always see the same thing. People's viewpoints, however, constantly change, and so the epistemological facts they perceive constantly change. This accounts for the apparent negotiability of facts in post-normal situations.

If values, like epistemological facts, are completely constrained by immutable ontological facts, subject to a given conceptual scheme, then they may as well be regarded as epistemological facts, or properties of the world. As I have just argued, in order to fully avert relativism, we must claim that values are subject to an external arbiter. Hence, if one wishes to fully avert relativism, one is forced to take the position that values are a form of epistemological fact, and in this sense the fact/value dichotomy is transcended. If, however, values are not completely constrained by ontological facts, then they are not a form of epistemological fact, and some degree of relativism is unavoidable.

Under an ontologically realist, non-relativist philosophy, the fact/value dichotomy is transcended in that we must recognize "values" as a form of epistemological fact, responsible to the external arbitration of ontological reality. In contrast, under facile relativism, "facts" become a form of "value", and are completely arbitrary. (I use "fact" and "value" here in the old senses of the terms.) All values and viewpoints are equally valid, and one can make fact claims about the world and hold values without fealty to ontological reality, as manifested through epistemological facts. In facile relativism, *all* fact claims are valid from *any* viewpoint, whereas in qualified epistemological relativism, only certain fact claims can be demonstrated true from a given viewpoint.

I do not think Funtowicz and Ravetz mean that the fact/value dichotomy is being transcended in the relativistic sense, that is, in the sense that facts and values are entirely arbitrary and we are free to make whatever fact claims and hold whatever values we wish. Rather, I think, when they say that the fact/value dichotomy is being transcended, they mean at the least that the old notion that facts and values are independent – that is, that we apprehend the same facts independently of our values – is being recognized as inaccurate. They may also mean that, in order to avert relativism, values must be recognized as epistemological facts, subject to external arbitration.

Though the meaning of the "fact/value dichotomy" being "transcended" may be clear to the philosophical community, I can almost guarantee that most scientists will misinterpret it, as I did at first, to mean facile relativism. Thus, to facilitate dialogue between philosophers and scientists, rather than saying "the fact/value dichotomy is being transcended", it might be better to say we are recognizing that facts and values are mutually interdependent, and cannot be considered in exclusion from each other.

Facts, Values, and Policy:

Let me now examine the relationship between scientific facts, values, and policy. By scientific facts, I mean epistemological facts about the properties and behaviour of the physical world as gathered using scientific conceptual schemes. I argue that sciences are the conceptual schemes best suited to predicting the physical response of systems such as the climate. We cannot make good climate policy in ignorance of scientific facts, for, as I argued above, it is scientific facts about climate dynamics that best allow us to predict the physical evolution of the climate, *given* a certain course of human action. Of course, scientists cannot say with complete certainty that the climate will warm, but then *no* fact claim can be made with complete certainty. Scientists can only claim that, relative to a given conceptual scheme (or set of conceptual schemes), if certain assumptions are correct, and *all other things being equal*, the best scientific evidence indicates that the climate will warm. "All other things being equal" has a similar function here to *syāt*, gesturing to the facets of reality current science neglects, and which may be non-negligible.

Nor can good policy-making proceed in ignorance of values (which I remind the reader are regarded here as epistemological facts). This is because the purpose of policy is to promote outcomes that serve the public interests. If it is not clear what interests should be served, then no matter how reliable the scientific epistemological facts, it is also unclear what decisions are good decisions, because no physical outcome is plainly most desirable.

I do not think the physical sciences are the best-equipped conceptual scheme with which to investigate human values. Other schemes, such as those represented by philosophy, social sciences, and psychology are far better equipped to understand and predict the behaviour of the human ethical and social systems that, in part, drive the climate. Through involvement in an extended peer community, people, including policy-makers, can also investigate each others' values through direct dialogue. Once some consensus obtains, the values upon which the dialogic members agree guide policy-formation. Hence, values, themselves epistemological facts, guide outcome-selection in light of scientific facts, which indicate the set of physical outcomes currently thought possible.

Sometimes one party's interests conflict with another's, or even the stated values of one party can conflict. When values conflict, one value can lead persons to ignore or deny outright epistemological facts that must be taken into account in order to safeguard the other value. In order to decide which epistemological facts are most salient, we must first establish in such situations which value ought to trump the other. If someone prioritizes his values in a hierarchy we do not agree with, we will regard him as having committed a moral error. For example, ostensibly, the industrialized peoples believe that all humans have the right to life and basic sustenance, and that it is wrong to act in such a way as to deprive other people of these rights. However, we also value our fossil-fuel enabled affluence. (Indeed, many wealthy people feel they have a *right* to this affluence.) Hence, we go merrily along our fossil-fuel belching way even though the best available scientific facts indicate that, in doing so, we will likely jeopardize the lives and livelihoods of millions of people.

I recognize what some may regard as a flaw in my argument, namely that it may recourse to the idea that human beings share a certain set of basic values, and there is a universally preferable hierarchy by which these values should be ranked. I do not necessarily regard this as a flaw for I believe that our common physiologies provide human beings with conceptual schemes that, while not identical, share certain basic features. These common conceptual schemes *predispose* all humans to valuing certain things, like pain avoidance and continued survival. Though the ways in which we rank shared desirable outcomes may differ, I think we possess enough common ground that democratic dialogue can identify and prioritize mutual central values.

The Extended Peer Community in the Climate Change Debate:

I return now to the extended peer community, specifically to examine how this community has shaped the climate change debate. Funtowicz and Ravetz (1993) argue that some scientists misunderstand the role of the extended peer community:

> When scientists with a traditionalist outlook bemoan the bad influence of 'the media,' it is sometimes because of

> their difficulty in comprehending this new feature of science when it is involved in policy. (p. 747)

I think this claim needs further discussion, notwithstanding the fact that Funtowicz and Ravetz qualify it with "sometimes".

The media has been of particular importance in the climate change debate, and I do not think the above is an accurate characterization of its role. To facilitate my exploration of this point, I quote another statement from Funtowicz and Ravetz (1993):

> Some participants in environmental struggles come to see scientists merely as hired guns, who should provide the data that 'we' need and consent to the suppression of the rest. Others will be personally impervious to any arguments and evidence that weaken their prejudged case. Are such participants legitimate members of an extended peer community? (p. 754)

The presentation of global warming by the media (at least the North American media) has been heavily influenced by individuals of both types, belonging largely to, or having vested interests in, the fossil fuel industry. These individuals have treated a very small subset of scientists – and many other willing "experts" without training in science – as hired guns, paying them to vocally deny the occurrence of global warming. It is worth noting that a number of these hired guns were also employed by the tobacco industry to deny that smoking causes lung cancer, and have not published in the peer-reviewed climate science literature for years, if ever. Many do not even have PhDs in natural sciences, and so are not equipped with the conceptual schemes necessary to adequately understand or critique the epistemological facts revealed by climate science.

When non-scientists criticize scientific fact claims (or any other fact claim) several things can be going on. If the purpose of the criticism is to show how certain values influence scientific findings, and to argue that a different set of values would produce findings more useful for a given purpose, then the criticism is valid. In such cases, however, the argument is not simply about "facts", but about conceptual schemes. The non-scientists are not arguing that the scientific facts are "wrong", for as I earlier argued only people who share conceptual schemes can validate or invalidate each others' facts. Hence, only scientists can validate or invalidate scientific facts. Rather, the non-scientists are arguing that scientists are viewing the world in a way that is counterproductive relative to some purpose.

Alternatively, if non-scientists, in critiquing science, are denying that the scientific viewpoint has any value, and that their own viewpoint is "correct", then they are *ekāntavādins*, and making an epistemological error. (According to the Jainas, they are also making a moral error, for they do violence to the worldview of another.)

There is a third possibility, however: sometimes when non-scientists (and also scientists) critique scientific results they are simply lying. This is a moral error. I think this third possibility describes much of contemporary commentary on climate science.

As I have already mentioned, members of the media and supposed "experts" have misrepresented the state of climate science, making it look as if climate change is a contentious issue within the scientific community when in reality the overwhelming majority of scientists believe that global warming is occurring. That is, the media and these "experts" are *lying* about the scientific consensus on climate science, and therefore about what the science currently says. Both the media and self-appointed "experts" are responsible for these lies. The media polarizes the issue because conflict and contention makes for a better story, and the "experts" create controversy where it does not exist in order to delay climate change mitigation, because they have vested interests in the fossil fuel industry.

Furthermore, many people have proven "personally impervious" to very strong scientific evidence that the climate is warming, and that the cause is anthropogenic. This imperviousness is likely due to vested interests of one sort or another in fossil fuels. However, because values are in conflict (immediate affluence vs. long-term welfare and security), these people cannot admit the validity of facts that indicate the climate is warming. To do so and continue emitting GHGs would entail openly violating values and ethical standards. In other words, a value conflict has led people to adopt a distorted vision of reality in order to evade, at least temporarily, the conflict, and preserve the appearance of ethical consistency.

The media has amplified the voice of vested interests disproportionately to the scientific community's voice because, as I have already mentioned, conflict sells. The hired guns have gotten equal air time with the overwhelming majority of scientists who believe climate change is real and anthropogenic. Hence, the media has not served to promote understanding of climate change science, but to polarize the issue and spread lies, masquerading as science. When scientists bemoan the influence of the media with regard to climate change, I do not think they are misunderstanding the role of the extended peer community. Rather, they are calling the hired guns and oil tycoons on their moral error, and questioning whether – because of this moral error – they are legitimate members of the peer community. The popular media tends to represent the status quo and to create the appearance of conflict where it does not exist. This is a problem that it seems to me Funtowicz and Ravetz have not provided for adequately in post-normal science.

Funtowicz and Ravetz specifically recommend the extended peer community to assure the quality of *scientific* inputs to the policy process. I would explicitly extend this recommendation to all inputs to the policy process, scientific and otherwise, and I suspect they would agree with me on this extension. Focusing on the scientific inputs would be one-sided.

Furthermore, a plurality of perspectives is not sufficient if some are not adequately heard. The problem of entrenched power must be addressed, and not just entrenched power in science. One of the major threats to the global biosphere today is not science, but entrenched energy interests, associated in the United States in particular with Christian fundamentalism and distrust of science. Societal power imbalances have allowed those with vested interests in continued fossil fuel use to distort science. In the policy debate around global warming, it is scientific inputs that have been marginalized until very recently, while the quality of other inputs has not been adequately controlled.

Legitimate Peers:

It should be evident by now that I do not think the dialogue of the extended peer community has effectively assured the quality of science and policy in the case of global warming. I think this is because the peer community has included illegitimate members, whose power allows them to dominate the debate.

Who is a legitimate member of the extended peer community? Based on *anekāntavāda*, I argue that only those participants in dialogue who are willing to make appropriately qualified, rather than *ekānta* claims, and who are willing to attempt genuine understanding of other viewpoints and the insights they bring to the table, should be viewed as legitimate members of the peer community. Liars are not legitimate members. This fills what I consider to be the largest lacuna in the theory of post-normal science. Under this criterion, neither the media nor the fossil fuel industry's hired guns have qualified as legitimate members of the extended peer community in the climate change debate.

The possibility of implementing such a criterion in practice, is, of course, complicated by the fact that illegitimate members may be able to bully their way into the negotiations based simply on political power or economic weight. Negotiations proceeding in absence of *ekāntavādin* illegitimate members may be ineffectual, because these members have the power to resist any conclusions reached and recommendations made by the community. In such cases, we can only hope that the process of dialogue exposes the *ekānta* members for what they are, and so weakens their power plays.

Contradiction, Technology, and Hegemony:

Forms of Contradiction:

Contradiction is central in both *anekāntavāda* and post-normal science. However, it is not conceived of identically in the two philosophical systems, and it plays somewhat different roles. For Funtowicz and Ravetz, it is a heuristic that promotes the view of conflict as normal, healthy, and necessary, and allows them to "...integrate apparently paradoxical concepts such as 'creative destruction' into a general framework" (1994, p. 569). The contradiction central to Jaina philosophy is logical, and is a symptom of conceptual knowledge's inadequacy for describing infinitely-faceted reality.

As discussed in chapter 2, Funtowicz and Ravetz identify three types of contradiction: complementarity, destructive conflict, and creative tension. Complementarity and destructive conflict are physical contradictions between material system elements. Complementarity involves the balance of opposed forces, and prevails in ordinary complexity. According to Funtowicz and Ravetz, it invokes both Eastern yin/yang and Newtonian action/reaction. Destructive conflict involves struggle between opposing elements, which causes system collapse. Creative tension involves the resolution of contradiction through qualitative transformation in the Hegelian sense. Hegel originally described this sort of contradiction in the context of advancing knowledge. Of these three kinds of contradiction, Funtowicz and Ravetz focus the most on complementarity and destructive conflict, the goal of post-normal science being to achieve complementarity in emergently complex systems through intentionality.

Contradiction arises in the Jaina theory in the form of their assertion that entities can possess contradictory properties simultaneously. The truth value 'inexpressible', which refers to the act of assigning the values true and false to one proposition simultaneously, captures this contradictory nature of reality. I reiterate that according to Matilal the proposition is not true and false in the same sense, but in two different senses.

According to my interpretation of Jaina theory, 'inexpressible' denotes an attempt to think simultaneously using two conceptual schemes. 'Inexpressible' both admits that entities can possess contradictory properties, and conveys that it is not possible to express or think these properties simultaneously. In Kuhnian terms, one would have to use two paradigms at once, and in post-normal terms, one would have to simultaneously utilize projections on two subspaces. In the Jaina theory, contradiction thus indicates the limitations of conceptual knowledge.

