The Professional Knowledge Base for Teaching:
A Philosophical Justification for a Plurality of Ways of Knowing

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ABSTRACT

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This thesis discusses how the related fields of educational inquiry and teacher education have been captured by a foundational view of what constitutes acceptable research methodology and adequate professional knowledge for teaching. The positivistic paradigm of educational inquiry employs the scientific method to obtain general laws explaining classroom phenomena. These generalizations are then taught to teachers as technical rules for sound pedagogy. This positivist/technicist paradigm has maintained its hegemonic status in education despite recent challenges from competing research paradigms, and charges that general rules often fail to guide practice in unique contexts.

I begin by demonstrating how the foundationalism of the positivist/technicist paradigm in education is a manifestation of the modern rational-scientific quest for certainty: a movement which cleaned the slate of 'unscientific' propositions, and advocated the utilization of rational synthesis and scientific method to build a systematic body of positive knowledge. Next, I show how logical empiricists have misrepresented the beginnings of modernity and the development of science in order to sustain this modern worldview. Following that, three 20th century versions of scientific rationalism are examined to show how each successive tradition weakened the certainty of scientific

knowledge. Next, insurmountable impediments to certain knowledge are discussed to demonstrate how rationalism has recently been challenged by a more humanistic view of science and philosophy. Finally, the postpositivistic philosophy of science is examined as a justification for the existence of a plurality of paradigms in educational research, and a multitude of knowledge forms to guide teaching practice.

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CHAPTER ONE

CAPTURED BY A UNIFOCAL VIEW OF THE PROFESSIONAL KNOWLEDGE BASE FOR TEACHING

> Compared to other sources of knowledge, such as experience, authority, inductive reasoning, and deductive reasoning, application of the scientific method is undoubtedly the most efficient and reliable.
>
> L.R. Gay, Educational Research

Introduction

This thesis will analyze and critique the dominant reality of modern educational theory and practice: that the related fields of educational inquiry and teacher education have been captured, since the late 19th century, by a unifocal view of what constitutes (a) acceptable research methodology, and (b) adequate professional knowledge for teaching. The positivistic paradigm of educational inquiry, and the related technical pattern of teacher education, began as an attempt by early educational psychologists such as Edward L. Thorndike (1965/1910) to emulate the astounding technical successes achieved in such practical fields as medicine and engineering by the application of the scientific methods of the natural sciences to their subject matters. Consequently, the traditional methods of pedagogy taught in 19th-century normal schools--derived from (a) the humanist scholarship of philosophers, historians, and early educationists, and (b) the common sense wisdom of practitioners--were soon replaced by the objective, standardized, systematized, generalized, algorithmic, technical products of scientific convention in the modern teachers college curriculum. However, despite modest successes in formalizing a professional (i.e., 'scientific') knowledge base for teaching through quantitative research (e.g., by the process-product research program), there has arisen, since the late 1970s, a ground swell of criticism to the effect that this 'positivist' or 'technicist' paradigm has had an impoverishing effect on the neophyte teacher's autonomy and ability to deal with the complex, uncertain, unstable, value-laden practical problems encountered in a pedagogical setting (e.g., see Bullough Jr. et al., 1992; Shulman, 1986a, 1986b, 1987; Zumwalt, 1982; Doyle, 1990; Tom & Valli, 1990; Garrison, 1988; Harris, 1993; Kennedy, 1987; Schön, 1983, 1987, 1989; Schwab, 1983; Eisner, 1977, 1983, 1991; among others).

Educational positivism finds its roots in the 17th century rationalist revolutions in philosophy and science (see Toulmin, 1990), in the 19th century 'positive philosophy' of Auguste Comte (see Comte, 1947/1842), and in the 20th century 'logical positivist' and 'logical empiricist' philosophies of science (see Wittgenstein 1974/1921; Hempel, 1965; Nagel, 1961). All of these traditions were variations on a *modern worldview* which attempted to establish a certain foundation for knowledge by (a) rejecting all common sense (i.e., practical) and ā *priori* (i.e., religious, metaphysical, aesthetical and ethical) knowledge claims as nonsensical; (b) embracing formal, rational systems of mathematics or logic (i.e., 'self-evident' truths), and empirical facts derived from passive scientific observation (*i.e., a posteriori* knowledge), as the only two types of propositions that make sense; and finally (c) achieving a unified body of positive knowledge dealing with all manner of phenomena--inorganic, organic, social, and psychological--by applying the scientific method of mathematical physics to them (Toulmin, 1990).

Giddens (1974) states that this modern view "...not only took scientific knowledge to be the paradigm of all (valid) knowledge, but also saw in science the solution to the major practical problems facing mankind" (pp. 1-2). According to Giddens, the 'positivistic attitude' in social science comprises three connected presuppositions: First, that the methodology of natural science can be directly applied to social and psychological phenomena; in other words, "...the phenomena of human subjectivity, of volition and will, do not offer any particular barriers to the treatment of social conduct as an 'object' on a par with the objects in the natural world" (Giddens, 1974, p. 3); second, that the outcomes of social scientific investigations can be formulated in 'laws' or 'law-like' generalizations analogous to those of the natural sciences; and third, that the products of scientific social inquiry have a technical character, or are instrumental in form; that is to say, these outcomes have no logically-given implications for practice or for the pursuit of any particular values. In short, the social sciences, like the natural sciences, are 'value-neutral.'

This thesis will contest the positivist claim that there is a single procedure (or logic)--that of the natural sciences--for arriving at valid, 'objective,' knowledge claims or propositions. Using pragmatic and postpositivistic theories, this epistemological monism will be shown to be untenable due to the fact that *all* knowledge is theory-laden, or partially derived from prior assumptions (presuppositions) about the world. In other words, all inquiries, and the knowledge claims they yield, involve transactions between subjects and the objective world. A scientist's theoretical and methodological commitments will define his or her problem-field, will cause him or her to attend to certain aspects of that field and ignore others, will result in the observed data being treated and interpreted in a specific, pre-determined fashion, and will prescribe what counts as an adequate problem-solution. These prior assumptions will thus provide an incomplete representation of the multiple, constructed meanings present in each phenomenon studied--in this instance, one of many teaching or learning processes. Therefore, a plurality of methods and theories will provide professional practitioners with a diversity of ways of knowing the objects (*or fellow subjects*) that their actions affect. As a result of this, a teachers college curriculum with a *unifocal* perspective on the knowledge required for teaching practice (e.g., scientific techniques) will be radically incomplete and will therefore be miseducative.

In this chapter I will begin by delimiting the current scope of the professional knowledge base for teaching. In this section we shall discover that, although a plurality of epistemologies is beginning to emerge in the field, what counts as *valid* knowledge is still a contentious issue. Following that, I will discuss my own personal experiences with teacher education and induction into the profession, experiences that I feel are typical of many novice educators, and which have led me to understand how crucial it is that all educators be exposed not to one, but to a plurality of ways of knowing. Next, I will discuss the dominant epistemology of research and teaching practice (i.e., positivism/technicism) and outline the main arguments against this paradigm's hegemonic status within the field of education. Finally, I will briefly mention three additional knowledge paradigms which constitute suitable addenda to a *pluralistic* professional knowledge base for teaching.

The Professional Knowledge Base for Teaching

Tom & Valli (1990) and Doyle (1990) consider that the knowledge base for teaching consists of the following components: (a) general subject matter knowledge in the liberal arts and sciences, (b) specialized subject matter knowledge, and (c) professional knowledge stemming from: (i) university-based instruction in pedagogy and foundations of education, and (ii) a school-based clinical practicum.

The path to certification in Canada is generally of two kinds: "(1) the integrated pattern in which professional and academic courses are taken simultaneously, and (2) the after-degree pattern in which the academic course work is taken first" (Leskiw & Ruddell Girhiny, 1992, p. 56; see also Wideen & Holborn, 1990). With the latter pattern, education courses are interspersed with short practice teaching periods (Wideen & Holborn, 1990). The former design usually affords longer periods of practicum due to the fact that the student will normally be exposed to classrooms from their first or second year of university onward. Irrespective of which pattern is followed, "...across Canada, a gradual move is being made toward five years of teacher preparation" (Leskiw & Ruddell Girhiny, 1992, p. 57).

Regardless of the pattern followed, Shulman (1986a, 1986b, 1987) argues that professional education unnaturally separates subject matter knowledge from pedagogical knowledge, with the academic course work providing expertise in subject matter content and the professional course work developing competency in pedagogical content, but neither fostering proficiency in *pedagogical content knowledge*--subject matter knowledge *for teaching*. He refers to this synthesis of pedagogical knowledge and subject matter knowledge, which "...distinguish[es] the understanding of the content specialist from that of the pedagogue" (Shulman, 1987, p. 8), as "the missing paradigm" in the literature on teacher education. In Shulman (1986a), he discusses how the curriculum focus of the 19th-century normal school was on subject matter content knowledge, with very little attention paid to pedagogical content; while that of the 20th-century teachers college has been on "...'research-based teacher competencies'..." (p. 5) derived from "...findings on the forms of teacher behavior that most effectively promote student learning" (p. 6), with almost no consideration of subject-matter content.

Today's policymakers, in determining the curriculum of the teachers college and standards of evaluation for teaching, have emphasized 'basic abilities' for 'effectiveness' in teaching (Shulman, 1986a); i.e., positivistic knowledge of effective teacher behaviours derived largely from the process-product research paradigm, which "...were accorded legitimacy because they had been 'confirmed by research'" (Shulman, 1987, p. 6). Shulman (1986a) considers this to be "...a thriving and successful..." (p. 6) research program; nevertheless, it does suffer from the basic limitation of any and all forms of inquiry: a narrowness of scope, a necessary simplification of complex reality[1], which has led it to ignore a central aspect of classroom life: the subject matter and how it was "...transformed from the knowledge of the teacher into the content of instruction" (p. 6).

The process-product research literature on teaching is replete with references to such things as direct instruction, wait time, and time on task, but there are few references to subject matter. Furthermore, those research programs which arose in opposition to the process-product paradigm (e.g., interpretivist paradigms) perpetuated this oversight, continuing to treat teaching in a generic fashion, as if what was being taught did not matter. Thus, in both quantitative and qualitative inquiries, "teaching processes were observed and evaluated without reference to the adequacy or accuracy of the ideas transmitted" (Shulman, 1987, p. 6). This is also a major concern of both Dewey (1965/1904) and Schwab (1978a), who feel that a proper knowledge of both the substantive content of the disciplines and the scientific methods through which that content is secured are crucial in order for teachers to be able to (a) structure that knowledge for proper teaching, and (b) recognize and evaluate the deep structures of knowledge within their students' minds.

It is evident then that Shulman (1986a, 1987) has provided a more detailed categorization of the knowledge base for teaching than either Tom & Valli (1990) or Doyle (1990). In addition to subject matter

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[1]See Schwab (1978c/1970, 1978d/1971) on the incompetences of theory.

knowledge and professional knowledge of pedagogical theory and practice, he has included the knowledge provided by "the missing paradigm": pedagogical content knowledge, or know-how in representing subject-matter content in a form easily learned by students. Moreover, his later analysis of the crucial knowledge base for effective teaching (Shulman, 1987) includes four additional categories. The first, curriculum knowledge, is knowledge of the curriculum of a teacher's own grade and subject, that of other grades and subjects, and "...of the materials and programs that serve as 'tools of the trade' for teachers" (p. 8). The second is knowledge of learners and their characteristics. The third is knowledge of educational contexts, which ranges "...from the workings of the group or classroom, the governance and financing of school districts, to the character of communities and cultures" (p. 8). The final one is "knowledge of educational ends, purposes, and values and their philosophical and historical grounds" (p. 8).

Shulman (1987) continues his analysis with a discussion of four different sources for this knowledge base, some of which are formally codified, and others of which are represented through more informal means. The first, a highly codified source, is scholarship in the content disciplines. The disciplines are a source for raw materials which are to be transformed by curriculum planning or deliberation into the understandings, skills, values and dispositions that are to be learned by school children. As mentioned above, Shulman (1987), after Dewey (1965/1904) and Schwab (1978a), considers that in order to foster effective teaching, it is crucial that a teacher know both the substantive content (i.e., knowledge claims) and methodological syntax, or grammar (i.e., research methods, and their underlying worldview), of these disciplines.

The second, moderately codified source for the knowledge base is educational materials and structures. These include:

> curricula with their scopes and sequences; tests and testing materials; institutions with their hierarchies, their explicit and implicit systems of rules and roles; professional teachers' organizations with their functions of negotiation, social change, and mutual protection; government agencies from the district through the state and federal levels; and general mechanisms of governance and finance. (Shulman, 1987, p. 9)

Shulman (1987) considers these a major source for the knowledge base "because teachers necessarily function within a matrix created by these elements, using and being used by them..." (p. 9).

The third source for the knowledge base, another highly codified foundation, is formal educational scholarship dealing with processes of schooling, teaching, and learning.

> This literature includes the findings and methods of empirical research in the areas of teaching, learning, and human development, as well as the normative, philosophical, and ethical foundations of education. (Shulman, 1987, p. 10)

In short, this source includes all the scholarship of the traditional foundations of education (psychology, sociology, history, and philosophy), as well as the newer empirical erudition provided by quantitative, qualitative, and critical methods of inquiry. As with scholarship from the subject matter disciplines, it is crucial that knowledge of formal educational scholarship include awareness of both the *substantive* and *syntactical* structures of these disciplines, to borrow Schwab's (1978a) terminology.

The final, and in Shulman's (1987) opinion, least codified source of knowledge is the wisdom of practice. This practical wisdom consists of "...the maxims that guide (or provide reflective rationalization for) the practices of able teachers" (p. 11). In short, this is the embedded (see Pratte & Rury, 1991), or tacit (see Polanyi, 1966), art and craft knowledge of teachers. While some (e.g., Tom & Valli, 1990) wonder if such experience constitutes a legitimate epistemology, or way of knowing, about teaching, and thus an essential component of the professional knowledge base for teaching, Shulman (1987) has no doubt that it is.

> We have concluded from our research with teachers at all levels of experience that the potentially codifiable knowledge that can be gleaned from the wisdom of practice is extensive. Practitioners simply know a great deal that they have never even tried to articulate. (Shulman, 1987, p. 12)

He concludes that much of the research agenda of the immediate future will be devoted to describing and analyzing (or interpreting) the practical wisdom of expert teachers in case studies using qualitative methods of inquiry.

To summarize, Shulman (1987) catalogues seven essential categories which comprise the knowledge base for teaching: (a) content knowledge, (b) general pedagogical knowledge, (c) curriculum knowledge, (d) pedagogical content knowledge, (e) knowledge of learners and their characteristics, (f) knowledge of educational contexts, and (g) knowledge of educational ends, purposes, and values. This knowledge base is to be found in the following sources, listed in order from most to least formal, with the first two being roughly comparable in terms of their degree of formality: (a) scholarship in content, (b) formal educational scholarship, (c) educational materials and structures, and (d) wisdom of practice.

In spite of Shulman's pluralistic conception of the professional knowledge base for teaching, it nevertheless must be stated that "...what counts as knowledge is vigorously contested..." (Tom & Valli, 1990, p. 373). Traditionally, only the first two of Shulman's (1987) categories, 'scholarship in content' and 'formal educational scholarship,' have been considered sources for the professional knowledge base for teaching; and, in the second category, only that formal educational scholarship which was derived from scientific methods of inquiry has traditionally been considered valid by researchers (e.g., see Brophy & Good, 1986; Campbell & Stanley, 1963; Gage, 1978, 1984; Skinner, 1982/1953; Thorndike 1965/1910), and by policymakers (see Doyle, 1990). In fact, among educational researchers, there has been an on-going debate--much of it polemical--concerning the relative fruitfulness and validity of quantitative (i.e., positivist) research methods on the one hand, and qualitative (i.e., interpretivist) research methods on the other (see Smith, 1983).

The wide scope taken by Shulman (1987) in delineating the knowledge base for teaching demonstrates that a more pluralistic view of both the practice of inquiry and the practice of teacher education has begun to emerge in the field. For example, it has become more acceptable to give the wisdom of practice a status commensurate with that conferred on the more formal (i.e., 'scientific') bases of knowledge. Such theorists as Shulman (1986b, 1987), Doyle (1990), Eisner (1977, 1983, 1988, 1991, 1993), Schwab (1975), and Erickson

(1986) now consider the wisdom of practice to be codifiable using qualitative, or interpretive research methods. As well, Schwab (1975) considers arts of deliberation and tactic, *devoted to wise action in practical contexts*, to be equally as rigorous and valuable as formal logic and strategy, *devoted to knowledge representation or codification in abstract, theoretical forms and terms*. This practical wisdom--variously known as the 'reflective epistemology of practice' (Schön, 1983, 1987), the 'logic of practice' (Reid, 1979), and 'the scientific temper in practical inquiry' (Garrison, 1988; Dewey, 1929, 1965/1904, 1966/1916)--is now considered by these and other educationists to be at least as important a source of knowledge for teaching practice as formal scholarship derived from scientific methods of inquiry.

In all, four epistemological traditions have emerged as potential contributors to a pluralistic professional knowledge base for teaching practice: (a) quantitative, scientific methods of inquiry with their attendant technical modes of practice, (b) qualitative interpretations of meaning with their corresponding interpretive modes of practice, (c) critically-oriented inquiries with their accompanying emancipatory modes of practice (praxis), and (d) the wisdom of practice with its concomitant deliberative or reflective modes of practice (Tom & Valli, 1990; Shulman, 1987). However, as we shall discover below, the notion of teacher education as 'training' in the use of general pedagogical techniques and propositions derived from scientific inquiries retains its primacy in the modern teachers college to this day.

My Personal Narrative of Preservice Teacher Education, and Induction into the Profession

The notion of adopting, as a starting point for research, the everyday lived experiences of the human beings involved in educational contexts, and of using hermeneutic-phenomenological approaches in such inquiry (see Van Manen, 1990), has been more or less legitimated in the field of education in the past twenty years (Eisner, 1988, 1993). As well, Eisner (1993) refers to "...a growing interest in narrative and in storied approaches to experience" recently within the field of educational research (p. 8). Bullough Jr. et al. (1992) provide a portrait of a new approach to teacher education which makes use of personal narratives written by student-teachers in order to develop more critically intelligent practitioners. In keeping with this tradition, I would like to now share my own experiences of teacher education and professional induction in order to demonstrate what the effects of a narrow *training* in the use of teaching techniques can be. First, however, I must comment on my use of a particular stylistic device in this thesis.

Like Eisner (1991), I will use the first person pronoun throughout this thesis, not as *accidental* or *unfortunate* occurrences which, in the eyes of many, would serve to compromise my 'objectivity' in inquiry, but as a deliberate stylistic device--"...*to serve epistemological interests* [italics added]" (Eisner, 1991, p. 4). Those interests include demonstrating that *all* experiences and knowledge claims are theory-laden, or derived from certain prior assumptions about the world. In other words, all inquiries, and the knowledge they yield, are ***achieved***, or partially ***created***, ***not discovered***, and involve *transactions* between inquiring subjects (human beings, with their held beliefs, values, ideas, and theories) and the experiential world (Kuhn, 1970; Eisner, 1988, 1991).[2]

As we shall see when I treat this crucial topic in greater detail in Chapters Two and Five, this view is in sharp contrast with that held by most positivists, who feel that certain, singular, objective Truth, with no contamination by fallible human beliefs (*doxa*), is possible through the use of scientific methods of observation. The most radical version of this view was held by the early Wittgenstein (1974/1921), one of the progenitors of logical positivism, who demonstrates in the following quote his belief that the knowing human being is not a 'subject,' but simply an agglomeration of his/her sense percepts of objective reality: "There is no such thing as the subject that thinks or entertains ideas" (p. 57). What follows is a personal narrative which examines what happens when teachers are *trained* to mechanically apply techniques in action, rather than liberally *educated* to 'think and entertain ideas' about their practice.

Odyssey[3]

I am a teacher of two years experience who followed the serial pattern to certification: I completed a B.A. with a concentration in politics in 1987, followed by a concentration in history completed as a

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[2]An example of how knowledge is constructed by a transaction between human beliefs and objective reality is provided by Einstein, who could not accept Heisenberg's Uncertainty Principle because of his metaphysical belief that God would not leave things as important as the structure of the cosmos to chance (Morgan, 1983).

[3]Credit for this subtitle, and the one that follows, *Confessional*, must go to Elliot Eisner (1993).

special student in 1988; I subsequently entered a one year B.Ed. program in 1989, obtaining a teacher's certificate in the spring of 1990. The educational path that I took to becoming a teacher was generally in line with the accepted North American preservice teacher education curriculum: (a) general education in the arts and sciences (the elective portion of my B.A. degree); (b) specialized education in the content areas to be taught (concentrations in history and politics); and (c) professional education, including (i) education in pedagogy and in the disciplines that inform professional practice, and (ii) a clinical practicum (experienced concurrently during the B.Ed. degree) (Tom & Valli, 1990; Doyle, 1990).

A brief examination of the course outlines from my B.Ed. program quickly reveals the curricular emphasis of the professional education that I received. It was largely technical--based on knowledge derived from scientific concepts and methods borrowed from educational psychology. Both Doyle (1990) and Leskiw & Ruddell Girhiny (1992) consider such an emphasis typical, given the dominance of educational psychology in research and teacher education this century.

> For most of the twentieth century educational psychology has dominated teacher education in the United States and Canada. Educational psychology was originally intended to lend academic respectability through scholarly and experimental studies, focused heavily on measurement and statistical analysis. This emphasis has become a powerful tradition which has made it very difficult to introduce new emphases and different balances within programs. (Leskiw & Ruddell Girhiny, 1992, p. 61)

In short, the curriculum I received was highly 'psychologized,' to borrow Doyle's (1990) term.

In this curriculum, the course *Foundations of Education* consisted of three subcourses: (a) *Education and Schooling*, (b) *Educational Psychology*, and (c) *Special Education*. The first subcourse, *Education and Schooling*, was further subdivided into four main topics: (i) classroom management, (ii) schooling in the post-industrial world, (iii) current and future issues in education, and (iv) legal foundations of education in Ontario. The content of the first topic, classroom management, basically consisted of scientific generalizations concerning "effective intervention strategies." The second topic, schooling in the post-industrial world, provided one of the few forays in this course into traditional foundations of education *other than educational psychology* (the others being philosophy, sociology, and history of education). The outline states that the objective of this section was ". . . to introduce students to the sociological and socio-historical context of education in the post-industrial world" (Lundy, 1989, unpublished document). However, this section was only a superficial coverage of some very complex topics such as the bureaucratization of schooling, the professionalization of teaching, and such things as a discussion of some modern social concerns in Ontario schooling (e.g., native education, sex-equity, class, exceptionalities, adolescent culture). The third topic was an examination of some current and future issues in education, such as mainstreaming, the drop-out issue, rising societal expectations of schooling, and racism. The fourth section involved reading and interpreting sections from the Ontario Education Act. Only one piece of scholarly work was read in this entire course, consisting of a journal article which was read and critiqued in writing as an assignment.[4] The other two subcourses in *Foundations of Education* (*Educational Psychology*, and *Special Education*) involved learning knowledge from textbooks that was almost entirely derived from scientific theories and empirical inquiries. In short, well over three quarters of the knowledge in this course was derived from *one* of the foundational subjects: educational psychology.

The course known as *Curriculum Methods* was divided into two subcourses: *Instructional Techniques* and *Instructional Technology*. Again, the knowledge base in this course consisted largely of scientific propositions and techniques derived from quantitative research methodology. It was in this course that we learned much of the knowledge provided by the process-product research program dealing with such things as questioning, wait time, and proper sequencing and organization of instruction. However, some content was gleaned from more doctrinal theories of education, such as the literature dealing with child-centered and activity-centered learning.

Curriculum Studies I was a course made up of two subcourses: *Curriculum Orientation*, and *Language Arts*. Less than ten percent of the time devoted to this course was spent on the *Curriculum Orientation* (only 10 hours). This subcourse consisted of a very superficial analysis of three different curriculum orientations: transmission, transaction, and transformation. The first, transmission, was a teacher/subject-centred orientation to curriculum. The second, transaction, was both teacher-/subject-, and child-centred in

[4]Indeed, this is the only article from a scholarly journal that I read in my entire professional education. The rest of my reading consisted of selections from textbooks.

orientation (i.e., there was a transaction between the needs and interests of the child and the standardized curriculum). The final orientation, transformation, was entirely child-centred: the child was to be entirely self-directed in his or her learning, and was to unilaterally determine what the curriculum would be. This ten-hour subcourse constituted all the time devoted to philosophy of education in this teachers college curriculum. The language arts portion of the course involved an examination of whole language techniques and organization of curriculum: knowledge largely derived from a doctrinal theory of language instruction; however, some elements of this theory have been subjected to verification using scientific methods.

The remaining courses, *Curriculum Studies II*, *Curriculum Studies III*, and the *Teaching Elective* (i.e. the teacher candidate's subject matter specialization) all dealt with the student-teacher's understanding of (a) subject matter from a pedagogical perspective, (b) the organization of curricula within different subjects, and (c) teaching techniques in various subject areas, e.g. music, physical and health education, art, mathematics, science, and social studies. In short, these courses were to provide what Shulman (1986a, 1986b, 1987) refers to as *pedagogical content knowledge*. To review, this knowledge base

> ...refers to the understanding of how particular topics, principles, strategies, and the like in specific subject areas are comprehended or typically misconstrued, are learned and likely to be forgotten. (Shulman, 1987, p. 26)

However, these courses generally failed to provide such a critical grasp of different specialized subject matters and their pedagogically-sound implementation for two main reasons. First, much of the course time was devoted to teaching the subject matter to the prospective teachers, who, more often than not, had little, if any, education in many of these curriculum areas. As a result, little time remained to develop the student-teachers' competencies in delivering the different disciplinary contents in a pedagogically-sound manner. Second, this single-year B.Ed. program was densely concentrated, with students attending classes usually between 35 and 40 hours a week, and having on average five to ten assignments due every week. As a result, the student-teachers had very little time for reflecting on the content they were presented with, and consequently, any learning which occurred often did not last long beyond the completion of the inevitable test or assignment.[5]

Evaluation in many of these courses or subcourses (e.g., *Foundations of Education*, and the *Curriculum Orientation*) consisted of tests, exams, essays and presentations, i.e., the assessment of propositional knowledge gleaned from lectures and textbooks--the dominant form of discourse in the academe (Eisner, 1988). The courses dealing with curriculum areas, e.g., math, social studies, etc., assessed student-teacher learning through tests and exams, but also included projects which generally involved the creation of instructional aids (e.g., a science kit), or lesson/unit plans which followed Tyler's (1950) time-honoured linear rational model of planning whereby one begins with ends (i.e., objectives), then determines means (i.e., teaching/learning activities), and finally, constructs evaluative measures. The subcourse known as *Instructional Techniques* evaluated

[5]This deficiency in the program can be extended to include *all* of the knowledge learned, not just pedagogical content knowledge.

student-teacher progress through tests and exams, and through assignments in which student-teachers had to tape themselves during a few practice teaching periods and later analyze their performances in terms of level of questioning (i.e., according to Bloom's taxonomy), wait time, and other such technical skills.

As we shall see below when I discuss my induction into the profession, much of the propositional knowledge I learned in teachers college contributed to my *knowing that*, but not to my *knowing how* (Ryle, 1949). In other words, I could readily recite all the latest theoretical knowledge that I had learned concerning lesson and unit planning, whole language, active learning, dealing with special needs students, etc., and did so voluminously in the interview through which I obtained my first teaching position, but I was not quite sure of how to actually implement these theories in the classroom (see Kennedy, 1987).

The same conclusion can be drawn with the techniques I learned. We were provided with no opportunites to 'drill and practice' such techniques as questioning, wait time, etc., through such means as 'microteaching' (see Gage, 1978), or other similar controlled lab experiences (see Dewey, 1965/1904). The result was that these techniques generally did not transfer to practice, with student-teachers' continuing their habitual modes of behaviour such as asking a follow-up question almost immediately after the first, unanswered, question was posed. Not only did my teachers college curriculum emphasize narrow--therefore insufficient--propositional and technical knowledge, the professors inculcated it in a substandard fashion. However, as Kennedy (1987) points out, even if such skills had been drilled into our heads, we would still have lacked the judgment to be sure of when exactly to apply them.

One reason for the insufficient treatment of propositional and technical knowledge was the almost exclusive reliance on textbooks as curriculum sources. Schwab (1978a) laments the fact that "...in the past thirty years...our textbooks (those for the training of teachers as well as those for students) have conspicuously omitted the structural context of statements" (p. 237). This attenuated treatment of the content of professional and subject matter disciplines in textbooks decreases critical understanding of knowledge because not enough of the context, or the structure of a discipline, is included "...to let a student know that he is dealing with a model or a possibility and not with a literal truth or literal falsehood..." (p. 236). In other words, not enough background is included in the text to alert the reader to the fact that a particular theory has shortcomings, or that an equally--*or more*--plausible alternative exists. Consequently, our daily exposure to "...the certain facticity of the text" (Eisner, 1993, p. 9) limited our ability to critically reflect on the strengths and shortcomings of different theories and techniques for professional practice.

The final component of this professional education was the clinical practicum: a series of two week placements in various schools interspersed with periods of scholarly study at the university (see Wideen & Holborn, 1990). The emphasis during these placements was largely on applying, in the classroom, the knowledge and techniques discussed in academic course work. My teaching was observed and evaluated four times in ten weeks by three different supervising professors, two of whom I had never met before. Following the observation period, there was normally a ten to fifteen minute debriefing with the professor during which he/she discussed his/her evaluation of my practice. There was no long-term tracking of my progress by one professor, through which that professor might have been able to actually help me correct some of my deficiencies as a teacher. The perfection of lesson planning--complete with the requisite behavioral objectives--seemed to be a preoccupation with most of the supervising professors, this despite the fact that in the practical context few of the experienced teachers formulated plans and objectives for each lesson--*there was no time for this*.

The practicum experience in teacher education, like that in most professions, reflects a widespread belief that some type of transition experience is required before a neophyte enters into independent practice (Kennedy, 1987). Practice teaching is simply a form of apprenticeship whereby a novice is supervised by an 'expert,' and the theory learned in the university-based portion of teacher education is to be applied in a practical context. This is all very well in theory, however in practice what often happens is that the student-teacher internalizes some very haphazard, incomplete, unthinking habits from cooperating teachers who are very often not experts, offer little criticism of their fledgling's performances, act as an ally in helping him or her to look good to any visiting professors, and have very little respect for the theoretical or technical learning that goes on in teachers colleges.

This feeling that the formal education of teachers college was inadequate was continually reinforced during practice teaching rounds, and during my induction into the profession, by comments made by a large proportion of the teachers that I encountered. These were comments which alluded to a belief that 'teachers college was a waste of time,' or that 'it didn't prepare you at all for practice,' or that 'it would have been much better to forget all that theoretical stuff and concentrate more on practice teaching.' Reid (1978) points out that practitioners, whose thinking is developed through involvement in unique, particular, idiosyncratic contexts often, and wrongly conclude that the thinking of the theoretician--rational, universal, abstract, orderly--is inconsequential in professional practice. Zumwalt (1982) discusses how practitioners will often ignore research findings in the form of rules, especially if they are inconvenient to implement, or contradict the teacher's own knowledge of (a) professional practice or (b) their present educational context. Similarly, Dewey (1929) discusses two results of the use of scientific data as fixed rules: (a) such rules would be followed inflexibly with the result being no real improvement in the situation; and (b) as a consequence of (a), the practitioner would become disgusted with the discrepancy between the purported outcome of a finding and the actual outcome, leading to an ultimate decision to ignore scientific research and rely on the empirical, or common sense, traditions of the profession. Finally, Schön (1983) suggests that in most professions a rift occurs between the academy and the practical context, between theory and practice, in which the theoretician and the practitioner each impugn the 'knowing' of the other.

These negative impressions about the value of the academic portion of teacher education have been substantiated in recent research literature:

> Teacher candidates may acknowledge that the university has a significant role in their preparation; yet many will claim that it has not performed this role to their satisfaction. On their first exposure to field experiences they learn that teachers and administrators feel the same way. (Leskiw & Ruddell Girhiny, 1992, p. 64).

Wideen and Holborn (1990) reach a similar conclusion from a review of research on teacher education in Canada:

> Campus courses are not held in high esteem by students, teachers or principals. The practicum remains the most favourably viewed component of teacher education. Where follow-up studies have been conducted by third parties, teachers report that the teacher training component occurring on campus had little impact upon their teaching. (pp. 20-21).

The question that remains, then, is: Why are teachers college courses on the theoretical knowledge required for teaching practice widely considered to be *impractical* and *not relevant to practice*?

Leskiw and Ruddell Girhiny (1992) speculate that the answer to this question lies within the realm of the 'theory-practice' problem encountered in most professional fields.

> The prime reason for severe criticism of pedagogical courses is the matter of transferability of knowledge from college courses to practice in the classroom. Many faculties leave the matter of transfer to individual candidates. Despite the heavy emphasis on field experiences in many programs, this transfer has not been facilitated. Neither faculty nor school supervisors have "led" the teacher candidate toward integrating both academic and pedagogical knowledge into the teacher's instructional plan. The key questions in this kind of integration have often gone unasked--and thus unsolved. How does the *teacher* decide what practices to

> use? How are these practices actually used? How can previous knowledge help the teacher in deciding and acting? What events that teachers see as useful can be utilized to help teachers understand classroom situations and then to carry this understanding into their teaching? (Leskiw & Ruddell Girhiny, 1992, pp. 64-65)

They go on to state that the reason for this lack of integration more than likely lies in the *type* of knowledge used in the teachers college curriculum: that which is derived from a preoccupation with causality and quantification, and which has relied heavily on statistics as the basic language of analysis and theory construction (Leskiw & Ruddell Girhiny, 1992). This type of knowledge has mostly been derived from the work of educational psychologists and behavioural scientists whose scientific methodology has come to dominate the field of educational research this century, producing correlational and experimental generalizations about teaching and learning processes which have, in turn, come to dominate the teachers college curriculum.

In summary, it can be said that the curriculum of the teachers college I attended largely focused on only one of the four traditional foundations of education, educational psychology, and emphasized the learning of scientific propositions and techniques from lectures and textbooks to be directly applied in practical situations. As a result of this, I consider myself to have been 'trained' to apply technical prescriptions rather than 'educated' to think reflectively about my practice. We now turn to an examination of the effects of this training on my probationary professional practice.

* * *

By all rights I should have emerged from this program of professional studies imbued with the theoretical and practical knowledge and skills required for successful teaching practice. Although I managed to survive my induction into the practice of teaching--as do the majority of neophyte practitioners[6]--I can not help thinking now that the training I received at teachers college in theories and techniques, *and even in practical situations*, did not adequately prepare me for the complexity of my chosen profession. I experienced a period of 'practice shock' during which I struggled to figure out how to apply the generalized knowledge and techniques I had learned in university in unique, ever-changing, idiosyncratic, complex, value-laden practical environments.

As a consequence of this technical training that I received, my probationary years of teaching largely consisted of a search for the *one true* system or technique that would allow me to control the learning and behaviour in my classroom. When I encountered discipline problems I adopted a variety of systems of behaviour modification--which usually did not work, I might add. Anti-social behaviour in my classroom led me to structure my curriculum around cooperative learning in order to increase the amount of pro-social behaviour in the environment, and this led to an inevitable loss of time devoted to disciplined, individual seat work. In adopting a whole language approach to teaching language arts, I emphasized creativity and enthusiasm for reading and writing rather than mastery of spelling and grammar because that is how whole language theory was interpreted by the teachers in my school--this in spite of the fact that many of the students in my class had obvious

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[6]Although, some *30 per cent* of beginning teachers do not make it to their second year of practice, and those who do leave are usually among the most able (Schlechty & Vance, cited in Bullough et al., 1992)

deficits in the mechanics of reading and writing.