I do not think any of the three forms of contradiction that Funtowicz and Ravetz explicitly define gestures similarly to the limits of conceptual knowledge. The only one that might seem at first to resemble the Jaina contradiction is creative tension, wherein a qualitative shift to a new conceptual scheme allows contradiction to be resolved. This, however, seems to me more analogous to a Kuhnian paradigm shift. Creative tension involves simply a transition to a new conceptual scheme; the mind of an individual whose thoughts go through a moment of creative tension does not necessarily bump up against the inadequacy of conceptual knowledge in general, but simply the inadequacy of one particular conceptual scheme for capturing two pertinent facets of reality in one theoretical structure. Creative tension does not involve progressively adding more and more conceptual schemes, retaining the insights of each, until finally a complete description of reality is attained. Rather, it involves abandoning an old conceptual scheme in favour of one that can resolve a problem or contradiction that seems particularly pertinent.

Funtowicz and Ravetz mention contradiction in one context analogous to its role in Jaina theory, but this is not one of the forms of contradiction they develop as central to the theory. Namely, higher dimensional objects in multidimensional phase space sometimes have properties that appear contradictory when viewed in lower dimensions. This sort of apparent contradiction arises specifically because we cannot view projections on various subspaces simultaneously. Higher dimensional beings presumably would be able to utilize a single higher-dimensional conceptual scheme, and the properties would then not appear contradictory. As in *anekāntavāda*, this form of contradiction hence points to the limits of single viewpoints or projections on subspaces for capturing reality.

Given that the focus of Funtowicz's and Ravetz's theory is irreducible uncertainty, stemming from the limits of conceptual knowledge, it seems to me that complementarity, destructive conflict, and creative tension are less useful and unifying heuristics than the form of contradiction explored in the last paragraph. Complementarity plays a central role in Futowicz's and Ravetz's analysis of ordinary complexity, and destructive conflict in their analysis of the oscillation between hegemony and fragmentation. However, it seems to me these forms of contradiction are descriptive rather than explanatory. Contradiction arising from the limitations of conceptual knowledge, however, locates the source of destructive conflict in a misunderstanding of the nature of reality, and also explains why hegemony and fragmentation are not as common in ordinarily complex systems.

I am, of course, not really suggesting that post-normal science be fundamentally modified, or that a culturally alien concept of contradiction be imported, for the form of contradiction analogous to the Jaina form is already present. I am just suggesting a shift in focus, in terms of the theory's central heuristic concepts. The inclusion of logical contradiction as central to post-normal science could substantially clarify and solidify Funtowicz's and Ravetz's discussion of contradiction, the central thrust of which is not immediately clear.

Knowledge, Ignorance, Technology, and Hegemony:

The seemingly contradictory inextricability of knowledge and ignorance is a form of contradiction falling into the Jaina class. Funtowicz and Ravetz do acknowledge that contradiction illuminates the co-creation of knowledge and ignorance:

> Within this style, one cannot envisage a beneficial progress without looking for its costs; the growth of knowledge without its interaction with ignorance; or the achievement of good without some production of evil. (1994, p. 572)

However, neither complementarity nor destructive conflict, being physical, illuminates this contradiction, which is so central to the irreducibility of uncertainty, and therefore to post-normal science.

Ravetz (1987) argues that the very source of much of our ignorance, paradoxically, is science:

> Now we face the paradox that while our knowledge continues to increase exponentially, our relevant ignorance does so, even more rapidly. And this is ignorance generated by science! (p. 423)

When science enables technological advances, these advances almost invariably bring new ignorance as well as new knowledge. This would not surprise a Jaina, for according to *anekāntavāda*, reality possesses many contradictory and mutually occurring properties, such as existence and non-existence. Just as one cannot invoke existence without also invoking non-existence, one cannot generate knowledge without also generating ignorance. This is because conceptual knowledge is incomplete. In taking a particular viewpoint on an object, we generate ignorance about those facets not captured by our viewpoint.

Hence, when science produces new concepts, which then become embodied in the form of technologies, it also produces new ignorances, which are the flip side of these concepts. This insight can help us to understand why hegemony emerges preferentially in human systems rather than in natural systems, and why it is so unstable and prone to collapse through the *ancien régime* state and autolysis. In my opinion, Funtowicz and Ravetz do not give any well-defined elaboration as to why this might be the case.

In brief, *the limitations of conceptual knowledge enter emergently complex physical systems through technology*, which is conceptual. In addition to being inherently conceptual, technology provides the means – the *power* – by which one subsystem can dominate all others, for example, by which human desires for affluence and convenience can marginalize non-human desires for continued existence. A case in point is the fact of the countless species whose existence is threatened by logging of the Amazon rainforest.

When technology is applied without taking adequate consideration of the many-sided nature of reality, it generates hegemony, an *ekānta* state characterized by the dominance of one worldview, and so of one element or subsystem. In ordinary complex systems, technological interference is absent or severely limited, and so the limitations of conceptual knowledge do not enter these systems. Note that this argument does not depend on asserting that non-human others lack conceptual knowledge, merely that they usually lack the power to elevate their conceptual schemes to dominance. This accounts for the "…complementarity of competition and cooperation, with a diversity of elements and subsystems" seen in ordinary complexity (Funtowicz and Ravetz, 1994, p. 568).

It is because hegemony is *ekānta* that is it so unstable, and liable to collapse into fragmentation. Because hegemony is *ekānta*, any hegemonic worldview is, in some sense, false. That is, it neglects many features of reality. Usually, one or more of these features is non-negligible for the continued existence of the hegemonic regime, and so the regime destabilizes under the influence of an inadequately accounted-for force. The instability of hegemony is yet another argument for ontological realism: any regime that neglects physical reality does so at its own peril, for physical reality and its forces are not negotiable, whether or not we perceive them in their totality.

Conclusion:

In this chapter, I have established substantial parallels between *anekāntavāda* and post-normal science. The central metaphors of the two systems are analogous, and the philosophies share realist ontologies and qualified relativist epistemologies.

I have also shown that, in claiming objectivity, traditional science is *ekānta*. Furthermore, science, being conceptual, cannot avoid being informed by values, for it is our interests that determine what features of reality are salient to us.

I have shown that ekāntavādism implies reductionism, but reductionism does not imply *ekāntavādism.* All statements that utilize concepts are reductionist. This includes *anekānta* statements, which differ only in that they explicitly acknowledge their reductionism. Hence, no scientific endeavour, including climate science, can claim to be non-reductionist. However, scientists can proceed in an *anekānta* fashion by acknowledging reductionism, respecting others' viewpoints and insights, and asserting, refuting, and confirming only qualified propositions.

The function of the extended peer community in post-normal science is to reduce reductionism. Through it, post-normal science is a philosophy of synthesis and reconciliation. Arguably, climate science is already, to a greater extent than most other sciences, *anekāntavadin*, and includes a far more extended peer community than seen in traditional sciences.

I have also argued that the dialogue of the extended peer community has not effectively assured the quality of science and policy in the case of global warming. This is due largely to the fact that illegitimate peers have been able to use political power to present a distorted picture of climate science to the public.

Funtowicz and Ravetz recommend the extended peer community specifically to assure the quality of scientific inputs to post-normal debates. Focussing on scientific inputs is one-sided, and quality would be better assured if *all* inputs to the debate were subject to equal scrutiny. In the policy debate around global warming, it is scientific inputs that have been marginalized until very recently, while the quality of other inputs has not been adequately controlled.

Anekāntavāda also suggests a criterion by which to identify legitimate members of the extended peer community, and so fill in an important lacuna in post-normal science. Only those participants willing to make appropriately qualified, rather than *ekānta* claims, and to attempt genuine understanding of other viewpoints are legitimate members of the peer community.

I have shown that facts and values are not inverted in post-normal science – they are in the same relationship in all kinds of science. Namely, we apprehend certain epistemological facts based on our values. In post-normal situations, the value-dependence of facts just becomes glaringly obvious.

I have also shown that, in order to completely avert facile relativism, it is necessary to view values as a form of epistemological fact, subject to an immutable ontological arbiter. It then becomes possible to distinguish between values as more or less desirable.

I have also explored the central roles that contradiction plays in *anekāntavāda* and post-normal science. While the Jaina form of contradiction points to the limits of conceptual knowledge, the forms of contradiction Funtowicz and Ravetz identify as central to post-normal science do not. These forms of physical contradiction are descriptive rather than explanatory, while contradiction arising from the limitations of conceptual knowledge locates the source of destructive conflict in a misunderstanding of the nature of reality. I thus suggest a shift in focus in terms of post-normal science's central heuristic concepts. This shift could substantially augment and solidify the role of contradiction in Funtowicz's and Ravetz's theory.

Finally, I offer an explanation, based on the Jaina form of contradiction, as to why hegemony and fragmentation are not as common in ordinary complex systems as they are in emergently complex systems: the limitations of conceptual knowledge enter emergently complex systems through technology, which is conceptual, and provides the power by which one worldview can dominate all others. When technology is applied without adequately considering the many-sided nature of reality, it generates hegemony, which is an *ekānta* state. In ordinary complex systems, by contrast, technological interference is absent or severely limited, and so the limitations of conceptual knowledge do not enter these systems. Because hegemony is *ekānta*, it neglects non-negligible features of reality, and so is highly unstable

I conclude this chapter with a question: if the limitations of conceptual knowledge enter emergent complexity through technology, what does this say about technical mitigation options like carbon capturing and sequestration? This will be a major focus of the next and final chapter.

Chapter Five: Carbon Dioxide Capture and Storage

Introduction:

In this chapter, I focus on a specific climate change mitigation option: carbon dioxide capture and storage (CCS). CCS involves capturing CO_2 from large point sources, such as industrial power plants, and then sequestering it in geological formations, in the oceans, or storing it in mineral carbonates. CCS is representative of a particular class of mitigation options, which I will call "technologically invasive". These options propose to mitigate climate change through further technological interventions into the climate system. Iron fertilization, for example, involves introducing iron to nutrient rich, iron-limited surface ocean waters to encourage phytoplankton blooms and increase oceanic CO_2 uptake.

I focus here on CCS because the science behind this option is quite developed, and because CCS has provoked much interest and discussion. In 2005, the Intergovernmental Panel on Climate Change (IPCC) prepared a technical report on CCS, the *IPCC Special Report on Carbon Dioxide Capture and Storage*. Though I develop my arguments specifically with CCS in mind, many of the philosophical points will be applicable for evaluating other technologically invasive mitigation options.

CCS has excited much interest because it can potentially allow us to continue using fossil fuels without triggering dangerous climate change. CCS could thus facilitate a smoother transition to a less carbon-intensive way of life, with minimal economic and societal disruption. In such a scenario, CCS could act as a stopgap measure while we develop other mitigation options, such as energy efficiency improvements and alternative energy sources. On the other hand, CCS could delay lifestyle changes and development of alternative energy sources, allowing us to prolong fossil fuel-enabled affluence and continue industrial production and consumption unabated, thus further straining the productive capacities of the biosphere.

I first give a general introduction to the science and technology of CCS, including the risks of various sequestration options, and discuss the motivations for using CCS. I then examine and evaluate CCS using Jaina ontology and the post-normal framework of emergent complexity, including notions of hegemony, fragmentation, and contradiction. Following this, I examine the IPCC report itself as an embodied dialogue – or prelude to dialogue – and compare it to the Jaina and post-normal dialogic ideals.

My purpose here is not to single-handedly develop an entire post-normal debate on CCS. Hence, I do not examine in complete detail all the technical, scientific, economic, social, and legal issues associated with the technologies. This will be the task of the extended peer community when CCS becomes feasible on a large scale. My purpose is rather to demonstrate how certain epistemological insights and principles can act as heuristics, guiding our evaluation of CCS and providing insight into its viability and desirability.

Overview of CCS:

In brief, carbon dioxide capture and storage involves separating CO_2 from the emissions of large point sources, transporting it to a storage location, and isolating it from the atmosphere (IPCC, 2005, p. 3). Large point sources of CO_2 include industrial plants, synthetic fuel and hydrogen production plants, and fossil fuel or biomass energy facilities (IPCC, 2005, p. 3). In contrast with small, diffused sources, like personal automobiles and home heating systems, large point sources emit substantial amounts of CO_2 in one location, which makes capture much easier.

Plants fitted with CCS systems require more energy than non-CCS plants, and so produce more CO_2 per unit product. However, CCS plants would still reduce immediate emissions to the atmosphere by 80-90% (IPCC, 2005, p. 4). The net emissions reduction would also depend on leakage during transport and, in the long run, how much CO_2 was retained in storage. Higher energy requirements also mean increases in solid waste and other environmental emissions per kWh. For instance, CCS plants would use larger amounts of ammonia and limestone to control emissions of nitrogen oxide and sulphur dioxide (IPCC, 2005, p. 27). However, the IPCC states, "more efficient new or rebuilt plants with CCS may actually yield net reductions in plant-level environmental emissions" (2005, p. 27).

CO_2 can be stored in geological formations, in the ocean, in mineral carbonates, or used in industrial processes. In geological sequestration, CO_2 is injected in dense form into saline formations, oil and gas fields, or unminable coal beds, where physical and geochemical trapping mechanisms theoretically prevent it from re-surfacing. Ocean sequestration would involve injecting and dissolving CO_2 into the water column below 1000 m, or depositing CO_2 at depths below 3000 m, where it is denser than water and "is expected to" pool in a lake (IPCC, 2005, p. 7). CO_2 can also be fixed into inorganic carbonates, and a small fraction used in industrial processes.

Carbon capture systems do not present fundamentally new challenges in terms of monitoring or environmental risk (IPCC, 2005, p. 27). Rather, the most significant new environmental risks arise in association with the actual storage of the CO_2.