Schwab (1978d/1971) considers such doctrinaire use, or unreflective eclectic use, of theory in professional practice to lead to 'tunnel vision,' and to perpetuate the 'bandwagon' or 'swinging pendulum' phenomenon in education. The search for, and use of, such one-sided educational approaches

> ...would lead to a curriculum [and teaching practices] so inadequate, so incomplete, that displacement would quickly ensue. The replacement, if it stemmed from another such one-sided commendation, would do something the first failed to do but would fail to do what the first accomplished. It too would be displaced in favor of another, and so on indefinitely. The child's education, in consequence, would be a series of abortive jerks and startings with no course charted and followed to a defensible destination. (Schwab, 1978d/1971, p. 328; parenthetical content added)

Unfortunately, my students had to suffer through a similar never-ending series of new systems, techniques, or algorithms, foisted on them by a teacher who did not realize that thinking should occur during the act of teaching, and not exclusively prior to action in the selection and learning of the next 'silver bullet' that was going to solve all of his practical dilemmas.

With time, I began to tire of this search for the one, certain technique or system for teaching or classroom management, particularly since none of them seemed to lead to the overall result that I desired. This, along with the toll exacted on my energy and morale, during my second year, by attempts to implement child-centred or active-learning approaches with a difficult group of youngsters who seemed to require a more structured learning environment, led me to eventually embrace more teacher-centred approaches to pedagogy.[7] By the time my first foray into the teaching profession ended I had almost completely eschewed practices recommended by theoreticians, and embraced the traditional routines of the profession: teacher-led instruction with students in rows, textbook learning, and individual seat work.

Dewey (1965/1904) felt that any induction into the profession which occurred prior to the development of knowledge of principles, scientific modes of thinking, and a critical spirit of inquiry could lead to (a) the perpetuation of traditional, and often incorrect and/or stultifying, methods of teaching; (b) the maintenance of the status quo in the profession; and (c) a lack of ability to grow from experience due to stunted intellectual development. I believe that this is what happened to me during my time in the profession: I was led to practice in an inflexible, unthinking manner due in large part to the technical nature of my teacher 'training.' My teachers college training had definitely failed to close the gap between theory and practice.

I will complete this sub-section by surveying the views of Bullough Jr. et al. (1992) on the tradition of teacher education as 'training'--an ethic firmly entrenched in institutional structures and relations, and the thinking of those involved in teacher education.[8]

> That teacher-education is firmly entrenched in a training orientation and approach is indicated by the ease with which the phrase, 'teacher training', flows from the lips of teachers and those who work with them and by the great emphasis upon the development of discrete and technical teaching skills; clearly training dominates the language of education. (Bullough Jr. et al., 1992, pp. 187-188)

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[7]More affectionately known as 'chalk 'n talk.'

[8]See also Zeichner (1983) on 'behavioristic teacher education.'

There are a number of assumptions embedded in the training view of teacher education which cause devastating results: (a) that "...there is, at least operationally, a single definition of the 'good teacher', to which teachers must aspire, that centers on the demonstration of a list of narrowly construed teaching skills..." (Bullough Jr. et al., 1992, p. 188); (b) that teaching is synonymous with instruction (the process of transmitting knowledge), with the dominant metaphor being that 'teaching is telling'; (c) a corollary of the first point, that all potential teachers should receive the same treatment in being trained, rather than treating them as individuals with different strengths, experiences, and self-conceptions as to teaching role or identity; (d) that the end of pre-service education is clear cut: i.e., "...once 'trained', the 'trainee' is a teacher..." (Bullough Jr. et al., 1992, p. 188); (e) as a result of (d), that there is rarely any attempt by teacher education professors to help with the suitable placement of their students, or to help them adjust to their new practice; (f) another result of (d), that very little attention is paid to teacher development once they are in the field: "...virtually all capital resources are put into achieving short-term gains, which are often those most easily attained but of least importance or enduring value" (Bullough Jr. et al., 1992, pp. 188-189); and finally, (g) that training is hierarchical and expert-driven, with teachers' lacking in such expertise, and therefore requiring the constant supervision of those who possess it. Without a doubt, these are crucial, and destructive, themes in teacher education.

Confessional

Zumwalt (1982) characterizes two types of beginning teacher: "...those who are 'turned on' by exploring alternative frameworks for viewing teaching and schooling, and those who are excited by practical problem solving" (p. 222). She also lists four different practitioner stances toward professional use of research findings:

> A list of things teachers 'should' do would be received by some as a welcome guideline for practice, by some as a starting point for experimenting in their own classroom, by some as something of minimal value, and by others as subversive. (Zumwalt, 1982, p. 223)

In my incipient practice of teaching I was definitely one of those turned on by exploring alternative frameworks for viewing classroom phenomena, and I welcomed 'teachers should' lists as guidelines for my practice (see Gage, 1978; Brophy & Good, 1986). Admittedly, I was the consummate "systems man," to borrow a term from Saul (1992), always seeking out the latest abstract, general, universal solutions to practical problems; never bothering to critically analyze the true, idiosyncratic nature of these problems; never realizing that systematic algorithms more often than not *did not* fit the 'messy' problems of a practical field like education. I never recognized the damage caused by applying blanket solutions to unique, unstable, value-laden, and thereby uncertain practical problems. It never occurred to me that a more deliberative, or reflective, stance in practice, using theoretical knowledge as a starting point for my own experiments, might have resulted in a more rational practice on my part.

At the time I had an intuitive sense of the fact that my stance toward practice was somehow more distant or transcendent than that of my wife, Donna, also a neophyte teacher; but, I wouldn't have considered it to be any less *rational* as a consequence of this. Now, with Reid (1978, 1979), Schön (1983, 1987, 1989), Schwab (1978c/1970, 1978d/1971, 1978e/1973, 1983), and many others, I would consider my devotion to theoretical dogma, and my search for wholly abstract solutions to practical problems, to be thoroughly *unreasonable*, however *rational* in a narrow, technical sense. The apparent contrasts in our approaches to teaching I put down to basic personality differences[9]: I was cerebral, abstract, and rational in my approach to most things, whereas Donna was more 'down-to-earth,' more 'hands-on,' more practical. As a result, I enjoyed the curriculum planning component of teaching practice and often loathed the delivery of my carefully constructed plans (which looked so good on paper) because the realities of their implementation and impact did not match my ideal picture of what should be happening. In contrast, Donna's planning was much less structured and implemented much more flexibly. I preferred to distance myself from my students and was often too impatient to work one-on-one with a child in order to evaluate just what a student did and did not know. Donna, on the other hand, seemed to thrive on getting to know each individual child's understandings and how they arrived at them. In short, my stance toward practice was reminiscent of Schön's (1983) model of *technical rationality* in which the practitioner, in order to control and manipulate his clients, maintains a clear boundary between himself and

[9]Cf. Kuhn (1970, pp. 4, 153, & 185), who considers 'individual makeup' to play a role in a scientist's theoretical commitments, his choice of paradigm, and, ultimately, his stance toward the professional practice of science.

the objects of his practical maneuvers.

However, despite my personal tendency to favour a more rational, technical approach to professional practice--and indeed to most things in life--I might have been able to overcome this disposition, and therefore might have been saved from teaching in an unreflective manner during those probationary years, if only I had been exposed to a view of the knowledge base for teaching that recognized that *something more* than scientific propositions and techniques were required. This did not happen because of the dominant view of teacher education as 'training in technical skills' within the teachers college I attended (Bullough Jr. et al., 1992), a conviction based on the hegemony of a *certain*--and *narrow*--epistemology of research and practice among academics and policymakers.

Today when I ponder my past predilection for *unreflective*, and ultimately *unsatisfying*, technical modes of practice, I try to remember that one's professional journey (indeed, one's life journey) is Odyssean: you land on the wrong islands, you live a discredited pattern of yourself. But you *are* going home, and hopefully one day, through life-long learning and education, you will acquire (practical) wisdom and integrity.[10]

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[10]Credit for first using this analogy of Odysseus's ten years of wandering before finally reaching his home island must go to Sir Laurens Van der Post, who made this comparison to the journey of life during a 1990 telecast of the C.B.C. television series *Man Alive*.

The Dominant Epistemology of Research and Teaching Practice and the Deskilling of Teachers

Is There a Knowledge Base for Teaching?

To this point in our examination of some of the literature concerning the knowledge base for teaching, it has seemed a commonplace that there is, in fact, such a thing. Since the late 19th century, *professional* knowledge has come to be equated with *scientific* knowledge, and professional status has generally been conferred only on those occupations that can lay claim to a specialized, technical body of knowledge (see Schön, 1983, 1989; Pratte & Rury, 1991; Saul, 1992). However, as Doyle (1990) points out, teaching is still considered by many (who view engineering and medicine as paradigm cases of professions) to fall short of being a profession due to the lack of such a core of specialized, technical knowledge. On this view, it is the role of scientific researchers to build this core of technical knowledge so that one day teaching will ascend to this exalted status. Kennedy (1987), a leading researcher on professional expertise, considers teaching to be a profession, but a relatively undeveloped one that has only recently acquired a body of general principles. Doyle (1990), in referring to the fact that teaching-effectiveness research began in the 1920s and continued with remarkable vigour for the next 50 years without yielding tangible results, supports this claim that the knowledge base for teaching is a recent, and relatively unevolved, phenomenon.

Indeed, there was much skepticism as to whether or not any formalized knowledge concerning teaching existed up until about twenty years ago (e.g., see Doyle, and Shavelson & Dempsey, both cited in Gage, 1978, p. 24). It was at this time that Gage (1978), using a new statistical technique known as the chi square method, began aggregating the results of many individual quantitative studies of teacher effects on student achievement--studies which had generally yielded weak or insignificant results, thereby suggesting to critics that one of two inferences could be drawn: on the one hand, that the teacher's role in attaining educational goods was inconsequential; or, on the other, that the complexities of teaching were intractable, thereby thwarting any hope of one day subsuming educational phenomena under general scientific laws.

Gage's response to the general failure of his adopted research program to solve its own problems was typical of any scientist operating within what Kuhn (1970) called a normal-scientific paradigm: he maintained his commitment to his disciplinary matrix of presuppositions and worked to refine his chosen conceptual and methodological tools. One result of this was his adoption of the above-mentioned technique of meta-analysis, which entailed first "...converting the exact probability value of the result of any single study into a value of the statistic called chi square" (Gage, 1978, p. 29), and then adding these values across studies to determine the significance of the sum. This innovation--which, incidentally, applied scientific method to *the products of scientific method* by generalizing across individual quantitative studies in much the same way that such inquiries generalized across discrete educational contexts (Shulman, 1986b)--immediately bore statistical fruit because of its efficiency in limiting the effects of design flaws, such as small sample sizes, in the single studies.

The promising results of Gage's methodological refinement led him to conclude that recent "...research on teaching [had] yielded some statistically significant, or nonchance, clusters of results" (Gage, 1978, p. 30), thereby undercutting critics' doubts that educational phenomena, such as teacher behaviour, could be studied scientifically. This meta-analytic work also demonstrated "...that teachers *did* make a difference. Variations in teacher behavior were found to be systematically related to variations in student achievement..." (Shulman, 1986b, p. 10). In the eyes of Gage and his fellow process-product researchers, a scientific basis, *or knowledge base*, for teaching practice--defined as "...knowledge of nonchance relationships in the realm of events with which the practice is concerned" (Gage, 1978, p. 20)--had at last been 'discovered.'

At this point the discerning reader might question why, in 1978, after (a) a few thousand years of inquiry into the practical wisdom (*phronesis*) of teaching by philosophers, historians and pedagogues such as Plato, Cicero, Comenius, Froebel, Pestalozzi, Herbart, and Montessori (Doyle, 1990)--often summarized in descriptions of *teaching methods* such as the Socratic method, the scholastic method, the project method, and the Montessori method[11]--and (b) the century-old traditional view of teaching as craft (the dominant view of teacher education in the 19th-century normal school) in which the often tacit cultural knowledge of

[11]See Colman (1967) for a discussion of these and many other traditional teaching methods.

master teachers is transmitted to novices through apprenticeship (Zeichner, 1983), Gage would *only then* declare that the assemblage of a professional knowledge base for teaching had begun? This question can only be answered by an examination of Gage's presuppositions regarding what constitutes *episteme*, or knowledge of teaching, and what the proper relation is between theoretical knowledge and practice.

Positivism and Technicism

The Dominant Epistemology of Research

Since the European Enlightenment of the 16th, 17th and 18th centuries, scientism[12] has infiltrated most academic disciplines, influencing inquirers to study all classes of phenomena--inorganic, organic, mental, and social--using scientific methods of observation, description, and verification, and leading to the eventual dissolution of humanist traditions of scholarship and liberal education (see Rust, 1977; Toulmin, 1990). Chambers (1992) considers statistical research of pedagogical practices to be part of "...a natural development of more general aspirations for a science of man..." (p. 93). An early aspirant for such a science was Comte (1947/1842), the founder of sociology, who considered the scientific method to provide *the only* positive knowledge of the world, and felt that the products of this method--both propositions and techniques--could be applied in the service of controlling humanity. By the late 19th century, such early educational positivists as Alexander Bain and James Sully were calling for a science of pedagogy. Statistical advances wrought by Francis Galton, Karl

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[12]Also known as 'the scientific world view' (Schön, 1983).

Pearson, and Edward L. Thorndike, during the late 19th and early 20th centuries, soon made such a science a possibility (Chambers, 1992).

Much of the scientific inquiry into teaching and learning processes this century has been undertaken with the presupposition that "...brick by brick a science of education could be built" (Eisner, 1983, p. 6). This presupposition, the progeny of such early educational psychologists as Thorndike, posited that science could be applied to educational practice in much the same way as it had been to medical practice, thereby replacing the previously subjective, commonsensical, folkloric, traditional, practical 'knowledge' of teachers with the objective, standardized, systematized, generalized products of scientific research practices. Thorndike (1965/1910) expressed his faith in the scientific model of educational research in the inaugural edition of *The Journal of Educational Psychology*:

> A complete science of psychology would tell every fact about everyone's intellect and character and behavior, would tell the cause of every change in human nature, would tell the result which every educational force--every act of every person that changed any other or the agent himself--would have. It would aid us to use human beings for the world's welfare with the same surety of the result that we now have when we use falling bodies or chemical elements. In proportion we get such a science we shall become masters of our own souls as we now are masters of heat and light. Progress toward such a science is being made. (p. 175)

However implausible the claim that scientists could: (a) isolate and study all the variables in teaching practice that affect complex human beings in such a complex social system as the classroom--*in all their interactive combinations*[13], and (b) generalize the findings of such

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[13]This is a claim that is moderated by, of all people, one of the staunchest advocates of scientific methods of inquiry in education, Nathaniel Gage (1978), who states that there is no 'science of

studies in order to predict human behaviour in unique classrooms (which have their own distinctive meaning structures) containing unique individuals (whose actions are *influenced* by these meaning structures and other indeterminate factors), such faith in the explanatory and predictive power of quantitative research remains to this day.[14]

In addition to the early research on teacher-effectiveness mentioned by Doyle (1990), and its later version, the process-product research program (see Gage, 1978, 1984; Brophy & Good, 1986), there have been numerous other research programs devoted to scientific methodologies of inquiry, e.g., teacher-selection research, time and learning (academic learning time) research, and student mediation (or process-process-product) research (Doyle, 1990; Shulman, 1986b). Gage (1978), using the four types of variables amenable to scientific investigation defined by Dunkin and Biddle (cited in Gage, 1978): (a) presage variables (i.e., teacher characteristics such as age, social class, educational background, etc.); (b) context variables (i.e., features of the teaching/learning context such as grade level, subject matter, type of community, etc.); (c) process variables (i.e., "...the realm of teaching methods and styles" [Gage, 1978, p. 23]); and finally (d) product or outcome variables (i.e., "...the amount of learning, or achievement of educational objectives" [Gage, 1978, p. 23]), lists six possible pairings of types: context-process, context-product, presage-process, presage-product, context-presage, and process-product.

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teaching,' only a 'scientific basis' for the 'art of teaching.'

[14]For an early critique of scientism in education, see Royce (1965/1891).

However, according to Gage, the preferred pairing for all those who want to improve teaching (e.g., teachers, teacher educators, and school administrators), is the process-product approach.

What these research programs share, besides a common methodology, are common forms of knowledge representation: techniques or theoretical propositions which are assumed to have direct applicability in practical situations. As stated above, these abstract, generalized, idealized findings concerning *observable* processes of teaching and learning (i.e., measurable teacher and student behaviours) have been widely considered by both researchers and policymakers this century to provide the only legitimate knowledge base upon which the academic portion of the teachers college curriculum should be built (see Doyle, 1990). This has therefore resulted in a technical view of professional knowledge and practice.

The Dominant Epistemology of Practice

According to Schön (1983, 1987, 1989), the dominant epistemology of professional practice since the late 19th century has been 'technical rationality,'[15] and "technical rationality is the *Positivist* epistemology of practice [italics added]" (Schön, 1983, p. 31). In other words, general laws derived from scientific research provide the only legitimate basis for professional practice. On this view "...professional activity consists in instrumental problem solving made rigorous by the application of scientific theory and technique"; or, in

[15]Kennedy (1987) terms this 'expertise as technical skill'; both Zeichner (1983) and Harris (1993) label this view of teaching 'applied science or technology'; and Zumwalt (1982) designates this a 'technological orientation' to teacher education and practice.

other words, professions are concerned with "...the instrumental adjustment of means to [pre-determined] ends..." through "...rigorously technical problem-solving based on specialized scientific knowledge" (Schön, 1983, pp. 21-22; parenthetical content added). Therefore, "to improve education, one *trains* teachers [cf. Bullough Jr. et al., 1992] to exhibit more of the knowledge, skills, and attitudes which will in turn lead to the desired outcomes for their students [italics added]" (Zumwalt, 1982, p. 224).

In the past two decades both researchers and practitioners have begun to question whether the practice of education can be informed solely by one form of knowledge consisting of scientific explanations and prescriptions, in both propositional and technical form (see Eisner, 1977, 1983, 1988, 1991, 1993; Zumwalt, 1982; Doyle, 1990; Tom & Valli, 1990; Shulman, 1986a, 1986b, 1987; Harris, 1993; Kennedy, 1987; Schön, 1983, 1987, 1989; Schwab, 1975, 1978c/1970, 1978d/1971, 1978e/1973, 1983; Makedon, 1990; Reid, 1978, 1979; Bullough Jr. et al., 1992; Tom, 1980; Zeichner, 1983; Battersby, 1987; Dawe, 1984; Fenstermacher, 1986; Leskiw & Ruddell Girhiny, 1992; Erickson, 1986). This is because educational practice involves unique individuals, each influenced by a unique combination of interacting variables, and each operating within a unique social context with unique cultural meanings. As a result, the variability of each educational context precludes the possibility of finding the one scientific rule or algorithm which can be generalized to all learning situations in order to ensure a standard learning outcome or product.

The unique combination of variables in one concrete context will never be the same as in another context, thereby (a) creating unique ends of education for every pedagogical situation, and (b) rendering the notion of theorists' ability to generalize scientific findings to every context highly suspect. Reid (1979) likens this to attempting to turn the uncertain practical problems of a more normative sphere, such as education (which has both complex practical *and* procedural problems), into the procedural problems of a less normative sphere of inquiry, such as those found in the 'harder' sciences (e.g., physics and chemistry). Schrag (Plurality) agrees that any attempt in the educational sphere to ". . . [secure] consensus on goals, resources, means and problems is a futile task" (p. 11). As such, the faith expressed by Thorndike that the discovery of a scientific technology of teaching could "... reduce the noise in the system" and thereby pattern the practice of teaching is a romantic notion (Eisner, 1983, p. 6) In sum, the sceptics of this scientific model of education believe that methods to achieve the various ends of education "...cannot be reduced to generally applicable rules. Rather, in each instance of their application, they must be modified and adjusted to the case in hand" (Schwab, 1978d/1971, p. 323).

This 'modifying' and 'adjusting' of scientific knowledge to fit 'the case in hand' is a form of knowledge, or a way of knowing about teaching, that is not represented by scientific generalizations. Herein lies the answer to the question I posed in the previous section[16] concerning the reason why theoretical knowledge is not held in high esteem by practitioners. The knowledge claims yielded from experimental

[16]Specifically, see the sub-section entitled *Odyssey*.

or correlational inquiries into educational phenomena (abstractions, generalizations, idealizations) do not always mesh well with the practical contexts of teaching, which have *some* regularities, but which are in large measure characterized by uniqueness, instability, uncertainty, and value conflict. As such, teachers must be provided with forms of knowledge that will (a) inform them about when (*if at all*) and how to apply formal knowledge in practical situations; (b) inform their practical thinking by providing them with craft knowledge, or exemplars of practice (Kuhn, 1970); (c) aid them in dealing with the unique situations of practice (i.e., situations in which formal, general knowledge breaks down) by providing them with 'artistic' abilities (Eisner, 1983), or a 'scientific temper' (Garrison, 1988; Dewey, 1929), or an ability to 'deliberate in practice' (Reid, 1978, 1979; Schwab, 1983), or proficiency in 'reflecting-in-action' (Schön, 1983, 1987, 1989); and (d) assist them in situations which involve making choices between conflicting courses of action (i.e., decision-making in value-laden areas of practice). In these grey, indeterminate zones of practice, generalized knowledge fails to provide practitioners with rules to follow--contrary to the claims of many positivistic social scientists (see Skinner 1982/1953; Thorndike, 1965/1910; Gage, 1978, 1984; Brophy & Good, 1986).

In conclusion, it must be stated that "...the model of Technical Rationality appears as radically incomplete" (Schön, 1983, p. 165). It fails to account for the practical competence that the more reflective teachers demonstrate in dealing with the 'divergent' situations of practice--situations involving uncertainty, instability, uniqueness, and value conflict (Schön, 1983). Clearly, some additions to the knowledge base for teaching are required, otherwise, the result will be teachers who are miseducated (Zumwalt, 1982), deskilled (Bullough et al., 1992), and disempowered (Doyle, 1990; Garrison, 1988).

Addenda to The Knowledge Base for Teaching

Leskiw and Ruddell Girhiny (1992) agree that a formal knowledge base for teaching, if it is to have lasting effects on the teacher's subsequent practice, must include forms of knowledge representation other than scientific laws.

> In both Canada and the United States, reaction from scholars and practitioners has been negative because of the inadequacy and inappropriateness of much of this research. More usable research should focus on what teachers know and do and on the kinds of decisions they make as they teach. Such research will obviously use a different range of methodologies that is more in keeping with field orientation. (Leskiw & Ruddell Girhiny, 1992, p. 67).

In the recent past, a number of other researchers, including Shulman (1986a, 1986b, 1987), Zumwalt (1982), Eisner (1977, 1983, 1988, 1991, 1993), Doyle (1990), Fenstermacher (1986), and Tom & Valli (1990), have concluded that methodological and theoretical pluralism in education will result in a multiplicity of forms of knowledge representation; will reveal that a multitude of potentialities are present in the phenomena engaged by educational researchers; and will thereby contribute a plurality of ways of knowing to the professional knowledge base for teaching.

As mentioned above, there have emerged at least four practical possibilities for a pluralistic professional knowledge base for teaching. First, the once hegemonic positivistic, or quantitative, research paradigm will continue to contribute its general propositions and techniques to the knowledge base for teaching, thereby alerting teachers to the regularities which exist in practical educational contexts. Second, interpretivist, or qualitative, research methodologies will contribute case studies containing 'thick descriptions' of the meaning structures already present in educational contexts (Geertz, 1973), and will provide teachers with *exemplars of practice*--a means of knowing what expert practitioners know and do in the classroom (Shulman, 1986). Third, critical methods of inquiry will provide teachers with (a) critiques of (or the means of conducting their own critiques of) current institutions of schooling designed to illustrate their systemic patterns of injustice, inequality, or bias against minority groups in society, and (b) alternatives to (or the means of generating their own alternatives to) the present system based on a dominant set of values such as democracy, justice, caring, or 'undistorted communications' (see Habermas, 1982). And fourth, the wisdom of practice (or scientific modes of thinking *in practice*, or reflective practice), will provide teachers with the means to deliberate about uncertain, unstable, unique, or value-laden practical problems *on the spot*; discover/create hypotheses or possible courses of action; test these artistic responses against experience; and finally apply them in practice. I will discuss these practical possibilities for a pluralistic epistemology of educational practice, *and* the philosophical or metaphysical views they presuppose, in greater detail in Chapter Six.

Conclusion: Captured by a Unifocal View of Teacher Education

To conclude: it is widely recognized that the field of education has, this century, turned its back on its more humanistic, traditional bases of knowledge; and has been largely captured by a positivistic view of research, and a technical view of teacher knowledge. This unifocal epistemology of research and practice is radically incomplete in that it (a) simplifies complex educational phenomena in order to allow them to be treated by scientific methods, and (b) prescribes the products of such methods (incomplete techniques and propositions) as blanket solutions to complex, unique, unstable, value-laden practical problems. Consequently, this hegemony of postivist and technicist epistemologies in research and teacher education has led to the widespread miseducating, deskilling, and disempowering of teachers this century.

Rust (1977) provides an apt characterization of the dangers inherent in eschewing moderate, skeptical, humanistic (a) modes of inquiry, (b) means of professional practice, and (c) curricular content, in preference for doctrinaire scientism and technicism.

> But if science permeates all fields of inquiry [and professional practice], it may produce victims of the most narrow indoctrination and the highest dogmatism, who are unable to deal with genuine alternatives or critical dilemmas. If we do not halt the trend toward a total take-over of science in [professional] education, where will we train those critical minded decision-makers who will be able to weigh adequately the issues that bear on civilization? (Rust, 1977, p. 102; parenthetical content added)

This thesis is dedicated to elucidating, first, the rise and decline in status of the modern scientific worldview; and second, the ascension, during the second half of the 20th century, of a competing humanistic view of science and philosophy which is tolerant of diversity, yet skeptical enough that *good reasons* must be given for believing in a principle, theory or fact.

The order of topics to be covered is as follows: In Chapter Two I will examine (a) how positivist, or scientific rationalistic, epistemology came to dominate the modern academy (and, concomitantly, how a technical epistemology of practice came to dominate professional education); (b) the presuppositions behind the positivistic worldview (i.e., decontextualization/the clean slate, rational-scientific methods, and foundationalism); and (c) the resurgence of a more moderate humanist worldview in contemporary thought and practice. In Chapter Three I will discuss the different forms that scientific rationalism has assumed during the modern age, i.e., 17th-century rationalism, and the 20th-century traditions of logical positivism, logical empiricism, and falsificationism. Furthermore, I will show how the latter two traditions reduced the epistemic status (i.e., certainty) of both formal and empirical knowledge. In Chapter Four, I will examine in detail how a number of epistemological and ontological uncertainties have undercut the legitimacy of the modern presuppositional scaffolding. In Chapter Five, I will discuss the return of humanistic epistemology, which is embodied in the postpositivistic philosophy of science, and which provides us with (a) a theoretical justification for embracing a plurality of epistemologies (or ways of *knowing that* and *knowing how* [Ryle, 1949]), and (b) a means of rationally evaluating and selecting from among this plurality. In Chapter Six, I will further demonstrate that the notion of having a plurality of epistemologies which contribute to the professional knowledge base for teaching is not merely an abstraction by briefly outlining three alternative research methodologies which have recently emerged in the field of education, and the types of knowledge that they contribute to professional practice. Finally, I will suggest a new form of teacher education designed to teach the professional practitioner how to use a plurality of ways of knowing in a reflective manner.

CHAPTER TWO

THE QUEST FOR CERTAINTY

> I do not believe in "last words" in human affairs, only better conversations.
>
> Elliot Eisner, The Enlightened Eye

Introduction

In Chapter One I discussed how the related fields of educational inquiry and teacher education have been captured, for most of this century, by a unifocal view of what constitutes, on the one hand, acceptable research methodology, and on the other, adequate professional knowledge for teaching. This hegemony of positivist research methods and technicist views of teacher education and knowledge was shown to have an impoverishing effect on the neophyte teacher's ability to deal with the complex practical problems characteristic of educational contexts. This is because: (a) the form of knowledge yielded by scientific methods of inquiry (abstract, general, 'certain' propositions and techniques) do not mesh well with the true nature of pedagogical contexts (characterized by complexity, instability, idiosyncracy, uniqueness, and uncertainty); and (b) as we shall see here in this chapter, and in Chapter Five, *each* method of represention, *including the scientific method*, contributes only one of many possible constructed realities, and therefore only one of many *ways of knowing*.

I also discussed how, from the late 19th century onward, rational, scientific methods of knowledge production supplanted the more traditional, humanistic bases of knowledge contained in 'teaching methods' (largely derived from humanist traditions of philosophy and history) and 'the wisdom of practice,' in the modern teachers college. This trend was shown to be in line with the general propensity of most fields and disciplines during the modern era to adopt scientific modes of inquiry, and of many occupations to seek professional status through the assemblage of a specialized knowledge base made up of scientific propositions and techniques.

These points raise a number of questions: Was it inevitable that scientism and technicism should supersede humanism in the modern academy and in professional practice? Is it true that the scientific method was, and is, undoubtedly the most efficient and reliable source of knowledge (to paraphrase the epigraph that opened the last chapter)? Can scientific inquiry into teaching and learning provide us with certain ends of education, and therefore clear-cut means to achieve those ends? Was it, and is it, self-evident that scientific modes of inquiry and forms of knowledge are more 'rational' than their more practical, humanistic counterparts? Finally, if we have at last come to the realization that the application of abstract, theoretical, general solutions to complex human problems often create side-effects for the modern world which outweigh any benefits, then why do we find it so difficult to accept more practical, humane approaches to problem-solving--approaches which honour the complexity and uncertainty of human affairs--*as rational*?

I propose that these questions can be answered by examining a dominant theme in the Western intellectual tradition, what Dewey (1960/1929) and Toulmin (1990) refer to as 'the quest for certainty.' This theme was first manifested in the Western tradition as the philosophical quest for certain knowledge of the true, immutable nature of being through metaphysical speculation. With the scientific revolution of the 17th century, this first, *ā priori*, quest for certainty through abstract, theological speculation was replaced by a second, *ā posteriori*, quest for certainty through passive perception of the phenomenal world, and through 'pure' logical reasoning. This rational revolution in the Western tradition displaced metaphysical knowledge claims as the dominant form of 'certain' knowledge, and, in fact, vilified them as irrational, nonsensical religious beliefs. The modern scientific quest for certainty not only eschewed traditional humanist sources of knowledge, but also the common sense wisdom of practice in such fields as administration, engineering, medicine and education. On this scientific view, only those claims that were verified by systematic methods of experimentation were to be counted as 'knowledge.'

In this chapter, I will show how the modern belief in the absolute objectivity and certainty of scientific generalizations led historians of science from the 1920s to the late 1950s to significantly misrepresent both the beginnings of modernity and the nature of scientific progress in their accounts of the modern age. Following that, I will present two revised accounts of the history of science and modernity which both aim at greater accuracy by avoiding the historical error of applying contemporary concepts and ideas to the past in an overly revisionist fashion.[1] First, however, I will begin with a discussion of the origins of the 'quest for certainty' in the Western intellectual tradition.

The Quest For Certainty in Western Thought

According to Dewey (1960/1929), "man who lives in a world of hazards is compelled to seek for security" (p. 3). In pre-Hellenic times, this was attained in two ways: (a) through the use of myth, superstition and ritual to tame unconquerable destiny, to put oneself "...on the side of the powers which dispense fortune..." (p. 3), "to secure the favor of the holy..." (p. 12); and (b) through the invention of arts which turn those forces which threaten man (e.g., fire) to his advantage. The latter is the method of changing the world through action--what the ancient Greeks termed *techne*--and the former "...is the method of changing the self in emotion and idea" (p. 3).

Dewey (1960/1929) indicates that traditionally intellectual activity (idea) has been elevated above practical action (making and doing). This is because the former is a sphere of security and certainty, whereas the latter "...deals with individualized and unique situations which are never exactly duplicable and about which, accordingly, no complete assurance is possible" (p. 6).

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[1]Of course, one cannot avoid beginning historical inquiries with some theoretic or conceptual framework which is at least partially influenced by one's own historical context. However, the trick in avoiding an overly revisionist version of the past is to allow new concepts and theories to emerge when one begins to discover that the 'Facts' of that past do not entirely mesh with the 'facts' as represented by the initial framework employed in inquiry.

> [The] exaltation of pure intellect and its activity above practical affairs is fundamentally connected with the quest for a certainty which shall be absolute and unshakeable. The distinctive characteristic of practical activity, one which is so inherent that it cannot be eliminated, is the uncertainty which attends it. Of it we are compelled to say: Act, but act at your peril. Judgment and belief regarding actions to be performed can never attain more than a precarious probability. Through thought, however, it has seemed that men might escape from the perils of uncertainty. (Dewey, 1960/1929, p. 6)

Indeed, the quest for complete certainty through pure knowing has constituted the most enduring philosophic tradition of the West--a legacy initiated by the philosophers of antiquity.

The ancient Greeks brought a new, revolutionary worldview to the West in which myth and superstition were eliminated and replaced by the ideals of science and the life of reason (Dewey, 1960/1929). A crucial distinction in Hellenic thought was that between *episteme* and *doxa*, or theoretical grasp (knowledge) and belief.

> Central to most traditional work on the theory of knowledge has been a distinction between knowledge and belief, it being assumed that beliefs can be either true or false while knowledge can only be true. If I claim to know that some proposition is true, for example, and further evidence shows that the proposition is false, we would not conclude that I had false knowledge, but that I did not know at all. Thus knowing is, by definition, infallible. (Brown, 1977, p. 145)

In other words, whatever is known is, by definition, true: if it were not true it could not be *known*, but it could be *believed* to be true (Eisner, 1991).

Platonic idealism overwhelmingly favoured *episteme*; but peripatetic realism, although it (a) considered metaphysical speculation on the nature of being to constitute the 'First Philosophy,' and (b) shared Plato's hope of one day discovering indubitable truths that held generally, also recognized the indispensability of a logic of action.

> [Aristotle] saw that our chance of acting wisely in a practical field depends upon our readiness, not just to calculate the timeless demands of intellectual formulae, but also to take decisions *pros ton kairon*--that is, 'as the occasion requires.' (Toulmin, 1990, p. 190)

As stated above, one cannot know for certain in practical situations; therefore, one must exercise wise judgment, or *phronesis*, through the selection of positions that "...are rationally warranted, reasonable, or defensible--that is, well-founded rather than groundless opinions, sound *doxai* rather than shaky ones" (Toulmin, 1982, p. 109).

According to Dewey (1960/1929), the ancient Greek search for pure, ultimate, self-sufficient, ideal, eternal, universal, certain, and therefore necessary knowledge through rational, theoretical activity became the dominant predisposition of philosophy, and later, of science --thoroughly overshadowing the other half of Aristotle's logic.

> Although this Greek formulation was made long ago and much of it is now strange in its specific terms, certain features of it are as relevant to present thought as they were significant in their original formulation. For in spite of the great, the enormous changes in the subject-matter and method of the sciences and the tremendous expansion of practical activities by means of arts and technologies, the main tradition of western culture has retained intact this framework of ideas. Perfect certainty is what man wants. It cannot be found by practical doing or making; these take effect in an uncertain future, and involve peril, the risk of misadventure, frustration and failure. Knowledge, on the other hand, is thought to be concerned with a region of being which is fixed in itself. Being eternal and unalterable, human knowing is not to make any difference in it. It can be approached through the medium of the apprehensions and demonstrations of thought, or by some other organ of mind, which does nothing to the real, except just to know it. (Dewey, 1960/1929, p. 21)

In Dewey's opinion this quest for certainty through rational knowledge set up the basic separation of theory and practice, and replaced the pre-Hellenic "...search for security by practical means..." with the classic philosophical "...quest for absolute certainty by cognitive means..." (Dewey, 1960/1929, pp. 24-25). Subsequently, the classic philosophical tradition with its *quest for certain knowledge through metaphysical systems* was superseded in the 17th century by the modern scientific tradition with its *quest for certain knowledge through passive perception and formal logic*. The quest for certainty has, then, "...set the primary problem of epistemology: the search for indubitable knowledge" (Brown, 1977, p. 146).

However, as Toulmin (1990) points out, the quest for certainty has not entirely dominated the history of philosophy; instead, there have been a series of pendulum swings between the rival agendas of theory and practice, *episteme* and *doxa*, rationalism and humanism; and as Dewey (1960/1929), Kuhn (1970), Toulmin (1982, 1990) and others demonstrate, the history of scientific *practice*, rather than *product*, has disclosed that the Platonic demand for, and the Aristotelian dream of, *episteme*--the search for indubitable knowledge--has always been too much to ask for. Moreover, as we shall discover below, the crusade for infallible knowledge has been such a strong *doxa* of modernity that the traditional histories of modernity have ignored the true nature of the scientific enterprise as "...a mode of *directed* practical doing [italics added]" (Dewey, 1960/1929, p. 24), involving very human--*therefore historical, cultural, and uncertain*--traditions and judgments.

The Traditional History of Modernity

Toulmin (1990), Kuhn (1970), and Brown (1977) all maintain that 20th-century students of the origins of science and modernity have been misled in fundamental ways by historians of science who have concentrated on the "...study of finished scientific achievements..." (Kuhn, 1970, p. 1). In these standard accounts, modernity began with the 17th-century transition from medieval modes of thought to "...rational methods in all serious fields of intellectual inquiry--by Galileo Galilei in physics, by René Descartes in epistemology--with their example soon being followed in political theory by Thomas Hobbes" (Toulmin, 1990, p. 13). The twin pillars of this intellectual revolution were (a) the birth of a new method of philosophy which encompassed both a theory of knowledge and a philosophy of mind, and (b) a scientific revolution leading to striking innovations in both physics and astronomy (Toulmin, 1990).

The classical, or pre-Cartesian, emphasis in philosophy was on metaphysical or religious thought that was both speculative and synthetic. Durkheim (1938/1895) provides a useful characterization of this first 'quest for certainty' in the Western tradition:

> Before the first rudiments of physics and chemistry appeared, men already had some notions concerning physico-chemical phenomena which transcended mere perception, such as are found, for example, mingled in all religions. The reason for this is that thought and reflection are prior to science, which merely uses them more methodically. Man cannot live in an environment without forming some ideas about it according to which he regulates his behavior. But, because these ideas are nearer to us and more within our mental reach than the realities to which they correspond, we tend naturally to substitute them for the latter and to make them the very subject of our speculations. Instead of observing, describing, and comparing things, we are content to focus our consciousness upon, to analyze, and to combine our ideas. Instead of a science concerned with realities, we produce no more than an ideological analysis. To be sure, this analysis does not necessarily exclude all observation. One may appeal to facts in order to confirm one's hypotheses or the final conclusions to which they lead. But in this case, facts intervene only secondarily as

> examples or confirmatory proofs; they are not the central subject of science. Such a science therefore proceeds from ideas to things, not from things to ideas. (pp. 14-15)

In short, metaphysical 'knowledge' was *ā priori*--derived by reasoning from self-evident propositions and imposed on reality--rather than a *posteriori*--derived by reasoning from observed facts. As such, metaphysical speculations, from Descartes on, were largely considered to be nothing more than superstitious, nonsensical beliefs or traditions with no basis in experiential reality. With the appearance of Descartes's epistemology in the 1630s, philosophy was considered to have been emancipated from the age-old tutelage of theology by making it a field of 'pure' inquiry (Toulmin, 1990)--one which was involved in "...the analytic description of the mode and process of experience itself" (Durant, 1961, p. xxiii).

Similarly, classical science, after a brief flowering in ancient Greece, was considered to have made little progress for almost 2000 years because of a lack of understanding of systematic methods of inquiry which eliminated the contamination of human beliefs by proceeding from things to ideas. On this traditional view, once the proper conditions for intellectual progress in science (i.e., the experimental method) were delineated by the 'new philosophers' (e.g., Galileo, Bacon and Descartes), there was exponential, incremental growth in scientific technique and knowledge in which ideas concerning nature became progressively more rational and realistic (Toulmin, 1990).[2]

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[2]There is a story told about Aristotle that demonstrates the limitations of ancient methods of observation of the natural world. According to this story, one day Aristotle caught a fly and carefully counted its legs. He then declared that flies had five legs and this became an accepted scientific axiom for many years. Of course, the fly

In short, the received view of modernity held that an intellectual revolution was fashioned in the 17th century consisting of the adoption of a new scientific method and a new pattern of 'pure' inquiry in philosophy (epistemology). According to this view, these were ideas which (a) emerged from the minds of brilliant innovators through "...a purely internal process" (Toulmin, 1990, p. 45), (b) held universally and generally because of their internal logical consistency, and (c) were widely accepted because they were, self-evidently, the *only* rational replacement for irrational common sense, metaphysical and religious traditions or superstitions.

Aside from the adoption of 'rational' methods in science and philosophy, the traditional account of modernity catalogued four other key features of the origin of the modern era: (a) that the 17th-century intellectual revolution occurred in a Europe that was prosperous largely due to the growth of cities and the development of trade; (b) that during this time Church controls over intellectual life were weakening because of the Protestant Reformation (with the 17th-century commitment to rationality being largely a function of this reduced pressure from the Church along with the economic prosperity discussed above [see Toulmin, 1990, p. 80]); (c) that a secular, vernacular culture was born which was literate and which judged doctrines by their inherent plausibility rather than being told what to believe by the Church; and (d) that "...modern social and political practice..." soon followed

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Aristotle caught and observed just happened to be missing a leg (Gay, 1992). Hence, science made little progress until Galileo's discovery of the scientific method of experimentation in which systematic observations are replicated across individual cases in order to verify a general hypothesis.

"...with the rise of the class-structured, sovereign nation-states" (Toulmin, 1990, p. 169), in which "...at least in theory, the warrant for a sovereign monarch's exercise of power lay less in the fact of an inherited feudal title than in the will of the people who consented to his rule..." (Toulmin, 1990, p. 9). Hence, the "new philosophers'" rational methods brought not only advances in the natural sphere, but also in the human domain with the appearance of new, rational, and fruitful (a) techniques, and (b) moral, political, and social ideas.

The standard account of the beginnings of modernity, then, set forth the image of "...a medieval world dominated by theology yielding to a modern world committed to rationality" (Toulmin, 1990, p. 12). It was an account that was, on the whole, triumphant.

> In a dozen areas, the modes of life and thought in modern Europe from 1700 on (modern science and medicine, engineering and institutions) were assumed to be more *rational* than those typical of medieval Europe, or those found in less developed societies and cultures today. Further, it was assumed that uniquely rational procedures exist for handling the intellectual and practical problems of any field of study, procedures which are available to anyone who sets superstition and mythology aside, and attacks those problems in ways free of local prejudice and transient fashion. (Toulmin, 1990, p. 11)

Hence, on this received view of modernity, discipline after discipline, and field after field, following the lead of the 'pure' sciences from the 1630s on, adopted scientific-rationalistic modes of inquiry. Once having done so they each exhibited "...a history of steady accumulation of knowledge" (Brown, 1977, p. 9). This scientific knowledge

> ...has a rational, necessary and unchanging form. It is certain....There is complete correspondence between knowledge in its true meaning and what is real. What is known, what is true for cognition, is what is real in being. (Dewey, 1960/1929, pp. 20-21)

Thus, the standard view of the progress of science in modernity as the incremental accumulation of indubitable theoretical knowledge can be considered the second 'quest for certainty' in the Western tradition (see Dewey, 1960/1929).

However, as Kuhn (1970) and Brown (1977) indicate, this standard view of scientific progress in disciplines and fields which adopted rational, scientific modes of inquiry has become more difficult to accept because of a shift in focus in the history of science from an emphasis on finished scientific achievements to a consideration of the history of research practice itself. This recontextualization of the products of research in terms of their practical contexts of discovery and development has raised serious questions about whether or not scientific progress actually involves the incremental accumulation of individual, *certain* discoveries made by wholly detached, 'objective' observers.

In addition, Toulmin (1990), in a broader *cosmopolitical* recontextualization of modern thought, raises further questions concerning: (a) whether we can consider the intellectual innovations of the 'new philosophers' to be purely internal logics fashioned by brilliant minds which hold true universally and generally, or whether we must include the broader historical and societal contexts in which these ideas were wrought in our reflections about modernity; (b) whether it is plausible to view the adoption of scientific rationalism as the dominant intellectual tradition of modernity as a purely *rational* process, or whether it is more credible to view this as constituting the overwhelming acceptance of an ideological, dogmatic belief in a metaphysical system (i.e., a purely self-justifying axiomatic system) for less rational reasons; and (c) whether or not rational, theoretical modes of thought and practice have done just as much harm as good in attempting to improve the human lot.

Kuhn and a First Level of Recontextualization

Thus far I have outlined two main 'quests for certainty' in the Western tradition. The first was the quest for certainty through the pure knowledge afforded by metaphysical speculation concerning the one true, certain, universal, immutable nature of being. As we saw above, metaphysicians throughout the ages have attempted to escape the uncertainties of experiential reality through abstract, rational, synthetic thought and have tended to substitute such ideas for concrete things. The second quest for certainty was the modern scientific approach to knowledge which attempted to demythologize humanity's thinking by proceeding from things to ideas using scientific, objective methods of observation which 'discovered' ultimate reality (Rust, 1977).

As mentioned above, the standard account of the progress of science since the *one* scientific revolution of the 17th century has been one of steady accumulation of objective truths. This account of modernity, dominant from the 1920s until the late 1950s, was the progeny of the logical positivists, philosophers of science who focused on the logical analysis of accepted research results (Toulmin, 1990).

> Philosophers working in this tradition concerned themselves primarily with logical problems, particularly the logical structure of theories, and the logical relations between statements which describe observations and the laws and theories that these statements confirm or refute. Questions which are not amenable to formal analysis, such as the nature of scientific discovery, were brushed aside as

> nonphilosophical. Similarly, logical empiricists[3] did not concern themselves with the nature of scientific progress, although they tended to accept the traditional view that modern science came into existence in the sixteenth and seventeenth centuries with the discovery of the "empirical method" and has exhibited a history of steady accumulation of knowledge. (Brown, 1977, p. 9)

These traditional histories of science, then, have usually recounted both the successive increments of theoretical and technical discoveries within scientific disciplines, and the obstacles (i.e., errors, myths, and superstitions) which have inhibited this accumulation (Kuhn, 1970). This approach has been concerned with clarifying "...the permanent contributions of an older science to our present vantage" (Kuhn, 1970, p. 3); in other words, it has been content to chronologize the succession of out-of-date theories--or rather, *unscientific myths*--which have led up to today's conventional view of a phenomenon. On this view, the only thing worthy of consideration was how these earlier 'theories' had been rejected, and became considered as 'mythical' or 'unscientific' representations, because they were not verified (proven to mesh with the experiential world) by the scientific method which was employed in testing them (a process of pure, objective observation in which the scientist suspends any preconceptions [*doxai*] that he/she might hold about the phenomenon studied). Consequently, this approach to the history of science *decontextualized* these falsified theories by not considering their importance within specific scientific communities at particular stages of their historical development.

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[3]At this point in my argument the reader can consider the terms 'logical positivism' and 'logical empiricism' to be roughly analogous. I will distinguish between them more fully in Chapter Three.

Kuhn (1970), in his revolutionary historiography of science, eschewed this conventional approach and instead embraced the heterodox strategy of examining the history of research *activity* (rather than *content*). Instead of beginning from the standpoint of present scientific convention in a discipline and chronicling the succession of failed theories which lead up to this present view, Kuhn was interested in displaying the historical integrity of a theory in its own time; that is, he wanted to recontextualize these theoretical views by examining (a) how they guided the practice of science in a discipline in the time and place that they were current, and (b) by what mechanism these theories were superseded by other views which more accurately represented the phenomena under study. In so doing, he concluded that "out-of-date theories are not in principle unscientific because they have been discarded" (Kuhn, 1970, pp. 2-3). He also came to the conclusions that the practice of science was not entirely objective and that the products of science could never be certain. *Episteme*--certain knowledge--could not be obtained through either metaphysical speculation on the one hand, nor through objective observation on the other; instead, the most humankind could hope to achieve in terms of knowledge were warranted assertions, Toulmin's (1982) 'sound *doxai*'--well-founded beliefs which resulted from an interactive process between ***metaphysical subjects*** and ***theory-neutral reality***.

Scientific Rationality is Not Wholly Objective

Kuhn (1970) begins his account of the history of scientific practice by noting that all perception is theory-laden rather than passive; in other words, there is no separation between theory and empirical fact which would serve to ensure the objectivity of science. The theory held by a scientist serves to bound the object of his/her inquiry, helping him/her to distinguish between meaningful and meaningless data in the phenomenological field. It follows then that observations are no longer entirely objective, and that it is therefore possible for two scientists holding different theoretical views to observe the same phenomenon and perceive two different things.[4] On this view, the scientist becomes a *participant-observer* involved in the critical interpretation of his/her subject matter (Toulmin, 1982). There is therefore an arbitrary 'metaphysical' element to observation (Kuhn, 1970): the theory-laden subject--the scientist whose observations are directed by his/her chosen theory--interacts with the theory-neutral object; theories are both constitutive of and reflective of things at the same time (Garrison, 1986); "...reality is as much in the eyes of the beholder as it is in the thing itself..." (Rust, 1977, p. 93); knowledge is partially constructed.

According to Kuhn (1970) scientists work within communities made up of practitioners of a scientific specialty. Aside from their subject matter, they share a common educational initiation into professional practice, they pursue a set of shared goals, and "within such groups communication is relatively full and professional judgment relatively unanimous" (Kuhn, 1970, p. 177). When Kuhn says that communication is

[4]However, Kuhn (1970) considers that this only occurs during periods of revolutionary science when a scientific community is split between those who have been converted to a new view of their subject matter, and those who are still committed to the orthodox view. As we shall see in Chapter Four, other theorists consider it possible for a plurality of views to exist concurrently during periods of normal science as well.

relatively full between the members of a scientific community he means that they share a *paradigm*.

A paradigm is "...the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community" (Kuhn, 1970, p. 175). It is a constellation of group commitments; a disciplinary, or presuppositional, matrix; or, in Toulmin's (1982) terminology, 'an interpretive standpoint,' that is constituted of more than just a common theoretical structure (or set of lawful generalizations). A disciplinary matrix also includes: (a) a metaphysical part--beliefs in particular models that provide the community with preferred analogies and metaphors for the phenomena they study, e.g., the doctrine that "...heat is the kinetic energy of the constituent parts of bodies..." (Kuhn, 1970, p. 184); (b) a set of methodological values, e.g., that "...quantitative predictions are preferable to qualitative ones..." (Kuhn, 1970, p. 185); and (c) exemplars--shared examples, or ways of viewing physical situations as like others previously encountered, rather than in terms of rules or laws (i.e., tacit knowing, knowing embedded in practice [see Polanyi, 1966]).

A shared presuppositional matrix will set the problems of a scientific community; prescribe the methodology to be used in inquiry; direct the observations of the members toward meaningful, and away from meaningless, data; determine how data are to be interpreted; circumscribe what counts as valid problem-solutions; and, indeed, shape the world within which the scientist operates, including what technical implements he/she will use in research. In short, the members of a scientific community share a specialized language, a history, a culture, and a way of seeing the world. This paradigm is passed on to neophyte practitioners in a rigorous and rigid process of educational initiation, or what Toulmin (1982) refers to as 'enculturation,' and it thereby "...come[s] to exert a deep hold on the scientific mind" (Kuhn, 1970, p. 5). The practice of scientific research is seen, on this view, "...as a strenuous and devoted attempt to force nature into the conceptual boxes supplied by professional education"; in fact, research could not proceed without such boxes (Kuhn, 1970, p. 5).

Scientific Progress is Not Cumulative

Kuhn (1970) makes a crucial distinction between periods of normal science and periods of revolutionary science. Periods of normal science are those in which a scientific community operates from within an accepted theoretical and presuppositional framework. Normal research is *paradigm-based research* in which scientists get down to the business of articulating their accepted theory by solving the puzzles set out by their disciplinary matrix. Paradigms usually delimit a narrow focus for inquiry, they restrict the vision of the scientist, and they define a narrow range of acceptable puzzle-solutions. In so narrowing the range of anticipated results, paradigms provide a built-in mechanism for the perception of anomalous data, this despite the fact that it is not the aim of normal research activity to produce such novelties of fact (which can, in turn, lead to novelties of theory).

Scientists, upon encountering anomalous data which can be considered counter-instances of their theories, usually suppress these novelties because they are subversive to their presuppositional commitments. On the traditional (i.e., 'logical positivist') view, the discovery of a counter-instance should lead to the disconfirmation or rejection of a theory, and the renunciation of its affiliated paradigm; in contrast, Kuhn (1970) found that such falsification *never* occurs in practice. Instead, such anomalies are initially treated as 'research problems' to be accounted for within the existing theoretical framework; and, in fact, *they most often are explained by it*. Kuhn (1970) considers such imperturbable commitment to paradigm to be (a) a function of the rigid and rigorous initiation to practice that a scientist receives, and (b) a crucial element in a scientific community's success in solving the difficult puzzles of some narrowly-defined aspect of nature.

Sometimes, however, an anomalous fact proves to be recalcitrant: it cannot be reconciled with the existing theoretical view, thereby making the profound failure of the normal-scientific tradition to solve its own problems conspicuously obvious to the professionals working within that paradigm. Such a recalcitrant anomaly will subvert the accepted theoretical view, thereby inciting a crisis within the affected scientific community during which a number of novel theories are invented to account for the novel fact. These tradition-shattering periods of 'revolutionary science' eventually end with the prevailing presuppositional framework of the field or discipline being replaced with another. Kuhn (1970) stresses the fact that a theory will only be declared invalid (i.e., *falsified*) when another, more fruitful, one is ready to take its place: one paradigm must follow another in guiding the perception of practitioners in order to have 'science.' The emergent paradigm is generally incompatible with the view that it is replacing, and produces a consequent shift in the conduct of science itself for those who have been converted: i.e., new methods and instruments, an altered problem-field, and new standards of problem-solution. As well, the world within which the scientist works is transformed mostly through conceptual changes which alter the scientist's meaning structure and way of seeing the world (Kuhn, 1970).

'Scientific revolutions,' then, are "...those non-cumulative developmental episodes in which an older paradigm is replaced in whole or in part by an incompatible new one" (Kuhn, 1970, p. 92). They are inaugurated by the failure of an existing paradigm to solve its own problems. The shift from an old position to a new one can be a long, drawn-out affair during which two paradigms compete for supremacy within a scientific community. These paradigms constitute different theoretical-language communities which are largely, but not wholly, incommensurable. They provide different ways of interpreting--of seeing--the subject matter of the community. Therefore, the decision to reject one theory in preference for another is an act of *judgment* (phronesis) that is based on more than the comparison of each theory with the world: the competing paradigms must also be compared with each other. In other words, the decision cannot be based solely on an evaluation of the internal logic of one position or the other, or the application of the logic of one position *to* the other. This is because the two groups have different conceptual maps or different theoretical languages which cause them to view the world differently and partially talk through each other.

Eventually, however, the majority of practitioners within a community will be persuaded to convert to the new paradigm if the vanguard in the field can demonstrate that it is more fruitful in solving the problems caused by the recalcitrant anomaly which initiated the crisis. They can only make this demonstration by effectively translating these findings for their uninitiated peers so that they can finally 'see' how progressive the new view is and thereupon be persuaded to adopt it. Nonetheless, in some cases it is not entirely clear that one view or the other is more comprehensive, as all of them are incomplete. The emerging paradigm may be more successful in solving the problem caused by the novelty of fact, but it might, at the same time, leave something out of its reckoning that is well explained by the traditional view. In such situations where there is some disagreement concerning the relative fruitfulness of two theories there is no algorithm to appeal to in making a decision. The scientists must make professional judgments, they must take leaps of faith that are not always correct and decide based on reasons that are mostly, but not always, rational (e.g., sometimes aesthetic or intuitive considerations enter in). However, this does not mean that *any* personal position will be accepted as scientific fact: a certain level of objectivity will be maintained because of the fact that the choice of what constitutes 'truth' will be a warranted, *communal* decision and not an individual one. Although complete certitude cannot be achieved, there is *more* certainty in numbers. On Kuhn's (1970) view, once the scientific community is converted to the replacement paradigm a new period of normal scientific puzzle-solving follows and continues until that time when the field is thrust into another period of crisis by one or more recalcitrant anomalies.

Interim Summary

Kuhn's (1970) study of the history of scientific practice has led him to conclude that the traditional historical view of the progress of science as the incremental accumulation of irrefutable empirical facts through a process of objective observation is flawed on two main accounts. First, scientific observations are theory-laden, not objective, and knowledge claims are partially constructed representations achieved by an interaction between the theory-laden perception of the scientist and the theory-independent world--by "...a continuing attempt to interpret nature in terms of a presupposed theoretical framework" (Brown, 1977, p. 10). Second, the overall progress of science has not been cumulative and scientific facts have not constituted *episteme* (immutable, certain, theoretical truths for the ages). Instead, the development of science has been characterized by periods of cumulative progress through normal-scientific inquiry, interspersed with non-cumulative periods of revolutionary science.

> The transition from a paradigm in crisis to a new one from which a new tradition of normal science can emerge is far from a cumulative process, one achieved by an articulation or extension of the old paradigm. Rather it is a reconstruction of the field from new fundamentals, a reconstruction that changes some of the field's most elementary theoretical generalizations as well as many of its paradigm methods and applications. During the transition period there will be a large but never complete overlap between the problems that can be solved by the old and by the new paradigm. But there will also be a decisive difference in the modes of solution. When the transition is complete, the profession will have changed its view of the field, its methods, and its goals. (Kuhn, 1970, pp. 84-85)

Hence there is no such thing as certain knowledge, there are only tentative representations of phenomena that serve a purpose for a time and which are eventually replaced by more fruitful views. Also, there is no foundational method of inquiry (e.g., the scientific method used in physics); instead, a diversity of procedures has emerged from the disparate experiences of scientific communities in studying their own unique objects of investigation.[5]

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[5]It must be noted that James (1969/1907) articulated a version of scientific discovery and change that is startlingly similar to that of Kuhn *some 55 years earlier*. Like Kuhn, James recognized that what we perceive is a function of the 'perspective' that we bring to observations (although James did not devise anything approaching Kuhn's conception of a 'paradigm' or a 'disciplinary matrix'). James also delineated an account of scientific progress that can be considered a rudimentary rendition of Kuhn's concepts of 'normal' and 'revolutionary' science. He discusses how all our 'truths,' 'formulas,' 'beliefs' or 'opinions' are provisional accounts of reality that guide us in verification processes. We consider these opinions to be 'true' because they *work* or they *pay* in the sense that they *lead us*, they are progressive in helping us to "...assimilate, validate, corroborate and verify" *for the time being*, not absolutely (James, 1969/1907, p. 133). This is similar to Kuhn's 'normal science': "The simplest case of new truth is of course the mere numerical addition of new kinds of facts, or of new single facts of old kinds, to our experience--an addition that involves no alteration in the old beliefs....[Here] truth is satisfied by the plain additive formula" (James, 1969/1907, p. 51).

Occasionally, however, in the process of 'acting on these beliefs' we are 'brought into sight' of 'novelties of experience' (i.e., 'recalcitrant anomalies') which cause us 'inward trouble' from which we seek to escape. Like Kuhn's normal scientific puzzle-solver, James's inquiring individual will at first act conservatively: s/he will try to account for the novelty by modifying her/his array of old opinions. Others will demonstrate their loyalty to their accepted truths by ignoring the novel phenomenon, or denigrating those who do recognize it. Eventually, the mismatch between previous truth and novel fact will lead the individual to modify "...his previous mass of opinions" (James, 1969/1907, p. 50). This resembles Kuhn's notion of a scientific revolution; however, James's view of the transition from the 'stock of old opinions' to 'new truth' is much more *cumulative* in that, on his view, such shifts preserve the former truths as far as possible, while stretching them enough to assimilate the novelty. Indeed, "the most violent revolutions in an individual's beliefs leave most of his old order standing....An *outrée* explanation, violating all our preconceptions, would never pass for a true account of a novelty"

In closing, it must be stated that Kuhn's (1970) work can be considered to have triggered a paradigm shift in the history and philosophy of science. The old, logical-positivist-inspired tradition viewed the historical development of science as a succession of falsified, unscientific hypotheses leading up to the present 'verified' theory. On this view the progress of science involved the incremental accumulation of empirically verified, logically structured, axiomatic systems which held universally, generally, and timelessly. Kuhn's historiography of scientific practice has replaced this analysis of the logical structure of completed, accepted theories with a program devoted to understanding "...the rational basis of scientific discovery and

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(James, 1969/1907, pp. 50-51). On this point, then, James seems to agree with post-Kuhnian philosophers of science such as Lakatos and Laudan (both cited in Gholson & Barker, 1985) who deem Kuhn a proponent of a doctrine of radical incommensurability that destroys the possibility of "...any real continuity in content from one paradigm to the next" (Gholson & Barker, 1985, p. 757). I will return to this controversy in Chapter Four.

Schwab (1978a) also devised a comparable theory of progress in the structure of disciplines that predated that of Kuhn (Kuhn's first edition of *The Structure of Scientific Revolutions* was published in 1962; Schwab's article was written in 1961, although it was not published at that time). Schwab, like James and Kuhn, views theory as a necessary guide in inquiry: it causes us to interpret data in a certain way, and allows us to distinguish between relevant and irrelevant facts. Unlike James, Schwab articulates a version of Kuhn's concept of a 'paradigm.' He discusses how 'research programs' (c.f. Kuhn's 'paradigm') have a certain 'structure' (c.f. Kuhn's 'disciplinary matrix') made up of criteria of methodology and validity, and "...a conception of the character of [the] subject matter" (Schwab, 1978a, p. 243). Similar to Kuhn's account of the progress of science (i.e., periods of 'normal science' interspersed with tradition-shattering phases of 'revolutionary science'), Schwab's research programs undergo periods of 'structure development' and 'structure replacement.' Truth, then, is plural, not final. However, unlike Kuhn, and similar to James, Schwab recognizes the possibility that a plurality of structures can exist concurrently in a field of inquiry--especially a social science. Like the debate concerning incommensurability, I will further clarify the nature of this controversy regarding plurality in Chapter Four.

theory change" within historical scientific communities (Brown, 1977, p. 10). However, as we shall now discover, Toulmin (1990) considers this recontextualization of scientific theories in terms of their historical contexts of discovery and development in scientific communities to be incomplete.

Toulmin and a Second Level of Recontextualization

Toulmin (1990) considers Kuhn's (1970) historiography of scientific practice to be an improvement on the standard accounts offered by historians of science during the period from the 1920s to the 1950s; however, he does not consider this recontextualization of scientific conventions in terms of their "...historical sequence of patterns of explanation ("paradigms")..." to have gone far enough (p. 84). On Toulmin's (1990) view, Kuhn's thesis that the members of scientific communities will, on occasion, replace their usual patterns of explanation with other, largely incommensurable, paradigms on the basis of explanatory success, ignores the fact that "...the success of science has rested, historically speaking, on *political* as well as explanatory considerations [italics added]" (p. 136):

> ...when it came to judging whether to accept or reject novel scientific ideas, even Kuhn did not allow social and political interpretations to enter the equation. Only in the 1980s have scholars gone far beyond changes in the internal content of the sciences, and asked how the external context influences their choice of problems and patterns of explanation. (p. 132)

There are, then, two main levels of context into which a scientific finding can be reintegrated: first, it can be reintroduced into its milieu of discovery, acceptance, and articulation within a scientific community at a particular stage of its historical development; and second, the discovery and endorsement of a theory by a scientific community can be considered in terms of the wider socio-political currents of thought and influence present in the historical era from which it emerged (i.e., all such theoretical systems are ineluctably the partial products of the time and place of their invention).[4]

In addition, Toulmin (1990) suggests that Kuhn (1970) and other 'new philosophers of science' have used an anachronistic conception of 'science' in their analyses of its history. In contemporary times, 'science' is viewed as an activity engaged in for conceptual or practical advancement within a field or discipline. In contrast, Toulmin points out that the conduct of science in earlier centuries can be considered as part of a broader 'form of life' or 'life world.' For example, scientists such as Newton were more interested in the theological implications of findings than in their technical applications. Moreover, the Enlightenment readers of these findings were more interested in the implications of scientific ideas for issues related to political obligation and social structure than in their theoretical or practical fruitfulness. So,

> instead of bringing our current standards of judgment to bear on the ideas of earlier generations, we do better to put ourselves into the heads of people living in a given historical situation, and try and recognize what gave scientific ideas the charms that won them a place in the "common sense" of the time. (Toulmin, 1990, p. 137)

Thus Toulmin (1990) is more interested in a *cosmopolitical* analysis of

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[4]For example, the theory of cognitive science in educational psychology employs a model of teaching and learning in which inputs are structured for efficient storage to, and retrieval from, memory. In my opinion it is no accident that such a theory is prevalent during a time of great advancement in, and use of, microcomputer technologies.

modern philosophy and science in which these narrower enterprises are seen to be both *influenced by* (i.e., in terms of the delimitation of their problem-field and their acceptance of certain patterns of explanation), and *influencing of* (i.e., in terms of suggesting and justifying social or political structures) the broader socio-political situation (i.e., historical events, beliefs, and ideologies).

In sum, Toulmin (1990) considers that it is not enough to examine scientific 'paradigms' when considering the beginnings of science and modernity in the 17th century. This is because the scientific work of that time was done as part of a broader 'life world': a comprehensive system of ideas that was as much a socio-political as a scientific device. That is to say, the scientific worldview was accepted as much for its justification of class-based nation-states as for its power in explaining natural phenomena. As well, Toulmin demonstrates that when recontextualized into their proper socio-political *milieux*, the 17th-century revolutions in philosophy and science *cannot* be considered to be the results of 'purely internal thought processes,' both abstract and context-free, embarked on by brilliant innovators--*reflective ésprits*--that could have come from any time or place. Finally, Toulmin disputes the positivist-inspired account of the beginnings of modernity as the 17th-century discovery--during a time of economic prosperity and religious freedom--of rational methods in science and philosophy. Instead, he sees the modern age as descended from two historical progenitors: 17th-century scientific rationalism *and 16th-century Renaissance humanism.*

1570-1610: Renaissance Humanism

Toulmin (1990) considers the logical positivist dating of the modern era from the 17th-century rational revolution in both science and philosophy onward to be incorrect. He asserts that "the key features of the modern age were products not of a single intellectual origin, but of two distinct beginnings" (Toulmin, 1990, p. 80). The first fount was embodied in the emancipatory ideas of the 16th-century Renaissance humanists, "...from Erasmus on, who lived in times of relative prosperity, and built up a culture of 'reasonableness' and religious toleration" (Toulmin, 1990, p. 80). The second source was embodied in the formalism of the 17th-century rationalists, from Descartes on, who eschewed the intellectual modesty of the humanists through a quest for absolute certainty in rational proofs. Indeed, if the dogmatic spirit and ideas of 17th-century thinkers are compared with the more moderate, tolerant erudition of the 16th-century writers, "...we may even find 17th-century innovations in science and philosophy beginning to look less like revolutionary advances, and more like a defensive counter-revolution" (Toulmin, 1990, p. 17).

The Renaissance humanists included such figures as Erasmus (b. 1467), Rabelais (b. 1494), Montaigne (b. 1533), and Shakespeare (b. 1564) (Toulmin, 1990). These writers demonstrated a practical concern for human life and its concrete details, and worked in content- and context-rich fields of study such as ethnography, history, and poetry. In their stance toward inquiry and life they displayed an urbane open-mindedness and a skeptical tolerance. They deplored the religious strife that was increasing in intensity during the 16th century and taught about the need for tolerance and human modesty. Specifically, there are three main interconnected themes in their thought: (a) the notion that 'reason' was the common sense ideal of being 'reasonable,' (b) an emphasis on both the theoretic and the practical realms, and (c) in philosophy, a healthy respect for diversity and complexity which led them to embrace skepticism.

"For 16th-century humanists, the central demand was that all of our thought and conduct be *reasonable [italics added]*" (Toulmin, 1990, p. 199). First, this meant developing modesty about one's capacities and, indeed, about the potential for all of humankind to 'know' for certain. Second, it meant that one cultivated a self-awareness in one's self-presentation. Third, it required that one learn to tolerate all forms of social, cultural, and intellectual diversity.

> It was unreasonable to condemn out of hand people with institutions, customs, or ideas different from ours, as heretical, superstitious, or barbarous. Instead, we should recognize that our own practices may look no less strange to others, and withhold judgment until we can ask how far those others reached their positions by honest, discriminating, and critical reflection on their experience. We can judge people's ideas or customs fairly only if we know not just where they ended up, but also (in the language of the 1960s) 'where they were coming from.' (Toulmin, 1990, p. 199)

As stated above, this toleration of diversity led them to abhor the dogmatic flavour of the debate between Protestant and Catholic factions in the period between the Reformation and the counter-Reformation. This debate was polemical, with both sides making opposing claims of having "...reach[ed] unquestioned Truth or unqualified Certainty over all matters of doctrine" (Toulmin, 1990, p. 25). Fourth and finally, one must emphasize sound rhetoric, which "...demands that we *speak to* the condition of an audience...", and which "...requires us to *listen to* their condition with equal care" in order to achieve "...honest human understanding..." (Toulmin, 1990, p. 199).

The 16th-century humanist tradition placed equal stress on both the theoretic and the practical spheres of thought and action.

> Renaissance scholars were quite as concerned with circumstantial questions of practice in medicine, law, or morals, as with any timeless, universal matters of philosophical theory. In their eyes, the rhetorical analysis of arguments, which focused on the presentation of cases and the character of audiences, was as worthwhile--indeed, as philosophical--as the formal analysis of their inner logic: Rhetoric and Logic were, to them, complementary disciplines. Reflecting on the detailed nature and circumstances of concrete human actions--considering their morality as 'cases'--also shared top billing with abstract issues of ethical theory: in their eyes casuistry and formal ethics were likewise complementary. (Toulmin, 1990, p. 27)

Although many 16th-century humanists were fascinated by metaphysical speculations, this theoretical streak went hand-in-hand with a taste for the variety of human experience and a respect for its rational possibilities. For example, Montaigne argued that

> ...it was best to suspend judgment about matters of general theory, and to concentrate on accumulating a rich perspective, both on the natural world and on human affairs, as we encounter them in our actual experience. (Toulmin, 1990, p. 27)

However, even while they held this reverence for the rational possibilities of human experience they nonetheless possessed a delicate feeling--a healthy skepticism--concerning the *limits* of that experience.

Montaigne considered it presumptuous and self-delusional to attempt to reach theoretical consensus about nature. This was a form of skepticism that was very different from the pejorative sense that the term acquired a century later as destructive nay-saying in which "...the skeptic *denies* the things that other philosophers *assert*" (Toulmin, 1990, p. 29). The Renaissance humanist position was more tolerant of speculative abstraction than this: "...they no more wished to *deny* general philosophical theses than to *assert* them..." (Toulmin, 1990, p. 29). In their opinion, general philosophical questions reached beyond the scope of experiential substantiation: "Faced with abstract, universal, timeless theoretical propositions, they saw no sufficient basis in experience, either for asserting, or for denying them" (Toulmin, 1990, p. 29).

Therefore, because it was beyond the ken of human beings to either confirm or disconfirm philosophical speculations, it was *reasonable* to accept a *plurality* of theological views.[7]

> In theology or philosophy, you may (with due intellectual modesty) adopt as personal working positions the ideas of your inherited culture; but you cannot deny others the right to adopt different working positions for themselves, let alone pretend that your experience 'proves' the truth of one such set of opinions, and the necessary falsity of all others. The 16th-century followers of classical skepticism never claimed to *refute* rival philosophical positions: such views do not lend themselves either to proof or to refutation. Rather, what they had to offer was a new way of understanding human life and motives....They taught readers to recognize how philosophical theories overreach the limits of human rationality. (Toulmin, 1990, p. 29)

On this view, we must temper our ambitions for certain, necessary, general, and timeless *episteme* by limiting knowledge to what humanity can know: sound *doxai* concerning "...the range of particular everyday phenomena,...the nature of the case,...on which human experience gives solid testimony..." (Toulmin, 1990, p. 30). There is no rational way to convert an interlocutor who honestly believes in a divergent position;

[7]This parallels James's (1969/1907) doctrine of 'epistemological pluralism.'

instead, we must learn to live with such variance of thought, we must accept the diversity of human life. Later, given more shared experience, we might one day resolve these differences and achieve a convergence of different schools of thought. Meanwhile,

> we need not be ashamed to limit our ambitions to the reach of humanity: such modesty does us credit....Tolerating ...plurality, ambiguity, or the lack of certainty is no error, let alone a sin. Honest reflection shows that it is part of the price that we inevitably pay for being human beings, and not gods. (Toulmin, 1990, p. 30)

In short, the 16th-century Renaissance humanists lived in times characterized by a tone of confidence, and embraced a tolerant rationality; that is to say, a humane reasonableness that was confidently modest, skeptical, and accepting of diversity. As an intellectual tradition, it embraced Aristotle's dual logic of *episteme* and *phronesis*, and was clearly not a 'quest for certainty.'