The Role of CCS as a Mitigation Option:

The stated goal of the 1992 United Nations Framework Convention on Climate Change is the "stabilization of greenhouse gas concentrations in the atmosphere at a level that would prevent dangerous anthropogenic interference with the climate system" (UNFCC, 1992, p. 5). The IPCC third assessment report, referenced in the *Special Report*, found that no single mitigation option would be adequate to stabilize GHG levels; rather, a portfolio of options is needed. Hence, CCS is not a stand-alone fix for climate change, but rather part of a mitigation portfolio, which includes improved energy efficiency, less carbon-intensive fuels, nuclear power, renewable energy sources, enhanced biological sinks, and reduced emission of other greenhouse gases (IPCC, 2005, p. 3).

Global energy use scenarios indicate that fossil fuels will continue to supply most of our energy at least until the middle of the century (IPCC, 2005, p. 20). Technological mitigation options exist and could be implemented to achieve a broad range of stabilization levels. However, this would require institutional and socio-economic changes. The *Special Report on Carbon Dioxide Capture and Storage* states, "In this context, the availability of CCS in the portfolio of options for reducing greenhouse gas emissions could facilitate the achievement of stabilization goals" (p. 21). That is, with CCS, we could reduce CO_2 emissions to the atmosphere without changing energy infrastructures, and thereby avoid the socio-economic and institutional changes associated with other mitigation options. Avoiding or reducing infrastructural changes would make it easier to reach stabilization

Geological Sequestration:

CO_2 can be stored in suitable geological formations in both onshore and offshore sedimentary basins using many of the same technologies as the oil and gas industry, such as well-drilling technology, injection technology, computer simulations of reservoir dynamics, and existing monitoring mechanisms (IPCC, 2005, p. 31). We already store natural gas in and inject liquid wastes and acid gases into such formations; these activities would provide relevant experience.

For storage in saline beds or oil and gas fields, CO_2 must be injected to depths below 800–1,000 m, where, due to high pressure, it becomes supercritical and resembles a liquid (IPCC, 2005, p. 31). This liquid-like state enables efficient use of storage space, and improves security. In addition, safe geological storage requires a low permeability shale and clay cap rock, which prevents the buoyant CO_2 from flowing upward out of the reservoir. Capillary forces can also hold the CO_2 in the pore spaces of the reservoir rock, but one or more sides of the reservoir usually remain open, allowing the CO_2 to migrate laterally and escape (IPCC, 2005, p. 31). In such cases, long-term CO_2 retention requires additional trapping mechanisms, such as geochemical trapping, in which CO_2 reacts with in situ fluids over hundreds to thousands of years to produce dense water that sinks rather than rising. The dissolved CO_2 then reacts with the rock to form solid carbonate minerals over millions of years.

CO_2 can also be stored in unminable coal beds, where it is adsorbed by the coal, displacing gases like methane (IPCC, 2005, p. 32), and remains trapped as long as pressures and temperatures stay stable. The feasibility of coal bed storage depends largely on the permeability of the bed. Because the CO_2 is adsorbed, coal bed storage can take place at shallower depths, as security does not require liquid-like density.

Injecting CO_2 into geological formations can also increase fossil fuel recovery (IPCC, 2005, p. 6). Enhanced Oil Recovery (EOR) involves injecting CO_2 into oil and gas beds, where it makes the fuel easier to extract via a variety of mechnisms. In Enhanced Coal Bed Methane Recovery (ECBM), the adsorbed CO_2 displaces methane from the coal, making it available for extraction.

Three geological storage projects on the order of 1 $MtCO_2$ per year already operate: the Weyburn EOR project in Canada, the Sleipner project in an offshore saline formation in Norway, and the In Salah project, which is located in a gas field in Algeria (IPCC, 2005, p. 31). These projects together capture and store 3-4 $MtCO_2$ per year that would otherwise be released to the atmosphere.

Risks to Human Health and the Environment:

The IPCC classifies the risks associated with CO_2 escape from geological reservoirs into two categories: global risks and local risks (2005, p. 34). Global risk consists essentially of the potential for enhanced climate change, should any significant fraction of CO_2 escape from storage reservoirs. Based on current knowledge, the probability that over 99% of CO_2 injected into appropriately selected and maintained reservoirs would remain there after 100 years exceeds 90%, and the probability that over 99% of it would remain after 1000 years is 60-90% (IPCC, 2005, p. 34). The risk of leakage is also expected to decrease over time, as other trapping mechanisms, such as geochemical trapping, take effect.

Local risks stem from heightened CO_2 concentrations in the near-surface environment, release of contaminants into groundwater through CO_2 reaction with the rock matrix, and displacement of native fluids by the injected CO_2 (IPCC, 2005, p. 242). Leakage can occur either abruptly or gradually, and be localized or spatially dispersed. There are three general pathways by which CO_2 can leave storage formations (IPCC, 2005, p. 242). Firstly, it can escape through the pore system in low permeability caprocks, should the CO_2 gas pressure exceed the value necessary to enter the caprock. Secondly, CO_2 can escape through openings, fractures, or faults in the cap rock. Finally, the gas can escape via poorly constructed or abandoned wells.

The risks from CO_2 leakage will depend on the spatial and temporal distribution of CO_2 fluxes, and on the CO_2 concentrations that would result from a particular flux (IPCC, 2005, p. 246). The effects of escaped CO_2 would depend on the storage site topography, vegetation type and density, prevailing winds and precipitation, and density of animal populations.

In general, abrupt, localized leaks will have more significant impacts per unit CO_2 than gradual, spatially dispersed leaks (IPCC, 2005, p. 242). Abrupt leakage up an abandoned injection well or through injection well failure could result in high CO_2 concentrations under stable atmospheric conditions, and given local topography capable of containing the CO_2. CO_2 is 50% denser than air, so would accumulate in depressions, resulting in higher concentrations than in flat areas (IPCC, 2005, p. 246). According to the Special

Report, "A sudden and large release of CO_2 would pose immediate dangers to human life and health, if there were exposure to concentrations of CO_2 greater than 7-10% by volume in air" (IPCC, 2005, p. 12). At concentrations above 2%, CO_2 compromises respiratory physiology, and at concentrations above 7-10%, it can cause unconsciousness and death (IPCC, 2005, p. 246).

The extent of ecosystem damages from CO_2 seepage is considerably uncertain – large seeps produce significant damage, but small seeps may produce no detectable impact (Sorey *et al.*, 1998). Gradual leakage up wells or through undetected cracks and faults into the subsurface could contaminate groundwater and kill plants and subsoil animals through processes like soil acidification. CO_2 in shallow soil causes vegetation die-off through root anoxia and low oxygen concentration (IPCC, 2005, p. 248). However, there is no evidence of any terrestrial impact from current CO_2 storage projects. Dissolved CO_2 forms carbonic acid, which can mobilize toxic metals, sulphate or chloride into drinking water (IPCC, 2005, p. 247). Brines displaced by injected CO_2 can also migrate and salinify aquifers (IPCC, 2005, p. 248), contaminating drinking water, impacting wildlife habitat, and compromising agricultural use of land. However, experience with injection of other fluids into geological formations demonstrates that contamination of drinking water by displaced brines is rare (IPCC, 2005, p. 248).

Subsurface microbes can be assumed present everywhere CO_2 injection is being considered, and the effect of CO_2 on these microbes is not well-studied (IPCC, 2005, p. 248). A variety of effects are possible, including favouring some species and harming others, or stimulating microbes that would reduce the CO_2 to methane (IPCC, 2005, p. 248).

Modelling studies have indicated that CO_2 movement through the subsurface will, in general, be very slow, and that release rates will be very low – for example, 10^{-6} of the stored CO_2 per year – once the CO_2 does migrate to the surface (IPCC, 2005, p. 246). In addition, simulations are available that explore the probability of CO_2 release to the biosphere via a variety of routes (IPCC, 2005, p. 246), and can be used to examine specific geological storage sites.

In saline formations, given adequate brine presence, most CO_2 will eventually dissolve in the brine on time scales short compared to the time taken for CO_2 to migrate out of the storage site (IPCC, 2005, p. 246). Once dissolved, the time scales over which CO_2 would leave the basin are so long as to be negligible for risk assessment purposes.

If CO_2 sequestered offshore were to leak through the ocean bottom, it could dissolve in the water and adversely affect marine organisms. In the case of an abrupt leak, the health and safety of offshore platform workers would also be at risk (IPCC, 2005, p. 243).

Injection of CO_2 at pressure substantially higher than the formation pressure can induce seismicity, creating fractures through which CO_2 can escape, and triggering earthquakes large enough to cause damage (IPCC, 2005, p. 249). Several geomechanical methods are available for assessing fault stability and maximum sustainable pore pressure. Monitoring for microseismicity can indicate whether pore pressures have exceeded the strength of faults, fractures, or intact rock, and reduce the chance of damage. Only a few seismic events resulting from deep injection of fluids have been recorded, suggesting that the risks are low (IPCC, 2005, p. 250).

Wells are one of the most probable leakage pathways for CO_2 (IPCC, 2005, p. 244). In the sedimentary basins where geological storage may occur, hundreds of wells are often located near possible injection sites (IPCC, 2005, p. 244). The high density of wells may compromise CO_2 storage security. Well drilling creates a passageway from subsurface to surface, and can compromise the integrity of the original rock. Unused wells are generally plugged with cement; however, leakage can occur between the cement and outside casing, between the cement and the inside of the metal casing, through the cement plug, through deteriorated metal casing or cement in the annulus, and in the gap between the outer cement casing and the rock formation, as shown in figure 2.

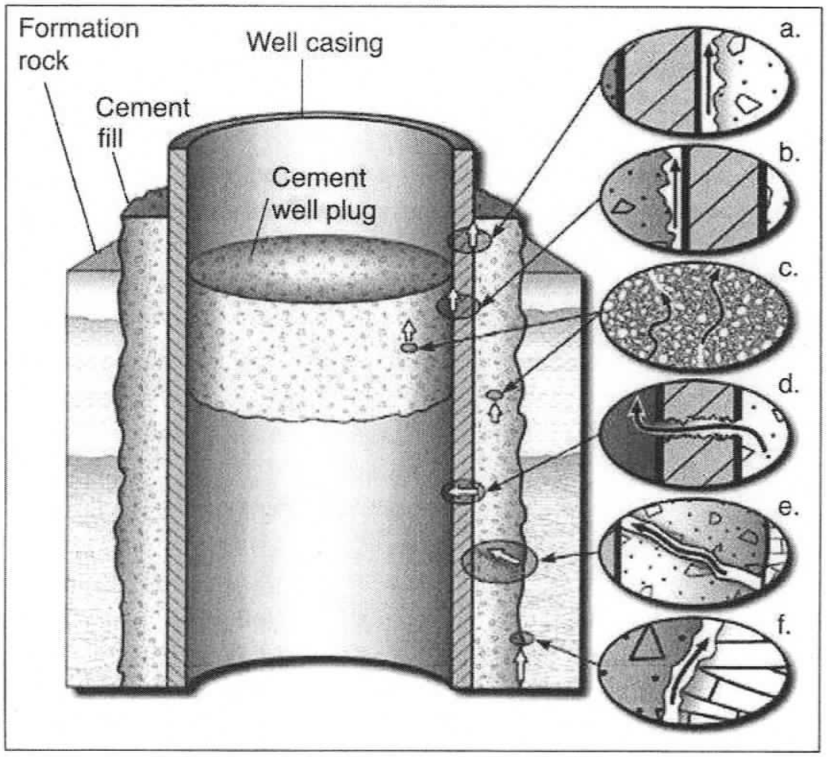

Figure 2 Possible routes by which CO_2 could escape from an abandoned well. (a) Between well casing and cement; (b) between casing and plug; (c) through degraded plug cement; (d) through corroded casing; (e) through fractured cement; (f) between cement and rock (after Gasda et al., 2004, p. 709).

Cement and metal casings could also degrade as a result of long-term exposure to CO_2; this is a topic of active research (IPCC, 2005, p. 244). In the long term, mining or drilling in regions with CO_2 storage could pose risks if the "institutional knowledge and precautions are not in place to avoid penetrating a storage formation" (IPCC, 2005, p. 247).

In the United States, 470 natural gas storage facilities currently operate, with nine documented significant leaks, and average annual release rates under 10^{-5}, expressed as a fraction of stored gas (IPCC, 2005, p. 245). These can provide an analogue to geological carbon storage; however; natural gas systems are subject to rapid pressure cycling, which increases the probability of leakage through the caprock, whereas CO_2 dissolves in pore waters, reducing leakage risk (IPCC, 2005, p. 245). CO_2 is also heavier than natural gas, and so less likely to move upward under buoyancy forces. CO_2 storage is riskier than natural gas storage only in that CO_2 is more corrosive to metallic components. Hence, the performance record of natural gas storage systems should be regarded as a lower bound on the probable performance of CO_2 storage systems (IPCC, 2005, p. 245).

Naturally occurring releases of CO_2 in volcanic areas also provide analogues, and give some basis for understanding CO_2 transport from the shallow subsurface to the atmosphere. However, such releases occur in highly fractured volcanic zones, not from deep storage in sedimentary basins (IPCC, 2005, p. 247). In general, fluxes and risks from natural CO_2 releases are much higher than would be expected from a CO_2 storage site (IPCC, 2005, p. 248).

Risk Assessment and Management:

Geological carbon storage will necessarily carry risks associated with pipeline, compressor, and wellhead operation, and other surface facilities. These operational risks are routinely assessed in the oil and gas industry, and are amenable to currently available risk assessment methods, such as quantitative risk assessment (IPCC, 2005, p. 250). The wealth of experience from the oil and gas industry means that we can give good estimates of risks from surface operations based on observed failure rates and consequences of failure (IPCC, 2005, p. 250). Hence, though the operational phase of geological CCS does present risks, these risks are not especially problematic from the point of view of assessment.

In contrast long-term geological carbon storage carries risks that are not amenable to the assessment methods used for surface operations (IPCC, 2005, p. 250). There is no well-established methodology to assess these risks. However, methodologies from related storage activities could be adapted to CO_2 storage. For example, nuclear waste performance assessment models assess risks from subsurface transport of nuclear waste over thousands to millions of years (North, 1999).

According to the IPCC, systematic risk assessment is well-founded on "a comprehensive catalogue of the risks and the mechanisms that underlie them" (2005, p. 250). Several potential carbon storage sites are currently under evaluation using the features, events, and processes (FEP) methodology. Parameters like permeability, caprock thickness, and number of wells fall under "features", while seismicity and well blow-outs fall under events, and physical and chemical processes such as chemical reactions and geomechanical stress changes fall under "processes" (IPCC, 2005, p. 250).

In addition to cataloguing risks, most risk assessments involve scenarios, each of which is an assembly of FEPs (IPCC, 2005, p. 250). After scenario-building, the next step in risk assessment, according to the IPCC, is the development of mathematical-physical models, which are based on the scenarios (IPCC, 2005, p. 250).

With regard to the risks associated with geological storage, the IPCC states, "…closely related industrial experience and scientific knowledge could serve as a basis for appropriate risk management, including remediation" (2005, p. 13), and that the health, safety, and environmental risks associated with well-planned and implemented geological storage would be comparable to those of natural gas storage, EOR and underground acid gas disposal, which are already carried out and hence presumably acceptable. Safe storage would require appropriate site selection, a monitoring system to detect leakage, a regulatory system, and methods to control any CO_2 leakages (IPCC, 2005, p. 12).

Natural CO_2 reservoirs already exist, so geological sequestration would not create formations unseen in nature. The existence of these reservoirs, which have trapped oil, gas, and CO_2 for millions of years, demonstrates that there are caprocks capable of retaining CO_2 on geological time scales (IPCC, 2005, p. 245). Further, natural reservoirs provide models for safe sequestration. By studying natural CO_2 reservoirs with low leakage, we can identify desirable features for geological storage sites, including an impermeable cap rock, lack of leakage paths, geological stability, and effective trapping mechanisms (IPCC, 2005, p. 13).

Three important criteria by which appropriate sites can be identified include the efficacy of the cap rock, the volume and permeability of the storage rock, and whether any pre-existing well will compromise security (IPCC, 2005, p. 33). A number of existing techniques are available for investigating such properties of geological storage sites. These include seismic imaging, pumping tests for evaluating storage capacity and seal quality, and cement integrity logs (IPCC, 2005, p. 33). In addition, computer programs can be used to model underground CO_2 movement. However, these programs were developed for oil and gas applications, and will need to be adapted for CO_2 storage (IPCC, 2005, p. 33).

Monitoring strategies currently available include routine techniques for measuring injection rate and well pressure and seismic surveys to track underground CO_2 movement (IPCC, 2005, p. 35). In addition, groundwater and soil sampling may help detect leakage, and CO_2 sensors with alarms can be placed in wells (IPCC, 2005, p. 35). Appropriate standard procedures and protocols will have to be developed, and are expected to develop along with the technology (IPCC, 2005, p. 35).

Techniques currently used to stop well blow-outs can quickly control abrupt leakage up an abandoned well. The oil and gas industry already regularly manages this type of hazard (IPCC, 2005, p. 34). For both abrupt and gradual leakage, potential leakage paths can be identified, and then appropriate monitoring and remediation strategies developed (IPCC, 2005, p. 34). Leaks from wells can be stopped by injecting heavy mud into the well casing, and leaking injection wells can be repaired by replacing injection tubers and packers (IPCC, 2005, p. 252). CO_2 could be extracted before it leaked into groundwater. Techniques are also available to remove CO_2 from subsurface soils, but they will be costly. Figure 2 displays a number of potential leakage pathways from saline formations in particular, and appropriate remediation strategies.

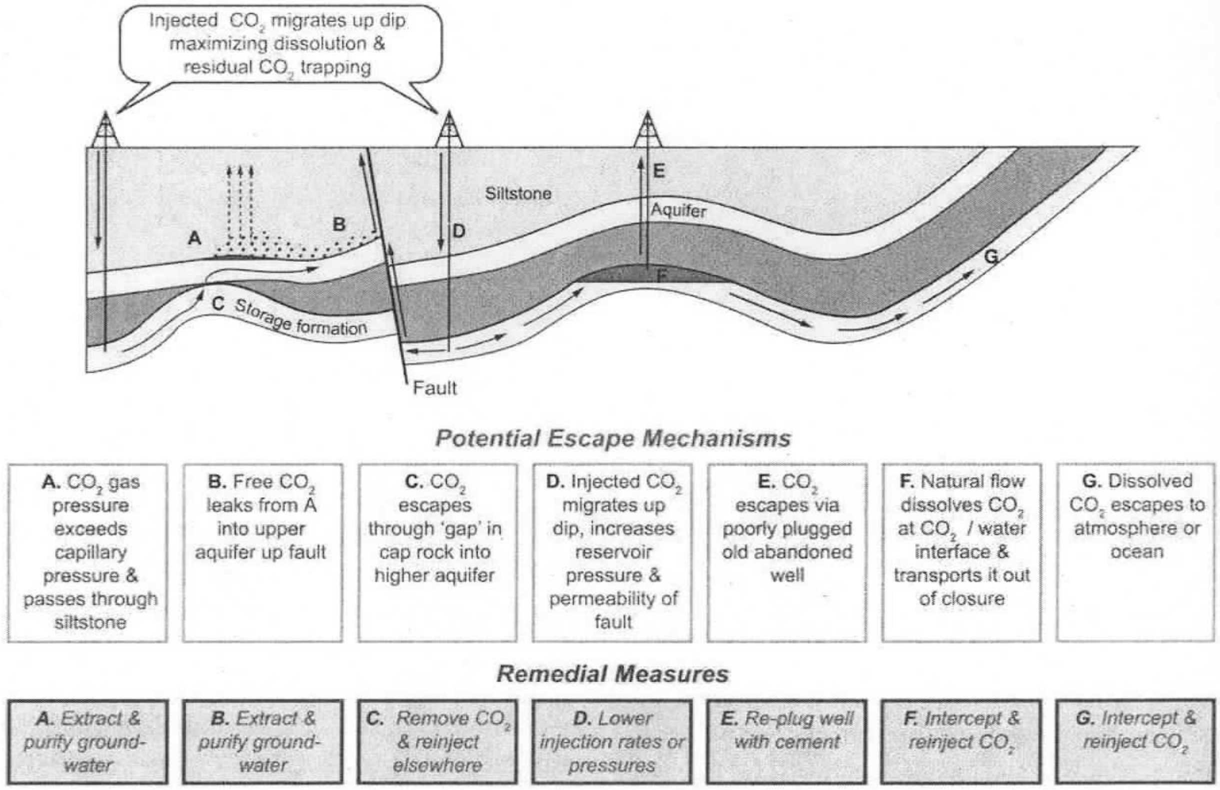

Figure 3 Potential CO_2 leakage routes from a saline formation, and associated remedial measures (IPCC, 2005, p. 243).

Because geological storage of CO_2 is extremely long term, provisions would need to be made for monitoring well into the future. In addition, the behaviour of abandoned wells in the presence of CO_2 on long timescales is poorly understood at present, and is being actively researched (IPCC, 2005, p. 251).

Ocean Storage:

Carbon dioxide dissolves in water to form carbonic acid, and the oceans naturally uptake CO_2 from the atmosphere. Ocean storage is still in the research stage, and has yet to be demonstrated on any significant scale. However, theoretical, laboratory, and modelling studies, as well as small-scale field experiments have been carried out over the last 25 years (IPCC, 2005, p. 37).

CO_2 could be stored in the ocean in two basic ways, and at two different depths (IPCC, 2005, p. 7). At depths below 1,000 m, the gas could be injected and dissolved into the water column using a permanent pipeline, or from a ship. Alternatively, CO_2 could be deposited via pipeline or offshore platform at depths below 3,000 m or onto the seafloor. At such depths and pressures, CO_2 is denser than water, and would pool in a lake, delaying dissolution.

CO_2 storage in the ocean would not be permanent. Instead, the ocean would release CO_2 gradually over hundreds of years. Models and ocean tracer data indicate that 65-100% of the CO_2 would remain after 100 years, and 30-85% after 500 years (IPCC, 2005, p. 14). The fraction retained depends on the injection depth, with higher fractions of CO_2 retained for greater depths.

Risks to Human Health and the Environment:

The potential ecological impacts of ocean sequestration are not well-understood, and are the subject of active research. When CO_2 dissolves in water, it forms carbonic acid. As a result of taking up 500 Gt of anthropogenic CO_2, the *p*H of the upper ocean has already decreased by about 0.1, while the *p*H of the deep ocean has so far not changed (IPCC, 2005, p. 37). The amount of CO_2 that could be stored in the ocean through injection would be bounded by equilibration between the ocean and atmosphere and by a maximum allowable decrease in pH (IPCC, 2005, p. 38). Most CO_2 deposition strategies would avoid high concentrations over large areas, either by diffusing CO_2 through the water column, or concentrating it in lakes on the bottom. The CO_2 would still diffuse over large areas on long time scales however, eventually spreading over the entire ocean volume.

The injection of hundreds of $GtCO_2$ would eventually produce observable *p*H changes over the entire ocean volume (IPCC, 2005, p. 38). According to the IPCC report on CCS, stabilization at 550 ppmv without ocean storage is projected to result in a pH decrease of 0.25, while stabilization at the same atmospheric CO_2 level with ocean storage is projected to result in a pH decrease of 0.4 (2005, p. 38). These *p*H changes far exceed pre-industrial variations in ocean acidity (IPCC, 2005, p. 38). Recent work indicates that burning all available fossil fuel reserves would, by the year 6800, cause oceanic *p*H to decrease by 0.7 units without oceanic carbon storage (Montenegro *et al.*, 2007). A decrease of 0.26-0.36 would reduce calcium carbonate levels to such an extent in the Southern Ocean as to significantly and negatively impact the biota (Montenegro *et al.*, 2007, p. 4). Shell-forming marine organisms at the base of the food chain would no longer have enough calcium carbonate to create their shells, and the reduction in their numbers would have ramifications up the food chain.

High concentrations of CO_2 are known, from experiments, to affect marine organisms, reducing calcification rates, reproduction, growth, circulatory oxygen supply, and mobility (IPCC, 2005, p. 38). Low *p*H increases biological uptake of trace metals, which could have toxicological implications (p. 303). Short-term exposure to high CO_2 concentrations causes respiratory distress, narcosis, and mortality, and lower concentrations may have important effects on long time scales (IPCC, 2005, p. 301).

Beyond changes in *p*H and pCO_2, changes in molecular CO_2, carbonate and bicarbonate concentrations may also affect ocean organisms (IPCC, 2005, p. 301).

Mechanisms that might compromise long-term performance and survival of ocean organisms in response to moderately elevated CO_2 are more poorly understood than those causing immediate mortality, but they are also more important, as they would affect larger ocean volumes as CO_2 diffused through the water on longer time scales. Reduced calcification in shelled organisms, long-term disturbances in ion equilibria, depression of aerobic energy metabolism due to acidosis, and inhibition of protein synthesis due to depressed metabolism, resulting in slower growth and reproduction are all possible long-term effects (IPCC, 2005, p. 304).

Complex relationships between animals mean that effects on individuals and species can affect the whole ecosystem. A continuum of impacts is likely, rather than a threshold above which CO_2 cannot be tolerated (IPCC, 2005, p. 306). CO_2 could reduce growth and reproduction, decrease resistance to infection, and limit ability to escape predation or catch prey. Few studies carried out in surface waters indicate that species benefit at the ecosystem level from elevated CO_2 concentrations, but species less sensitive to CO_2 could become dominant in high CO_2 concentrations. Less food availability for higher trophic levels would imply reduced fisheries productivity, which would directly affect humans.

We have limited knowledge of deep-sea population, community structure, and ecological interactions. Because the deep ocean is an energy-limited environment, deep sea organisms reproduce slowly, and may take a long time to recover, should CO_2 injection reduce populations (IPCC, 2005, p. 299). Most deep sea life is adapted to low ambient CO_2 levels, and it is not known whether species would adapt successfully to permanently elevated CO_2 levels (IPCC, 2005, p. 301). Because the chemical and physical attributes of the deep ocean vary little, evolution has probably eliminated most individuals who could endure environmental perturbation (IPCC, 2005, p. 298).

According to the IPCC, injecting CO_2 into the ocean would have "ecosystem consequences" (2005, p. 14). However, we do not know what ecological effect CO_2 injection would have over large ocean areas and long time periods, and no environmental criteria have been identified to avoid adverse consequences. The IPCC recommends continued research to determine the potential for adaptation on evolutionary time scales.

Natural oceanic systems with high CO_2 levels can provide information on the possible physical and biological effects of oceanic CO_2 storage (IPCC, 2005, p. 300). Among these systems are hydrothermal vents, which release CO_2-rich fluids. Examination of communities near hydrothermal events would show how organisms have adapted to high CO_2 concentrations, but not how organisms adapted to normal ocean water would react to high CO_2 concentrations.

Deep saline brine pools are examples of large volumes of dense fluids on the seafloor, in which marine life cannot survive, and which emanate plumes. Brine pools might provide information related to the environmental impacts of a CO_2 lake on the seafloor.

In addition, changes in CO_2 concentration have occurred on geological time scales. There is evidence that changes in carbonate mineral saturation may be related to changes in calcifying organism distribution in the geological record, and CO_2 is thought to be a factor in the late Permian/Triassic mass extinction (IPCC, 2005, p. 300).

CO_2 release experiments have been performed on the continental slope and rise off California. These experiments have revealed a wide-range of behavioural and physiological responses in marine organisms in response to increased CO_2 and decreased *p*H. Abyssal animals located within 1 m of the CO_2 pools experienced mortality rates of >80% as a result of being exposed to periodic *p*H reductions of 1-1.5, while some fish survived month-long exposure (IPCC, 2005, p. 302).