1610-1650: Catastrophe and Counter-Renaissance

On the accepted view of the beginnings of modernity, the rational revolution of the early 17th century occurred during times that were prosperous and tolerant of religious diversity. Toulmin (1990) vehemently challenges this account as a scientific rationalist fabrication, for during this time the 16th-century tone of confidence was replaced with a tone of catastrophe. First, Toulmin (1990) maintains that the years from 1605 to 1650 were not comfortable, rather, they were among the most wretched in European history. Whereas the 16th century had been prosperous, the 17th century suffered a grave economic depression, especially during the period from 1619-1622. At this time general unemployment created a pool of idle men who would later function as mercenaries for hire during the Thirty Years' War. This was also a time of world-wide worsening of climate, famine, plague, and rapid urbanization. Second, Toulmin (1990) asserts that the yoke of religion on scientists and other intellectual innovators during the period from 1620-1660 was not weakened, but *intensified*, largely due to the depth of the religious fervour engendered by the Counter-Reformation.

Third, the cultural break with the Middle Ages, characterized by the rise of a vernacular, secular culture, actually began 100 to 150 years earlier than reported in the traditional account. The spread of education and literacy among the laity, which in turn helped to destroy the Church's monopoly in science and scholarship, was not entirely a 17th century phenomenon, but had begun a century earlier: "...Galileo and Descartes were late products of changes that were already well under way in Western Europe by 1520, and in Italy a good time before" (Toulmin, 1990, p. 19). In other words, the lay culture of modernity had its genesis in the 16th century, as exemplified by the scholarship of humanists such as Erasmus, Rabelais, Shakespeare, de Montaigne, and Bacon, and was not born of the 17th century alone. Thus,

> ...it is time to give up any assumption that the 17th century was a time--the first time--when lay scholars in Europe were prosperous, comfortable, and free enough from ecclesiastical pressure to have original ideas... (Toulmin, 1990, p. 21).

The rational revolutions in science (i.e., Galileo's scientific method) and philosophy (i.e., Descartes's epistemology) were clearly *not* the products of comfortable times.

As we saw above, the traditional account of modernity treated Descartes as a rational *ésprit* who was supremely logical and above the crises of his times: "...we do not need to refer to the outward events of his life, since these did not essentially influence the history of his thoughts. That was a purely internal process." (Toulmin, 1990, p. 45). Toulmin (1990) refers to the received account of Descartes's intellectual career as the 'ivory tower biography,' because it served to decontextualize the man's thought from his life experiences. For example, the account of his life and ideas contained in the *Grande Encyclopédie* either praises him for turning his back on the tragedies of his times, or ignores important events in his life, such as his intimate exposure to the brutality of the Thiry Years' War. Toulmin disagrees with such an approach: in order to fully understand Descartes's ideas we must consider them in terms of their historical context.

> Each new philosopher presents theses to an audience that lives, with him, in a situation different from those of his predecessors. Their *contexts* of writing often differ in major respects; and, by ignoring these differences, we impoverish our understanding of the *content* of their ideas. (Toulmin, 1990, p. 87)

When Descartes's ideas are recontextualized it becomes plausible to assert that his version of reason--scientific rationalism--was greatly influenced by the monumental catastrophe of his times: the Thirty Years' War.

The Counter-Reformation exploded into full-scale war in 1618, when Descartes was in his early 20s, and ended in 1648, when he had but two years to live. It therefore seems plausible that Descartes's life and intellectual commitments were influenced by this Thirty Years' War. To secure this connection, Toulmin (1990) gives two examples of how Descartes had personal contact with this catastrophe. First, the young René spent his formative years being educated at La Flèche, a Jesuit college preoccupied with the 'epoch-marking' assassination of King Henri IV of France in 1610. Henry was a trusted colleague of Montaigne who shared the latter's modest skepticism and tolerance for diversity, who tried to reduce the role of religion in politics, and whose policies reflected his commitment to religious toleration. "Henri IV's assassination struck a mortal blow to the hopes of those, who, in France and elsewhere, looked to toleration as a way of defusing denominational rivalry" (Toulmin, 1990, p. 52), and "...dashed the last hope of escaping from irresoluble conflicts" (p. 48). Descartes was a student at La Flèche during the years following the assassination and would therefore have been present at many annual celebrations of Henry's memory (termed the *Henriade*) held there. Second, in the first dozen years of the war, Descartes followed its progress in person, initially "...as a gentleman observer studying Prince Maurits of Nassau's new military techniques...", and later as a member of the Duke of Bavaria's army staff (Toulmin, 1990, p. 61). There is thus little doubt that Descartes's ideas were greatly influenced by this upheaval in Christianity, as "...his whole mature life was spent under its shadow" (Toulmin, 1990, p. 61).

In light of the facts of Descartes's life, Toulmin (1990) considers it no great wonder that he was compelled to reject, indeed to condemn, Renaissance humanist reasonableness, emphasis on the practical, and intellectual tolerance as inconclusive, overly permissive, and open to abuse. Instead, he pursued a quest for absolute rational certainty by first searching for a 'single certain thing,' i.e. his *cogito ergo sum*, which would serve to make other certainties possible. Following this, Descartes pursued a rational method for resolving scientific problems which had a dual focus:

> For Descartes, rational thought could not rely on inherited tradition: empirical procedures rooted in experience rather than theory were in his view compromised, since they perpetuated the folklore of a given culture and period, and rested finally on superstition, not reason. He felt that if everyone cleaned their slate, and started from the same sensory 'impressions' or 'clear and distinct ideas', there would be no need to ask what personal or cultural idiosyncracies each of them brought to their common debate. Wherever possible, then, the 'rational' thing to do was to start from scratch, and to insist on the certainty of geometrical inference and the logicality of formal proofs. Only so could a way be found, he believed, to avoid both the interminable quarrels of the dogmatic theologians, and the uncertainties and contradictions implict in Montaigne's skepticism. (Toulmin, 1990, p. 199)

Descartes's new research program was, then, both mathematical and experimental; however, he gave higher priority to the pursuit of mathematical certainty than to empirical support of intellectually disconnected facts. In his opinion, the pinnacle of certain human knowledge was found in Euclidean geometry--an ideal rational system with no successor (i.e., it was *positive and timeless*). In solving any kind of problem, all one needed to do to arrive at *the* correct solution was to strip away the inessentials, such as our inherited systems of concepts and the historical contexts of problems (i.e., decontextualize the situation), and begin again with a clean slate by using rationally validated methods which, on the one hand "...fram[ed] one's basic theories around ideas whose merits were clear, distinct, and certain..." and, on the other, "...us[ed] only demonstrable arguments, having the necessity of geometrical proofs" (Toulmin, 1990, p. 81).

At the heart of Descartes's epistemology lay a crucial distinction between "... the *rational freedom* of moral or intellectual decision in the human world of thought and action, and the *causal necessity* of mechanical processes in the natural world of physical phenomena" (Toulmin, 1990, p. 107). This Cartesian dichotomy, which became one of the main tenets of the modern worldview, "...began by cutting rationality off from causality...[and] ended by separating the world of (rational) human experience from the world of (mechanical) natural phenomena" (p. 107). Some of the main beliefs on the 'nature' side of the division (which will be important for my later arguments) were that (a) nature is governed by fixed laws set up at the creation; (b) the objects of nature are composed of inert matter; and (c) at the creation, God combined natural objects into hierarchical, stable systems of 'higher' and 'lower' things, with motion flowing downward from the higher to the lower phenomena. One of the most important beliefs from the 'human' side of the dichotomy was that humans live mixed lives, partially causal and partially rational. Our rational side is contained in our intellectual capacity to think and reason, and was to be trusted and encouraged.[8] Our causal side is contained in our emotions, which were bodily and carnal, and considered to frustrate the work of reason in the individual: they were therefore to be distrusted and restrained. According to Brown (1977), this suspicion concerning the ability of human beings to control their emotions and remain 'objective' was at the heart of many rationalist attempts throughout the modern age to develop techniques of inquiry which limited the human element in them.

[8]This is another affirmation of Dewey's (1960/1929) assertion that throughout history man has traditionally elevated the realm of thought above all else.

In short, Descartes's intellectual program embraced the quest for certain knowledge through (i) foundationalism (i.e., the search for a set of authoritative, rational principles through formal logic to serve as an abstract, timeless, universal foundation for human knowledge), and (ii) the discovery of the fixed laws of the inherently mechanical, stable systems of nature, which were made up of inert matter. Both of these rational processes involved starting from scratch by cleaning the slate and therefore removing the clutter of inherited traditions. These three things--certainty, systematicity, and decontextualization (the clean slate)--together formed the pillars of the second, scientific and epistemological, phase of modernity from 1630 on, and were so successful that they almost completely banished all traces of the first, literary or humanistic, phase which lasted a mere forty years (from 1570 to roughly 1610). Thus, Descartes successfully set aside Renaissance humanist beliefs, and persuaded fellow philosophers to abandon and denounce such context- and content-rich fields as history and ethnography, in preference for abstract, decontextualized fields such as geometry, dynamics, and epistemology. This constituted a move from a partially practical to a purely theoretical view of philosophy.

Toulmin (1990) lists four fundamental ways in which the long-standing preoccupations of the Renaissance humanist tradition were set aside by the 17th-century rational revolution. The first was a shift from the oral to the written; from a concern for both rhetoric and logic, to a stress on logic alone. Pre-1600, two standards of evaluating the soundness or validity of arguments were considered: (a) the circumstantial (i.e., among particular people in specific situations and dealing with concrete cases where various things were at stake) merits or defects of persuasive, public utterances; and (b) "...the internal steps relied on in the relevant 'arguments'--i.e., strings of statements" (Toulmin, 1990, p. 30). In the Cartesian tradition, this focus was narrowed so that questions of validity pertained solely to the formal analysis of "...written chains of statements whose validity rested on their internal relations" (Toulmin, 1990, p. 31). In short, "...*formal logic was in, rhetoric was out*" (p. 31).

The second shift was in the scope of philosophical reference: from the particular to the universal. In the age-old humanist tradition, issues in such areas as morality were handled using case studies: indeed, Aristotle considered that the 'good' had no universal form, but was to be considered relative to the circumstances of particular cases. However, with the Cartesian revolution, this focus was narrowed:

> ...philosophers again limited their own scope: the careful examination of 'particular practical cases' was ruled out of ethics by definition. Modern moral philosophy was concerned not with minute 'case studies' or particular moral discriminations, but rather with the comprehensive general principles of ethical theory. (Toulmin, 1990, p. 32)

In sum, "...*general principles were in, particular cases were out*" (Toulmin, 1990, p. 32).

The third deviation from Renaissance humanist wisdom was the eschewal of the local in preference for the general. The 16th century humanists found sources of material in such disciplines as ethnography, geography, and history: fields where geometrical methods of analysis had no power. As such, they honoured the peripatetic philosophy's view that human life is not well addressed by abstract, general approaches. Descartes, on the other hand, felt that "the demands of rationality impose on philosophy a need to seek out abstract, general ideas and principles, by which particulars can be connected together" (Toulmin, 1990, p. 33). Thus, "...*abstract axioms were in, concrete diversity was out*" (Toulmin, 1990, p. 33).

The fourth and final 'change of mind' was from the timely to the timeless. Prior to 1600, the Renaissance humanists gave equal weight to (a) concrete issues, such as legal, medical or confessional practice; and (b) abstract issues of theory. In practical fields all problems are timely: they refer to specific moments in time and are decided as the occasion requires; in other words, *time is of the essence*. For example, a navigator's decision to change course ten degrees to starboard is a decision that is as rational as any mathematical deduction; however, the rationality of the decision is based on more than just the computation, it is also based on the timeliness of the action. If the action were delayed until such time as turning that precise amount in that direction would lead the ship into the path of another vessel, then the decision is no longer rational. The Cartesian revolution removed the timely as a concern in philosophy; instead, "...attention was focused on timeless principles that hold good at all times equally..." (Toulmin, 1990, p. 34). Hence, "...*the permanent was in, the transitory was out*" (p. 34).

According to Toulmin (1990) this historical shift from practical philosophy to theoretical philosophy (and science), reflected in the aforementioned 'changes of mind,' constituted the *narrowing* of rationality, *not* its invention.

> ...we also need to look again at the deeper belief that 17th-century science and philosophy developed an original concern for rationality and the claims of Reason. This belief is misleading in two ways. Rather than expanding the

> scope for rational or reasonable debate, 17th-century scientists narrowed it. To Aristotle, both Theory and Practice were open to rational analysis, in ways that differed from one field of study to another. He recognized that the kinds of arguments relevant to different issues depend on the nature of those issues, and differ in degrees of formality or certainty: what is 'reasonable' in clinical medicine is judged in different terms from what is 'logical' in geometrical theory. Seventeenth-century philosophers and scientists, by contrast, followed the example of Plato. They limited 'rationality' to theoretical arguments that achieve a quasi-geometrical certainty or necessity: for them, theoretical physics was thus a field for rational study and debate, in a way that ethics and law are not. Instead of pursuing a concern for 'reasonable' procedures of all kinds, Descartes and his successors hoped eventually to bring all subjects into the ambit of some formal theory: as a result, being impressed only by formally valid demonstrations, they ended by changing the very language of Reason--notably, key words like 'reason', 'rational', and 'rationality'--in subtle but influential ways. (Toulmin, 1990, p. 20)

It is interesting to note that the 16th-century Renaissance humanists considered law, and not science, to be the model of a rational enterprise. As a practical undertaking, it honoured "...the significance of local diversity...", "...the rhetorical power of oral reasoning...", "...the relevance of particularity...", and the need for timely action (Toulmin, 1990, p. 34). A century later, the definition of rationality narrowed by the Cartesian and scientific revolutions, such practical pursuits as jurisprudence were impugned as irrational because they were 'contaminated by human error, myth, and superstition.' Indeed, one is hard pressed to understand how the limiting of human 'knowing' in the modern age to formally analyzable proofs can be considered anything other than a dogmatic and extreme narrowing of rationality.

However extreme Descartes's epistemology might now be considered to be, given the climate of the times in which Western Europeans were faced with a seemingly interminable war fueled by incommensurate religious dogmas, it seems understandable that many would flock *away* from uncertainty and skepticism, and *towards* another 'faith,'--a secular 'quest for certainty' which was considered as neutral between the competing religious creeds.

> The shift within philosophy, away from practical issues to an exclusive concern with the theoretical...was no quirk of Descartes. All the protagonists of modern philosophy promoted theory, devalued practice, and insisted equally on the need to find foundations for knowledge that were clear, distinct, and certain. Facing dogmatic claims by rival theologians, it was hard for onlookers of goodwill to restrict themselves to the cool modesty of an Erasmus or a Montaigne, who would have argued (with Pyrrho and Sextus) that it was a mistake for theologians to claim certainty on either side, and that human candor should lead us to admit that matters of faith are intellectually *un*provable and accordingly *un*certain. The protagonists in the religious wars had no stake in skepticism; nor would they call off their war for Lacedemonian reasons; living in a time of high theological passion, the only other thing thinking people could do was to look for a new way of establishing their central truths and ideas: one that was independent of, and neutral between, particular religious loyalties. (Toulmin, 1990, pp. 69-70)

In short, "...in the bloody theological deadlock of the Thirty Years' War, philosophical skepticism became *less*, and certainty *more*, attractive"; it offered a generation its only "...real hope of *reasoning* their way out of political and theological chaos..." (Toulmin, 1990, p. 71).

To recapitulate, when the scientific and philosophical revolutions of the 1630s are recontextualized by a consideration of the socio-political conditions of the times, a number of tenets of the traditional view of the beginnings of modernity must be rejected. First, these revolutions did not occur during times of economic prosperity, but rather during a period of depression and famine. Second, the modern age had dual origins: the decisive transposition from Church-dominated medieval culture to secular modern culture occurred not in the 17th century but in the 16th with the Renaissance humanist revolt against ecclesiastical and feudal authority. On this revised view, a world dominated by theology *first* yielded to a world committed to 'reasonableness,' and *then* to a world devoted to formal rationality. Third,

> the 17th-century philosophers' 'Quest for Certainty' was no mere proposal to construct abstract and timeless intellectual schemas, dreamed up as objects of pure, detached intellectual study. Instead, it was a timely response to a specific historical challenge--the political, social, and theological chaos embodied in the Thirty Years' War. (Toulmin, 1990, p. 70)

Thus the works of the 'new philosophers' were not the products of pure *ésprits* untouched by the historical events of their time; instead, they were the progeny of intellectuals desparate to reason their way out of a political and theological quagmire. Fourth, their work did not expand the scope of rational thought, but instead shrank it by repudiating the realm of the practical. Finally, scientific rationalism was adopted less because of its 'self-evident' logical consistency and rationality, and more because it provided an independent and neutral alternative to either one of the two irreconcilable religious dogmas.

On the whole, the received view downplayed the contributions of the Renaissance humanists to modernity, and considered their scholarship to be the products of late medieval thinkers who stopped short of taking "...the definitive step forward into the 'modern' world of logic and rationality" (Toulmin, 1990, p. 169). Next, we shall see how the scientific worldview was further entrenched as the dominant doxa of modernity because of the justification that it provided for an emerging socio-political structure which restored stability to the devastated European continent.

1650-1750: The Emergence of a New Cosmopolis

Aside from the political and economic breakdown in Europe between 1610 and 1650 which precipitated the epistemological revolution in which 16th-century Renaissance humanism was superseded by 17th-century rationalism, there was also a concurrent breakdown of public confidence in the accepted order of nature.

> The religious conflict triggered by the Reformation took place at just the same time when the traditional cosmology--the Sun and Planets moving around a stable, stationary Earth--at last came under sustained attack. (Toulmin, 1990, p. 82)

In fact, Ptolemaic cosmology had been undermined by Copernican astronomy a century before, but it was slow in succumbing: it underwent further challenges at the hand of Galileo at the beginning of the 17th century, and suffered its final death knell toward the end of the century with the release of Newton's *Mathematical Principles of Natural Philosophy*.

In Toulmin's (1990) opinion, the political catastrophe engendered by sectarian conflict, combined with the uncertainty generated by the revolution in cosmology, led to an overall *cosmopolitical* crisis which made the absolute certainty of formal rationality an attractive proposition to 17th-century intellectuals.

> This historical coincidence created an impression. The more acute the differences between Protestant and Catholic zealots, the more dogmatically they denounced one another, and the more urgently did cooler heads embrace the project for a 'rational' method to establish truths whose certainty was clear to reflective thinkers of any denomination. Meanwhile, the more vigorously Galileo advocated the new

> Copernican system--the Earth being just one more planet moving around the Sun--the more pressing was the need for a full renovation of natural philosophy. (Toulmin, 1990, pp. 82-83)

This simultaneous collapse of epistemology and cosmology, in which *everything* seemed to be threatened all at once, led the new philosophers to attempt to restore and underpin both of these fields with geometrical foundations: "Natural philosophy itself must be rebuilt on geometrical foundations, if the epistemological foundations of a new cosmology are to be guaranteed" (Toulmin, 1990, p. 83).

The long-awaited 'renovation of natural philosophy' was fashioned by the first giant of modern Science, Sir Isaac Newton, whose new, comprehensive system of dynamical theory and planetary astronomy (ca. 1687) at last answered the astronomical questions left by Copernicus, and finally garnered respect for the theretofore maligned 'Royal Society of London.' Newton continued the rational project of Descartes by combining a mathematical structure and an experiential basis to develop his system of cosmology and matter theory. Moreover, he perpetuated the rational dream of purifying the operations of human reason by: (a) decontextualizing these operations from the details of particular cultural or historical situations, and (b) achieving certain knowledge through the pursuit of a formal rational method, a unified science, and an exact language. Therefore, like Descartes's rational program--with its victory over Montaigne's modest skepticism--Newton's system--with its rejection of the more moderate empiricist methods of Bacon's naturalism--represented a triumph of the theoretical over the practical. Further, as with Descartes's revolution in epistemology, Newton's revolution in physics was not the product of a purely detached rationality, and its widespread acceptance rested on *political* as well as explanatory considerations (Toulmin, 1990).

As stated above in the introduction to this section, Newton's conduct of science must be considered as part of a larger 'life world' in which the desired ends of inquiry were more *theological* than practical. Newton's primary concern was that his theories demonstrate an "...intellectual coherence with a respectable picture of God's material creation, as obeying Divine laws"; that is to say, the "...scientific refinement of 'pure' ideas...was separated from concern with practical fruits...", such as technical advancements (Toulmin, 1990, p. 105). Thus, he was concerned with providing a comprehensive account of the workings of the world which bound things together in theological terms as much as in explanatory terms. This account hardly meshes with the traditional view of the 'new philosophers' as purely rational thinkers freed at last from the bonds of ecclesiastical constraints.

As well, the Enlightenment readers of Newton's *Principia* were more interested in the implications of his cosmology for issues related to political obligation and social structure than in their theoretical or practical fruitfulness.

> The confidence with which most people adopted this framework went far beyond the mathematical and experimental grounding that Cartesian or Newtonian physics had earned at the beginning of the 18th century. If we dig below the surface, the reception given to this picture of nature from 1700 on (like that given to the Quest for Certainty in the 1650s) rested on other, parallel *subtexts*, whose meaning had little to do with deducing mathematical theorems or explaining natural phenomena. (Toulmin, 1990, pp. 117-118)

Hence, there was much more at stake in the early 18th-century acceptance of the Newtonian worldview than the simple affirmation of a seemingly progressive explanation of physical and cosmological phenomena. We are instead dealing with the endorsement of an entire *cosmopolitical system* which bound the world together in political and, as mentioned above, theological, terms, as well as in scientific ones (Toulmin, 1990).

According to Toulmin (1990), there were two main principles which guided the reconstruction of European society and culture following the Peace of Westphalia: first, a commitment to *stability* in and among the sovereign (i.e.. free from Church control) nation-states which were beginning to emerge; and second, a dedication to *hierarchical* social structures within each state. This pattern of socio-political organization was accepted as the price to be paid for détente and stability in a Europe which, for over a generation, had known only the chaos engendered by doctrinal conflict. Nevertheless, the furtherance of such a socio-political system would be *more justifiable* if it could be shown that the 18th-century commitment to stability and hierarchy was, in fact, *consonant with the Divinely-ordained natural order of things*.

> For those who carried this task forward, it was important to believe that the principles of stability and hierarchy were found in all of the Divine plan, down from the astronomical cosmos to the individual family. Behind the inertness of matter, they saw in Nature, as in Society, that the actions of 'lower' things depended on, and were subordinate to, oversight and command by 'higher' creatures, and ultimately by the Creator. The more confident one was about 'subordination and authority' in Nature, the less anxious one need accordingly be about social inequalities. (Toulmin, 1990, p. 128)

The paradigm case of a modern, rational nation-state with a hierarchical social structure was the France of Louis XIV--the 'Sun

King' who reigned from 1643 to 1715. In fact, because Louis was only five years old when he ascended to the throne, his Prime Minister, Cardinal Richelieu, can be considered the more important architect in determining the structure that this first modern nation-state would take by building "...into its methods all of Descartes's deductive ideas" (Saul, 1992, p. 49). Nevertheless, it was Louis who eventually created the system of rungs descending from his throne of grandeur on which "...each aristocrat had a perch in declining order of visible magnificence" (Saul, 1992, p. 63). This is clearly reminiscent of the image of nature provided by Newtonian theory as "...a stable physical system of bodies moving in fixed orbits around a single, central source of power--the Sun and the planets as a model for the Sun King and his subjects" (Toulmin, 1990, p. 184).

To summarize: the widespread acceptance of the Newtonian worldview in the early 18th century among the educated of England and France signalled the definitive preeminence of formal rationality in philosophy, science, *and* sociopolitical organization:

> ...scientific theories and nation states alike were fully rational only if they formed stable 'systems': in one case logical systems *à la* Euclid, in the other institutional systems with determinate relations. (Toulmin, 1990, p. 183)

Moreover, on Toulmin's view, the rational revolutions in philosophy and science, with their attendant infrastructure: certainty, formal rationality (foundationalism), and the clean slate (decontextualization), were as much *socio-political* as scientific devices. This second, rationalist phase of modernity, with its comprehensive system of ideas about nature and humanity, triumphed in the West "...for the legitimacy it apparently gave to the political system of nation-states as much as for its power to explain the motions of planets, or the rise and fall of the tides" (Toulmin, 1990, p. 128).

At this point, it is quite clear that Toulmin (1990) takes a more radical approach than does Kuhn (1970) in explaining the acceptance of scientific theories, such as Descartes's epistemology or Newton's physics, by their respective intellectual communities. What was at stake in Descartes's or Newton's time went far beyond the 20th-century conception of scientific activity as 'explanatory': "in particular, the cosmopolitical function of the worldview counted for as much as its explanatory function, *and probably more*...[italics added]" (Toulmin, 1990, p. 132). Thus, on Toulmin's view, we can only give an accurate account of the acceptance of a theory as a provisional 'truth' by recontextualizing it into the milieu from which it emerged. When this is done with Descartes's and Newton's theories, it places the welcome they received "...squarely in the social and political framework of [their] time" (Toulmin, 1990, p. 132).

1750-1914: The Moderation of Rationalist Dogma

According to Toulmin (1990), from the 1720s until the mid-20th century most philosophers and natural scientists defended, at least in theory, the modern infrastructure of ideas, i.e. *certainty*, *formal rationality*, and *the clean slate*, as *the ideal* to be attained in all 'rational' fields of inquiry and action. Indeed, "the apotheosis of logic and formal rationality [had] struck deep roots..." in modern Western thought (Toulmin, 1990, p. 148). However, in this section we shall discover that in practice, the rationalist dream of foundationalism--a *unified* science or comprehensive system of sciences all using the scientific method as found in physics and forming a body of knowledge with formal, geometric structuring--proved to be just that, a dream which was appealing during the insecure times of the 17th century, but which gradually faded away as things became more stable. Moreover, we shall determine that most of the ontological and epistemological presuppositions first embodied in the Cartesian dichotomy, and later sanctioned in Newtonian mechanics, such as the inherent inertness of matter, had been disproved by the beginning of the 20th century.

Methodological Pluralism

After 1700, scientists extended the range of their subject matter beyond physics into the realms of chemical, biological, social, and psychological phenomena. However, as they did so they did *not* extend a common scientific method, i.e. that used in physics, into every new domain of study. Instead, as they attacked each new field, the first thing they did was to try to find out *how* to study it (Toulmin, 1990). What proved preeminently important in furthering the progress of science was not formal logic and rationality, but the pragmatic

> ...ability of scientists to move into fresh fields, and develop techniques for handling aspects of experience that were not previously in their reach, [which] rest[ed] on their capacity to renegotiate (so to say) the relations between different branches of scientific theory, so as to meet the novel demands of each new field. (Toulmin, 1990, p. 183)

Thus, the reality of science became, by the early 20th century, one of '*methodological plurality*' in which every science employed "...those specific methods that have proved, in concrete experience, to match the characteristic demands of its own intellectual problems" (Toulmin, 1990,

p. 193); rather than one of 'unified science,' of "...formal systems based on abstract theoretical ideas alone, with a 'certainty' borrowed from geometry..." (p. 181), in which the totality of findings in all scientific fields of inquiry formed a comprehensive system. In sum, all the sciences, from physics to psychology, came to stand on their own, using research methods which were based on their own first-hand experience, and no longer relying on the 17th-century faith in a unified science (Toulmin, 1990).

As an illustration, the first 'science of man' which exemplified this shift *from* foundationalism *to* methods deeply grounded in experience was *sociology*--that field of inquiry which was initiated by Auguste Comte in the mid-19th century. Initially, sociological inquiry, as envisioned by Comte (1947/1842), was to involve the use of the scientific method as employed in those fields of knowledge which had, in his opinion, already reached a positive state, such as astronomical science, terrestrial physics, chemistry, and physiology. On this view, once these methods were properly applied to complicated social phenomena, the result would be the discovery of invariable natural laws, which would then serve to complete the 'positive philosophy,' Comte's comprehensive system of general ideas in which the different sciences are not separate, but are instead the branches of one, homogeneous stream of positive doctrine dealing with all forms of physical and social phenomena.

It was not long before Comte's foundationalism and 'quest for certainty,' which aped that of Descartes, was to be considered untenable. Later in the 19th century Emile Durkheim (1938/1895) used scientific methods to study 'social facts' as observable, quantifiable 'things.' This quickly led him to conclude that Comte's 'positive philosophy' was little more than a metaphysical doctrine because the direct experimentation common in such disciplines as physics and chemistry was unsuitable for sociological inquiry. This method was ineffectual due to (a) the impossibility of artificially producing, by experimenter manipulation, social facts to prove that one phenomenon is the cause of another; and (b) the great complexity of social phenomena which makes control difficult because the elimination of all causes, save one, is a next to impossible ideal. Consequently, Durkheim embraced the weaker scientific method of 'concomitant variation'--which produced only measures of correlation between social facts and not causal laws--as the research tool of sociology. Although Durkheim (1938/1895) recognized that the requirement of using such a method--a condition imposed by the nature of the social phenomena studied--could be construed as a weakness by scientists working within other fields because of the inherent *uncertainty* of such results, he considered it an inconvenience which was easily "...compensated by the wealth of variations at the disposal of the sociologist, of which we find no example in the other realms of nature" (p. 134).

The trend in sociology which saw scientific practitioners in this discipline move away from the foundationalism of Comte towards methods more grounded in the actual experience of social phenomena led to the eventual adoption of not only correlational research methods, but also modes of inquiry which were rooted in hermeneutic-phenomenological philosophy. Leading this movement was Weber (1978/1922), whose 'interpretative sociology' synthesized scientific and hermeneutic methods of inquiry. On this view, it is not sufficient to give a causal explanation of the outward course of a social action, one must also interpret the *meanings* that agents give to these actions in order to properly understand and explain them. This is so because the behaviour of individuals involved in social systems can be understood in a way that the behaviour of physical phenomena cannot: that is to say, in terms of *meanings and intentions* rather than simply in terms of *mechanical functioning*. Like Durkheim, Weber recognized that the results of such methods of inquiry, when scrutinized by an interlocutor, would be perceived as speculative and incomplete; however, this uncertainty was seen as the inescapable result of efforts to preserve what was distinctive about sociological phenomena.

In sum, within a mere 75 years of Comte's adjuration to apply the formal methods of disciplines such as physics and chemistry to all manner of phenomena in order to produce a unified body of positive knowledge, experience had taught such sociological practitioners as Durkheim and Weber to adopt methods that more effectively matched the characteristic nature of social phenomena. Soon after, the development of inquiry in education followed a similar pattern to that found in sociology, beginning with the strict foundationalism of early behaviorists and educational psychologists, such as B.F. Skinner and E.L. Thorndike, who put their emphasis on the certainty of their results by stressing control through internal validity; continuing with more moderate quantitative researchers such as N.L. Gage who attempted more accurate representations of classroom phenomena by taking their experiments from the lab into the classroom, thereby stressing a balance between internal and ecological validity; and finally, ending with qualitative researchers such as Elliot Eisner who placed emphasis on providing more accurate accounts of the meaning structures actually present in classrooms rather than imposing general theoretical languages on such contexts from the outside.[9]

Rejecting the Presuppositions of The Modern Program

We saw in the previous sub-section that, on Toulmin's (1990) view, the natural (and social) sciences, between 1750 and 1914, moved a long way from the formal systematization of Descartes's 'natural philosophy,' which defined in an absolute way what counted as 'rationality' for a 75 year period after the release of his *Discourse on Method*. This moderation of rationalist dogma was largely achieved because practical-minded scientists heeded the evidence provided by actual experience of the distinctive nature of phenomena in different fields of inquiry and, subsequently, embraced the *reasonable* positions that (a) different sorts of phenomena required different methods of inquiry in order that science might progress, and (b) the progress within different disciplines would be diverse enough in form that the possibility of a science unified under Euclidian geometry would be precluded. In this way, modern science redeemed itself of its own accord in eschewing the foundationalism of the 17th century 'new philosophers.' However, as we shall presently see, other presuppositions of the scientific-

[9]Bredo & Feinberg (1982) refer to this practice of imposing abstract theoretical constructs on a practical context as a 'monological approach' to inquiry.

rationalistic worldview proved recalcitrant, and required definitive revolutions in thinking before they were replaced by more moderate views.

One such presupposition was the notion that matter was purely inert and stable, which was undercut in theoretical physics shortly after 1900 by Schrödinger's wave mechanics and Heisenberg's quantum mechanics. Another was that nature was governed by fixed laws set up at creation, which was undercut by a revolution in physics which, after more than two centuries, finally replaced Descartes's and Newton's worldviews (Toulmin, 1990).

> From 1890 to 1910, the physicists J.J. Thomson, Albert Einstein, and Max Planck broke the links between current physical theory and earlier Newtonian orthodoxy. The new physics so created--particles smaller than the lightest atom, space and time that lack sharp-edged distinctness, matter and energy that seemed interchangeable--undercut the last pretence that Euclidian geometry and Newtonian mechanics are certain, final, and indispensable to the rational understanding of Nature. (Toulmin, 1990, p. 151)

Descartes's search for a 'neutral scratch point,' which would clean the slate by cutting ourselves off from our inherited cultural framework of ideas and replacing it with the shared basic concepts of Euclidian geometry, proved to be untenable due to the fact that he had neglected to ask whether reflective thinkers in all cultures and epochs would describe space and spatial relations in ways that conformed to this ancient Greek geometry (Toulmin, 1990). Clearly the physicists of the late 19th and early 20th centuries did not; in fact, Einstein's theories were based more on 19th-century Riemannian geometry than on that of Euclid.

A third assumption originating with the Cartesian revolution in philosophy (and later developed by such British Empiricists as Locke and Hume) which was later rejected was that sensory evidence provided a second neutral scratch line for certain knowing. Since Descartes's time, scientific-rationalistic thinkers had been working to remove the human element from scientific observation because of the inherent danger of having one's irrational, emotional side on the one hand, or one's culturally-determined framework of ideas on the other, colour one's perceptions. As we shall discover in greater detail in Chapter Four when I discuss impediments to positive knowledge, work in ethnography concerning the diversity of colour terminology in different languages and cultures, along with work in psychology which showed that all perception is theory-laden, undercut this notion that passive sensory observation generates universal ideas of reflection (Toulmin, 1990).

Clearly, humankind cannot 'clean the slate' either by beginning with shared basic concepts such as those found in Euclidian geometry, or by beginning with neutral sensory evidence which produces shared concepts.

> The burden of proof has shifted; the dream of finding a scratch line, to serve as a starting point for any 'rational' philosophy, is unfulfillable. *There is no scratch*. The belief that, by cutting ourselves off from the inherited ideas of our culture, we can 'clean the slate' and make a fresh start, is as illusory as the hope for a comprehensive system of theory that is capable of giving us timeless certainty and coherence. (Toulmin, 1990, p. 178)

One late 19th and early 20th century philosophy which helped to bury this modern program was pragmatism (see James 1969/1907; Dewey, 1929, 1960/1929, 1965/1904, 1966/1916), a tradition which (a) rejected the notion that we can cut ourselves free of our conceptual inheritance, or the information, opinions and ideas available in a local situation; and (b) posited that instead all we can do is begin "...*from where we are, at the time we are there*..." and "...use our experience critically and discriminatingly, *refining and improving* our inherited ideas, and determining more exactly the limits to their scope" (Toulmin, 1990, p. 179). This pragmatic 'reasonableness' marked a return to the modest skepticism and practical philosophy of the 16th-century Renaissance humanists, as evidenced by its concern for the oral, the particular, the local, and the timely.

In summary, by the early 20th century, natural and social scientists had largely given up the dream of a unified science and had instead pursued their inquiries by independent methods which matched the nature of their respective objects of study. A Revolution in physics had at last undercut the centuries-old belief that Euclidean geometry and Newtonian cosmology were certain and final rational foundations for explaining nature. In philosophy, the advent of pragmatism marked the return of a conception of philosophy that embraced both the theoretical and the practical realms. Given these retreats from the strict rationality of the modern worldview introduced by the 17th century 'new philosophers,' "...the 1910s and 1920s might have seen a definitive demolition of the modern scaffolding" (Toulmin, 1990, p. 151). Instead, the return to another thirty years (1914-1945) of economic and political catastrophe in the West postponed the full reinstatement of a humanist worldview for almost fifty years.

1914-1950s: Catastrophe Revisited and a Renewed Quest for Certainty

Toulmin (1990), in keeping with his practice of examining the acceptance of revolutionary (or, in this case, *retrograde*) intellectual ideas not only in terms of their explanatory success but also in terms of the broader socio-political (or historical) context in which they are born, recounts how the return to Renaissance values which seemed imminent in 1910 was deferred by another thirty-year period of strife which echoed that of the 17th century. In 1910, the times were ripe--more so than at any time since 1610--for a renewed toleration of diversity, ambiguity, and uncertainty. This movement was, however, deferred by a return to formal rigour, exactitude, and foundationalism which was precipitated, *once again*, by simultaneous political, economic and cosmological upheavals in the West.

The entire European system of nation-states was dragged, in 1914, into a regional conflict by a structure of treaties and alliances which was, ironically, designed to ensure the security of all. The war ended, in 1918, with an inequitable peace which, along with the general economic collapse of 1929, doomed the nascent democratic government of the main aggressor, Germany. This state's aggression was rejuvenated, a generation later, by the fascist Nazi Party, which seized power in 1933, unleashing an imperialist version of nationalism which, in turn, triggered a second World War from 1939-1945. Thus, thirty years of slaughter in the name of nationhood came almost exactly 300 years after the thirty years of slaughter in the name of religion which had helped to set up the system of nation-states in the first place (Toulmin, 1990).

At the same time that this traditional system of European states was in dispute, the received cosmology was being discredited: Einsteinian relativism was replacing Newtonian certainty (Toulmin, 1990). The result of these concurrent cosmo-political upheavals was that "by the late 1930s, the political and cultural situation in Western Europe was little better than in the 1630s: the 'middle of the road' was nearly as empty as it had been after the murder of Henri IV" (Toulmin, 1990, p. 158). Rather than finally renouncing philosophical foundationalism and the 'quest for certainty,' and accepting the pluralism that had arisen in the sciences, European and American intellectuals instead sought safety in a *second* crusade "...for a permanent and unique set of authoritative principles in human knowledge..." (Toulmin, 1990, p. 174).

Leading this crusade were the Vienna Circle philosophers of the 1920s and 1930s, who in "...response to the disaster of the First World War chose not to move in a humanistic direction but rather to return to formalism" through a philosophy of science known as 'logical positivism' (Toulmin, 1990, p. 153). Toulmin (1990) points out that this view of science was dominated by rationalism and shared all of the assumptions of the 17th century 'new philosophers': that is, it sought to *reformulate* the issues of natural science and philosophy (the 'clean slate') in *abstract*, *universal* terms (the 'quest for certainty'), and was dedicated to the building of a *unified* science (foundationalism or systematicity).

There were, however, two major differences between the two main formalist movements of the modern age: First, whereas Descartes based his foundationalism on Euclid's geometry, an 'ideal rational system with no successor,' the logical positivists built their system of unified science around the symbolic logic of Whitehead's and Russell's (1970/1910) *Principia Mathematica, another* 'self-validating system of knowledge with no plausible heir.' Second, the logical positivists' formalism was not of the same magnitude as that of the 17th-century rationalists:

> As developed in the 1920s and 1930s, the myth of modernity and the dream of a fresh start did not replicate the 17th-century rationalist research program perfectly; nor did they reaffirm without change the model of formal exactitude that underlay 17th-century natural philosophy. Rather, the ideas of strict 'rationality' modeled on formal logic, and of a universal 'method' for developing new ideas in any field of natural science, were adopted in the 1920s and 1930s with *even greater* enthusiasm, and in an *even more extreme* form, than had been the case in the mid-17th century....The Vienna Circle program was, thus, even more formal, exact, and rigorous than those of Descartes or Leibniz. Freed from all irrelevant representation, content, and emotion, the mid-20th-century *avant garde* trumped the 17th-century rationalists in spades. (Toulmin, 1990, p. 159)

Thus, the effect of this dogmatic extreme was a return to a monopolistic position in science: theoretical physics had the most in common with pure mathematics (or the *formal logic* to which it had been reduced in the *Principia* of Whitehead and Russell); therefore, its method was chosen as foundational. The effects of this program were so overwhelming that by the 1950s "...many scientists and philosophers of science still conceded the imperial claim of physical theory to impose its explanatory patterns on all branches of science" (Toulmin, 1990, p. 164). Indeed, it took until the mid-1970s before patterns of explanation of psychological and educational phenomena based on the most formal of the natural sciences (e.g., physics and chemistry) began to give way to new paradigms based on the first-hand experience of inquirers working within these fields (see Cronbach, 1975; Gage, 1978; Eisner, 1977).

After World War I, then, Europeans felt the need to clean the slate and pursue their own quest for certainty. To support this program, they developed the inaccurate, impoverishing account of modernity refuted by both Kuhn (1970) and Toulmin (1990). As we saw above, this erroneous version shoved the reasonableness of the 16th-century humanists aside, and thereby supported the logical positivist investment in rationalism. In so doing, it devalued the legacy of the 16th century, a time of "...greater prosperity and maturer humanism", and misrepresented the true nature of the 17th century--a time of economic stagnation, *not* prosperity; religious intolerance, *not* freedom; and ideological slaughter, *not* cool-headed rationality (Toulmin, 1990, p. 170). Thus the received view of modernity and its origins--like the rationalist approach to solving intellectual and practical problems by 'cleaning the slate' of any information or ideas grounded in the context of the situation and beginning from a 'neutral scratch point' (i.e., either formal logic or passive perception, or both)--was a rationalist construction which decontextualized the histories of philosophy and science by abstracting accounts of their development from their historical circumstances.

In summary, the moderation of rationalist dogma that occurred between 1750 and 1910--which largely restored Aristotle's wisdom regarding the need to match methods to problems and to consider both the theoretical and the practical realms--was set aside by a return to formalism and foundationalism inaugurated by the logical positivists of the 1920s and 30s, who embraced the Platonist demand for a single, universal method of knowing. In Toulmin's (1990) account, this sudden 'reversion' to a narrow formal rationality was more a reaction to a cosmopolitical crisis (i.e., thirty years of economic strife and war occuring concurrently with a succession in cosmology from Newtonian to Einsteinian theory) similar to that which befell the 17th century, than a case of a more strict and therefore more progressive view of rationality replacing another less rigorous and therefore less legitimate version. However, this second 'quest for certainty' through formal rationality[10] was to last only one generation, whereas that of the 17th century 'new philosophers' influenced the majority of intellectuals for a period of almost two centuries. In the next section I shall examine the second moderation of rationalist dogma to occur in the modern age; that is, a second, and this time successful, attempt at *Re-Renaissance* (Toulmin, 1990).

1950s Onward: Re-Renaissance--The Third Phase of Modernity

Toulmin (1990) considers foundationalism to be a dream "...which has its appeal in moments of intellectual crisis, but fades away when matters are viewed under a calmer and clearer light" (p. 174). As mentioned above, the foundationalism of the logical positivist program began to wane only ten years after 1945, whereas that of the 17th century rationalists faded more gradually over a couple of centuries. Toulmin (1990) views this 20th-century re-awakening of strict formalism

[10]Or third overall if the first, metaphysical, 'quest for certainty' is included in this count.

as a brief period of regression into obsolete, and largely refuted, modes of thought caused by another thirty years of calamity in Europe--a relapse which could not long withstand the inexorable movement in philosophy and science to reinstate "...the humane and liberal standpoint of the late Renaissance" (Toulmin, 1990, p. 161).

By the late 1950s, the times were at last ripe for a resurgence of humanist philosophy, and a concomitant abatement of rationalist beliefs in (a) the possibility of a certain, neutral scratch point--an escape from philosophical or common sense traditions--through either formal logic or passive perception; (b) the possibility of a systematized body of scientific knowledge organized under a foundation such as Euclidean geometry or symbolic logic; and (c) the necessity of cleaning the slate or decontextualizing situations from their practical, historical realities (Toulmin, 1990). The third phase of modernity, the long-awaited 'Re-Renaissance,' would (a) witness the renunciation of this rationalist attempt to ground *all* our knowledge in universal, abstract, timeless systems (i.e., a doctrine of epistemological monism); (b) behold the degeneration of rationalist doctrines from the status of 'self-justifying, tradition-free truths' to one of 'metaphysical beliefs' (or, sometimes, 'myths') which, in many ways, do not mesh with experience[11]; (c) disparage ***formal, theoretical rationality*** as narrow

[11]This turn of events is ironic given that the 17th-century rationalist revolution was largely a movement to replace metaphysical speculations with a certain, rational basis for knowledge. Since this time, it has come to be widely accepted that no system of thought, including empirical science, is universal and self-justifying. Rather, all are simply belief systems or historical traditions of thought which, to a certain point, are upheld for ***sound reasons*** (i.e., they largely mesh with experiential reality), but which, beyond this, must be largely accepted as articles of faith (see Kuhn, 1970; Brown, 1977).

and often harmful when applied to complex human problems; and (d) embrace *practical 'reasonableness'* as a more expansive form of rationality which honours both the need for systematic thinking and for timely action which takes contextual variables into account.

The tide toward Re-Renaissance was led by a new generation of philosophers, including Toulmin (1982) and Kuhn (1970), who were experienced in natural science rather than in mathematics or symbolic logic. As we saw above, their approach to the analysis of scientific activity and progress was open to historical, contextual issues and not just logical concerns. By the 1960s, this 'new philosophy of science' had shown that standards of progress in science are variables which are subject to paradigm shifts, and that such shifts involved processes of practical rationality (e.g., human creativity, intuition, and judgment) which were historically conditioned; i.e., they involved "...mak[ing] the best of starting with what we have got, here and now" (Toulmin, 1990, p. 179). In other words, theoretical systems such as Euclidean geometry or Newtonian physics are not universal, timeless axiomatic systems, but are instead the creations of *a phase of science* (on Kuhn's [1970] view), or, to recontextualize further, *a given age* (on Toulmin's [1990] view). Further, the practice of science does not involve the use of a single explanatory method applied in all disciplines at all times, but a plurality of methods "...each of which is limited in scope and lifetime" (Toulmin, 1990, p. 84). Finally, the logical positivist practice of formal analysis of the logical structure of theories was superseded by the 'postpositivist' practice of historical analysis of (a) a plurality of theoretical languages, made up of variable concepts, and (b) the scientific, political and economic criteria which contributed to their acceptance in different sciences at different times (Toulmin, 1990).

This 'new philosophy of science' de-emphasized the abstract, timeless, universal, theoretical issues embodied in the logical positivist commitments to (a) certainty through 'passive' perception of 'stable' phenomena using the method of the 'master' science (physics), (b) systematicity through symbolic logic, and (c) decontextualization through abstraction; and accentuated the concrete, particular, timely, and rhetorical (or persuasive) issues involved in the actual practice of science and its historical development through non-cumulative sequences of largely incommensurable paradigms (or *uncertain*, *ephemeral*, *diverse* patterns of explanation of partially *constructed* realities). Moreover, this revolution in the history and philosophy of science, with its acceptance of methodological and theoretical diversity, is considered by Toulmin (1990) to constitute the 're-humanization of science,' in which

> the task is not to build new, more comprehensive systems of theory with universal and timeless relevance, but to limit the scope of even the best-framed theories, [to] fight the intellectual reductionism that became entrenched during the ascendency of rationalism, ...and [to] reinstate respect for the pragmatic methods appropriate in dealing with concrete human problems. (p. 193)

In short, Aristotle's supplications to match methods of inquiry to objects of inquiry and to embrace both theoretical grasp and practical wisdom had, for the first time since 1610, been given due consideration by philosophers and scientists. The seductions of "...abstract neatness and theoretical simplicity..." were at last giving way to an awareness of "...the unavoidable complexities of concrete human experience" (Toulmin, 1990, p. 201).

Thus, in contemporary times, all of the philosophical 'changes of mind' which occurred in the 17th-century rationalist revolution were, at long last, reversed. The third phase of modernity can be considered a period of 'intellectual expansionism' because it has

> ...redirected attention to the practical, local, transitory, and *context bound* issues that were close to the heart of 16th-century humanists, but were set aside by 17th-century rationalists for abstract, timeless, universal, and *context free* issues. In our day, formal calculative rationality can no longer be the only measure of intellectual adequacy: one must also evaluate all practical matters by their human 'reasonableness.' (Toulmin, 1990, p. 184-5)

In other words, this phase of modernity has broadened the concept of 'rationality' by once again *keeping in mind* both the theoretical *and* the practical realms. The Aristotelian, and, later, the 16th-century humanist, requirement to balance the legitimate claims of *episteme* and *phronesis*, the concrete and the abstract, the particular and the universal, the oral and the written, and the timely and the timeless, in human rationality, has finally been re-confirmed.

Indeed, as Toulmin (1990) points out, the last 20 years in philosophy have witnessed a revival of interest in (a) the concrete study of case ethics (as opposed to abstract moral theorizing); (b) local 'traditions' or 'forms of life' which are best studied using historical or anthropological methods (as opposed to the search for universal foundations for knowledge); (c) questions about rhetoric and discourse, e.g., by the analytical philosophers in Britain and the U.S.A. (as opposed to questions of formal logic); and (d) problems whose rational significance is dependent on the timeliness of solutions, such as those found in medical ethics (as opposed to problems whose significance is eternal). Toulmin (1990) even suggests that this concentration on practical issues is not just 'applied' philosophy, but *philosophy itself*--"...the 'legitimate heir' of the purely theoretical enterprise that used to be called philosophy..." (p. 190).

This movement toward acceptance of the wider notion of rationality as 'reasonableness,' has also made an appearance in fields other than science and philosophy. Many theorists working within such diverse fields as business administration, professional education, curriculum theory, and political theory have, like Kuhn and Toulmin, recently criticized the modern hegemony of formal rationality as an overly narrow conception of human knowing which is unresponsive to the characteristic uncertainty, ambiguity, and value-ladenness of human problem-fields. For example, Schwab (1978c/1970, 1978d/1971, 1978e/1973, 1983) and Reid (1978, 1979) criticize theoretic approaches to curriculum planning and recommend that practical rationality replace such methods. Schön (1983, 1987, 1989) rebukes the ascendency of technical rationality in professional practice and education, and works to displace it with a wider, more pragmatic epistemology of practice. Nohria and Berkley (1994) warn against 'flavor of the month' business administration through the adoption of a never-ending succession of 'scientific management innovations,' and, like Schön, recommend more pragmatic practices. Finally, Saul (1992) laments the 'dictatorship of reason' in the West (i.e., the modern control of populations by scientific-minded technocrats who manipulate bureaucratic structures to suit their own ends) and encourages a return to more prudent and democratic standards of 'reasonableness.'

What all of these 'third-phase' theorists have realized is that formal rationality is not always *reasonable*: that is, in certain cases--and perhaps *all* cases beyond a certain point--it is no longer reasonable to "...appeal to 'systematic' rules and 'rational' demonstrations" in seeking solutions to human problems (Toulmin, 1990, p. 219).

> Rationally adequate thought or action cannot, in all cases equally, start by cleaning the slate, and building up a formal system: in practice, the rigor of theory is useful only up to a certain point, and in certain circumstances. Claims to certainty, for instance, are at home *within* abstract theories, and so open to consensus; but all abstraction involves *omission*, turning a blind eye to elements in experience that do not lie within the scope of the given theory, and so guaranteeing the rigor of its formal implications[12]....Once we move outside the theory's formal scope, and ask questions about its relevance to the external demands of practice, however, we enter into a realm of legitimate uncertainty, ambiguity, and disagreement. (Toulmin, 1990, p. 200)

For instance, Schön (1983, 1989) mentions a 'crisis of public confidence' in professionals who apply 'formal, technical (i.e., abstract, timeless, universal and context-free) rationality' to the often uncertain, unstable, unique or value-laden practical problems they deal with, which, consequently, leads them to 'cut' practical situations to fit their theoretical frameworks. In education, this process has

[12]Wick (1976) makes an analogous point when he states that a theoretical problem-solution may be considered 'good in itself' but not 'good on the whole' due to the inevitable partiality of all rational abstractions. For example, cooperative learning may be 'good in itself' in that it fosters development of pro-social behaviours in students, but not 'good on the whole' because of the opportunity costs of emphasizing group work at the expense of other legitimate educational aims such as the individual, disciplined study of texts. Therefore, to put forth cooperative learning as a theoretical panacea for whatever ails education would be to make an entirely *unreasonable* 'rational' claim (see Toulmin, 1990).

often caused students who do not fit stereotypical patterns of 'intelligence' or 'good behaviour,' to be labelled as 'slow learners' or 'behaviourally disordered' persons, *and thus marginalized*, largely because they 'resisted' the neat, abstract categories of a 'rational' practitioner.

Hence in its third, humanist phase the intellectual scaffolding of modernity has been demolished, "...and modernity has at last *come of age*" (Toulmin, 1990, p. 172). In fields as diverse as science, philosophy, business studies and education we have returned "...from a theory-centred conception, dominated by a concern for *stability* and *rigor*, to a renewed acceptance of practice, which requires us to *adapt* action to the special demands of particular occasions" (Toulmin, 1990, p. 192).

At no time in the history of humankind has it been more crucial that we adopt "...the 16th-century commitment to intellectual modesty, uncertainty, and toleration...", especially since modern political and bureaucratic structures are rapidly becoming obsolete due to world-wide trends towards globalization and interdependence which are hastened by technological advancements (which are, in turn, the products of modern scientific rationality) (Toulmin, 1990, p. 174). These trends have (a) resulted in a mixture of cultures, ideas, and worldviews on a level unprecedented in human history; (b) brought unparalleled levels of competition for increasingly scarce resources and for the control of expanding (and sometimes contracting) world markets; (c) caused universal problems such as the ethical issues associated with new medical technologies, the threat of nuclear war (or terrorism), and environmental degradation (Toulmin, 1990). Clearly, the application of scientific-rationalistic modes of thought to human problems has brought great advances, but it has also resulted in a long list of deleterious side-effects. Ironically, it is now becoming apparent that these issues can be addressed effectively only by operating on both an abstract, theoretical level *and* a local, practical level: we must encourage humanity to *'think globally (generally), but act locally.'* In contemporary times, effective problem solving involves *less* the application of systematized, standardized, 'certain' solutions, constructed out of context, and supposedly imposing stability from 'on high[13],' and *more* a process of local deliberation which takes into account both rational thought and the idiosyncracies of a human milieu, thereby leading to functional, adaptable solutions which are good *for now in this particular situation* (Toulmin, 1990).

Conclusion

Together, Kuhn (1970) and Toulmin (1982, 1990) have demonstrated that the traditional histories of science and modernity, influenced by the logical positivists of the 1920s and 30s, are rational constructions--indeed, *fabrications*--which have decontextualized the products of scientific activity by analyzing them in a logical fashion, and apart from their historical milieux of discovery, recognition, and articulation. These accounts have been, on the whole, triumphant, portraying the emergence of scientific rationalism in the 17th century

[13]Cf. Schwab (1983), who comments on the common practice within educational bureaucracies of transporting theoretical problem-solutions from 'Moscow to the provinces.'

as a movement which (a) occurred during times of economic prosperity and religious freedom; (b) emerged as the internal logical products of rational *ésprits*; and (c) supplanted the irrational, theological traditions of medieval times. The progress of science from the 17th century onwards was seen, on this view, as the incremental accumulation of certain laws describing relations between stable natural phenomena.

Kuhn (1970) secured a paradigm shift in the history and philosophy of science by replacing the logical positivist emphasis on the logical analysis of 'finished' scientific achievements with an historiographical study of the actual practice of science within communities of inquirers. This constituted a first level of recontextualization of scientific theories in that it examined the import that theoretical frameworks had for particular communities at specific stages of their historical development. This account also refuted three main tenets of the positivist worldview: (a) that the knowledge obtained by the application of the empirical method was *objective*, and therefore *certain*; (b) that the progress of science was cumulative; and (c) that the scientific method as employed in physics or chemistry could, and would, be applied uniformly and fruitfully to all manner of phenomena, natural and human.

The first doctrine was undercut by Kuhn's (1970) assertion that observations are not passive, but are instead theory-laden; therefore, scientific facts are partially constructed by interactions between metaphysical subjects and the phenomenological world. Consequently, the logical positivist conviction that the discovery of rational methods during the 17th century had replaced speculative, indeterminate, traditional grounds for knowledge with a positive, universal, self-justifying foundation is, itself, nothing more than a metaphysical *doxa*--a belief in a traditional, culture-bound tenet which it is self-delusional to presume is omnipresent in the world. The second precept was undermined by the revelation that the progress of science is not wholly aggregational, but involves processes of cumulative articulation of *provisional* theories during periods of normal science interspersed with non-cumulative, revolutionary episodes during which accepted patterns of explanation are razed and replaced by other, presumably more fruitful, configurations. The final maxim was subverted by the discovery that during periods of revolutionary science it is not only theoretical patterns which are supplanted, but also methodological designs, which are amended to facilitate the efficient gathering of facts which more accurately represent empirical reality.

Toulmin (1982, 1990) is in broad agreement with Kuhn's accounts of (a) the conduct of science within communities of inquirers, and (b) the progress of science as an historical sequence of largely incommensurable patterns of explanation. However, the later Toulmin considers Kuhn's work to be incomplete as it overlooked the possibility that the wider socio-political milieu might substantially influence (a) the lines of inquiry that individual scientists pursue in the process of theoretical innovation, and (b) the subsequent acceptance or rejection of novel frameworks by the communities within which they work.

Furthermore, when Toulmin (1990) examined histories of the 16th and 17th centuries *other than those provided by early 20th-century historians of science* he discovered that the latter's accounts of the beginnings of modernity were somewhat inaccurate. First, the decisive break with medieval modes of thought came not with the 17th-century rational revolution, but with the 16th-century Renaissance humanist revolt against ecclesiastical and feudal constraints. Therefore, on Toulmin's view, modernity had dual origins: first, in 16th-century humanism with its reverence for both the theoretic and the practical realms; and second, in the 17th-century rational revolution which entrenched certainty, systematicity and 'the clean slate' as the dominant *doxai* of the era. Second, the 17th century was *not* a time of political stability, economic prosperity and religious freedom which was instrumental in facilitating the modern intellectual revolution which eschewed medieval superstition and widened human rationality, but rather a time of political chaos, economic downturn, and religious dogmatism which caused the renunciation of modest Renaissance skepticism and thereby *narrowed* human rationality. Third, the 17th-century progenitors of the modern worldview, such as Descartes and Newton, were not wholly detached, rational thinkers, but were instead individuals living in catastrophic times--caused by simultaneous upheavals in politics and cosmology--who tried to *formally reason* their way out of uncertainty. Fourth, Descartes's and Newton's program of formal rationality built around a structure of mathematical physics enjoyed widespread acceptance in the 17th century *less* for its explanatory fruitfulness, and *more* for the justification that it lent to the emerging socio-political system of nation-states made up of hierarchical social classes.

Toulmin (1990) goes on to illustrate how strict, theoretical rationality is attractive during times of great insecurity, but eventually loses support during times of increased stability. Hence, the rationalist paradigm emerged as the dominant intellectual tradition of modernity, enjoying widespread support and acceptance by philosophers and scientists following two thirty-year periods of political and cosmological upheaval in Western Europe (i.e., [a] 1618-1648: the Thirty Years' War and the emergence of Newtonian physics; and [b] 1914-1945: World Wars I and II and the appearance of Einsteinian physics). In the years following both of these catastrophes, the modern scaffolding gradually yielded to more humanistic versions of philosophy (i.e., as embodied in a renewed interest in both the theoretic *and* the practical realms) and science (i.e., as evidenced by the emergence of modes of inquiry based in experience) as the socio-political climate of the West improved. Thus, in the West a Medieval world dominated by metaphysical speculation yielded to a Renaissance world characterized by moderate skepticism (phase one of modernity: 1570-1610), which in turn acquiesced to an Enlightenment world dominated by formal rationality (phase two of modernity: 1610-1950). This attachment to formal rationality gradually faded until, in the late 19th century, the Western world was on the verge of a return to the moderate skepticism of the 16th century. However, this 'Re-Renaissance' was deferred for some fifty years by the cataclysms of 1914-1945. Only recently have the lost humanist topics--sidetracked by Descartes's and Galileo's revolutions in the 1630s--re-emerged in the intellectual tradition of the West (phase three of modernity: 1950s onward).

To conclude, Toulmin (1990) indicates that there are 'twin trajectories of modernity' which have traced out quite different paths. The first doctrinal, or ***metaphysical***, trajectory outlines the direction that formal doctrines underpinning human thought and practice have taken since 1600.[14] These have followed an Omega-shaped trajectory: after 300 years of strict rationality we have returned close to our *humanist* starting point. The second, scientific or experiential trajectory is very different: modern natural philosophy has established its roots firmly in experience, leading to a plurality of theories and methodologies which indicate a progression toward ever more fruitful and accurate representations of the phenomena found in an ever-expanding number of fields.[15] In Chapter Three, I will reexamine the main presuppositions of the modern worldview; specifically, I will survey the forms that these *ā priori* assumptions have assumed in four traditions: 17th-century rationalism, and three 20th-century philosophies of science (logical positivism, logical empiricism, and falsificationism).

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[14]In referring to this trajectory as a *metaphysical* one, Toulmin has affirmed the notion that scientific rationalism is simply the dominant *faith* of Modernity. Undoubtedly, it is a more technically progressive creed than Christianity or Idealism, but it is, nonetheless, a leap of faith which must be taken, in the final analysis, on uncertain, unverifiable grounds. The ability to transcend human experience and observe nature objectively, through the eyes of God, is beyond the limits of human rationality (Eisner, 1988).

[15]Toulmin (1990, p. 167) refers to the progress of science as *cumulative* and *continuous*. I find his choice of words here unfortunate given that Kuhn's (1970) analysis of science--which Toulmin affirms--went to great pains to demonstrate that this progress was not, in fact, cumulative. I will examine the postpositivist view of scientific progress more closely in Chapter Four.

CHAPTER THREE

THE SCIENTIFIC-RATIONALISTIC WORLDVIEW

> The correct method in philosophy would really be the following: to say nothing except what can be said, i.e. propositions of natural science--i.e. something that has nothing to do with philosophy--and then, whenever someone else wanted to say something metaphysical, to demonstrate to him that he had failed to give a meaning to certain signs in his propositions....What can be said at all can be said clearly, and what we cannot talk about we must pass over in silence.
>
> L. Wittgenstein, Tractatus Logico-Philosophicus

Introduction

In Chapter Two, I demonstrated, with Toulmin's (1990), Kuhn's (1970), and Brown's (1977) assistance, that the dominant presuppositional scaffolding of modernity has included commitments, among philosophers, scientists, and empirical researchers in other fields, to *certainty* through formal rationality and the empirical method, to the foundationalism of mathematical physics, and to the practice of cleaning the slate when dealing with intellectual and practical problems. The purpose of the present chapter is two-fold: first, to outline the various patterns that scientific rationalism has assumed in modernity: i.e., 17th-century rationalism, logical positivism, logical empiricism, and falsificationism; and second, to demonstrate how logical empiricists and falsificationists were forced by logical inconsistency and experience to moderate some of the dogmas of the modern scaffolding: i.e., that *direct* passive perception provides a *certain* means of *verifying* scientific propositions, and that knowledge can be organized in terms of *pure* deductive relations between higher-order theories and lower-order theories, between lower-order theories and hypotheses, and between hypotheses and observation statements.

I will begin by outlining two common approaches taken by 17th-century rationalists and 20th-century logical positivists in attempting to acquire indubitable knowledge of the phenomenological world, i.e., formalism and direct, passive perception. Next, I will show how logical empiricists accepted and dealt with the following realities: (a) that many phenomena cannot be directly observed, only indirectly tested; (b) that empirical observations can never provide certain verification of scientific hypotheses, only varying levels of confirmation; (c) that not all regularities can be expressed in general or deterministic laws; and finally (d) that not all confirmed hypotheses or theories can be *exactly* explained by, or subsumed under, higher-level theories. Following that, I will discuss falsificationism, which posits that scientific propositions are incapable of being verified--or even highly confirmed--by experience: that is to say, they can only be corroborated *for the time being*, i.e. until they are falsified by experience, or replaced by theoretical innovations. In the end, I shall conclude that although the logical positivist worldview has been seriously tempered by logical empiricism and falsificationism, all three traditions have maintained, in loose form at least, the major tenets of the modern worldview.

The Path To *Episteme*

Dewey (1960/1929), in discussing, *and rejecting*, both the philosophical and the scientific 'quests for certainty' through either metaphysical speculation, rational abstraction, or empirical observation, mentions two conditions which are, on these views, essential to the attainment of 'certain' knowledge (*episteme*). The first is that there must be direct correspondence between 'true' knowledge and what is real: "What is known, what is true for cognition, is what is real in being" (Dewey, 1960/1929, p. 21). James (1969/1907), who, like Dewey, rejects both strict rationalism on the one hand, and extreme empiricism on the other, makes an analogous point: "the popular notion is that a true idea must copy its reality" (p. 132). The second condition, an unexpressed premise of the first, is the Cartesian belief that the objects of physical nature are composed of inert matter (Toulmin, 1990): i.e., "...only the completely fixed and unchanging can be real" (Dewey, 1960/1929, p. 21).

> For knowledge to be certain [it] must relate to that which has antecedent existence or essential being....[These things] are alone inherently the proper objects of knowledge and science. (Dewey, 1960/1929, p. 22)

In sum, on this view, "...truth means an essentially inert static relation", an agreement between the true idea and the *eternal, immutable* reality to which it refers (James, 1969/1907, p. 133).

Apart from the ontological assumption that the objects of inquiry are preexisting and static, and the epistemological presumption that they can be directly represented in formal, propositional knowledge claims, it must be mentioned that the modern worldview attributes a systematic, deterministic nature to both the natural and human elements of reality. In other words, the eternal phenomena of nature and humanity are arranged in systems of relations which are characterized by linear causality: therefore, "...an uncaused event is impossible" (Kalin, 1992), or, "...there are no effects without causes and no causes without effects" (Lincoln & Guba, 1985, p. 28). On this view, both natural phenomena *and* human persons or groups are

> ...treated as a system to be studied by noting relations between perturbations in the state of one of its elements and changes in the states of other elements. By observing associations between these changed states the researcher seeks to infer the 'laws' according to which the system operates. From this point of view there is no difference, at least in principle, between studying persons (or social phenomena) and studying natural phenomena. Each can be studied as a type of closed system. (Bredo & Feinberg, 1982, p. 6)

Thus, the scientific goals of explanation, prediction, and/or control of phenomena are "...based on the assumption that all behaviors and events are orderly and that they are effects that have discoverable causes" (Gay, 1992, p. 6). Indeed, the noted positivist Emile Durkheim (1938/1895) considered the description of social phenomena in terms of cause-effect relationships to be an indication that a field of inquiry is 'rational.'

> ...The only designation we can accept is that of 'rationalist.' As a matter of fact, our principal objective is to extend scientific rationalism to human behavior. It can be shown that behavior of the past, when analyzed, can be reduced to relationships of cause and effect. These relationships can then be transformed, by an equally logical operation, into rules of action for the future. (pp. xxxix-xl)

This view of reality is undoubtedly a residue of the Cartesian belief that God combined natural objects into stable, hierarchical systems governed by fixed laws set up at the creation (Toulmin, 1990); and it constitutes a second major ontological presupposition of the modern worldview.

Dewey (1960/1929) outlines two approaches which have traditionally been taken in trying to achieve exact similitude between knowledge representations and their unchanging, preexisting objects: Whereas some "...ascribe the ultimate test of knowledge to impressions passively received, forced upon us whether we will or no," others "...ascribe the guarantee of knowledge to synthetic activity of the intellect" (p. 22). The former method--empiricism or *a posteriorism*--is normally associated with science; the latter tactic--rationalism or *a priorism*--is a practice prevalent in both classical philosophy and modern scientific rationalism (although most modern thinkers would vehemently deny that their thought is metaphysical in any way, shape, or form). Regardless of the approach taken, what is essential is that the observing or inquiring mind not affect, not produce change in, not interact with, the object to be known. The object must remain "...unaffected by these acts; otherwise it would not be fixed and unchangeable", and ergo *could not* be represented as 'pure' knowledge (Dewey, 1960/1929, p. 23). This is the modern fact-theory (or fact-value) distinction, long considered an essential condition for 'objective' knowledge, which had its origin in the Cartesian dichotomy between rational human experience (which is prone to contamination by emotional attachments to metaphysical/religious traditions or illogical 'common sense' beliefs) and mechanical natural (and, later, human) phenomena.

Both of the scientific-rationalistic movements of modernity discussed so far (i.e., 17th-century rationalism and 20th-century logical positivism) have blended formalism with the empirical method in their search for a neutral starting point or 'scratch line' for certain knowledge (Toulmin, 1990). As mentioned above, Descartes combined the mathematical structure provided by Euclidean geometry with an experiential basis furnished by experimental methods of inquiry. Similarly, the logical positivists of this century fused symbolic logic with scientific observation to create a certain foundation for inquiry and knowledge. At this point, the discerning reader might wonder how modern theorists could consider Euclidean geometry or symbolic logic, the products of rational synthesis and *not* empirical observation, to be something other than metaphysical beliefs. I shall address this query below in the subsection to follow.

In addition to employing rationalist and empiricist methods to achieve a 'neutral scratch line,' both positivist movements (17th century and 20th century) supported 'decontextualizing' or 'abstracting' knowledge by maintaining "...the temporal and contextual independence of observations, so that what is true at one time and place may, under appropriate circumstances...also be true at another time and place" (Lincoln & Guba, 1985, p. 28). As well, they promoted the practice of 'cleaning the slate' by repudiating both *ā priori* (i.e., metaphysical, ethical, aesthetical, or religious) knowledge claims and 'common sense' knowledge claims, as nonsensical, mythical, meaningless, illogical beliefs due to the fact that they were neither formally meaningful (i.e., true by definition, by virtue of their internal logic, or *tautologous*), or empirically meaningful (i.e., true by virtue of scientific observation) (Bredo & Feinberg, 1982).

Nagel (1961), Durkheim (1938/1895), and Skinner (1982/1953) all discuss common sense knowledge, its 'dangers,' and its 'inadequacies.' Durkheim warns about the threat of common sense propositions colouring the perception of the untrained mind working to explain sociological phenomena.