Risk Assessment and Management:

There is no peer-reviewed literature on risk assessment or management in the case of intentional ocean carbon storage. The IPCC states that the response of deep ocean biological systems to injected CO_2 needs further study, and should be studied for longer durations and on larger spatial scales than previously (2005, p. 49). In addition, the IPCC states that techniques and sensors are needed to monitor CO_2 plumes and their biological and geochemical consequences (2005, p. 49).

If CO_2 were converted to bicarbonates or hydrates prior to or during release, ocean *p*H would be less affected, but costs and other environmental impacts also increased (IPCC, 2005, p. 14). For example, dissolving mineral carbonates such as limestone in CO_2 lakes could both minimize ocean *p*H change and increase the CO_2 storage time to 10,000 years. However, this would require large amounts of limestone and energy expenditure.

Liquid CO_2 could potentially be recovered from lakes on the seafloor (IPCC, 2005, p. 307), should unacceptable adverse effects arise. Hence, injection onto the seafloor and storage in lakes is, in theory, more reversible than injection into the water column, and concomitantly less risky.

Mineral Carbonates:

CO_2 forms stable carbonates when reacted with metal oxides, which are abundant in silicate minerals. In fact, the amount of silicate minerals found in the Earth's crust exceeds the amount that would be needed to fix all CO_2 produced by fossil fuel combustion (IPCC, 2005, p. 39). However, the reaction proceeds slowly, and must be enhanced through pre-treatment of the minerals, which is, at present, very energy-intensive.

Risks to Human Health and the Environment:

The environmental impacts associated with CO_2 storage in mineral carbonates arise from the necessary silicate mining operations, transport, and landfills to dispose of the useless products. Environmental impacts would include land-clearing, decreased air quality, decreased water quality, damage to vegetation, tailings, leaching of metals, and habitat degradation. Mining and its potential environmental impacts would no doubt cause some concern. However, prevention and mitigation practices are basic, and have been developed through years of experience (IPCC, 2005, p. 329). Thus, large-scale silicate mines would not present any fundamentally new risks.

Since the silica and carbonates produced are stable over long time periods, the CO_2 would not be released to the atmosphere. Once the carbonates had been formed, the need for monitoring would thus be virtually eliminated.

Industrial Uses of CO_2:

CO_2 can be used as a gas or liquid in industrial processes, or as a feedstock to produce products containing carbon. However, the amount of CO_2 required for such processes is small, the CO_2 is generally retained only for short times periods, and the processes do not always achieve net emissions reductions over the lifecycle of the product (IPCC, 2005, p. 8). Therefore, industrial use of CO_2 is not expected to contribute significantly to CO_2 emissions reductions, and I will not consider it further.

Transportation and Environmental Risk:

Many large point sources of CO_2 are located within 300 km of potential geological sequestration sites (IPCC, 2005, p. 8), which increases the feasibility of and decreases risks associated with transportation. However, only a small number of large point sources are located near potential ocean storage locations (IPCC, 2005, p. 8). The number of large point sources is likely to increase through the twenty-first century, and the proximity of future point sources to sequestration sites has not been studied; the IPCC foresees the need for detailed regional assessments to improve information (2005, p. 9).

According to the IPCC, CO_2 pipeline transport could pose similar or lower risks to the environment than do hydrocarbon pipelines. CO_2 pipelines have been in operation since the 1970s, and currently over 2,500 km of pipeline in the United States transports CO_2 to oil fields, where it is used for EOR (IPCC, 2005, p. 29). Because a sudden CO_2 release can imperil human life and health, risks to humans are lower for pipelines running through sparsely populated areas. However, leakage from pipelines is in general very small. If pipelines are routed through heavily populated areas, they will require a lower maximum allowable H_2S content, overpressure protection, and leak detection systems, among other features, as well as carefully selected routes (IPCC, 2005, p. 12). There has been less than one reported accident per year with operating CO_2 pipelines, and no human injuries or fatalities (IPCC, 2005, p. 30).

Marine tankers currently engage in large-scale transport of liquefied petroleum gases (LPG), and CO_2 could be similarly transported by ship, since the properties of liquefied CO_2 are similar to those of LPG (IPCC, 2005, p. 30). LPG tankers are potentially dangerous, but design, construction, and operation standards have rendered serious incidents rare.

Road and rail tankers are technically feasible, but uneconomical options as compared to pipelines and ships. As such, they are unlikely to play a large role in CO_2 transport.

CCS and Hegemony:

In the last chapter, I argued that through the technological application of scientific epistemological facts, the limitations of conceptual knowledge enter emergently complex systems. Technologies not only impart great power to those who wield them, but also physically embody the incompleteness of conceptual knowledge. Incautious application of technology can induce hegemonies in systems, compromising the complex balance characteristic of ordinary complexity. This can happen unintentionally, due simply to technology's nature as embodied conceptual knowledge: the technology will not have been designed to take all infinite facets of the system into account, and can thus induce an *ekānta* state when improperly applied. That is, technologies can potentially initiate physical hegemonies mirroring the limited conceptual schemes these technologies embody. When the system with which one deals is complex and nonlinear, the chances that one will neglect a non-negligible facet are far higher. Alternatively, technological power can also be harnessed to intentionally promote the hegemonic domination of one group's interests.

For example, monocrop agriculture de-complexifies – that is, *reduces* – a pre-existing complex ecological balance by replacing it with one dominant organism, whose dominance depends on continued technological intervention. Similarly, injecting CO_2 into the ocean would alter the chemical balance, inducing "carbon dioxide hegemony" – that is, CO_2 would come to dominate the chemical concentration in a way not in keeping with the natural balance of ordinary complexity. The result would likely be a *reduction* in marine ecosystem diversity and the emergence of species hegemony where none previously existed. That is, species resistant to high levels of CO_2 or low *p*H might come to dominate the ecosystem. The reductionist nature of the conceptually-based technology can thus result in literal reduction of the ecosystem.

In the case of ocean sequestration, the hegemony is partially predictable: we know that increased CO_2 levels will alter the chemical environment of the deep ocean (and eventually the whole ocean) and very likely kill some marine organisms, such as shells and corals, but we do not know the long-term, large-scale consequences for ecosystems.

As I pointed out in the last chapter, all conceptual knowledge, not just science and its embodiment in technology, is incomplete and reductionist. One thus might argue that human beings (indeed, any being governed by a conceptual scheme and priorities) have always had a reductionist effect on their environments, that technology's effects are no different, and are therefore unobjectionable. However, modern technology provides human beings with a sheer *power* unseen in natural history. Prior to the development of technology, human beings were not able to influence natural systems to the same extent that we now can; hence, natural forces could more often correct for or adapt to our reductionist effects. In contrast, since the invention of tools and agriculture, our ability to shore up increasingly large-scale hegemonies has increased along with our technological development. The effects of our reductionist technologies are qualitatively different because of this power: in general, we alter natural systems on a larger scale and far more quickly than ever before. The speed and scale of the changes, along with the inherently reductionist tendencies of technology, mean that our interventions decrease the resiliency of natural systems, for species, chemical balances, and ultimately the systems themselves are unable to adapt.

Carbon dioxide capture and storage involves direct technological intervention into the complex Earth systems. In light of the relationship between technology and hegemony, I suggest we approach CCS with caution. The cumulative effect of human technological interference in the environment has been to shore up the hegemonic domination of the human species, and our immediate concerns and interests, and to dangerously disrupt ecological systems. The use of fossil fuels, without regard for consequences to the biosphere, has enabled us to satisfy our needs and wants to an extent unseen in history. The result has been runaway consumerism in industrialized countries, along with unsustainable exploitation of natural resources, increasing pollution, and destabilization of ecosystems resulting in mass extinctions of other species.

Now, fossil fuel use is threatening our future not only indirectly, by enabling runaway exploitation of the biosphere, but also directly, by triggering climate change. Carbon capturing and sequestration would avert or reduce the risk of dangerous climate change, but it could simultaneously prolong our exploitation of the biosphere, by allowing fossil fuel use to continue, perhaps unabated, for some time. CCS could thus function as a short-term band-aid solution, for the source of our problems – including climate change – is not ultimately fossil fuel use, but our *ekānta* approach to interacting with our environment, which, combined with technological power, makes us very dangerous both to other species and to ourselves. CCS will not have served us well if it simply allows us to avoid, for a little longer, facing the fact that our ways of interacting with our environments are unsustainable.

We will not, however, accomplish the transition to a more non-one-sided interaction with the biosphere overnight, and it is because of this that CCS has the potential for legitimate use. Namely, it can function as a transitional measure, while we make efforts – in all due haste – to transform our lifestyle into one that can be maintained in the long run. Achieving what Funtowicz and Ravetz call "complementarity" will require not only a switch to cleaner and less carbon-intensive fuels, but re-examination of our entire approach to elevating our needs and wants over those of our fellow creatures and environment. As such, climate change can function as a 'precursor incident', warning of danger to our future, due to the imminent instability of human civilization – which we can see as one big industrial system – in its present form. If, however, CCS simply allows us to prolong the life of our hegemony, especially if it does so at the expense of causing further environmental damage – such as wide-spread mortality of marine organisms from ocean sequestration, more intensive non-GHG environmental impacts from power plants due to carbon capture systems, or pollution from mining silicate minerals for use in fixing CO_2 – then we will have failed to recognize and heed climate change as a warning signal, and thereby avert the autolysis of our industrial civilization.

Anekānta Use of Technologies:

The outlook for CCS as a viable mitigation measure might seem, at this point, grim on purely epistemological grounds. However, as I have already pointed out, all conceptual knowledge is incomplete and reductionist. It is not the reductionist tendencies of technology per se that are the problem, but ignorance of these tendencies – that is, *ekāntavādism.* Because technology imparts such power to reshape our physical environment, *ekānta* use of technologies is particularly dangerous. If we proceed under the impression that our technologies are not reductionist, we proceed in a one-sided fashion, and this – combined with the inherent power of technology – can result in hegemony and ultimately autolysis.

I believe that it is possible to design and use technology with the incompleteness of conceptual knowledge in mind, that is, in an *anekānta* fashion. Just as making *anekānta* statements involves acknowledging the partiality of one's view, applying technology in an *anekānta* manner would involve acknowledging and providing for its potentially reductionist effects. Because of the sheer power of technology, however, we must be doubly cautious in its use.

The Need for "Sufficient" Scientific Knowledge:

Sufficient scientific knowledge – or, more properly, *apparently* sufficient scientific knowledge – is a prerequisite for averting technological disaster. The qualification is, of course, *anekāntavādin*, with "apparently" gesturing to possibly non-negligible and multitudinous unknown facets of reality.

I specifically discuss scientific knowledge here, but that does not preclude the importance of other knowledges, such as local knowledge, or knowledge of ethics. Scientific knowledge is, however, an essential piece of the picture because it is the conceptual scheme best equipped to understand and predict the behaviour of physical systems. As I argued in the last chapter, the ontological realism of *anekāntavāda* and post-normal methodology allows me to acknowledge the proper sphere and unique insights of science without hegemonically elevating it as the only valuable form of knowledge.

Even when we think we possess the requisite scientific understanding to use a technology safely, unforeseen consequences may arise. Knowingly proceeding in absence of apparently sufficient scientific knowledge is simply asking for disaster. Apparently sufficient scientific knowledge is thus one criterion by which we should evaluate the various CCS options. Geological sequestration and mineral carbonates have a clear edge over ocean sequestration in this regard. The IPCC report states that scientific understanding of geological storage "rests on a large body of knowledge in hydrogeology, petroleum geology, reservoir engineering and related geosciences" (IPCC, 2005, p. 245). Natural geological reservoirs demonstrate the possibility of long-term CO_2 storage in rock formations, and provide models of safe storage sites. Techniques from the oil and gas industry are available for investigating the suitability of potential reservoirs, and computer programs can model underground CO_2 movement. Natural gas storage projects, deep underground waste disposal, and existing CO_2 storage projects provide experience, technology, and techniques directly relevant to geological CO_2 storage.

Similarly, a wealth of knowledge gained in the mining industry would guide the mining operations needed to recover silicate minerals for storing CO_2 in mineral carbonates. The probable environmental effects and risks of such large-scale mining are thought known, and strategies have been developed to deal with them. Whether these effects and remediation strategies are acceptable is another matter entirely.

In contrast to the above two storage options, little is known about whether or how CO_2 can be safely stored in the oceans. Elevated CO_2 levels and decreased *p*H over large parts of the oceans would very likely have long-term consequences for marine ecosystems, but the exact consequences are unknown. The little knowledge we do have indicates that elevated CO_2 concentrations negatively impact many species, and that species do not benefit at the ecosystem level. We know little about deep sea communities, but we do know that their member species tend to grow and reproduce slowly, and are adapted to low CO_2 levels. The impact of increased acidity on calcifying micro-organisms, while not fully understood, has the potential to be very negative, while the impact on coral reefs will almost certainly be disastrous. With regard to impact of ocean acidification on non-calcifying organisms (organisms that do not form shells), The Royal Society writes:

> The influence of ocean acidification on marine organisms other than those exerted through calcification, could

> include decreased reproductive potential, slower growth or increased susceptibility to disease. These responses could have cascading effects through food webs, with possible consequences for ecosystem structure and elemental cycling. (2005, p. 23)

Our current knowledge suggests that oceanic sequestration would have negative effects – potentially disastrous – and seriously calls into doubt, in my opinion, the possibility of safe oceanic storage.

The Need for "Continuous, Iterative Control":

Carbon dioxide capture and storage is a perfect example of science and technology creating new ignorance. Technological developments enable us to sequester CO_2 in the oceans and in geological formations, but along with this new knowledge, we have new ignorance about the long-term effects and safety of sequestration.