> The impulses of common sense are so deeply ingrained in us that it is difficult to eradicate them from sociological discussion. When we consciously free our thoughts of them, they still mold our unconscious judgments; and against such error we have no defense. Only long and special training can teach us to avoid it. The reader must bear in mind that the ways of thinking to which he is most inclined are adverse, rather than favorable, to the scientific study of social phenomena; and he must consequently be on his guard against his first impressions. (Durkheim, 1938/1895, pp. xxxvii-xxxviii)

In similar fashion, Skinner (1982/1953) warns that common sense knowledge about human behaviour is not to be trusted:

> We all know thousands of facts about behaviour...But this familiarity is something of a disadvantage, for it means that we have probably jumped to conclusions which will not be supported by the cautious methods of science...We may show considerable skill in making plausible guesses about what our friends and acquaintances will do under various circumstances or what we ourselves will do. We may make plausible generalizations about the conduct of people in general. But very few of these will survive careful analysis. (p. 46)

Nagel (1961) refers to common sense knowledge as *'prescientific' techne* about the environment which, despite the fact that it can be 'organized' or 'classified,' does not constitute science because "...it is seldom accompanied by any explanation of why the facts are as alleged" (p. 3). For example, many societies have discovered the wheel and put it to practical use without knowing anything about frictional forces, or why goods loaded on wagons fitted with wheels are easier to move about than goods dragged on the ground. Furthermore, when 'common sense' does attempt to explain its facts, "...the explanations are frequently without critical tests of their relevance to the facts" (p. 4). Thus the mark of *scientific* inquiry is its quest "to explain, to establish some relation of dependence between propositions superficially unrelated, to exhibit systematically connections between apparently miscellaneous..." facts (p. 5). In spite of these differences between prescientific and scientific thinkers, they nevertheless share a common stimulus to inquiry: the desire "...to find reliable ways of foreseeing changes in [their] environment and, if possible, controlling them to [their] advantage" (Hempel, 1965, p. 333).

I have already discussed metaphysical speculations, the very real danger of imposing *ā priori* ideas and theories on reality, and the positivist opposition to them, in Chapter Two. All I will add to this is, first, an acknowledgement that both natural science and social science got their start by substituting ideas for things (Durkheim, 1938/1895); and second, a quote from Durkheim which warns against this *a priorism*.

> These *idola*, which are illusions that distort the real aspect of things, are nevertheless mistaken for the things themselves. Therefore the mind, encountering no resistance in this imaginary world and conscious of no restraint, gives itself up to boundless ambitions and comes to believe in the possibility of constructing, or rather reconstructing, the world, by virtue of its own resources exclusively and at the whim of its desires. (Durkheim, 1938/1895, p. 17)

I now turn to an investigation of what, on the positivist view, distinguishes 'pure rational systems,' such as Euclidian geometry and symbolic logic, from these metaphysical '*idola*.'

Euclidian Geometry

The success of ancient Greek mathematicians, e.g., Pythagoras (ca. 525 B.C.) and Euclid (ca. 300 B.C.), "...can be attributed mainly to their development of two techniques: abstraction and generalization" (Asimov, 1972, p. 8); which, together, would later form the basis of the modern 'quests for certainty.' Euclid's attempt at rational synthesis began with a small number of geometric axioms so obvious, so self-evidently true, that it seemed they were beyond the need for proof; and ended with a system of spatial relations so refined that it has withstood over 2,000 years of scrutiny (Asimov, 1972). This "geometry seemed to reveal the possibility of a science which owed nothing to observation and sense beyond mere exemplification in figures or diagrams", and "...disclose[d] a world of ideal (or non-sensible) forms which were connected with one another by eternal and necessary relations which reason alone could trace"; in other words, it "...formed a complete system of immutable and necessary truth" (Dewey, 1960/1929, p. 16).

For Descartes, however, Euclidean geometry did not constitute a 'pure' mathematics alone, but also "...a science of spatial relations, dealing with Space *as encountered in experience*" (Toulmin, 1990, p. 171). Therefore, Descartes "...appeal[ed] to Euclid's axioms as the 'foundations' of a physics intended to make comprehensive sense of all material nature" (Toulmin, 1990, p. 171). On Descartes's view, this system of geometry constituted not a set of metaphysical beliefs, but a neutral 'scratch line' of *shared basic concepts* or *clear and distinct* *ideas* which were self-evident and self-justifying (i.e., beyond the need for proof), and which were "...equally available to reflective thinkers in all epochs and cultures" (Toulmin, 1990, p. 177). As such, this system of mathematical physics was designated the foundation, or *paradigm*, for inquiry and knowledge construction in all theoretical disciplines and practical fields devoted to rationality.

Symbolic Logic

Whitehead's and Russell's (1970/1910) *Principia Mathematica* was devoted to establishing a vigorous logical foundation for mathematics through the development of a system of symbolic logic--an ideal language which would reveal the true logical forms of all propositions (Williams, 1992). Specifically, the *Principia* was devoted to reducing mathematics to logic, and thereby accounting for the distinctive character of mathematical truths without appealing to problematic metaphysical axioms (i.e., those beyond the need for formal or empirical proofs) (Williams, 1992). Whitehead and Russell (1970/1910) believed that the more we limit mathematics (i.e., in terms of its statements of knowledge) by "...diminishing to the utmost the number of undefined ideas and undemonstrated propositions...from which it starts", the more secure knowledge is (p. 1). However, they also envisioned a wider use for their ideal symbolic language:

> It is a subsidiary object of this work to show that, with the aid of symbolism, deductive reasoning can be extended to regions of thought not usually supposed amenable to mathematical treatment. (p. 3)

Indeed, this form of reasoning was later extended by Wittgenstein (1974/1921) into philosophy, which was, as a consequence, further reduced in scope from the *already* diminished (by Descartes in the 17th century) body of doctrine known as 'epistemology' (concerned with the processes of experience and knowledge), to the *activity* (*sans* doctrinal content) of logical analysis and clarification of scientific propositions which were either verifiable or tautologous. In other words, philosophy was thereafter to concern itself only with the logical structure of scientific theories, and the logical relations between observation-statements and the laws and theories that they verify or disprove (Brown, 1977).

According to Wittgenstein (1974/1921), there are only two types of meaningful statements: (a) the empirical proposition which is "...true or false only in virtue of being a picture of reality" (p. 23)[1]; and (b) the tautology, which is true by virtue of its logical form alone. Tautologies say nothing, "they have no 'subject-matter'" (Wittgenstein, 1974/1921, p. 63); they "...are not pictures of reality..."; "they do not represent any possible situations" (pp. 34-35). However, despite this lack of experiential grounding, and *precisely because* they do not describe or refer to any state of affairs in the experiential world, they do not constitute metaphysical beliefs. They are instead analytic propositions which are true by definition, by virtue of their symbolic form, i.e., on the basis of the formal relations between signs in abstraction from any signification or meaning (Brown, 1977). An example of this is the statement 'all bachelors are unmarried' (Bredo & Feinberg, 1982), which, although it refers to no specific state of

[1]I will deal with this class of statements more in the next sub-section.

affairs, still indicates something about the world by the specific logical combination of its symbols (Wittgenstein, 1974/1921). In short, tautologies, which include all the laws of logic and mathematics, are considered to be 'pure,' certain truths.

Passive Perception

Aside from pure mathematical or logical statements, there is one other type of proposition that is considered to be meaningful by both of the rationalist traditions mentioned: namely, *statements of fact* obtained through a process of direct sensory perception of the experiential world. As stated above, Descartes believed that some human reason apart from experience can be considered legitimate; however, although he included *both* in his quest for certainty through a foundation of mathematical physics, he favoured mathematical certainty over empirical truth. In contrast, the 17th-century empiricist, John Locke (the first of a group of 'British Empiricists' which also included Berkeley and Hume), rejected the notion of there being 'pure reason' (Hearn Jr., 1992), and posited instead that 'shared sensory evidence' or 'ideas of fact' constituted the one and only 'neutral scratch line' (Toulmin, 1990). On this view, it was assumed that similar ideas of sense (sensory inputs) would generate, with repetition, similar ideas of reflection (concepts), in people of all times and all places (Toulmin, 1990).

Hume, an 18th-century empiricist, also believed that perception of the objects of awareness was immediate, or passive, and contributed meaningful propositions known as 'matters of fact' (Brown, 1977). In addition, he recognized one other type of proposition as meaningful:

specifically 'statements of relations of ideas'--an *ā priori* form of knowledge (the only one Hume would admit)--whose truth-value could be shown to be logically necessary (Brown, 1977). This version of classical empiricism greatly influenced Wittgenstein (1974/1921), whose two classes of meaningful propositions, tautologies and verifiable statements, were roughly analogous with Hume's 'relations of ideas' and 'matters of fact.'[3]

Wittgenstein (1974/1921) considered true empirical propositions to provide direct pictures of states of affairs in the experiential world. For example, the observation-statement 'there are no bachelors in this room' is a proposition that is true or false by virtue of experience (Bredo & Feinberg, 1982). In other words, the statement is either confirmed or disconfirmed by an existing state of affairs. Thus, according to logical positivist doctrine, passive perception constitutes a second source of *meaningful propositions*; that is to say, those with *potential* truth-value: whereas "a tautology's truth is certain, a proposition's [is] possible..." *if* confirmed by empirical observation (Wittgenstein, 1974/1921, p. 35). This notion of 'passive perception' is also known as 'the verificationist theory of meaning,' and, as we shall soon uncover, it has had a controversial history (MacKinnon et al., 1987).

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[3]However, Hume's concept of a 'relation of ideas' was never clear and well developed, whereas Wittgenstein's concept of a 'tautology' was explicitly delineated (Brown, 1977).

Logical Positivism

Before moving on, it will be useful to consolidate the logicist and empiricist elements of logical positivism by means of a definitive summary statement. To put it briefly, Wittgenstein and the logical positivists recognize only two types of statements as meaningful: purely formal, unconditionally true, propositions known as tautologies; and statements of fact, or empirical propositions whose truth-value is determined once and for all by means of scientific observation. All other statements, including common sense, religious, and philosophical propositions, are to be passed over as *nonsensical* "...*pseudo-propositions*, mere meaningless combinations of sounds or signs without cognitive content [italics added]" (Brown, 1977, p. 23). On this view, what can be said at all must be said *clearly*, and the sum of what can be said clearly or *meaningfully*, i.e. in terms of being verified by experience or logically true, is embodied in the propositions of natural science. "What we cannot speak about [in this manner] we must pass over in silence" (Wittgenstein, 1974/1921, p. 74). Thus, "the totality of true propositions is the whole of natural science (or the whole corpus of the natural sciences)" (Wittgenstein, 1974/1921, p. 25).

Logical Empiricism

As with Comte's positive philosophy, and Descartes's mathematical physics before that, it was not long before Wittgenstein's radical empiricism was found lacking when confronted by human experience in the practice of science.

> The central difficulty for logical positivism as a philosophy of science is that scientific laws which are formulated as universal propositions cannot be conclusively

verified by any finite set of observation statements. (Brown, 1977, p. 23)

Garrison (1986) labels this 'the problem of confirmation or verification'; Popper (1965/1934), after Hume, refers to it as 'the problem of induction.'[3] Referring to the schema reproduced here in Figure 1, Garrison (1986) discusses how confirmation of any hypothesis or theory involves drawing a logical connection (usually causal, as symbolized by the arrow) between that hypothesis or theory (T) and some experimental conclusion (E), which can be stated in the form 'If T is true, then E will be observed.' If E is indeed observed in the empirical world, then a researcher would likely claim that T has been verified. However, the problem with this schema is that it is logically invalid, due to what is commonly referred to as 'the fallacy of affirming the consequent.' The two premises (P_1 and P_2) may be true and the conclusion (C) still false--*even if the theory is well confirmed* (Garrison, 1986). This is due to the impossibility of gathering an *infinite* number of observation statements which would confirm a theory indubitably; in other words, the logical positivist belief in 'absolutely certain induction' is delusional.

$$\begin{array}{ll} P_1 & T \rightarrow E \\ P_2 & \underline{E \qquad\quad} \\ C & T \end{array}$$

Figure 1. A logical schema for verificationism (Garrison, 1986, p. 13).

A second problem with the verificationist theory of meaning stems from its assertion that "...all valid knowledge must correspond with an *observable* reality [italics added]" (MacKinnon et al., 1987, p. 5). The

[3]I will examine Popper's response to this problem below in the subsection which deals with falsificationism.

predicament raised by this contention is that many phenomena in both the natural and the social sciences--particularly theoretical concepts referring to mechanisms which 'lie behind' observed events--elude direct observation (Bredo & Feinberg, 1982). For example, several theoretical terms of modern physics, such as 'electron,' 'entropy,' and 'state-function' do not refer to observables (Brown, 1977). Similarly, all social sciences, including educational science, deal with internal thought processes or psychological states which defy direct observation (Kalin, 1992). In short, it would seem that in many cases the 'observing mind' is denied *immediate* access to the independent data against which theories are tested.

In order to deal with these intractable dilemmas, it was necessary that logical positivism, and its 'verificationist theory of meaning,' be moderated. What emerged was a doctrine known as 'logical empiricism,' which 'solved' both of the problems associated with the logical positivist theory of meaning by giving up the requirement of strict empirical confirmation, and replacing it "...with the requirement that a meaningful proposition must be testable by reference to observation and experiment" (Brown, 1977, p. 23). Specifically, Carnap (cited in Brown, 1977), one of the founders of logical empiricism, advocated replacing the notion of strict verification with the idea of a 'gradually increasing confirmation' of hypotheses through experimentation. This solved 'the problem of induction' by no longer demanding that the results of experiments be conclusive (or *certain*); however, it *was* a requirement that these tests "...provide the sole ground for determining the truth or falsity of scientific propositions" (Brown, 1977, p. 23).

The problem of phenomena which elude direct observation was resolved by the logical empiricists' willingness to allow such things to be verified by *indirect inference*, that is to say, by "...the construction of tests which, if the original supposition is correct, will result in *some* observable phenomena [italics added]" (MacKinnon et al., 1987, p. 5). As an example in social science, quantitative research in education often reduces internal psychological states and thought processes to observable behaviours (i.e., dependent variables) that can be measured, statistically treated to determine the significance of measurements, and causally related to antecedent events (i.e., experimental treatments). To summarize, the logical empiricist philosophy of science replaced the logical positivist requirement of *conclusive verification through direct scientific observation* with the less certain notion of *'testability' by reference to observation (direct or indirect) and/or experiment*.

Scientific Explanation

With logical empiricism, as with logical positivism and Descartes's mathematical physics before that, the goal is to achieve 'objective' scientific knowledge by 'cleaning the slate,' or eradicating all the various 'pseudo-propositions' of naturally existing languages and/or metaphysical traditions, and replacing them with *cognitively meaningful* (a) logical (or mathematical) truths, and (b) confirmed statements describing the interrelations between immutable natural (or human) phenomena. On this view, it is the responsibility of science to assemble the corpus of true empirical propositions, and the task of philosophy to (a) logically analyze and clarify these scientific propositions; (b) organize these laws by checking their logical consistency with, and subsuming them under, higher-order theories (Bredo & Feinberg, 1982); and (c) where possible given our present level of sophistication in establishing relations of dependence between seemingly unconnected facts (Nagel, 1961), subsume these theories under even wider theories (Brown, 1977). In this way a logically unified body of scientific knowledge, dealing with as many varieties of empirical phenomena (both natural and human) as possible, is to be assembled (Nagel, 1961). I turn now to a description of these empirical and logical processes by examining the work of some of the later logical empiricists, e.g. Hempel (1965) and Nagel (1961); and the writings of others who have commented on this philosophy of science, such as Martin (1982/1972), Bredo & Feinberg (1982) and Brown (1977).

Deductive-Nomological Explanation

Although there exists a diversity of procedures which are used by working scientists to validate or refute their hypotheses[4], Hempel (1965) stresses that *all* scientific explanation must adhere to a fundamental logic which is, not surprisingly, based on two stalwart tenets of the modern scaffolding: formalism and empirical confirmation. There are two main patterns of scientific explanation, deductive-nomological explanation and inductive-statistical explanation, of which the former is the *basic* or *ideal* configuration due to the fact that its explanations of observed events are deducible from universal (i.e.,

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[4]E.g., Durkheim (1938/1895) mentions the *experiment* and the *comparative method*, which has a number of variations such as the 'method of agreement,' the 'method of difference,' and finally the 'method of concomitant variation' or 'correlation.'

nomological) statements which constitute, in most cases, *deterministic (i.e., causal) laws.*

A deductive-nomological explanation consists of two main

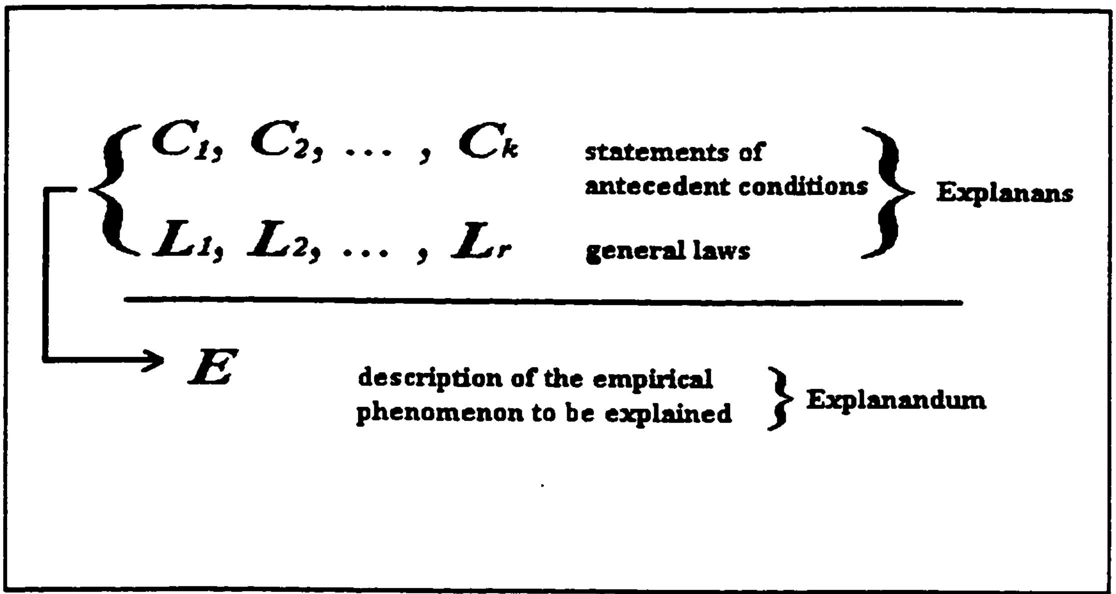

Figure 2. A schema summarizing the characteristics of deductive-nomological explanation (Hempel, 1965, p. 249).

components (see Figure 2): (a) the *explanandum*--a sentence *describing* the phenomenon to be explained (i.e., *not* the phenomenon itself); and (b) the *explanans*--"...the class of those sentences which are adduced to account for the phenomenon" (Hempel, 1965, p. 247). The explanans is further divided into two subclasses: (a) sentences (i.e., C_1, C_2, ..., C_k) which state particular antecedent conditions; and (b) sentences (i.e., L_1, L_2, ..., L_r) which represent general laws. A specific explanation must invoke, at minimum, one general (or 'covering') law to explain a single phenomenon; however, as the schema indicates, there is no upper limit on the number of covering laws which may be utilized in an explanation (Hempel, 1965).

For a deductive-nomological (D-N) explanation to be sound, its constituent parts must satisfy certain conditions of adequacy, i.e., three logical conditions and one empirical condition. The first logical condition (R_1) is that the explanandum must be a logical consequence of the explanans (Hempel, 1965). In other words, a D-N explanation has the formal structure of a deductive argument whereby the empirical phenomenon described is a logically necessary consequence of the statements of antecedent conditions and covering laws; for otherwise, the premises contained in the explanans would *not* state a sufficient (and sometimes--though not always--necessary) condition for the truth of the explanandum (Nagel, 1961; Hempel, 1965). The second logical condition (R_2) is that "the explanans must contain general laws, and these must actually be required for the derivation of the explanandum" (Hempel, 1965, p. 248). Although the vast majority of D-N explanations contain statements of antecedent conditions, they do not constitute a *necessary* condition for a logically-valid explanation. For example, the explanans covering the motion of double stars in celestial mechanics contains only statements of general laws. However, if statements of antecedent conditions are present, they, like the covering law(s), must be essential for the deduction of the explanandum (Hempel, 1965). The final logical condition (R_3)--one which is implicit in R_1--is that the explanans must be capable of test by experiment or observation; that is, *it must have empirical content* (Hempel, 1965).

Hempel's (1965) lone *empirical* condition of adequacy (R_4) states that "the sentences constituting the explanans must be *true* [italics added]" (p. 248). However, as we saw above, and as Hempel no doubt realized, the 'problem of induction' precludes the possibility of securing positive truths. Thus, he appends this proviso to R_4:

> That in a sound explanation, the statements constituting the explanans have to satisfy some condition of factual correctness is obvious. But it might seem more appropriate to stipulate that the explanans has to be *highly confirmed* by all the relevant evidence available rather than that it should be true [italics added]. (Hempel, 1965, pp. 248-249)

In this way, Hempel differentiates his logical empiricism from Wittgenstein's logical positivism. Similarly, Martin (1982/1972) distinguishes between R_4, in which "all the sentences in the explanation must be *true*" (i.e., *logical postivism*), and R'_4, in which "all the sentences must be *well confirmed* relative to available evidence [italics added]" (i.e., *logical empiricism*) (p. 34). Thus, "R_1, R_2, R_3, R_4 specify the requirements for a *true causal explanation*; R_1, R_2, R_3, R'_4 specify the requirements for a *justified causal explanation*" (Martin, 1982/1972, p. 35). However, whereas Martin makes no commitment to either position, Hempel clearly eschews Wittgenstein's 'verification theory of meaning' and recognizes Carnap's notion of 'gradually increasing confirmation.'

Hempel (1965) also remarks that this stipulation concerning the amount of empirical confirmation required for an adequate explanation leads to some awkward consequences with regards to how we should characterize scientific explanations which were, during an earlier historical period, 'highly confirmed,' but which now, in light of more recent empirical findings, are 'highly disconfirmed.' As Hempel explains, it might be tempting, given such a case, to consider the original explanatory account to have been initially 'correct,' and later, upon the discovery of unfavourable evidence, 'incorrect.' However, on Hempel's view, this version does not accord with 'sound common usage' (i.e., *the positivist account of scientific progress*),

> which directs us to say that on the basis of the limited initial evidence, the truth of the explanans, and thus the soundness of the explanation, had been quite probable, but that the ampler evidence now available makes it highly probable that the explanans is not true, and hence that the account in question is not--and never has been--a correct explanation. (Hempel, 1965, p. 249)

As we saw above in Chapter Two, Kuhn (1970), Toulmin (1990), and Brown (1977) consider this logical empiricist handling of theories which are first 'confirmed' and later 'disconfirmed' (i.e., treating them as the 'erroneous myths' of previous historical periods when compared with today's 'confirmed truths') to constitute historical revisionism given that all such theories were, *at some time, within some scientific community*, considered to be 'correct..'

Martin (1982/1972), referring to a schema similar to that contained in Figure 3, provides a clear and concise example of a D-N explanation. In this case, it is observed that a rod has expanded and we wish to know why this phenomenon has occurred. The explanandum (E) here is that a rod has lengthened. The explanans invoked to describe and explain the event consists of the following statements: C_1, the rod is made of copper; C_2, the rod was heated (statements of initial conditions); and L_1, copper expands when

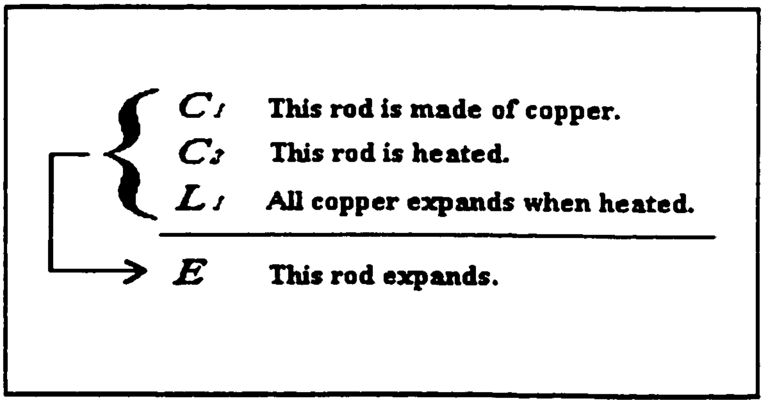

Figure 3. An example D-N explanation (see Martin, 1982/1972, p. 33).

it is heated (covering law). Recalling Hempel's (1965) conditions of adequacy, this constitutes a sound scientific explanation given that: *R_1* is satisfied--the explanandum 'this rod expands' logically follows from the premises contained in the explanans; *R_2* is fulfilled--the explanans contains at least one covering law, and the covering law and statements of antecedent conditions are essential for the derivation of the explanandum; *R_3* holds--the sentences are testable, they have empirical content; and finally, *R'_4* is met--"...for in light of the available evidence the law and initial conditions are well confirmed" (Martin, 1982/1972, p. 35).

D-N Explanation and Causation

The preceding discussion of deductive-nomological explanation has mostly dealt with a mode of explanation known as 'causal' or 'deterministic' explanation. In this mode of D-N argument, the laws expressing certain empirical regularities are *causal*, *deterministic* laws.

> If E describes a particular event, then the antecedent circumstances described in the sentences C_1, C_2, ..., C_k may be said jointly to 'cause' that event, in the sense that there are certain empirical regularities, expressed by the laws L_1, L_2, ..., L_r, which imply that whenever conditions of the kind indicated by C_1, C_2, ..., C_k occur, an event of the kind described in E will take place. Statements such as L_1, L_2, ..., L_r, which assert general and unexceptional connections between specified characteristics of events, are customarily called causal, or deterministic laws. (Hempel, 1965, p. 250)

Thus, a causal explanation implicitly claims that general laws exist in virtue of which the occurrence of certain antecedent conditions (i.e., the complex set of circumstances and events described in the *statements* of antecedent conditions) can be said to provide a sufficient condition for, or to *cause*, a certain *effect*--the explanandum event (Hempel, 1965). Further,

> ...as is suggested by the principle 'same cause, same effect,' the assertion that those circumstances jointly caused a given event implies that whenever and wherever circumstances of the kind in question occur, an event of the kind to be explained takes place. (Hempel, 1965, p. 348)

Finally, Hempel (1965) discusses two subclasses of causal explanation: (a) causes as narrowly circumscribed antecedent events, and (b) causes as antecedent states of total systems (or *deterministic theories*). Although causal explanations *need not* refer to comprehensive theories describing relations between events occurring within physical systems (therefore, such an explanation takes the form of a *'narrowly described antecedent event'*), they more often than not take the form of a *'deterministic theory'* which describes the antecedent state of a system at any one time in order to determine the state of this system "...at any other, earlier or later, time" (Hempel, 1965, p. 351).

Other Modes of Explanation Which Follow the D-N Model

While all 'causal explanations' are, at least implicitly, *deductive-nomological*, it does not follow that *all* D-N explanations are *causal*. Hempel (1965) points out that there are a number of modes of explanation which employ the deductive pattern, but which do not describe causal relations between events. First, there are D-N explanations which employ *laws of coexistence* rather than causal laws. An example of this is Ohm's law which states that the period t (e.g., 2 seconds) that it takes a simple pendulum to complete one full swing is *connected* to its length (e.g., 100 centimeters) by a law expressing a mathematical relationship between period and length. However, we would not say that the period of 2 seconds was *caused* by the length of 100 cm; instead, we would say that there exists *a relationship of coexistence* between the two facts.

A second mode of D-N explanation which is not causal in nature is the deductive subsumption (explanation) of a covering law under (by) theoretical principles, or of theories under (by) more comprehensive theories (Hempel, 1965; Brown, 1977). Thus, on this view, knowledge can exist at three or more levels of abstraction: at the level of particular observations, at the level of laws, and at the level of theories (which are occasionally subsumed under wider theories). Each incremental level of abstraction checks the logical consistency of statements in the level below it; that is to say, more inclusive theories check the logical consistency of narrower theories, theoretical principles check the logical consistency of laws, and laws check the logical consistency of observation statements (Bredo & Feinberg, 1982).

However, it must be mentioned that more inclusive theories often only provide *approximative D-N explanations* of the laws or theories subsumed under them; in other words, these "...laws hold only within a limited range, and even there, only approximately" (Hempel, 1965, p. 344). For example, while we might say that Newton's law of gravitation, strictly speaking, contradicts that of Galileo, "...for free fall over short distances near the surface of the earth...", Newton's law shows that "...Galileo's law holds to a high degree of approximation" (Hempel, 1965, p. 344). Hence, Newton's "...theory might be said to provide an *approximative D-N explanation* of Galileo's law" (Hempel, 1965, p. 344).

It is through the subsumption of theories (or laws) under wider theories that scientists broaden and deepen their understanding of the empirical phenomena they observe (Hempel, 1965). First, our understanding is broadened because a more inclusive theory will cover (i.e., explain) a wider range of facts than do narrower theories or empirical laws. For example, Newton's theory of gravitation and of motion explains not only free fall on earth, but also on other celestial bodies, and goes beyond accounts of planetary motions in that it also explains: (a) the relative motion of double stars, (b) the orbits of comets and artificial satellites, (c) the movements of pendulums, (d) certain aspects of the tides, and (e) the rise of liquids inside thin tubes, among other phenomena (Hempel, 1965; Nagel, 1961). And second, our knowledge of empirical reality is deepened because a wider theory "...reveals the different regularities exhibited by a variety of phenomena, such as those just mentioned in reference to Newton's theory, as manifestations of a few basic laws" (Hempel, 1965, p. 345). In other words,

> patterns of relations may be discovered that are pervasive in vast ranges of fact, so that with the help of a small number of explanatory principles an indefinitely large number of propositions about these facts can be shown to constitute a logically unified body of knowledge. (Nagel, 1961, p. 4)

This is, without a doubt, a restatement of the modern scientific-rationalistic dream of building a systematized body of scientific knowledge explaining diverse phenomena, and organizing this corpus under a foundation of mathematical physics (Toulmin, 1990).

Although it is an important objective of science to "...establish theories of broad scope under which narrower generalizations may then be subsumed as special cases...", as indicated above, we must sometimes settle for *approximative D-N explanations* of these narrower theories (Hempel, 1965, p. 347). Further, Hempel (1965) indicates that the subsumption of narrower theories under more comprehensive theories deepens our grasp of empirical phenomena in that we come to understand how generalizations previously accepted as correct, but which are now considered to be, at worst, erroneous myths, and at best, 'lawlike statements' which are explained only approximately by more comprehensive succeeding theories, should have been confirmed in the first place. This occurs because a wider theory, i.e., one which affords a more expansive view of the subject matter, demonstrates that the scope of an earlier theory was confined within a certain limited range of cases, thus allowing it, in its own time, to be confirmed. In other words, the *incorrect* or *approximative* nature of the past generalization is only revealed when the narrower theory is supplanted by the wider theory.

> When a scientific theory is superseded by another in the sense in which classical mechanics and electrodynamics were superseded by the special theory of relativity, then the succeeding theory will generally have a wider explanatory range, including phenomena the earlier theory could not account for; *and it will as a rule provide approximative explanations for the empirical laws implied by its predecessor*. Thus, special relativity theory implies that the laws of the classical theory are very nearly satisfied in cases involving motion only at velocities which are small compared to that of light [italics added]. (Hempel, 1965, p. 345)

In short, this is a view of science which sees its progress as a *cumulative* process in which succeeding, and more comprehensive, theories are able to explain or subsume large portions of their narrower predecessors--albeit often only in an approximative manner. As we saw in Chapter Two, Kuhn (1970) refutes this cumulative account of the development of science by demonstrating that succeeding theories are formed from different *a priori* assumptions, have dissimilar semantic structures, and are largely incommensurable with their forerunners. Therefore, on this view, it makes no sense to try to explain and/or evaluate earlier theories by employing the terms and meaning structures of successor theories.

D-N Explanation and Prediction

Not only do laws and theoretical principles explain events which have already occurred, but because they make *general* claims, they also "...range over cases not as yet examined and have definite implications for them" (Hempel, 1965, p. 364). Hempel (1965) considers D-N explanation to show a close affinity to prediction; in fact, they are considered to have the same logical form and therefore to be symmetrically related.

> If E is given, i.e. if we know that the phenomenon described by E has occurred, and a suitable set of statements C_1, C_2, ..., C_k, L_1, L_2, ..., L_r is provided afterwards, we speak of an explanation of the phenomenon in question. If the latter statements are given and E is derived prior to the occurrence of the phenomenon it describes, we speak of prediction. (Hempel, 1965, p. 249)

Hempel (1965) refers to the inferences or arguments by which predictions are obtained as 'D-N predictions' in that they, like D-N explanations, are of deductive-nomological form (i.e., their premises comprise both explanatory laws and statements of particular fact). In short, the difference between D-N explanations and D-N predictions is purely pragmatic (Hempel, 1965): the very same knowledge can be used, on the one hand, to explain a prior event, and on the other, to predict, control, or bring about a future event (Bredo & Feinberg, 1982).

Additionally, Hempel (1965) asserts that checking the predictions derived from covering laws invoked in D-N explanations is an important way of testing the soundness of these generalizations:

> It may be said...that an explanation of a particular event is not fully adequate unless its explanans, if taken account of in time, could have served as a basis for predicting the event in question. (p. 249)

For example, Hempel (1965) points to correlational explanations as incomplete explanatory accounts which are considered as such because, although they may indicate some positive correlation between the antecedent conditions cited, they do not provide a sufficient basis from which to make predictions concerning the explanandum phenomenon. To illustrate, certain peculiarities in the work of an artist might be explained as the by-product of a particular neurosis; however, although the two facts are correlated, this explanation would not allow us to predict these peculiarities. Therefore, this explanation must be considered inadequate and incomplete. Notwithstanding this, such incomplete correlational arguments are useful in that they point out a direction for future research, which, with 'appropriate effort,' will more than likely complete the explanatory account and thereby allow predictions to be made (Hempel, 1965).

In spite of Hempel's (1965) assertion concerning the logical parity of D-N explanations and D-N predictions (i.e., the 'thesis of the symmetry of explanation and prediction'), he makes the following stipulations: (a) he warns about the need to take the claim regarding the use of deterministic laws and theories to predict future events 'with a grain of salt'; and (b) he cautions that although this thesis allows "...that *every adequate explanation is potentially a* *prediction*...", the converse, that "...*every adequate prediction is potentially an explanation*...", does not follow (Hempel, 1965, p. 367).

Regarding the first stipulation, Hempel (1965) states that even the most sound of D-N predictions do not allow us to forecast future events using only information concerning present conditions. Such a predictive argument will be adequate only if we include the premise that there will be an absence of disturbing influences in the future. Since it is impossible to foresee all of the boundary conditions which might, in the future, impinge upon the territory covered by an explanation, we must temper our claim to be able "...to predict certain aspects of the future from information about the present..." (Hempel, 1965, p. 366).

With regards to the second qualification, that D-N predictions do not always yield adequate D-N explanations, Hempel (1965) discusses a sample D-N prediction involving one of the early symptoms of measles: the appearance of small, white spots, termed 'Koplik spots,' on the mucous linings of the inner cheeks. This deductive argument includes: (a) the general law (L_1) that the appearance of Koplik spots is *always* followed by the later symptoms of the measles; (b) the statement (C_1) that 'patient *i* has Koplik spots at time *t*'; and (c) the explanadum statement '*i* subsequently shows the later symptoms of the measles,' (e.g., a high fever). Although such a D-N argument might be adequate for predictive purposes, its explanatory adequacy can be questioned. For instance, we would not want to say "...that *i* had developed high fever and other symptoms of the measles because he had previously had Koplik spots" (Hempel, 1965, p. 374). This is so because it is possible that the later symptoms were caused by some other illness or condition.

Explanatory Sketches and Potential D-N Explanations

Thus far our discussion of the D-N model of explanation has centred on what can be referred to as 'completed' or 'confirmed' explanations, or those with explicitly circumscribed antecedent conditions and covering laws. But what of those situations where "...the relevant conditions or laws remain largely indefinite..." (Hempel, 1965, p. 349)? Is there something *like* the D-N schema which can be used to analyze and clarify these incomplete arguments? The answer is 'yes': the D-N model *can* be used in *sketch form* to outline: (a) what relevant antecedent conditions we know so far, (b) what supporting evidence exists for these conditions, and (c) what laws, if any, we have assumed. We can then scrutinize this '*explanation sketch*' in order to bring its logical and epistemological deficiencies to light (Martin, 1982/1972). Referring to Figure 4, we can see that the covering laws for this explanatory discourse are missing or remain indefinite. Three statements of initial conditions (C_1, C_2, and C_3) are assumed, but might be lacking in supporting empirical evidence. As well, other relevant conditions might

Laws assumed	**?**
Statements of initial	C_1
conditions apparently	C_2
assumed	C_3
Other statements of	
initial conditions	**?**

$$\therefore E$$

Figure 4. An explanatory sketch (see Martin, 1982/1972, p. 36).

exist, but as of yet they remain indeterminate. In sum, what we have here is not a full-fledged D-N explanation, but rather a 'program,' a 'sketch,' or "...a 'working hypothesis' which may prove its worth by giving new, and fruitful, direction to further research" (Hempel, 1965, p. 350). In other words, such sketches provide scientists with guidelines for research activity which is designed to fill in the sketch and thereby complete the explanation. Furthermore, they can furnish teachers and students of science with insights into the surviving gaps in scientific knowledge (Martin, 1982/1972).