One way of compensating for the reductionist tendencies of, and the new ignorance generated by technology is through what Ravetz calls "continuous, iterative control" (1987, p. 420), which involves constant monitoring, and systems that can respond in a timely fashion to danger signals. Continuous, iterative control does not eliminate ignorance, but guards against unapparent gaps in our knowledge. Hence, another way of evaluating CCS is by asking to what extent continuous, iterative control is possible. Once again, the various storage options are not equal in this regard.

In the case of geological storage, a number of well-developed techniques already exist for monitoring injection rate and well pressure, tracking underground CO_2 movement, and detecting both rapid and slow leaks. Continuous monitoring is only half of control, however. Detecting problems is next to useless if nothing can then be done about them. In geological storage, it *is* possible to remedy leaks. For instance, standard oil and gas industry techniques for stopping well blow-outs could also halt abrupt CO_2 well leakage. CO_2 could be intercepted and extracted from the storage rock, and even removed from subsurface soil. Hence, geological storage is, to some extent, reversible, should serious problems emerge.

Continuous, iterative control thus appears feasible in the case of geological storage, and the requisite technologies are already well-developed. It seems it may be possible, in geological sequestration, to guard against gaps in knowledge that might otherwise have significant negative consequences – that is, "to make ignorance usable".

Once CO_2 is stored in mineral carbonates, which are stable, the probability of release to the atmosphere approaches zero. There is thus no need for monitoring or control once the carbonates are created. There would, however, be a need for monitoring and control of the large-scale mining operations needed to procure enough silicates. The degree to which the environmental and societal risks of the requisite mining operation could be controlled will determine the relative desirability of mineral carbonation as opposed to geological and ocean storage. The IPCC notes that "the preventing and mitigating practices are relatively basic and well developed" (2005, p. 329). Hence, storage in mineral carbonates also seems like it might possibly be associated with an adequate level of control.

Once CO_2 is injected into the oceans, there is limited potential for removal. It might be possible to recover CO_2 from lakes on the seafloor, but common sense indicates that CO_2 diffused through the ocean would be much harder to remove. This is an especially important point considering that serious problems might not emerge until substantial portions of the CO_2 deposited on the seafloor had already diffused, and were no longer available for recovery. Hence, ocean storage is not as reversible as geological sequestration. Even if we gather apparently sufficient scientific knowledge and develop adequate monitoring systems, limited potential for removing injected CO_2 from the oceans restricts our capacity to mitigate unforeseen problems. Since geological and ocean storage involve the same carbon capture systems and similar transport systems, the risks associated with capture and transport are similar for geological and oceanic storage. Ocean storage thus seems inferior overall to geological storage in terms of the possibility for continuous, iterative control.

Reducing Reductionism and the Probability of Unforeseen Incidents:

Another way to compensate for technological reductionism is to reduce it by including as many conceptual schemes as possible in designing and deploying a technology, bringing to bear the insights of an extended peer community. To ensure that our understanding of the *physical* system involved is as complete – that is, as non-reductionist – as possible, we should include many different scientific perspectives, as well as local and traditional knowledge. Geological engineers cannot alone ensure that offshore geological CO_2 injection proceeds safely, though they may possess essential knowledge: safety will require input from other scientists, such as marine biologists. As demonstrated by the IPCC report, the development and evaluation of CCS technologies already calls on an extended scientific peer community. The same can be said for climate science in general.

Technological intervention in natural systems may also cause unforeseen societal and ethical problems. Scientific knowledge can predict, to some extent, the physical effects associated with a technological intervention, but it alone cannot provide insight into whether the consequences of that intervention are ethically acceptable or desirable. Moreover, science and technology are not value-free: value assumptions are always encoded in any scientific knowledge and in technological applications. To avoid and/or mitigate ethical and social problems that might arise in connection with CCS, we should include as many different non-scientific viewpoints as possible when designing, evaluating, and deploying a proposed technological intervention. Hence, an open public dialogue is essential to clarify what values must be taken into account in planning CCS, which values be safeguarded, and which outcomes promoted.

With the involvement of an extended peer community including non-specialist members, science becomes genuinely post-normal. Scientific problems and technological goals are defined and redefined with input from this community, and with a broader societal and ethical context in mind.

The post-normal approach, with its associated extended peer community, is thus a third and essential way to ensure that technological intervention into natural systems is not *ekānta*. The extended peer community acknowledges reductionism by asserting the validity of multiple viewpoints, and the inadequacy of any one of them, and attempts to reduce reductionism by including as many viewpoints as possible and thereby capturing as many facets of reality as possible. It is with this in mind that I now turn to an examination of the IPCC *Special Report* in the context of post-normal methodology.

The IPCC Report and Post-Normal Science:

According to Ravetz (2004), post-normal situations involve ethics, society, ecology, safety and sustainability, and in post-normal situations, "we can no longer separate nature, science, and society," (p. 348). Post-normal science must cope with complex real-world problems, rather than the abstracted and controlled environment of the laboratory.

Carbon dioxide capture and storage will quite clearly be a post-normal issue: it involves ethics, society, ecology, and safety, and the scientists and engineers who work on it are working in the complex real world. Further, CCS quite clearly falls into the category "precautionary science", as defined by Ravetz (2004), for it is a reaction to the harmful effects of progress, and lies at the interface of science and policy.

If we accept Funtowicz's and Ravetz's arguments about the need for post-normal science, then CCS quite clearly calls for a post-normal approach. I now examine the IPCC special report as precautionary science, and evaluate how closely it resembles an instantiated piece of post-normal dialogue.

The stated purpose of the *Special Report* is to assess current knowledge regarding the "technical, scientific, environmental, economic and societal dimensions of CCS" (2005, p. 19). In addition to evaluating CCS technology and potential for mitigating GHG emissions, the IPCC identifies costs, health, safety, and environment risks, legal issues, and the public perception of CCS as important issues for the report (2005, p. 22).

The report states the following about climate change in general:

> The climate change issue involves complex interactions between climatic, environmental, economic, political, institutional, social, scientific, and technological processes.

> It cannot be addressed in isolation from broader societal goals, such as equity or sustainable development (IPCC, 2001a), or other existing or probable future sources of environmental, economic, or social stress. In keeping with this complexity, a multiplicity of approaches has emerged to analyze climate change and related challenges. (p. 70)

The group of specialists responsible for this report clearly understands the impossibility of separating "nature, science, and society" when it comes to CCS and climate change; all facets of the issue, including economic, societal, and environmental are important in evaluating the viability and desirability of CCS as a mitigation option. Moreover, the *Special Report* links complexity with the use of multiple analytic approaches, which strongly echoes Funtowicz's and Ravetz's call for an extended peer community, and the Jaina assertion that multiple viewpoints more accurately capture multifaceted realities. If the report had instead simply focussed on the scientific and technological feasibility of CCS, and neglected social context and the need for multiple analytic approaches, it would clearly not be in agreement with post-normal principles.

Several foci of the IPCC Special Report are particularly relevant for assessing the extent of its orientation toward post-normal methodology. The chapters on geological, oceanic, and mineral carbonate storage include sections on societal requirements, including public perception of CCS, and on risk assessment methodologies. I will now focus on these sections.

Societal Requirements:

Other than cost-effectiveness and the ability to usefully supply energy, the *Special Report* identifies legal requirements and public acceptance as two aspects that must be addressed before CCS can be widely used (2005, p. 69). The IPCC identifies some prerequisites for public acceptance of CCS, including assurance of reduced overall emissions, negligible threats to human health and ecosystems, safe transport, and adequate provisions for long-term monitoring (2005, p. 70).

New legal challenges likely to be associated with CCS include licensing procedures, containment criteria, geological stability, potential hazards, interference with other activities and with property rights, and long-term responsibility for safety (2005, p. 69). For example, should corporations or governments be responsible in the long run for ensuring that CO_2 remains safely stored? Who should assume liability for any damages resulting from leaks? These questions obviously call for a post-normal dialogue, with all stakeholders involved on even footing. If corporations and governments alone solve these legal issues, the process will not qualify as *anekānta*.

In terms of public acceptance, the *Special Report* identifies the following as relevant questions:

> What form of public consultation will be needed before approval of a CCS project? Will the public compare CCS with other activities below ground such as the underground storage of natural gas or will CCS be compared to nuclear waste disposal? Will they have different concerns about different forms of storage, such as geological or ocean storage of CO_2? Will the general attitude towards building pipelines affect the development of CO_2 pipelines? (p. 69)

In terms of the form of public consultation required, Funtowicz and Ravetz would of course answer that a post-normal dialogue is required.

The report also mentions the dearth of studies carried out on public attitudes toward CCS. The above list of questions and the comments on "studies of public attitudes" immediately beg several remarks. Firstly, the list of questions demonstrates that the IPCC recognizes public acceptance as necessary for implementing CCS, and that some form of public consultation is necessary. This is a significant step toward a post-normal approach.

Secondly, the authors identify the public as "they". This demonstrates that the public has not been involved in producing the IPCC Special Report. Indeed, examining the list of authors and reviewers at the back of the Report reveals academics, statisticians, economists, representatives of national environmental assessment agencies, institutes of science and technology, research centres, and large corporations like Shell, and other credentialed specialists. Conspicuously absent are environmental interest groups and other members of the "public", such as ordinary citizens representing specific geographical areas. Significant members of the extended peer community are not represented in this assessment of CCS, so the *Special Report* does not instantiate genuinely post-normal dialogue. In fact, the peer community responsible for the report is composed of mainly scientists, economists, and professionals associated with the fossil fuel industry. A scientists or other specialist might argue that environmental organizations and other special interest groups ought not to be involved in preparation of the report, for they would bring with them values and vested interests with the potential to "skew" the science. However, just because the report was written by credentialed specialists and scientists does not make it value-free, nor guarantee that the science is not "skewed"; as I have already argued, science, being conceptual, cannot avoid being informed by values, and the values of specialists are just as capable of "skewing" science as are any other. Of course, one could argue – and I think this is the case – that the IPCC is already controlling the quality of its reports to a greater extent than any other scientific documents in history. The body is doing this by involving many different viewpoints – though they are expert – in preparation of the reports.

The focus on "studies" of public perception of CCS could maintain a division between scientists and non-experts, with non-experts as objects of study to be analyzed and then accounted for in planning, rather than genuine participants in a dialogue. These studies represent public opinion in the *Special Report*; whether or not this is objectionable depends on what one thinks the report's ultimate role should be in post-normal dialogue: should the report instantiate post-normal dialogue, or just precede it? One thing is clear, however: exclusive use of "studies" as a form of public consultation would not allow an open discussion between specialists and members of the general public, with non-specialists and specialists on genuinely equal footing, and so would be highly disagreeable from a post-normal perspective.

Another societal issue the *Special Report* addresses is sustainable development: environmental conservation, social equity, economic growth, and poverty are all to be accounted for in relation to climate change mitigation (2005, p. 70). For CCS in particular, we must show that the technology is compatible with long-term sustainability, in that the potential for leaks can be adequately controlled. Among the relevant questions, the Report identifies the following:

> Will CO_2 capture and storage favour the creation of job opportunities for particular countries? Will it favour technological and financial elitism or will it enhance equity by reducing the cost of energy? In terms of sustainable development, does the maintenance of the current market structures aid those countries that traditionally market fossil fuels, relative to those that import them? Is this something that mitigation policies should be developed to assist? There are no simple answers to these questions but policymakers may want to consider them. (p. 70)

Once more, the *Special Report* discusses sustainable development in relation to CCS, demonstrating a positively post-normal awareness of the broader context of science and technology. However, I point out that, in a genuinely post-normal approach, scientists as well as policymakers would want to consider the above questions, rather than simply providing scientific inputs to policy. That is, the above questions should shape scientific and technological efforts to develop mitigation options, as well as policy to implement them.

Risk Assessment Methodologies:

The chapter on geological sequestration includes a section on risk assessment methodology. According to this section, risk assessment involves developing a detailed inventory of risks and their underlying mechanisms, scenario-building, and, ultimately, mathematical-physical models. This section makes no mention of public involvement; rather, the kinds of risk assessment covered in the *Special Report* are conducted by scientists, engineers, and other credentialed professionals. The report thus presents expert points of view on risk assessment, rather than the points of view of a post-normal peer group.

A post-normal risk assessment methodology would involve members of the extended peer community from the beginning of the risk assessment process. For instance, non-scientists and interest groups could be involved in developing risk assessment methodologies and models. This would not entail abandoning scientific rigour, for, in addition to having their own perspectives respected, legitimate members of such a process would have to be willing to listen to and respect the special insights and knowledge of scientists and engineers. This could potentially enrich the risk assessment process by bringing to the table different values and perspectives, which might alter the risk assessment methodologies adopted. Involving non-scientists would increase transparency and public understanding of science and engineering, and perhaps even create public investment in and ownership of CCS as a mitigation option. Again, whether or not the *Special Report* should be faulted for its portrayal of risk assessment depends on what one believes its role should be in post-normal dialogue. I will expand upon this point further in the conclusion to this chapter.

Public Perception of Geological and Ocean Storage:

The IPCC notes that the public possesses insufficient knowledge about climate change and about various mitigation options (2005, p. 257). "Insufficient knowledge" presumably means insufficient scientific knowledge to evaluate CCS compared to other mitigation options, and engage in a dialogue about whether to implement it. The few studies that have been done on public perception of CCS indicate that people generally view geological storage less favourably than other climate change mitigation options, like energy efficiency improvements and the use of less carbon-intensive fuels (2005, p. 257). In addition, the public views ocean storage more negatively than geological storage (2005, p. 258). For instance, during an interview on CCS, a 44-year-old female stated the following:

> …if we were to put it…in the ocean, we could be messing with some form of life that's on the bottom. I don't think we have much knowledge of what's down there. Because we really can't explore that deep. So we'd be messing with something we have no knowledge of. (Palmgren *et al.*, 2004, p. 6442)

According to the IPCC, people may be less than enthusiastic about CCS because they see it as "treating the symptoms not the cause, delaying the point at which the decision to move away from the use of fossil fuels is taken, diverting attention from the development of renewable energy options and holding potential long-term risks that are difficult to assess with certainty" (2005, p. 258). These concerns echo my own about the potential of CCS to temporarily prop up human global hegemony, which is, in the long run, unworkable.

The discussion of public perception of CCS in the *Special Report*, and much of the research done in this area, seems largely geared toward predicting the trajectory, dynamics, and salient issues of the debate that will emerge when CCS becomes a technologically mature mitigation option. For example, Reiner and Herzog (2004) attempted to find analogues to CCS with similar regulatory characteristics, in order to predict public acceptance. Such attempts at prediction can be seen as attempts to understand the viewpoints that various members of the public might have. If this is the case, then efforts to study and understand public opinions are *anekāntavadin* and post-normal in character.

Besides public acceptance of the need to decrease CO_2 emissions, and more extensive scientific knowledge, Palmgren *et al.* (2004) identified open, respectful communication as necessary for public acceptance of CCS:

> High levels of public acceptance will almost certainly require....an approach to public communication, regulation, monitoring, and emergency response that is open and respectful of public concerns. An open and inclusive approach does not guarantee success. However, an arrogant approach such as the one adopted in the past by the industries responsible for nuclear power and genetically modified crops could create a level of public distrust that

> makes the widespread implementation of carbon sequestration in the United States difficult, if not impossible. (p. 6449)

This is reminiscent of the Jaina use of *syāt* as a depolarizing, conciliatory and at the same time persuasive technique in argumentation, getting an opponent to agree by conceding part of his or her argument, and persuading him or her to see a different point of view. It also displays awareness that public acceptance is a force to be reckoned with. Funtowicz and Ravetz call for a less arrogant approach in order to assure the quality of science for policy; Palmgren *et al.* realize that arrogance is not conducive to agreement. Open, respectful dialogue is thus in the interests of CCS proponents, as well as other stakeholders.

The wording used in Reiner and Herzog (2004) is less encouraging from a post-normal, and from a Jaina perspective:

> A community (or a subset of activists or homeowners) can fend off siting of an undesired facility under cover of many legitimate guises including charges of environmental justice or racism, lack of public participation, scientific uncertainties, and the need to pursue a permitting process. That does not mean that there are not valid, even egregious, cases, but being able to distinguish bona fide cases is especially difficult in the midst of often intense local disputes. Several key questions surround the regulatory conditions for successful siting. Does public and non-governmental organization (NGO) participation facilitate resolution or entrench and encourage conflicts? Does the potential for human health damages (as assessed by experts) increase opposition or is the basis for opposition not influenced by peer-reviewed scientific studies? Can the permitting process facilitate progress or is it primarily the source of delay and obstruction? (p. 1563)

This wording implies that community objection to a CCS facility can be illegitimate: the community members or activists can fend off the facility "under guise of" legitimate objections. This is a judgment against the validity of another's viewpoint, and is therefore one-sided. Reiner and Herzog seem to ask whether public participation is desirable at all, or if it just gets in the way. Later in the article, they state the following:

> Strategies that offer concrete benefits or promote trust in affected communities and that remove legitimate arguments as camouflage for self-interest can overcome public goods problems. Committing to compensation, openness, information sharing, monitoring and enforcement can help diffuse legitimate grievances. This strategy will add to the costs and lead to delays, but so too will a permitting process where the public feels disenfranchised. (Reiner and Herzog, 2004, p. 1569)

Hence, Reiner and Herzog also recognize openness as a method of obtaining public consent for CCS facilities. They note that such strategies take time and lead to delays, but that so do more disenfranchising tactics. However, the wording of this paragraph, according to my reading, indicates that Reiner and Herzog regard so-called "legitimate grievances" as camouflage. NIMBY (not in my backyard) battles are not about the purported issues, but rather some vague ulterior motive called "self-interest". This seems to me patronizing, and also suspect. As a Jaina philosopher would no doubt point out, not only is the legitimacy of concerns viewpoint-dependent, but all concerns are ultimately based on "self-interest". Thus, while Reiner and Herzog recognize the utility of open discussion, they retain a viewpoint wherein specialists and experts can judge the legitimacy of others' concerns. This is definitely more arrogant than the attitude taken by Palmgren *et al.*

These points perhaps reflect less upon the *Special Report* itself, however, than they do on the potential uses of the report, and of the research it summarizes. That is, research into public perception of CCS could be used in either an *ekānta* or an *anekānta* fashion, and the form of usage will depend not just on scientists, but also on policy-makers and other powerful participants in the CCS dialogue. If genuinely open discussion follows public perception research, the process will be post-normal. Otherwise, it will not be. The existence of at least some awareness among specialists of the need for open dialogue is, however, an encouraging sign.

The Role of the Special Report in Post-Normal Dialogue:

While the IPCC authors display keen awareness of the multifaceted nature of climate change, and the societal context of CCS science and technology, the report lacks salient feature of a genuinely post-normal dialogue. Notably, it was prepared entirely by specialists, not a truly extended peer community, which would include, among others, ordinary citizens. This exclusion of non-expert members of the general public from the peer community is evinced by the report's reference to members of the public as "they", and its survey of expert-conducted studies on public attitudes toward CCS.

Hence, the IPCC report does not genuinely instantiate post-normal methodology. The question remains: is this a failing for which the report should be condemned? If one views the report as a prelude to post-normal dialogue, one that does not preclude post-normal dialogue, then one might argue that the report should not be criticized for its specialist nature. According to such a viewpoint, the *Special Report* collects and summarizes expert knowledge regarding CCS, laying part of the groundwork for and thereby facilitating a post-normal dialogue. The report is to be seen as one voice among many – the collective voice of academic and industrial experts – rather than itself an opportunity for post-normal dialogue.

CCS became a salient topic for investigation by physical scientists, social scientists, and engineers because of perceived need to mitigate climate change, which is a global social and ethical problem. Because CCS research is driven by such a socially pressing, ethically complicated, and value-charged issue as climate change, one might argue that the extended peer community has already had a role in selecting the scientific, engineering, and social scientific problems. In conducting the research preceding, and then in preparing the *Special Report*, experts were strongly aware of the ethical and social context of their work – this much is quite clear. Nonetheless, in preparing the report, the specialists involved were working as an *expert* peer community. The "Introduction" to the IPCC, found on the *About IPCC* page of the organization's website, states the following:

> IPCC reports are written by teams of authors, which are nominated by governments and international organizations and selected for a specific task according to their expertise. They come from universities, research centers, business and environmental associations and other organizations from more than 100 countries. (2004, p. 4)

Hence, IPCC authors are selected specifically "for their expertise". Policy considerations guide their expert review of the literature:

> The IPCC usually starts a new assessment by developing a general outline, often during a "scoping" meeting of experts. Policymakers and other users of IPCC reports are consulted in order to identify the key policy-relevant issues. After the outline is approved, teams of lead authors are assembled for each chapter. Chapter teams should include experts from all regions and represent a range of expertise and prevailing scientific-technical views. (2004, p. 4)

Though policy considerations guide the literature review process and production of reports, these reports are still expert productions, with no policymakers or laypersons involved in the assembly of knowledge and writing processes. I note that an effort is made to include diverse viewpoints among the experts, including persons from all regions, different expertises, and varying scientific-technical views. Though certain viewpoints, such as those of laypersons and philosophers, are not yet involved, this is a step toward non-one-sidedness.

One might alternatively argue that post-normal approaches should involve multiple specialist and non-specialist viewpoints at every stage of the process. According to such a viewpoint, laypersons and philosophers, for example, should have been directly involved in the research preceding and in the preparation of the *Special Report*, specifically because CCS is an issue of wider societal importance. The motivation for this argument is as follows: it is in the early stages of scientific and technical research, when methodologies, approaches, and concepts are first defined, that values get encoded in scientific frameworks. For this reason, discussion of ethics and values cannot follow definition of a science or technology, but must occur concurrently. Informed by *anekāntavāda*, it is easy to understand why this might be the case. Conceptual schemes, by their very nature, are shaped by values and interests, and it is in the early stages of a scientific or technical enterprise that the conceptual schemes guiding that enterprise are shaped. Hence, when scientific and technical issues, such as CCS, are of broad societal importance, a wide variety of stakeholders should be involved from the very outset, to ensure that the science and technology reflect and safeguard, to the greatest extent possible, the most important values of all stakeholders.

One possible counterargument goes as follows: the quality of any post-normal process is determined, in large part, by the quality of inputs provided by the various participants (as well as by the willingness and ability of the participants to listen to each other). There are at least two components to the quality of any input. Firstly – and this strategy has already been extensively explored by Funtowicz and Ravetz, as well as the Jainas – inputs will generally be of higher quality when they have been subject to examination by multiple viewpoints to fill in the inevitable blind spots of any conceptual scheme. However, specialized knowledge is also high quality – that is, useful for its intended purpose – *specifically because it is specialized.* To ground this claim conceptually, I return to the Jaina distinction between *pramanas* and *nayas*, where a *pramana* is a whole ocean, and a *naya* a cup of ocean water. Recall that a *pramana* is a sweeping statement that attempts to gesture to all facets of reality at once. Recall also that, according to the Jainas, *pramanas* tend to conceal the finer details of entities. These finer details are exactly what *nayas*, or standpoints, reveal. Conceptual schemes, or viewpoints, bring reality to light as much as they obscure it. In this way, knowledge and ignorance are truly inseparable. Scientific and other specialized academic viewpoints illuminate certain details of reality, just as other viewpoints illuminate other details.

Given the value of *nayas*, does it make sense to involve laypersons, who are non-specialists, in the production of "scientific" knowledge? If one believes that scientific *nayas*, in themselves, have value for the post-normal process, one might argue that it does not. One might argue that there is immense value in the production of expert knowledge (or, in the case of IPCC reports, *collection* of expert knowledge) by communities of experts and by individual experts – whether they are scientists, native persons, organic farmers, economists, or any other concerned members of the public – *without the intrusion of other viewpoints.* One might further argues that involving oneself in the production of specialized knowledge arising from another viewpoint one does not share is not mutual dialogue and understanding, but imperialism. Toleration involves respecting boundaries (though these will often be fuzzy), and the value of others' knowledge. Mutual understanding arises when parties try to understand each other's viewpoints, and specialized knowledges, not when they try to invade each others' knowledge-generation processes.

This would not, of course, mean that scientists working on post-normal issues should not be in constant dialogue with non-scientists. As I already noted, the *Special Report* was prepared in response to a pressing societal issue. It is full of references to broader ethical and social context. This awareness should precede, inform, and permeate scientific knowledge-production in post-normal situations, but it would still be scientists who produce and collect scientific knowledge. One might argue that it cannot be otherwise, for if scientists did not produce the knowledge, it would not be scientific, and would no longer carry the special insights of the scientific *naya*.

It is also important to take into account the length and complexity of the current IPCC process, which involves hundreds of individuals and takes several years. To represent in this process every viewpoint would result in an unmanageably large extended peer community, and an unrealistically long process. From a practical point of view, it is hence not possible to include a perfectly representative extended peer community. To do so would not only dilute beyond recognition the value of the scientific *naya*, but handicap the IPCC process beyond usefulness.

Determining the size of the extended peer community seems to me an optimization problem. The peer community should be large enough to include all pertinent viewpoints, but not so large that it completely dilutes the specialized knowledge of those viewpoints included, or results in an unmanageable process. Whether or not the IPCC peer communities are optimal is a question for further consideration, and not to be decided simply academically, but through public dialogue.

The preceding argument regarding the value of *nayas* might be considered problematic because science has traditionally been valued more highly in our society than other forms of knowledge. Though this power imbalance may be undesirable (and I think it is), I do not think it immediately implies the desirability of invading scientific knowledge production with other forms of knowledge, as science has at times invaded them.

Let me now re-examine some potential criticisms of the *Special Report*. I mentioned the references in the report to studies of public perception of CCS, and argued that exclusive use of such studies as a form of public participation would not constitute a genuine post-normal dialogue. The inclusion of such studies in the IPCC report is not itself objectionable, however, if the report is seen as an expert report – an input to post-normal dialogue rather than a dialogue itself. However, a genuine post-normal dialogue would have to follow the report. If the report were followed by more studies of public perception and application of CCS technologies without consultation with a truly extended peer community, the process would not count as post-normal. On the other hand, if one believes that philosophers and other scientific laypersons should have been involved in the very production of the IPCC report, representation of non-expert opinion by inclusion of the above studies is objectionable.

The IPCC writes that its reports have "become standard works of reference, widely used by policymakers, scientists, other experts and students" (2004, p. 1). For a dialogue to classify as post-normal, at a minimum, the report would have to be used by laypersons as well. What should the role of the IPCC be in facilitating post-normal dialogue following production of its reports, and, possibly, in involving scientific non-experts in actual production of the report? That is, to what extent is it up to the IPCC to ensure that laypersons have access to the report and the opportunity to engage in real dialogue on CCS?

Part of non-one-sidedness is, I think, aware of one's own position with respect to others, including power differentials and possible biases in one's own opinion. The Jainas, with their emphasis on nonviolence toward the viewpoints of others, might argue that persons in positions of relative power, if they wish to act in an *anekānta* manner, should endeavour to empower less powerful participants in any dialogue. Hence, one might argue that, in any discussion of CCS, the onus would be on the powerful members to ensure that those less powerful had equal participation in the dialogue. These powerful members would include policy-makers, corporate interests, and the IPCC. Hence, one could argue that, from an *ankekāntavādin* standpoint, there is some onus on the IPCC to ensure that less powerful members of the dialogue participate equally.