A *potential D-N explanation* is any argument which has the form of a D-N explanation "...except that the sentences constituting its explanans need not be true" (Hempel, 1965, p. 338). In the potential D-N explanation, L_1, L_2, ..., L_r are not *laws*, but are instead *lawlike sentences*: statements of essentially generalized form which are *like* laws, except that they are potentially false (Hempel, 1965). The notion of a potential D-N explanation is used in two different situations: (a) when we hypothesize as to whether or not "...a novel and as yet untested law or theory would provide an explanation for some empirical phenomenon..."; and (b) when we discuss theories which were once considered confirmed, but which have subsequently been discarded in light of new empirical evidence or theoretical innovations (Hempel, 1965, p. 338). The potential D-N explanation is distinguishable from the *explanatory sketch* in that with the former all sentences describing antecedent conditions and lawlike statements are present, whereas with the latter some gaps exist.

Inductive-Statistical Explanation

While the D-N model of explanation "...has been widely regarded as the paradigm for any 'genuine' explanation, and has often been adopted as the ideal form to which all efforts at explanation should strive", it is not always possible in the natural sciences, and *especially* in the social sciences, to secure explanations of deductive form (Nagel, 1961, p. 21). Bredo and Feinberg (1982) state that it is almost impossible to find relationships between variables in social science that take the universal and determinate form of many of the laws of natural science. Hempel (1965) mentions that the regularities (i.e., laws and theories) invoked in psychology to explain human behaviour "...frequently...cannot be stated with the same generality and precision as in physics or chemistry..." (p. 251). Despite this fact, Hempel (1965) insists that "the terms 'empirical science' and 'scientific explanation'...refer to the entire field of empirical inquiry, including the natural and social sciences as well as historical research" (p. 333). What has allowed this incursion of scientific methods into the social sciences and humanities is a *liberalization* of the somewhat 'positivistic' D-N form of explanation (Bredo & Feinberg, 1982). Nagel (1961) agrees that limiting explanation to a strictly deductive model would have unnecessarily excluded important areas of investigation from the discussion surrounding the logic of explanation.

The *inductive-statistical* (I-S) (see Hempel, 1965), or *statistical-probabilistic* (S-P) (see Martin, 1982/1972), or *probabilistic* (see Nagel, 1961), model of explanation parallels its D-N counterpart except for two things (see Figure 5): (a) its laws are

$$\left.\begin{array}{ll} C_1, C_2, \ldots, C_n & \text{statements of antecedent conditions} \\ L_1, L_2, \ldots, L_n & \text{statistical laws} \end{array}\right\} \text{Explanans}$$

[makes highly probable]

$$E \qquad \left.\begin{array}{l} \text{description of the empirical} \\ \text{phenomenon to be explained} \end{array}\right\} \text{Explanandum}$$

Figure 5. A schema summarizing the characteristics of inductive-statistical explanation (see Hempel, 1965, p. 383; Martin, 1982/1972, pp. 39-40)

statistical rather than general, and (b) the premises contained in the explanans do *not formally imply* the explanandum, they only make it *highly likely*, or *nearly certain*. Whereas the D-N model assumes that *general laws*--that is, laws of the form '<u>all</u> A are B'--are required for complete, therefore deterministic, explanations, the I-S model allows laws of statistic-probabilistic form (Martin, 1982/1972). Statistical laws can be stated less precisely, e.g., '<u>most</u> A's are B,' or 'the proportion of A's that are B is close to one,' or 'any A has a close chance of being B'; *or*, they may be stated quantitatively, e.g., '90 per cent of A's are B' (Martin, 1982/1972).

As statistical laws are really only 'law-like statments' (i.e., statements of essentially generalized form which are *highly probable*, or, stated conversely, *potentially false*), the explanandum cannot be logically deduced from the explanans: "The relation between the premises and the sentences describing the event, state or process to be explained is probabilistic rather than deductive" (Martin, 1982/1972, p. 39). In other words, the relation of the explanans to the explanadum is not a deductive or deterministic one (i.e., *if* the premises of the former are true, the phenomenon described in the latter necessarily follows) but rather one of *inductive support*. Referring to Figure 5, the fact that such arguments "...are *inductive* or *probabilistic* in the sense that the explanans confers upon the explanandum a more or less high degree of...logical (inductive) probability" is symbolized by the double line separating the premises from the conclusion (Hempel, 1965, pp. 385-386). The strength of the inductive support for the argument is indicated in the square brackets. Hempel (1965) states that it is impossible, without being completely arbitrary, to designate a particular number, say .8, as the minimum value of inductive support, or probability, permissible in an I-S explanation.

The most often quoted example of an I-S explanation is that of 'Jones's streptococcal infection,' schematized in Figure 6 (see Hempel, 1965, pp. 382-383). To explain why a patient, Jones, recovered from a

C_1 **Jones had a streptococcal infection.**
C_2 **Jones was treated with penicillin.**
L_1 **Nearly all people having streptococcal infections who are treated with penicillin recover.**

═════════════════════════

E **Jones recovered**

Figure 6. An example I-S explanation (See Hempel, 1965, p. 383; Martin, 1982/1972, p. 39; Brown, 1977, p. 58).

streptococcal infection, we might be told that he had been treated with penicillin. However, it would be incorrect to invoke a general law connecting penicillin treatment and recovery from a streptococcus infection. This is because penicillin treatment does not *invariably* lead to recovery from this illness, rather it "...will effect a cure *in a high percentage of cases*, or *with a high statistical probability*"--as indicated in the statistical law L_1 (Hempel, 1965, p. 382). Thus the conclusion here cannot be logically deduced from the premises as in a D-N explanation. Though the premises C_1, C_2, and L_1 are true, the conclusion E could be false: Jones might not regain his health (Martin, 1982/1972). Nevertheless, the explanandum event is highly likely, or to be expected with near-certainty, relative to the premises (Martin, 1982/1972; Hempel, 1965).

An important point concerning the distinction between D-N and I-S modes of explanation is made by Hempel (1965), who warns against asserting that because all general laws or theories, such as Newton's law of gravitation, rest upon an "...inevitably incomplete body of evidence..." in that heretofore undetected exceptions to the rule may surface in the future, such nomological claims should be qualified as only 'probabilistic' (p. 378). Hempel points this out as a category error in which *empirical statements which have a more or less high inductive probability bestowed upon them by the existing evidence* are confused with *the type of claim* made by a given statement, i.e. either nomological or statistical-probabilistic. In short, the distinction between general laws and statistical laws pertains not to the evidential support for these generalizations, but to the claims they make: "...the former attribute (truly or falsely) a certain characteristic to all members of a certain class; the latter, to a specified proportion of its members" (Hempel, 1965, p. 379).

The Ambiguity of I-S Explanation and The Requirement of Maximal Specificity

Hempel (1965) indicates an apparent ambiguity or inconsistency with I-S explanations by, once again, referring to Jones's illness. As we saw above, the statistical law invoked in this example claims that a high proportion of patients infected with a streptococcal infection who are treated with penicillin will recover. However, there exist certain mitigating factors, such as the existence of streptococcal strains which resist penicillin or the fact that Jones is an octogenarian with a weak heart, which would make the opposite conclusion highly probable, i.e. that Jones will *not* recover. Thus, the 'ambiguity of inductive-statistical explanation' derives from the fact that

> ...for a proposed probabilistic explanation with true explanans which confers near certainty upon a particular event, there will often exist a rival argument of the same probabilistic form and with equally true premises which confers near-certainty upon the nonoccurrence of the same event. And any statistical explanation for the occurrence of an event must seem suspect if there is the possibility of a logically and empirically equally sound probabilistic account for its nonoccurrence. (Hempel, 1965, pp. 394-395)

Or in other words, it is possible to offer a valid statistical explanation for both Jones's recovery and for his non-recovery (Brown, 1977). Hempel (1965) states that there is no analogous inconsistency in the case of D-N explanation: "...for if the premises of a proposed deductive explanation are true then so is its conclusion; and its contradictory, being false, cannot be a logical consequence of a rival set of premises that are equally true" (p. 395).

Hempel (1965) deals with this ambiguity by imposing Carnap's requirement of *total evidence* upon statistical explanations. For example, the original explanation of Jones's recovery included the information that Jones had a streptococcal infection which was treated with penicillin and the statistical law that recovery in such cases is highly probable. However, this explanation becomes untenable if we also become aware of the fact either that Jones's infection was of a type resistant to penicillin, or that Jones was an octogenarian with a weak heart. Therefore, in formulating an acceptable I-S explanation of Jones's response to medical treatment, we would want to base it on a statistical law which takes into account *all* information which has potential explanatory relevance to the explanandum phenomenon. Such information would be used to narrow the class to which we assign Jones's illness from that of 'a person who has a streptococcal infection' to 'an *octogenarian with a weak heart* who has a streptococcal infection,' or to 'a person who has a streptococcal infection *which is resistant to treatment with penicillin.*' In so doing, we improve the probability that the explanandum event will follow from the explanans in an I-S explanation. Hempel (1965) refers to this as "...the requirement of maximal specificity for inductive-statistical explanations" (p. 399).

I-S Explanation and Prediction

Hempel (1965, p. 406) asks the following question: "Can it be maintained that an inductive-statistical explanation of a particular event, much like a deductive-nomological one, constitutes a potential prediction of that event?" He answers that I-S explanations *can* be used to predict events provided (a) like D-N arguments, there is adequate empirical confirmation for the premises, and (b) the I-S argument conforms to the requirement of maximal specificity, which is the most rational way to form any expectations concerning the explanandum event.

The Dream of Replacing Statistical Laws With General Laws

Hempel (1965) asserts that, although not logically conclusive, we must accept statistical-probabilistic arguments as valid because otherwise we would be forced to eliminate many important explanatory accounts which make explicit use of statistical laws. In spite of this, he clearly regards such explanations as incomplete, and those of deductive form as ideal or paradigmatic in that they state the relations between facts or variables in 'general,' 'universal,' or 'deterministic' (and therefore *more certain*) terms. The following example clearly demonstrates Hempel's (1965) belief that "...probabilistic explanations are only temporary halfway stations on the road to the deductive ideal" (Nagel, 1961, p. 23):

> In [some] cases, when the nomological claim implicit in a causal statement is merely to the effect that *there are* relevant factors and suitable laws connecting *X* and *Y*, it may be possible to lend some credibility to this claim by showing that under certain conditions, an event of kind *X* is at least very frequently accompanied by an event of kind *Y*: this might justify the working hypothesis that the background conditions could be further narrowed down in a way that would eventually yield a strictly causal connection. It is this kind of statistical evidence, for example, that is adduced in support of such claims as that cigarette smoke is 'a cause of' or 'a causative factor in' cancer of the lungs. In this case, the supposed causal laws cannot at present be explicitly stated. Thus, the nomological claim implied by this causal conjecture is of the existential type; it has the character of a working hypothesis for further research. The statistical evidence adduced lends support to the hypothesis and suggests further investigation, aimed at determining more precisely the

> conditions under which smoking will lead to cancer of the lungs. (Hempel, 1965, p. 350-351)

Undoubtedly, then, Hempel believes that with continued rigorous experimentation the statistical, lawlike statements of any I-S explanation will eventually be replaced by the nomological laws which are characteristic of the D-N mode of explanation.

According to Martin (1982/1972) the replacement of a statistical law by a general law usually involves the discovery of some additional property of a phenomenon. (In some instances this replacement is induced by the discovery of some new instrument of observation or experimentation which allows some previously unobservable phenomenon to be quantified and measured). For instance, a statistical law might read 'For every x, if x has A, then *with frequency F*, x has B.' Upon the discovery of an additional property of x (P), the statistical law is replaced by the general law 'For every x, if x has A *and P*, then x has B' (Martin, 1982/1972, p. 41).

> Thus the use of statistical laws in science is logically compatible with the existence and eventual discovery of such a property P, and with the existence and eventual discovery of general laws. (Martin, 1982/1972, pp. 41-42)

Despite this 'logical compatibility,' both Martin (1982/1972) and Nagel (1961) doubt whether universal laws will ever be discovered in particular subject matters.

In education, the positivist researchers Gage (1978) and Brophy & Good (1986) characterize the development of educational science as a quest to isolate ever more powerful variables (i.e., *properties* of learners, teachers, curricula, and educational contexts), to develop ever more reliable instruments or measures of these variables, and to subject the field's abundant correlational results to experimental test in order to yield general, causal laws which can be used to predict and control educational outcomes. Certainly, then, they would agree with Hempel (1965) that the *ideal* is to achieve the certainty which is associated with nomological explanation, even if they recognize that few of the generalizations in a complex field such as education "...will permit highly exact prediction or unerring control of educational results" (Gage, 1978, p. 93).

The Role of Theory in Explanation

Bredo and Feinberg (1982) provide a useful account of the logical empiricist application of theory in inquiry. As we saw above, higher-level theoretical statements deductively explain lower-level laws, and similarly, laws (or lawlike statistical statements) explain observations. An *empirical generalization* is distinguished from a law in that the functioning of the latter, unlike the former, is deductively explained by a higher-level theory. Theoretical statements more often than not refer to properties of unobservable hypothetical mechanisms which 'lie behind' observable events. For example, Skinner's concept of 'reinforcement' is one which is not amenable to direct observation, and functions as a theoretical principle which explains a set of empirical generalizations. These 'unobservable hypothetical entities' are crucial to explanation because they help us to distinguish between those events which are spuriously related and those which have some causal connection. For example, two events might *seem* to be positively correlated, but are in fact both 'caused' by a third event. Without an underlying theory to describe the relations between these events, and to suggest possible confounding influences which can then be controlled for in experimentation, scientists will find it difficult to differentiate between relations which are merely symptomatic and those which are causal (Bredo & Feinberg, 1982).

Brown (1977) and Bredo & Feinberg (1982) discuss a problem with theories as *abstract*, integrated deductive systems: the terms appearing in theoretical propositions have no empirical meaning, they are not stated in a way that they are directly observable (e.g., Skinner's idealized conception of 'reinforcement'). Therefore, the implications of these abstract theoretical principles must be partially translated, using *correspondence rules*, into terms which *have* empirical meaning, and which are therefore observable. The first step in this process is that of theorizing, in abstract terms, the relations between certain facts; or, in other words, postulating a conception of a particular phenomenon. Next, "a set of correspondence rules serves to relate this idealized conception to observable conditions" (Bredo & Feinberg, 1982, p. 20). Following that, laws are derived from the initial theory and the correspondence rules. Finally, as I described above in my treatment of D-N and I-S explanations, laws (or lawlike statements) are related to particular observations (i.e., explanandum events) via statements of initial conditions; again, this relation is either a deductive or a statistical-probabilistic one.

Like the inductive-statistical model of explanation, theoretical statements have an ambiguous status in relation to the logic of scientific discourse. This is so because such statements are neither tautologous, i.e., true by virtue of their logical form; nor are they verifiable by direct observation since they are stated in an essentially abstract form. Thus, by using theoretical terms, scientists "...seem to be engaging in the very form of metaphysical speculation that proponents of positivism have sought to eliminate" (Bredo & Feinberg, 1982, p. 20). One solution to this inconsistency has been to consider theoretical principles and terms as mere hypothetical postulations rather than as real objects; in other words, they constitute a set of *useful fictions* which have no direct deductive relationship to observed data, only an indirect one through the deductive subsumption of a set of laws. This is why logical empiricism has been designated the 'hypothetico-deductive' view of theories (Bredo & Feinberg, 1982).

Falsificationism

I have already discussed the intractable problem of *ā posteriori knowledge* (that derived from sense experience by *induction*) known variously as 'the problem of confirmation, verification, or induction.' The reader will recall that the problem with relying on induction to provide a certain foundation for knowledge is that "...even a well-confirmed theory may still prove to be fallacious" due to the impossibility of verifying *each and every* instance of the phenomenon described by the theory (Garrison, 1986, p. 13). As mentioned above, the logical empiricist response to this recalcitrant dilemma was to replace logical positivism's 'verificationist theory of meaning' with the less-certain notion of testability by reference to observations and experiments which are designed to gradually increase the level of confirmation for hypotheses. Thus both logical positivism and logical empiricism posit that the demarcating characteristic of a *scientific* proposition (as opposed to a *metaphysical* or a *pseudo-scientific*[5] proposition) is that it can be *confirmed (or disconfirmed)* inductively by experience: *indisputably* confirmed according to the former tradition, and *highly or most probably* confirmed according to the latter tradition (Brown, 1977). As we shall now discover, Karl Popper, "...rejects both these forms of verificationism, and indeed any attempt to construct an inductive logic" (Brown, 1977, p. 68).

The Problem With Inductivism

Popper (1965/1934) begins his refutation of verificationism by examining the nature of inductive reasoning: "...an inference [is] 'inductive' if it passes from *singular statements*..., such as accounts of observations or experiments, to *universal statements* such as hypotheses or theories" (p. 27). The problem of induction is the question of how one can establish the truth of *universal statements* from *singular statements* known by experience to be true (e.g., those produced by scientific experimentation). This is akin to saying that "...the truth of [a] universal statement can somehow be reduced to the truth of singular ones..." (Popper, 1965/1934). Popper (1965/1934) maintains that we *cannot* obtain universal, certain, verified truths, or highly probable propositions, in this manner:

> Now it is far from obvious, from a logical point of view, that we are justified in inferring universal statements from singular ones, no matter how numerous; for any conclusion drawn in this way may always turn out to be false: no matter how many instances of white swans we may have observed, this does not justify the conclusion that *all* swans are white. (p. 27)

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[5]An example of a 'pseudo-scientific' theory would be astrology.

On Popper's (1965/1934) view, then, theories can never be verified once and for all; instead, a single instance of hypothesis testing can only *corroborate* a theory's claim(s) *for the time being* in that we do not *yet* have a good reason to discard it. Therefore, it is only logically possible to *falsify* theories, as a positive result obtained from a single experiment can always be overturned by a later negative result. In short, inductive inferences are *not* logically justified.

Deductivism

It is Popper's (1965/1934) assertion that while induction--arguing from the truth of singular statements to the truth of theories--is logically impossible, the *deductive* testing of theories *can* be carried out. On this view, there are four ways in which a theory can be subjected to deductive testing: first, by the logical comparison of conclusions among themselves to test the internal consistency of the system; second, by the investigation of the logical form of the theory to determine if it is scientific or empirical in character as opposed to being, say, tautological; third, by the comparison of the theory with others to determine if it would constitute a scientific advance if it survives its tests; and fourth, by the application, through scientific experimentation, of conclusions deduced from the theory in order to see if it stands up to empirical reality (Popper, 1965/1934).

The procedure for testing a theory against experience is deductive in that singular predictions which are easily testable (i.e., observation statements) are deduced from the theory and then compared with the results of experiments. If, on the one hand, the decision is that the singular statement is acceptable, or *verified*, when compared with experimental results, then the theory has, *for now*, passed its test. However,

> it should be noticed that a positive decision can only temporarily support the theory, for subsequent negative decisions may always overthrow it. So long as a theory withstands detailed and severe tests and is not superseded by another theory in the course of scientific progress, we may say that it has 'proved its mettle' or that it is '*corroborated*'. (Popper, 1965/1934, p. 33)

If, on the other hand, the decision is negative in that the singular conclusions do not concur with the experimental findings, then these conclusions have been *falsified*, and the theory from which they were logically deduced is also refuted (Popper, 1965/1934).

In sum, it is not logically possible to verify a theory, or to establish it as 'probable,' by induction from singular statements which are confirmed by experiment. It is only logically possible to falsify the conclusions deduced from a theory by subjecting them to experimental tests. In other words, Popper's claim that theories can only be falsified by experience, never verified, *is one which is born out by formal logic* (Garrison, 1986). For example, Figure 7 shows that if an observation statement logically deduced from a theory (T) is adduced to be connected (usually causally) with some experimental conclusion (E), and E is not, as a matter of empirical fact, observed (not E), then it follows that the theory is falsified (not T). This form of deductive reasoning is known as *modus tollens*, and, as stated above, it is the only form of

$$\begin{array}{ll} P_1 & T \rightarrow E \\ P_2 & \underline{\text{not } E} \\ C & \text{not } T \end{array}$$

Figure 7. A logical schema for refutation (Garrison, 1986, p. 13).

reasoning which is considered by Popper to be logically valid (Garrison, 1986). Thus, "...all reasoning is either deductive or defective..." (Garrison, 1986, p. 13). As we shall see in the next subsection, this reconstruction of the logic of science, in which "...deductive logic alone is sufficient for the evaluation of scientific claims", "...yields a new demarcation criterion" for the scientific character of a theory (Brown, 1977, p. 69).

Falsifiability as a Criterion of Demarcation for Empirical Theoretical Systems

The positivist demarcation criterion--by which scientific theories are distinguished from metaphysical and pseudo-scientific doctrines--is that 'meaningful' (i.e., empirical) statements "...must be capable of being finally decided, with respect to their truth *and* falsity..."; that is to say, "...their form must be such that *to verify them and to falsify them* must both be logically possible" (Popper, 1965/1934, p. 40). As we saw in the preceding subsection, Popper (1965/1934) denies the possibility of inductive logic:

> ...inference to theories, from singular statements which are 'verified by experience' (whatever that may mean), is logically inadmissable. Theories are, therefore, *never* empirically verifiable. (p. 40)

Thus, to uphold the criterion of demarcation inherent in positivism is to *eliminate the theoretical systems of natural science* from the class of propositions which are meaningful, i.e., empirical. We must therefore choose a criterion which allows statements that defy verification to be admitted to the domain of empirical science. Such a criterion is the *falsifiability* of a system:

> I shall not require of a scientific system that it shall be capable of being singled out, once and for all, in a positive sense; but I shall require that its logical form shall be such that it can be singled out, by means of empirical tests, in a negative sense: *it must be possible for an empirical scientific system to be refuted by experience*. (Popper, 1965/1934, pp. 40-41)

Hence *empirical*, or *scientific*, propositions are those which are falsifiable: "*In so far as a scientific statement speaks about reality, it must be falsifiable: and in so far as it is not falsifiable, it does not speak about reality*" (Popper, 1965/1934, p. 314). To illustrate, the statement 'It will rain or not rain here tomorrow' is not empirical because it cannot be disproved; in contrast, the statement 'It will rain here tomorrow' *is* refutable and is therefore empirical (Popper, 1965/1934, pp. 40-41).

Strict Versus Modified Falsificationism

Brown (1977) points out that Popper's theory has traditionally been interpreted in two different ways. The *strict* version of falsificationism, which I have heretofore outlined, is that deductive logic *alone* is adequate for the evaluation of scientific claims: if an observation statement deduced from a theoretical system is not corroborated by empirical facts, then the statement and the theory from which it is derived are refuted. The refutation of a theory results, then, from *a single falsifying test* of the predicted observable conclusions deduced from that theory: "...if the conclusions have been *falsified*, then their falsification also falsifies the theory from which they were logically deduced" (Popper, 1965/1934, p. 33). In this way the progress of science advances "...upon the firmest of logical foundations": theories constitute only provisional, hypothetical representations of phenomena, and "...the growth of science consists in a continuing series of conjectures and refutations" (Garrison, 1986, p. 13).

However, Popper (1965/1934) *also* suggests that it takes *more* than a single negative test of predicted observable conclusions to consider the theory from which they were deduced falsified:

> We say that a theory is falsified only if we have accepted basic statements which contradict it...This condition is necessary, but not sufficient; for we have seen that non-reproducible single occurrences are of no significance to science. Thus a few stray basic statements contradicting a theory will hardly induce us to reject it as falsified. We shall take it as falsified only if we discover a *reproducible* effect which refutes the theory. In other words, we only accept the falsification if a low-level empirical hypothesis which describes such an effect is proposed and corroborated. This kind of hypothesis may be called a *falsifying hypothesis*. (pp. 86-87)

According to Brown (1977), this ambiguity concerning the amount of empirical corroboration required for falsification has led to a second, *modified*, interpretation of Popper's theory--one which Popper himself later came to accept.

'Modified falsificationism' can best be approached by examining the epistemic status of the basic statements that serve as the premises in falsifying arguments (Brown, 1977). Brown (1977) points out an apparent incongruity in Popper's falsificationism in that, on the one hand, it is based on a logical relation which allows us to infer the *refutation* of a universal statement from a *singular premise*; while on the other, it rejects inductive logic for allowing universal statements to be *verified* by *singular statements*. But, as we shall see, the *modus tollens* deductive argument cannot, by itself, establish the finality of a falsification, because in order to accomplish this the singular basic statements serving as premises in arguments which falsify universal statements would have to be established *conclusively*, i.e., *once and for all*. And if this inference from the particular to the general is impossible in the case of verification, then it seems logical that it would also be unjustified in the case of falsification. In fact, "Popper denies that this can be done; indeed, to hold that basic statements could be known indubitably would be inconsistent with his entire methodology" (Brown, 1977, p. 72).

As Brown (1977) indicates, Popper recognized that indubitably established basic statements (in support of a verification *or* a falsification argument) are inadmissable for three basic reasons. First, experimental results can always be questioned as we can argue that a result is not reliable, or that any discrepancy between a result and a theory is only *apparent* and will thus disappear when we improve our understanding of the subject matter. Second, scientists accept or reject basic statements based on experience, "...but it is impossible for experience to prove or disprove any statement" (Brown, 1977, p. 72). Recalling first that deductive logic constitutes the only notion of 'proof' that Popper admits, second, that logical relations only hold between propositions, and third, that experiences are not statements, but psychological events which have no logical relation to statements, it then becomes apparent that experiences can *motivate* one's acceptance of basic statements, *but cannot logically prove them*. Finally, given Popper's demarcation criterion, "...to hold that science rests on *indubitable observation statements*...[italics added]" (as the verificationists do) is "...to make science rest on a *non-scientific* foundation" (Brown, 1977, p. 73). Thus, *all* scientific propositions--those used to corroborate a theory (for the time being), *and* those used to falsify a theory (*also* for the time being)--"...are falsifiable conjectures" (Brown, 1977, p. 73).

As a consequence of all this, the basic statements in a falsification argument are but *hypotheses* which can never be conclusively corroborated. Clearly, then, something *other than deductive logic alone* is required for the evaluation of scientific claims. As Brown (1977) points out: "For Popper falsification takes place *only after scientists agree to accept a basic statement as adequately corroborated* [italics added]" (p. 73). As a result, science rests not on formal proofs, but on practical judgments made by scientists concerning whether or not basic statements adduced to support falsification hypotheses are adequate: such statements are therefore *conventions* (Brown, 1977). In short, by showing that all cases of falsification require prior substantiation by basic statements, and that these statements in support of falsification arguments have no stronger epistemic status than those which support corroboration arguments, Popper short-circuits his own attempt to build a purely deductive logic of science (Brown, 1977).

Falsificationism as a Transitional View

Brown (1977) considers Popper's falsificationism (especially the second, modified, strain) to be a transitional view between logical empiricism and the 'new philosophy of science.' First, it breaks with logical positivism and logical empiricism because it considers the demarcation criterion of a *scientific* proposition to be that it is *falsifiable*, rather than both falsifiable *and* verifiable. Second, despite being an attempt to construct a purely deductive logic of science, falsificationism--like the 'new philosophy of science' which succeeded it--recognizes that scientific communities (and human judgments) play a role in deciding the fate of theories. Finally, Popper's view of the history of science as a series of conjectures and falsifications can be considered a primitive precursor to Kuhn's (1970) thesis that the development of science is not cumulative and involves alternating periods of 'normal' and 'revolutionary' inquiry.

However, despite having broken from the logical positivist and logical empiricist research traditions on the one hand, and having a few things in common with the postpositivistic philosophy of science on the other, falsificationism still maintains *most* of the presuppositions of the modern worldview, as outlined by Toulmin (1990). Firstly, it persists in characterizing the role of philosophy as the logical analysis of scientific propositions, and upholds Whitehead's and Russell's *Principia Mathematica* as the standard or paradigm of logic (Brown, 1977). While it rejects the attempt to base science on propositions which are 'verified,' or at least 'highly confirmed,' it "...does so for a perfectly respectable logical empiricist reason: that no adequate inductive *logic* can be constructed" (Brown, 1977, p. 71). In short, it retains the formalism which was a founding attribute of the modern worldview. Secondly, it carries forth the scientific rationalist presupposition "...that the objectivity of science is completely derived from its appeal to observation [i.e., *passive perception*] or, at least, to some special set of statements which have a particularly close tie to observation" (Brown, 1977, p. 75). Finally, it continues to uphold the practice of 'cleaning the slate' by seeking to make general statements about phenomena which hold across individual cases. However, unlike logical positivism and logical empiricism, it does not consider metaphysical doctrines to be 'meaningless' (Brown, 1977). In short, while "Popper played an important role in moving the philosophy of science in [a] new direction, ...he did not complete the transition..." by repudiating the dominant *ā priori* assumptions of modernity (Brown, 1977, p. 76).

Conclusion

In this chapter I outlined two conditions for *episteme*: (a) that there be direct correspondence between (i) 'true' knowledge and (ii) what is real in being; and (b) that the latter have an unchanging, antecedent existence. As well, I detailed the scientific rationalist belief in a systematic, deterministic, tangible reality. Moreover, I noted the positivist assumption that to maintain an exact correspondence between true knowledge and its object, the inquiring or observing mind must not interact with the object to be known (i.e., the processes of rational inquiry and/or passive observation must 'clean the slate' of any common sense or *ā priori* assumptions, must maintain the temporal and contextual independence of theory and/or observation, and must attempt to eliminate 'fallible human judgment' in inquiry). Further, I described two paths to certain (*or highly* certain) knowledge: through (a) formalism (i.e., mathematical or logical truths), and (b) empiricism (i.e., verified truths *or* highly confirmed explanations *or* corroborated theories). These are paths that were followed by 17th-century rationalists, and by 20th-century logical positivists, logical empiricists, and falsificationists. Finally, I described the operations of formal logic, empirical observation, and 'hypothetico-deductive systems,' in the processes of scientific inquiry, explanation, and prediction.

In Chapter Two I summarized Toulmin's (1990) thesis that the modern presuppositional matrix--consisting of beliefs in certainty, systematicity, and decontextualization--was born of insecure times (1618-1648); was gradually tempered as stability returned to Western Europe (1750-1910); was revived this century *in an even more extreme form* by another period of catastrophe (1914-1945); and finally, faded away once more as stability and prosperity returned to the West following World War II. In keeping with Toulmin's (1990) assertion that support for the modern presuppositional matrix eventually fades during times of increased stability (and as it is confronted by empirical reality), this section has shown that, in the 20th century, the *extreme version of scientific rationalism* known as 'logical positivism' was tempered in many ways by two successor traditions: logical empiricism and falsificationism.

The most profound liberalization of the modern scaffolding by these two traditions has involved the 'quest for *certain* knowledge' through empirical observation. The central problem here involves the criterion chosen for demarcating *scientific* theories from those that are metaphysical or pseudo-scientific. On the logical positivist view, the distinguishing characteristic of scientific statements is that they are capable of *complete verification by direct empirical observation*.

Logical empiricism addressed two problems which arose concerning this thesis: (a) that finite scientific observations are incapable of conclusively verifying general statements (i.e., the problem of induction); and (b) that not all scientific phenomena are directly observable. The first problem was solved by only requiring *a high degree of inductive support, or confirmation*, for a scientific claim. The second problem was addressed by allowing scientific statements to be confirmed *by experimental tests* which do not necessarily involve the *direct* observation of phenomena.

Falsificationists reaffirmed this conviction that the objectivity of scientific statements is sustained by experimental testing, but rejected the notion that scientific claims can be completely or highly verified by inductive inference. On this view, if such claims pass an experimental test, then they are corroborated *only for the time being*, as they will undoubtedly be falsified at some time in the future. Falsificationists therefore posited that scientific theories are only logically capable of being falsified, *never confirmed*, by experience--and not even this can be achieved with any high degree of certainty. Thus while each of the three traditions discussed asserts that the objectivity of science rests on empirical observations that are *passive* in that a strict separation between the knowing subject and the object known is supposedly maintained, the epistemic status (i.e., *certainty*) of scientific propositions has been weakened by logical empiricism, and even more so by falsificationism.

A second moderation of the logical positivist philosophy of science involved the type of claims made by scientific propositions, and served to weaken the strict, mathematical relationship adduced to exist between higher-order theories and lower-order theories or laws, and between laws and observation statements. Specifically, logical empiricists allowed incomplete, uncertain laws of *statistical-probabilistic form* to stand as explanations of the relations between natural (or social/psychological) events. Indeed, the phrase 'statistical law' can be considered a contradiction in terms if strict formalism is adhered to because laws, by definition, describe *general, nomothetic, deterministic* regularities of nature. Furthermore, the subsumption of narrower theories under wider theories was shown to be an *inexact* process in that more inclusive theories often only provide *approximative* D-N explanations of the laws or theories that they cover--*if they are able to provide any such explanation at all*. Thus it would seem that the progress of science does not exactly involve the accumulation, or 'unification,' or subsumption of an ever-increasing number of scientific facts under the 'pure' theoretical structure provided by mathematical physics.

The falsificationists' view of the history of science as a series of conjectures and falsifications also contradicts the logical positivist (or the logical empiricist) account of scientific progress as the incremental accumulation of verified facts (or highly confirmed facts). Furthermore, the *modified* interpretation of falsificationism (unlike logical positivism, logical empiricism, and the *strict* version of falsificationism) hints that the evaluation of scientific claims involves *something more* than just "...the application of formal rules and effective criteria...", i.e., it also involves the judgment of the scientific community (Brown, 1977, p. 76).

Despite these modifications of logical positivist doctrines by logical empiricism and falsificationism, all three traditions uphold the main tenets of the modern worldview: i.e., the special role of formal logic in the analysis of scientific claims; the empiricist assumption that the objectivity of science rests on the passive observation of things and events; the need to distinguish the sphere of scientific discourse from metaphysical, or pseudo-scientific, discourse[6]; and finally, the practice of forming knowledge claims in abstract, theoretical terms which can be generalized across contexts. As stated in Chapter Two, this scientific-rationalistic view of inquiry is considered to be self-evident and self-justifying--what Brown (1977) calls a 'presupposition-free philosophy'--largely because it rests on the 'pure' knowledge afforded by *Principia* logic and scientific empiricism. However, in Chapter Five I shall question whether this view of modern science is free of *ā priori* assumptions; and I will outline an alternative view of science based on more moderate, humanistic *axiōma*--one of which postulates that "...theory and observation are much more nearly co-equal partners in the *construction* of..." scientific knowledge (Brown, 1977, p. 76, italics added).

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[6]However, whereas logical positivism and logical empiricism would consider metaphysical discourse to be 'meaningless,' Popper makes no such claim.