The risk assessment methodology outlined in the report, which does not mention the inclusion of laypersons, is also not objectionable if viewed as the contribution by scientists and engineers to an overall risk assessment methodology. Of course, to count as post-normal (and simply for wise political procedure), the risk assessment process for any given CCS site would have to include, for example, local citizens, scientists or not, who would be most immediately affected by any accidents. This is doubly so because risk is highly subjective – different people with different values will regard different sorts of risks and levels of risk as acceptable. However, if one takes into account that values are likely encoded in risk assessment procedures early on, then perhaps scientific laypersons should be involved in the development of risk assessment methodology from the very beginning.

I will end with two points on which I think post-normal criticism of the IPCC *Special Report*, and the IPCC reports in general, are most likely justified from either of the above positions. The first is the review process, which involves experts and members of governments:

> To ensure that they are credible, transparent and objective, the IPCC reports must pass through a rigorous two-stage scientific and technical review process. For the first review, the drafts are circulated to specialists with significant expertise and publications in the field. Revised drafts are distributed for the second review to governments and to all authors and expert reviewers. (IPCC, 2004, p. 4)

It seems to me that if governments are reviewing the report, then in order to avoid skewed governmental and political influence on science, it might be wise to have other stakeholders also review the report. The review process may be an appropriate point for the extended peer community to enter dialogue with the IPCC specialists. Alternatively, perhaps governments should not review the report.

The second point for potential criticism is the use of the word "objective" both in the above quote and in the following quote, which describes the rationale behind the IPCC's existence:

> Because climate change is such a complex and challenging issue, policymakers need an objective source of information about the causes of climate change, its potential environmental and socio-economic impacts, and possible response options. (IPCC, 2004, p. 1)

It should be evident from my extensive discussion of conceptual schemes that all viewpoints are informed by values and interests. Indeed, values and interests cause us to notice certain features of reality in the first place. Hence, objectivity – a value-neutral viewpoint – is an impossible oxymoron. In fact, it is specifically in scientific claims to objectivity, and so to special status, that scientific *ekāntavādism* is located, for claims to objectivity imply belief that one's viewpoint is not context-dependent. One might thus argue that setting up the IPCC reports as "objective" elevates them over other forms of knowledge, and is *ekānta*.

The use of the word "objective" here is unfortunate, yet I think its intent may not be to permanently elevate the scientific, social scientific and technical knowledge collected by the IPCC above all other viewpoints. The *Special Report* demonstrates awareness that multiple perspectives are needed to address the complexity of climate change, and raises the question of what sort of public consultation is called for with regard to CCS. That is, the authors are aware that laypersons' opinions – and so their knowledge – must be taken into account.

I think, as it is used by the IPCC, the word "objective" has several functions in the above quotes. With regard to the review process as a means to ensure "objectivity" in the report itself, I think objectivity means, firstly, an acceptable level of consensus. Secondly, it means controlling any political or other agendas that might lead participants to lie about the state of the physical science, as outspoken persons with vested interests in the fossil fuel industry have done throughout the global warming "debate". This is a matter of internal policing on the part of the scientists and experts (and government members) engaged in the review. Nonetheless, "objective" is perhaps not the best word.

It might be better to say, "true to the accepted methodologies and values of the expert communities involved". These methodologies and values are accepted, of course, because they are judged to produce high quality knowledge of whatever specialized type the expert community produces.

I think "objective" in the second quote really means "qualified", that is, equipped with the requisite conceptual schemes to provide knowledge about certain features of reality. Thus, this use of "objective" raises, once again, the problem of legitimate membership in the extended peer community, and gestures to the fact that arguably illegitimate peers have dominated the climate debate. "Objective" here asserts that the experts involved in the IPCC reports are qualified to provide "information about the causes of climate change, its potential environmental and socio-economic impacts, and possible response options", and also that they are the *most* qualified to do so. From a post-normal perspective, this statement is objectionable, for people whose conceptual schemes have not been shaped by formal academic training may – simply because they hold a different viewpoint – see social dynamics, response options, and potential environmental impacts that academically-trained specialists would miss.

Conclusion:

My analysis in this chapter has been, in broad strokes, two-pronged. Firstly, informed by an understanding of technology as embodied conceptual knowledge, I have evaluated CCS based on its potential to promote *ekānta* hegemonies in the Earth system, with associated negative consequences. Because CCS technology is powerful and cannot possibly account for the infinitely many facets of complex Earth systems, its application may have inadvertent reductionist effects on these systems, promoting hegemonies and ultimately negatively affecting human beings. Alternatively, we may use CCS intentionally to prop up and prolong human *ekānta* hegemonic exploitation of the biosphere, just as we have in the past used technologies to promote our interests, with inadequate provision for effects on the rest of the Earth system.

The reductionist potential of CCS does not, however, mean that we should reject it out of hand, for all conceptual knowledge is reductionist, and our interactions with our environment inevitably have reductionist tendencies. Through application of Jaina principles and the post-normal methodology, we can avoid *ekāntavādism* in our application of CCS technology, and thereby minimize the danger of destructive side effects. In this more positive scenario, CCS may function as a stopgap measure, smoothing our transition to a less exploitative, one-sided and correspondingly short-sighted interaction with our environment. In such a case, climate change will have functioned as a "precursor incident", allowing us to avoid the imminent autolysis of the global human industrial system.

We can achieve *anekānta* use of CCS technology by firstly ensuring that we have the requisite scientific knowledge, and then ensuring that "continuous iterative control" is possible, so that we can make our ignorance usable and mitigate unforeseen consequences. According to these criteria, geological and mineral carbonate storage are clearly preferable to oceanic CO_2 storage. Even if we gather apparently sufficient scientific knowledge about the effects of injecting carbon dioxide into the oceans and develop adequate monitoring technologies, we will have limited potential to mitigate unforeseen problems arising as a result of oceanic CO_2 storage. Finally, by involving multiple perspectives – both specialist and non-specialist – in the development and deployment of CCS, we can reduce reductionism by capturing as many facets of our complex physical and social reality as possible.

My second focus in this chapter has been on the IPPC *Special Report* itself, specifically the degree to which it instantiates post-normal methodology. The *Special Report* is not a post-normal document, but this is not necessarily a failing. The report can be seen as a primer on science, technology, environment, economics and societal issues as seen from the points of view of specialists. This position is motivated by the argument that part of what makes specialist knowledge high quality is the fact that it is specialist. This corresponds, in the Jaina conceptual scheme, to recognizing the value of *nayas* as well as *pramanas*. According to this viewpoint, the report is not an end to post-normal debate, but a potential starting point for such debate. Whether or not post-normal debate develops does not depend only on the IPCC. Public consultation regarding CCS could be *ekānta* or *anekānta*, and the usage of the *Special Report* will depend not just on scientists, but also on policy-makers and other powerful participants in the CCS dialogue.

On the other hand, values get encoded in scientific enterprises early on, during that phase when the science is being defined. One might argue that when a scientific enterprise, such as CCS, has broad implications for society as a whole, as many different viewpoints as possible – including non-specialist viewpoints – should thus be involved in development of the science from the very beginning. According to this argument, scientific non-specialists, such as philosophers and other laypersons without academic training, should have been involved in preparation of the *Special Report*.

Chapter 6: Conclusion

Philosophy and climate science remain at some distance from each other in the academic peer literature. Climate science is one of the most interdisciplinary scientific fields currently active. Integrated assessment even involves specialists working in fields other than the physical sciences, such as social sciences and economics. However, exchange of ideas between philosophers and climate specialists remains limited, so that philosophers are not really recognized members of the climate peer community.

I hope that, in this thesis, I have shown how philosophy can contribute to the climate dialogue. The epistemologies of *anekāntavāda* and post-normal science provide insights into many aspects of the climate problem, including models of an extended peer community adequate to ensure the quality of climate policy, a dialogic model for this peer community, and criteria by which to judge various mitigation options. The ontologies of *anekāntavāda* and post-normal science are congruent with what modern climate studies are discovering about the climate – that it is a highly complex system with many facets, both social and physical, and that multiple analytic approaches are required to adequately address climate change. *Anekāntavāda* and post-normal science complement this insight, by suggesting specific approaches, grounded in epistemological understanding, to ensure the quality of climate science and policy. The extended peer community brings to bear on the problem multiple viewpoints and their attendant unique insights. This can increase understanding not only of the physical climate system, through avenues such as local knowledge and community-based research, but also provides equally essential awareness of the complex web of values and interests associated with climate change science and mitigation.

In this thesis, I have firstly compared *anekāntavāda* and post-normal science. Post-normal science identifies traditional scientific reductionism as the source of environmental problems, but the viewpoint of *anekāntavāda* reveals that not only scientific, but all conceptual knowledge is reductionist: conceptual schemes necessarily pick out only certain features of reality, and then construct explanatory frameworks based on these features. The problem with traditional scientific knowledge is not that it is reductionist, but that it has been *ekānta*, believing its partial view of reality to be complete. The dangers associated with this one-sidedness are exacerbated by the sheer power of technology, so that *ekāntavādin* technological interventions into the earth system have the potential to cause serious harm.

I believe, however, that it is possible to use technology in an *anekānta* fashion. In *anekāntavāda*, one acknowledges the context-dependent, partial nature of one's knowledge by qualifying it. In this way, one avoids doing violence to other viewpoints. Similarly, *anekānta* use of technologies would involve compensating for their reductionist tendencies by recognizing, in various ways, their context-dependent applicability, and the incompleteness of the conceptual knowledge responsible for generating them.

Secondly, I have applied *anekāntavāda* and post-normal science specifically to carbon dioxide capture and storage, to show how these philosophies provide criteria by which to evaluate mitigation options. The post-normal methodology offers continuous, iterative control and the insights of the extended peer community as ways of compensating for the reductionist tendencies of science and technology. Continuous, iterative control is also one criterion by which to evaluate the various carbon capture and storage options. Because we know any technology is liable to neglect certain features of the complex environment, technologies that offer the possibility of continuous, iterative control are preferable to technologies that do not. By this criterion, geological storage and storage in mineral carbonates are preferable to ocean storage, for which reversibility is limited and long-term consequences unknown, and potentially severe.

The extended peer community dialogue offers a second way to evaluate technologies. The *IPCC Special Report on Carbon Dioxide Capture and Storage* displays post-normal awareness of the broader social context of CCS science, and the need for public dialogue, but the report lacks essential features of post-normal methodology. The peer community responsible for the report is composed of specialists, and lacks community members and representatives of grassroots environmental groups, for example. This is not objectionable if the report is seen as an expert input, one among many inputs, and is followed by a post-normal dialogue, with all stakeholders on even footing. However, since values enter and shape scientific undertakings early on in the process, one might argue that scientific laypersons should have been involved in preparation of the *Special Report* itself.

I have left at least one significant outstanding issue: who decides, and how, which stakeholders are legitimate members of the peer community? I have already identified one requisite; namely, to qualify as a legitimate member, one must be willing to listen to, respect, and understand others' viewpoints and special knowledges. I add to this a certain level of capability – that is, one must be capable of understanding the other viewpoints involved. However, these criteria do not identify any procedure by which someone should or should not be granted membership in the community. Who should have the power to grant such membership, and through what process? Should the community as a whole grant membership? How can toleration and genuine openness be ensured, or at least promoted? Exploration of these questions would, I think, require another thesis.

The goals and methods of this project have been, on several levels, *anekāntavadin.* The thesis is, appropriately, interdisciplinary, my background being in science, social sciences, and, to some extent, philosophy. As well as being a philosophical examination of climate science, the thesis is an indirect argument for a closer dialogue between philosophers and climate specialists. As evidenced by the confusion and bemusement I often encountered when explaining my thesis topic to others, there has not yet been any frequent or voluminous exchange of ideas between these two academic fields. It is difficult to see how this can help either philosophers of science or climate specialists, for both have insights relevant to the others' work. One reason for the division is, perhaps, the highly practical, applied, and immediately relevant nature of climate studies, which might seem to clash with philosophy's theoretical, abstract, removed image. Yet I believe philosophical principles can be every bit as relevant to practical affairs. In fact, in the complex field of climate change, which pushes against the very limits of knowledge, to formulate policy without understanding and making provisions for the nature of knowledge is patently unwise.

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Appendix A

The Blind Men and the Elephant

John Godfrey Saxe (1816-1887)

It was six men of Indostan,
To learning much inclined,
Who went to see the Elephant
(Though all of them were blind),
That each by observation
Might satisfy his mind.

The *First* approach'd the Elephant,
And happening to fall
Against his broad and sturdy side,
At once began to bawl:
"God bless me! but the Elephant
Is very like a wall!"

The *Second*, feeling of the tusk,
Cried, -"Ho! what have we here
So very round and smooth and sharp?
To me 'tis mighty clear,
This wonder of an Elephant
Is very like a spear!"

The *Third* approach'd the animal,
And happening to take
The squirming trunk within his hands,
Thus boldly up and spake:
"I see," -quoth he- "the Elephant
Is very like a snake!"

The *Fourth* reached out an eager hand,
And felt about the knee:
"What most this wondrous beast is like
Is mighty plain," -quoth he,-
"'Tis clear enough the Elephant
Is very like a tree!"

The *Fifth*, who chanced to touch the ear,
Said- "E'en the blindest man
Can tell what this resembles most;
Deny the fact who can,
This marvel of an Elephant
Is very like a fan!"

The *Sixth* no sooner had begun
About the beast to grope,
Then, seizing on the swinging tail
That fell within his scope,
"I see," -quoth he,- "the Elephant
Is very like a rope!"

And so these men of Indostan
Disputed loud and long,
Each in his own opinion
Exceeding stiff and strong,
Though each was partly in the right,
And all were in the wrong!

MORAL,

So, oft in theologic wars
The disputants, I ween,
Rail on in utter ignorance
Of what each other mean;
And prate about an Elephant
Not one of them has seen!