CHAPTER FOUR

CHALLENGING THE *Ā PRIORI* ASSUMPTIONS OF MODERNITY

> The axioms of Modernity assumed that the surface complexity of nature and humanity distracts us from an underlying Order, which is intrinsically simple and permanent. By now, however, physical scientists recognize as well as anyone that natural phenomena *in fact* embody an "intrinsically simple" order only to a limited degree: novel theories of physical, biological, or social *dis*order (or "chaos") allow us to balance the intellectual books. We may temporarily ("for the purposes of calculation") shelve the contexts of our problems, but, eventually, their complete resolution obliges us to put these calculations back into their larger human frame, with all its concrete features and complexities.
>
> Stephen Toulmin, Cosmopolis

Introduction

In Chapter Two, I discussed how the dominant presuppositional scaffolding of modernity has been largely replaced, in the late 20th century, by a more *humanistic* view of the related fields of philosophy and science--one which has concluded that the rationalist aims of certainty, systematicity and decontextualization in the accumulation of knowledge are unattainable, and often destructive, dreams. In Chapter Three I outlined the various patterns that scientific rationalism has assumed in the modern age, i.e., 17th-century rationalism, and the 20th-century traditions of logical positivism, logical empiricism, and falsificationism. I also demonstrated how logical empiricists and falsificationists were forced by logical inconsistency and experience to moderate some of the dogmas of the modern scaffolding, including its system of strict deductive logic, and the epistemic status of ā *posteriori* knowledge.

This chapter and the next constitute a continuous body of thought, and so I will introduce them together here. Their purpose will be to elaborate on issues first introduced in Chapter Two concerning the collapse of the modern scaffolding and the re-Renaissance of humanist philosophy and science that has taken place during the latter half of this century. I will show how, in the post-war years, the formal scientific logic of modernity has been challenged by a newly-resurrected humanist view of philosophy, science, and related fields *such as teacher education*--one which recognizes that human rationality cannot escape context, history, and *phronesis*. As we shall see, these humanistic principles are ones shared not only by 'new philosophers of science' such as Kuhn (1970), Brown (1977), and Toulmin (1982, 1990); but also social scientists such as Gareth Morgan (1983); pragmatist philosophers such as William James (1969/1907) and John Dewey (1929, 1960/1929); and educationists influenced by both pragmatic philosophy and the 'new philosophy of science,' such as Joseph Schwab (1978a, 1978b/1960, 1978c/1970), Elliot Eisner (1988, 1991, 1993), and James Garrison (1986).

In this chapter I will elaborate on a multitude of ontological and epistemological impediments to certain (or nearly certain) knowing (some of which were briefly introduced in Chapter 2). These are barriers to positive knowledge which have emerged from human experience (which is psychological and culturally-determined, *not 'objective' and* logical), and which have destroyed the rationalist dream of attaining a unified body of scientific knowledge--the *'only'* valid kind--by the application of the empirical method as found in physics and chemistry. I will also discuss how no system of rational thought--mathematical or logical--is presupposition-free, or self-justifying, or quite obviously superior to all others. Following that, in Chapter Five, I will elaborate on Kuhn's (1970) amended view of scientific discovery, progress and truth (also introduced in Chapter Two), which involves a plurality of largely incommensurable methodologies and theories, and the application of practical wisdom. Finally, in the second half of Chapter Five I will discuss issues (e.g., the nature of plurality, the rational evaluation and choice of competing paradigms, and the trustworthiness of knowledge) raised in dealing with the *uncertainty* (indeed, some would say *'radical relativism' or 'irrationalism'*) caused by the humanist refutation of the rational-scientific worldview.

What will emerge at the end of Chapter Five is a *moderate* view of science and philosophy. This view parallels that of the 16th-century Renaissance humanists. First, it embraces both elements of Aristotle's dual logic--the need for formal, rigorous, theoretical thought *and* wise practical action which takes into account the idiosyncracies of the local context (i.e., a view which embodies both logic and rhetoric, the particular and the universal, the local and the general, and the timely and the timeless). Second, it honours the diversity and complexity of human experience by including a plurality of doctrines and methods of inquiry. Finally, it respects the rational possibilities, *and limits*, of human experience by demanding that our thought and conduct be *reasonable*.

The choice of a more moderate conception of rationality, i.e. *rationality as 'reasonableness,'* does not indicate a preference for *irrationality*. The 'rational' is a wider category than the 'logical' (Eisner, 1991); reason is not an 'all or nothing' thing, it constitutes a continuum (see Reid, 1978). A human reasonableness that is tolerant, yet skeptical, constitutes a 'middle way' between two extremes: on the one hand, 'certain' knowledge through formal, rigorous logic and strict empirical verification or testing; and on the other, absolute uncertainty owing to naïve opinion, credulity, subjectivity, and/or radical relativism. For example, the 'new philosophy of science,' with its pragmatic, 'instrumental' standard for the trustworthiness of knowledge, constitutes a 'middle way' between the exact formalism of logical empiricism and the extreme relativism of an unreflective eclectic set of theories and methods. Or, as Eisner (1993) suggests, the key is to "...avoid the verificationist's constipation of conceptual categories on the one hand and the radical relativist's free-for-all, anything-goes, no-holds-barred nihilism on the other" (p. 8).

Insurmountable Impediments to Positive Knowledge

In this chapter I shall begin, on the one hand, my refutation of the modern presuppositional framework, and, on the other, my delineation of humanist philosophy and science. I will demonstrate that doubts exist as to whether *all* natural and human problem-fields are constituted of discrete, stable phenomena which can be isolated, manipulated and passively observed in order to yield systematic, deterministic, cause-effect representations of these facts. I will show how both logic and experience fail to dictate theory justification and choice. I will discuss how the modern fact-theory/value distinction is a myth: interaction between the *knower* (whose perceptions are, in part, shaped by theoretical, cultural and local influences) and the *known* is an inescapable part of scientific observation. I will explain how the objects of inquiry do not constitute antecedent existents, i.e., they are realities which are *partially constructed* in the *human* act of inquiry. I will examine how the rationalist attempt to 'clean the slate' is both futile and detrimental to the process of inquiry. I will demonstrate that no system of thought, rational or otherwise, is self-validating or free of presuppositions. Finally, I will establish that no integration of scientific knowledge is possible given the demands of experience.

Epistemological and Ontological Uncertainties

There exist 'ontological uncertainties' which arise from the nature of particular natural and social phenomena, and 'epistemological uncertainties' which emanate from the *inevitable* application of human perception and judgment in inquiry (Morgan, 1983). This reality demonstrates that "...science is best regarded as an *uncertain endeavor* dealing with an *uncertain world* [italics added]" (Morgan, 1983, p. 386). In the end, we shall see that these epistemological and ontological uncertainties together suggest that phenomena, both natural and social, are rich in potentialities which can be revealed in a plurality of ways by using different theoretical and methodological tools (Morgan, 1983).

Indeterminacy and Unpredictability in Natural Science

A major source of ontological uncertainty involves the notion of 'physical determinism' which originated with the ancient Greek atomism of Democritus and Lucretius (Kalin, 1992), and which, as I mentioned in Chapter Two, later appeared as a major tenet of both Descartes's epistemology and Newton's physics. To review, determinism is the metaphysical principle that matter is inert, stable or passive, exists 'out there,' and is organized (*by God* according to Descartes) into deterministic, mechanistic systems in which every phenomenon is caused by a preceding event. From the 1630s to the 20th century, this Cartesian view of the universe as a 'Great Machine' constituted of intricate automata was dominant in the West (Zukav, 1979). As Toulmin (1990) indicated, from 1900 on this presupposition was finally undercut by a branch of theoretical physics known as quantum mechanics, in which it was discovered that subatomic phenomena "...are by no means as fixed and certain as the Greek atomists and those who have built science on atomic and mechanistic principles have led us to believe" (Morgan, 1983, p. 386).

The New Physics

Relativity. The ascendancy of classical Newtonian physics, and the "...view of the universe as a giant, predetermined clock..." (Cziko, 1989, p. 17), was at last undermined by Albert Einstein early this century with the publishing of his *Special Theory* in 1905, and his

General Theory of Relativity in 1915.[1] Together, these revolutionary theories "...showed that phenomena appear differently according to where you stand and how fast you are moving" (Reid, 1978, p. 97). This discovery clearly refuted the modern assumption that there exists a neutral scratch-point for observing an event occurring in one space at one time: what you see depends on your motion and your viewpoint.

The dual nature of light energy. Einstein was also instrumental in demonstrating the dual nature of light energy--another discovery that chipped away at the foundations of the modern worldview. In 1803, the question of the nature of light was settled 'once and for all' by Thomas Young who, using a simple experiment involving a light source, a double slit screen and a phenomenon known as 'interference,' demonstrated that light was wave-like. Just over 100 years later in 1905, Einstein posited that light was composed of tiny particles called photons which together formed a beam of light analogous to a stream of bullets. Using an experiment involving a phenomenon called 'the photoelectric effect,' Einstein substantiated his theory *without, at the same time, being able to disprove Young's explanation*. To this day Young's wave theory of light and Einstein's particle theory of light both stand as 'proven' explanations (Zukav, 1979). This duality creates the curious situation in which, depending on the theoretical starting point of physicists and their subsequent scientific treatment of light energy, two distinct nomological explanations can be verified *for a single empirical*

[1]Cziko (1989) points out the irony that Einstein, whose work contributed to the birth of quantum mechanics and the death of Descartes's and Newton's 'Great Machine,' could not shake his belief in a deterministic universe and, to his death, attributed the uncertainties in quantum phenomena to incomplete knowledge.

phenomenon. This is significant because light energy is a physical phenomenon, and such events have traditionally been considered to be highly stable, immutable, and eternal, and therefore amenable to *true causal explanation*; however, the 'truth' about light, because of its duality, is no longer positive. To this day the highest degree of certainty that scientists can obtain vis-à-vis light energy is that it is both a particle *and* a wave.

Zukav (1979) outlines more recent discoveries about the wave-particle duality of light energy which have further undermined classical notions of causality and predictability in science. Scientists have taken Young's double-slit screen experiment, which he used to prove that light was wave-like, and run the procedure with single photons. However, before we can grasp the startling results of these recent experiments we must first understand Young's original 1803 experiment. Young began with a light source and a wall onto which the light could shine. Between these two he placed a screen with two vertical cut-outs no larger than a razor slit. One of the cut-outs was initially covered over and the light source was allowed to pass through the other slit. The cut out was a razor slit so that its width was roughly the same size as the wavelength of the light passing through it, causing the light to *diffract* on the wall. In other words, rather than travelling in a straight line to the wall, the light fanned out into a rough circle, leaving a bright area in the centre which gradually faded to darkness at the extremities. Next, Young opened the second slit and turned on his light source. Instead of a projection on the wall that was the sum of the light which passed through the two slits, there appeared alternating bands of light and darkness. Young attributed this curious result to the fact that the diffracted light waves from each slit were interfering, much the same as the waves created by two pebbles dropped simultaneously into water would interfere. Where light waves overlapped at their crests the result is an intense light, i.e. the bands of light on the wall; and where light crests met troughs, the two waves cancelled each other out causing the bands of darkness on the wall. In short, Young's experiment proved that light energy exhibited the characteristics of a wave "...because only waves can create interference patterns" (Zukav, 1979, p. 87).

As mentioned above, recently physicists have run Young's double-slit experiment using single particles of light, or photons, fired from a light gun. First, however, Young's original experiment was duplicated: a light source (emitting many photons) was turned on and both slits in the screen were left open. A familiar interference pattern of light and dark bands was observed on the wall, proving that waves have passed through the slits. If we discuss what happened using the terms of particle theory, it can be said that photons have hit the wall where the light bands have appeared, and that no photons have landed where the dark bands have materialized. Next, a photographic plate was placed on the wall to measure where single photons would hit after being fired from a light gun. In a second experiment, only one slit was left open, and a single photon was fired through it from the same location as the light source in the first experiment. The location where it hit the wall was recorded on the photographic plate. It was noted that the photon landed in an area that would have been dark (i.e., where there would not have been any photons) had the second slit been left open. This is to be expected since only one slit was open and therefore no interference pattern of light and dark bands should exist. In a third experiment, all of the initial conditions of the second experiment were duplicated exactly: the origin of the photon was the same, and its velocity and direction were identical to those in the second experiment prior to passing through the *same slit*. However, this time the second slit was left open, which, you will recall, caused the familiar interference pattern of light and dark bands to appear in the first experiment. Needless to say, the photon did not land in exactly the same spot as in the second experiment, as that spot was in the middle of a dark band which was caused by interference in the initial experiment.

This result is highly significant for two reasons. First of all, *how did the photon 'know' that the second slit was covered in the second experiment, or that it was left open in the third experiment?* If the second slit is covered, then there is no interference and the photon can hit anywhere the wall is illuminated; but if the second slit is open then interference will occur and there will be places that photons will never go, i.e., where there are dark bands. Zukav (1979) points out that one of the central mysteries of the new physics is how the photon 'knew' in the second experiment "...that it could go to an area that must be dark if the other slit were open", and how it 'knew' in the third experiment that the second slit was open and that therefore it *must not* land where a dark band appears (p. 87). This phenomenon contrasts sharply with the classical ontology, i.e., a universe made up of 'automata' in which all events are caused mechanistically by a set of antecedent conditions. In the case of the second and third experiments, although the antecedent conditions are identical, the photons do not act as automata by striking the wall in exactly the same place; instead, they seem to have the ability to process information about whether or not the second screen is open and they act accordingly!

The second reason these experiments involving the double-slit screen and single photons are significant is because they shatter all conventional notions of causality and predictability.

> The wave-particle duality was the end of the line for classical causality. According to that way of thinking, if we know certain initial conditions, we can predict the future of events because we know the laws that govern them. In double-slit experiments we know all that we can know about initial conditions and we still can't predict correctly what happens to single photons. (Zukav, 1979, p. 88)

In other words, the paths of individual photons cannot be determined with any certainty. All we can predict is what pattern large groups of photons will make and how they will be distributed in that pattern. All that can be said about a single photon is the *probability* of it landing in a given location on the wall (Zukav, 1979). Undoubtedly it is because of ontological uncertainties such as these that logical empiricists (e.g., see Hempel, 1965; Nagel, 1961; and Martin, 1982/1972) were forced to admit that 'ideal' explanations of the deductive-nomological type were not always possible and therefore explanations of the inductive-statistical-probabilistic type must also be regarded as 'scientific.'

The Uncertainty Principle. Another major discovery which undercut the mechanistic worldview was Heisenberg's uncertainty principle, which proved that, at the subatomic level at least, there was no such thing as 'an *exact* science' (Zukav, 1979).

> The uncertainty principle refers to the degree of indeterminateness in the possible present knowledge of the simultaneous values of various quantities with which the quantum theory deals; it does not restrict, for example, the exactness of a position measurement alone or a velocity measurement alone. Thus suppose that the velocity of a free electron is precisely known, while the position is completely unknown. Then the principle states that every subsequent observation of the position will alter the momentum by an unknown and undeterminable amount such that after carrying out the experiment our knowledge of the electronic motion is restricted by the uncertainty relation. This may be expressed in concise and general terms by saying that every experiment destroys some of the knowledge of the system which was obtained by previous experiments. This formulation makes it clear that the uncertainty relation does not refer to the past; if the velocity of the electron is at first known and the position then exactly measured, the position for times previous to the measurement may be calculated. Then for these past times (delta)*p* (delta)*q* is smaller than the usual limiting value, but this knowledge of the past is of a purely speculative character, since it can never (because of the unknown change in momentum caused by the position measurement) be used as an initial condition in any calculation of the future progress of the electron and thus cannot be subjected to experimental verification. It is a matter of personal belief whether such a calculation concerning the past history of the electron can be ascribed any physical reality or not. (Heisenberg, 1949/1930, p. 20)

Zukav (1979) provides this simplified explanation of Heisenberg's remarkable discovery:

> In the subatomic realm, we cannot know both the position *and* the momentum...(momentum is a combination of how big an object is, how fast it is going, and the direction that it is moving)...of a particle with absolute precision. We can know both, approximately, but the more we know about one, the less we know about the other. We can know either of them precisely, but in that case, we can know nothing about the other. (p. 52)

The reason, then, that we can never know both the position and the momentum of a moving particle is that by observing either of these things we inevitably alter the other: "...the process of measuring the position x of a particle disturbs the particle's momentum p, and vice versa..." (Strauss, 1992, p. 1). Since we cannot accurately determine the position and momentum of a subatomic particle at the same time, we cannot *predict* much about them either. However, while we can never know for certain what will happen with a particle that we are 'observing,' we *can* predict *probabilities*: "All that we can know for sure are the probabilities for it to behave in certain ways" (Zukav, 1979, p. 53). In sum, contrary to Descartes's and Newton's view that natural events can be precisely determined and mathematically described, the quantum physicists have shown that subatomic events cannot be predicted with any certainty: they are ontologically indeterminate.

Hence, when dealing with subatomic particles, the scientist is as much a participant as an observer, he or she "...interferes with the neutrality and very nature of the object in question", and makes it highly dubious that "...one can know an objective effect if one is always a part of its cause" (Kalin, 1992, p. 1). Inquiry, then, involves choices based on human values: Do we want to measure the *position* of a particle more accurately, or its *momentum*? "...*We must choose* which of these two properties we want to determine" (Zukav, 1979, p. 54). Humankind's status in the universe is therefore raised, by Heisenberg's discovery, from that of "...helpless cogs in a machine..." to that of beings who partially influence or create their own reality (Zukav, 1979, p. 53). Morgan (1983) agrees "...that *uncertainty ultimately involves choice*" and, consequently, a greater measure of responsibility on the part of the scientist for what we know *and* what we do with that knowledge (p. 390). The decision of how to treat a scientific phenomenon on one level determines how it will be revealed, and on another the ultimate consequences of that knowledge (Morgan, 1983). In sum, at the level of subatomic events, Descartes's and Newton's 'Great Machine' does not exist, science is no longer exact, and the distinction between subject and object has vanished (Zukav, 1979).

Chaos Theory

A second area of theoretical discourse mentioned by Toulmin (1990) which undermines the notion of a deterministic world is 'chaos theory,' which "...provides a framework for understanding irregular or erratic fluctuations in nature" (Gollub & Solomon, 1992, p. 1). The possibility of there being chaotic elements in natural (and by traditional definition *deterministic*) systems was first explored in the late 19th century by the French mathematician Poincaré in relation to planetary orbits. This line of inquiry was forgotten until the early 1960s when the American meteorologist Edward Lorenz demonstrated that chaotic dynamics existed in a simple deterministic model of thermal convection in the earth's atmosphere. Since then, chaotic dynamics (i.e., those involving the evolution of a system in time) have been shown to exist in: (a) the orbits of certain bodies in the solar system; (b) the pulsations of variable stars; (c) models describing convection and mixing in (i) fluids, (ii) wave motion, (iii) oscillating chemical reactions, and (iv) electrical currents in semiconductors; (d) the dynamics of animal populations; and (e) medical disorders such as heart arrhythmia and epilepsy. As well, efforts are being made to apply chaos theory in the social sciences to such areas as the modelling of arms races and the study of business cycles (Gollub and Solomon, 1992).

Chaotic systems exhibit both deterministic *and* chaotic characteristics.

> A process demonstrating chaos is one in which strict deterministic causality holds at each *individual* step in an unfolding process, and yet it is impossible to predict the outcome over any *sequence* of steps in the process. (Cziko, 1989, p. 19)

The reason that it is impossible to make predictions about the future states of such a system (beyond a single step) is because these processes demonstrate sensitivity to initial conditions.

> That is, any uncertainty in the initial state of the given system, no matter how small, will lead to rapidly growing errors in any effort to predict the future behavior. (Gollub & Solomon, 1992, p. 1)

Within a chaotic system, the behaviour of particular events can be predicted only if the initial conditions are ascertainable to *an infinite degree of accuracy*--a standard which is impossible in reality. Thus in chaotic systems the source of indeterminacy is "...the clumsy nature of our measuring devices or the extremely small size of the entities that we attempt to measure" (Zukav, 1979, pp. 132-133). This is distinguishable from Heisenberg's uncertainty principle, where the indeterminacy is imposed "...by the very way that nature presents itself to us" (Zukav, 1979, p. 133). Chaos theory can therefore be considered a source of *epistemological* uncertainty because it arises from limitations inherent in the knowing subject rather than from the nature of the known object itself.

As an example, the motion of a dust particle which is floating on the surface of two oscillating whirlpools will display chaotic behavior in that it will move in well-defined circles around the centres of the whirlpools and alternate between the two in an *irregular* manner. In trying to predict the motion of the particle, an observer would have to measure its initial position. If the measurement is not infinitely precise the observer will not be able to predict the exact location of the particle at some future time. As stated above, making infinitely accurate measurements is impossible, therefore it is impossible to make a long-term projection of the trajectory of the particle (Gollub & Solomon, 1992). In other words, Hempel's (1965) assertion that a deterministic theory can be used to describe the antecedent state of a system at one time in order to exactly determine the state of the same system at any another time *does not apply to all natural systems equally*.

The Physical World: Neither Certain Nor Capricious

While some, e.g. Heisenberg and Bohr (cited in Morgan, 1983), have generalized the uncertainty or indeterminacy found at the subatomic (micro) level to all classes of events, and have asserted that its effects are only evident at the former level of being owing to the extreme minuteness of the entities that are measured there, others, e.g. Morgan (1983), contend that we must regard these arguments as *suggestive* rather than *conclusive*. I am in agreement with Morgan on this matter because, first of all, the inductive support for such a general claim (or for a hypothesis which refutes this claim) is always finite, and therefore *uncertain* (see Popper, 1965/1934); and second of all, it seems to me that, from the viewpoint of an *empiricist at least* (a designation which I assume describes both Heisenberg and Bohr), to say that there is *no discernible evidence of indeterminacy* in systems which involve entities at the macro level is the same thing as saying that this indeterminacy *does not exist*, and that therefore such systems are describable in a *determinate*, or *deductive-nomological*, fashion (see Hempel, 1965).

Hence, while we are unable to maintain the philosophy of determinism at the subatomic level, and within certain systems which demonstrate sensitivity to initial conditions (i.e., chaotic systems), this does not necessarily mean that *all* classes of scientific phenomena are indeterminate, and thereby defy attempts to explain them in a mechanistic way using causal or statistical laws. In fact, according to Asimov (1972), in most cases where indeterminacy does exist in scientific observations it "...is so small compared with the scale of the measurements involved that it can be neglected for all practical purposes" (p. 380). He also points out that uncertainty at the quantum level should not be generalized to all phenomena, and has in no way affected the stance that scientists take when investigating the natural world:

> One often reads that the principle of indeterminacy removes all certainty from nature and shows that science after all does not and never can know what is really going on, that scientific knowledge is at the mercy of the unpredictable whims of a universe in which effect does not necessarily follow cause. Whether or not this interpretation is valid from the standpoint of philosophy, the principle of uncertainty has in no way shaken the attitude of scientists toward scientific investigation. If, for instance, the behavior of the individual molecules in a gas cannot be predicted with certainty, nevertheless on the average the molecules do obey certain laws, and their behavior can be predicted on a statistical basis, just as insurance companies can calculate reliable mortality tables even though it is impossible to predict when any particular individual will die. (Asimov, 1972, p. 380)

In other words, even though subatomic and chaotic phenomena demonstrate some degree of indeterminacy, irregularity, or unpredictability, this does not mean that they are *completely* capricious, arbitrary or uncertain (Skinner, 1982/1953). We can still attempt to describe and predict the behaviour of these phenomena using probabilistic statements. In fact, it is specifically for events such as these that Hempel (1965) has detailed his second form of *scientific* (not *prescientific*) explanation, i.e., inductive-statistical arguments which utilize probabilistic, law-like propositions rather than deductive-nomological, or determinate, laws.

Cziko (1989) agrees with Asimov that the "probabilistic randomness" which exists at the quantum level should not be generalized to all physical phenomena. In his view, to state that *all* physical events--at the micro *and* macro levels--are indeterminate and unpredictable hardly seems consistent with the incredible explanation, prediction and control of the physical world which has been achieved by science this century. Cziko (1989) states that with most scientific phenomena (i.e., those investigated at the macro level such as rocket engines and computer chips), if randomness does exist, its effects are negligible because "at the macro level we are dealing with large agglomerations of particles and atoms whose quantum effects in a sense cancel each other out" (p. 22). Interestingly, Cziko (1989) speculates that because of the ever-increasing miniaturization of computer and other electronic components, we may one day see the quantum randomness of subatomic particles manifest itself in malfunctions of these technologies.

To conclude, it must be stated that virtually all phenomena (natural, social and psychological) exhibit *some* regularities which can be isolated, abstracted and mathematically described--even if only in an uncertain, statistical manner. However, as *suggested* by Heisenberg's uncertainty principle, and later established by the postpositivistic philosophy of science, this basic ordering of phenomena reveals itself to inquirers in a diversity of ways "...according to the structures of thought and experience through which they are engaged and known" (Morgan, 1983, p. 387).[2]

Indeterminacy and Unpredictability in Social Science

Skinner (1982/1953), Hempel (1965), Dewey (1929), and Morgan (1983) agree that the subject matters of the social sciences are more complex and less deterministic than those of the natural sciences. Skinner (1982/1953) states the following about the scientific explanation of behaviour:

> Since [behavior] is a process, rather than a thing, it cannot easily be held still for observation. It is changing, fluid, and evanescent, and for this reason it makes great technical demands upon the ingenuity and energy of the scientist. (p. 46)

Hempel (1965) concedes that the regularities invoked to explain the behaviour of human subjects "...cannot be stated with the same generality and precision as in physics or chemistry..." (p. 251). Dewey (1929) asserts that the physical sciences "...deal with subjects that are intrinsically less complex, involving fewer variables" than those dealt with in the social sciences (p. 22). Furthermore, Dewey (1929)

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[2]I will further explicate this point below in the subsection entitled *Perception Yields Culturally-Determined Facts*.

agrees with Skinner that the phenomena of social science are fluid and changing, and not uniform and standard. Aside from the variables of the social sciences being more numerous (i.e., social systems are more 'open' than natural systems and therefore their states are 'determined' by a greater number of variables which often interact), and less fixed or determined, both Hempel (1965) and Morgan (1983) point out that they are also considered by many to be less *perceptible or concrete* than their physical counterparts. Despite this agreement in principle that social and psychological phenomena are less deterministic than physical events, these theorists take different approaches in dealing with this uncertainty: on the one hand, Skinner and Hempel maintain that the social sciences can and must use the same structure of explanation as the natural sciences; and on the other, Dewey and Morgan contend that these uncertainties suggest a need for a *plurality* of research methods.

Foundationalism Despite Uncertainty

Both Hempel (1965) and Skinner (1982/1953) reject the argument that a social or behavioural science is impossible due to the fact that human events are unique, idiosyncratic, irrepeatable and therefore inaccessible to general, causal explanation. Hempel (1965) answers this objection by stating that all events subsumed under general laws, including physical phenomena, are unique and irrepeatable. With all events, natural or human, what is explained is "...some more or less complex characteristic in a specific spatio-temporal location or in a certain individual object, and not...*all* the characteristics of that object, or...all that goes on in that space-time region" (Hempel, 1965, p. 253). For his part, Skinner (1982/1953) argues that in all incipient sciences it is by no means clear at the outset that general laws will emerge which are capable of explaining particular cases. For example, this argument was used in the nascent stage of medical science, but later faded as single cases began to be discussed in general terms by referring to factors common to many cases. Therefore, on this view, as a science of man progresses and the ramifications of its explanations become clear, the argument that it is impossible to subsume unique human events under general laws will lose its forcefulness. As we saw in Chapter One, the ramifications of the early results of quantitative inquiries in education only became clear, according to Gage (1978), when these generally weak and inconclusive results were subjected to new techniques of meta-analysis which demonstrated that research on teaching had produced statistically-significant results.

A second objection to a science of man mentioned by Hempel (1965) is that phenomena involving purposive behaviour require 'motivational' (i.e., teleological) analysis rather than 'causal' explanation. Hempel rebuts this objection by stating that the motives of human beings can be treated as causes. A motivational explanation, if adequately formulated, meets all the conditions for a causal explanation, and is, in fact, "a causal explanation in which some antecedent conditions are the motives of the agent whose actions are being explained" (Hempel, 1965, p. 255). In other words, motives and beliefs can be determined, operationally defined, and classified among the antecedent conditions of a motivational explanation, and such an explanation is formally analogous to a causal explanation.

A third argument against applying scientific explanation to social or psychological events is that most of these phenomena, which involve the internal states of human beings, are not subject to direct observation (i.e., they are less concrete) and therefore cannot be precisely determined. Hempel (1965) insists that this is not essentially different from "...determining factors adduced in physical explanations..." which are also "...very frequently inaccessible to direct observation" (p. 254). In such cases, methods of indirect test are used to guarantee the empirical nature of an explanation; however, these methods must be "...'operationally determined' with reasonable clarity and precision..." (Hempel, 1965, p. 255). Skinner's behavioural science provides an example of the operationalization of internalized psychological states as publicly observable variables which are then used in 'causal' explanations (Kalin, 1992).

A fourth problem with a science of man involves the extraordinary complexity of behaviour: "Even though behavior may be lawful, it may be too complex to be dealt with in terms of law" (Skinner, 1982/1953, p. 50). Skinner (1982/1953) responds that such statements are about the limitations of scientists or their aspirations rather than about the suitability of applying scientific methods in the study of behaviour.

> We often succeed in reducing complexity to a reasonable degree by simplifying conditions in the laboratory; but where this is impossible, a statistical analysis may be used to achieve an inferior, but in many ways acceptable, prediction. (Skinner, 1982/1953, p. 51)

In short, "apparently hopeless cases often become manageable in time" (Skinner, 1982/1953, pp. 50-51). Indeed, Thorndike (1965/1910) goes so far as to describe "school-room life..." as "...a vast laboratory in which are made thousands of experiments of the utmost interest to 'pure psychology'"; thereby suggesting the possibility of controlling the complexity of the classroom, and the desirability of conducting scientific inquiries in such natural settings (p. 182).

A fifth problem mentioned and discredited is that there exist insurmountable control problems in social science which are not faced by natural scientists:

> Even if we assume that behavior is lawful and that the methods of science will reveal the rules which govern it, we may be unable to make any technological use of these rules unless certain conditions can be brought under control. (Skinner, 1982/1953, p. 51)

Skinner (1982/1953) responds that lab situations can simplify some conditions and eliminate irrelevant ones. However, if the results of these studies are to apply in contexts where comparable simplification and control are impossible, then it would seem that these results are of little value. To this Skinner (1982/1953) answers that such scientific studies will enable us to make optimal use of that control we *do* possess: "the laboratory simplification reveals the relevance of factors which we might otherwise overlook" (p. 52). As well, Skinner points out that we can also control many conditions relevant to human behavior, e.g. the conditions of work in schools, which will allow us to generalize laboratory results to these contexts. Thorndike (1965/1910) agrees that experimental results are directly generalizable to classroom contexts: "...laws derived by psychology from simple, specially arranged experiments help us to interpret and control mental action under the conditions of school-room life" (p. 182).

Both Skinner and Thorndike, then, do not consider the complexity and control problems inherent in social and psychological phenomena to be insurmountable impediments to a science of behaviour. Using laboratory experiments, they believe it is possible to reduce the complexity of such phenomena by controlling certain variables and eliminating others. In this way, laws of behaviour are derived which can supposedly be used to control and predict behaviour in practical contexts.

More recently, in the field of education, Campbell & Stanley (1963) realized that simplifying phenomena in lab experiments by limiting the number of variables dealt with concomitantly reduces the utility of the results obtained for real classrooms because in these contexts it is not usually possible to control or artificially limit the high number of variables present in order to match the conditions which existed during the original experiment. An important distinction they make is between the *internal validity* and the *external validity* of an experimental result. The former is concerned with the following question: Is the observed effect attributable to the experimental treatment, or did some undetermined extraneous variable--alone or in concert with any other variable--actually cause the event? The latter is concerned with the following questions: Is the result generalizable? And if so, to which population(s) and setting(s)? While both types of criteria are important and must be stressed, the first, internal validity, is usually considered more significant because it relates to the *certainty* of an explanation. As well, Campbell and Stanley (1963) point out that the two criteria "...are frequently at odds in that features increasing one may jeopardize the other" (p. 175). Following Campbell's & Stanley's lead, quantitative researchers in education such as Gage (1978) and Brophy & Good (1986) have surrendered some control over variables in taking their inquiries out of the laboratory and into the classroom, all in the interest of obtaining results which are more ecologically valid (i.e., applicable in particular contexts). Whether the control offered by the laboratory is stressed, or a balance between internal and external validity is emphasized by taking research into more natural settings, the ultimate goal of all positivists in social scientific research is/has been to produce an orderly, scientific knowledge base (i.e., one which establishes cause-effect relationships between variables) concerning all manner of social and psychological phenomena by use of the controlled experiment.

To recapitulate to here: despite the complexity and greater indeterminacy of social contexts and psychological phenomena, Hempel and Skinner (among others) consider the causal type of explanation to be adequate (indeed *mandatory* for the assemblage of a valid knowledge base which will allow for the control and prediction of these sorts of events) in fields which deal with such occurrences. They argue that: (a) all phenomena, natural and human, are unique in some degree, but nevertheless contain regularities which can be subsumed under causal laws; (b) human motivations or intentions can be treated as causes and stated as antecedent conditions in causal explanations; (c) unobservable phenomena such as beliefs and motivations can be operationalized as observable events and subjected to indirect tests to determine the causal relations between them and other events; (d) the complexity of social or psychological phenomena can be reduced or simplified, and variables can be isolated and measured, in order to achieve adequate explanations and predictions; and finally (e) conditions in practical contexts can be controlled enough to allow the technological use of scientific rules. In short, on this view, although social phenomena exhibit greater complexity and instability than physical phenomena, "...there is nothing essentially insoluble about the problems which arise from this fact" (Skinner, 1982/1953, p. 46). However, in spite of this faith in the possibility of obtaining causal explanations or predictions in the human sciences which can be used to control behaviour in a technical fashion, I shall illustrate below that the actual conduct and technical use of scientific research in the social sciences have been severely limited by the complexity and indeterminacy of most social and psychological events. In practice, scientists have found it exceedingly difficult to bring such phenomena under control, and thus to causally explain and predict them.

Pluralism As a Response to Uncertainty

In Chapter Two I outlined Toulmin's (1990) assertion that the rationalist dream of a unified body of scientific knowledge, comprising all classes of phenomena (natural and human), and structured in a formal, or geometric, manner (i.e., in terms of deterministic laws explaining the mechanistic workings of systems) was untenable due to the fact that scientists, in extending the range of inquiry into new realms, were forced by experience to use a diversity of methods which matched the characteristic demands of these unique problem-fields.

> The task is not to build new, more comprehensive systems of theory with universal and timeless relevance, but to limit the scope of even the best-framed theories, and fight the intellectual reductionism that became entrenched during the ascendancy of rationalism. The intellectual tasks for a science in which all the branches are accepted as equally serious call for more subdisciplinary, transdisciplinary, and multidisciplinary reasoning. Like the informal procedures of the common law when it is functioning at its best, these interlocking modes of investigation and explanation check exaggerated claims on behalf of all universal theories, and reinstate respect for the pragmatic methods appropriate in dealing with concrete human problems. (Toulmin, 1990, p. 193)

Here, I wish to demonstrate that the emergence of methodological pluralism in social science was a direct result of first the indeterminate nature of many (some would say *most*) social and psychological phenomena, and second the pragmatic approach adopted by social scientists who have recognized the need, in every science, "...to employ those specific methods that have proved, in concrete experience, to match the characteristic demands of its own intellectual problems" (Toulmin, 1990, p. 193).

As I mentioned in Chapter Two, Durkheim (1938/1895) realized early on that Comte's (1947/1842) foundationalism, which called for the application of direct, or artificial, experimentation in social science (i.e., the method used in physics), was an impossible ideal:

> We have only one way to demonstrate that a given phenomenon is the cause of another, viz., to compare the cases in which they are simultaneously present or absent, to see if the variations they present in these different combinations of circumstances indicate that one depends on the other. When they can be artificially produced at the will of the observer, the method is that of experiment, properly so called. When, on the contrary, the production of facts is not within our control and we can only bring them together in the way that they have been spontaneously produced, the method employed is that of indirect experiment, or the comparative method. (p. 125)

While in Durkheim's (1938/1895) opinion the sole purpose of sociological explanation was to establish relations of *causality* between social facts, he realized that such phenomena "...escape the control of the experimenter..." and that therefore "...the comparative method is the only one suited to sociology" (p. 125).

Durkheim (1938/1895) points out that there is more than one type of comparative method: e.g., the method of agreement, the method of difference, and the method of concomitant variation (or correlation). The first two methods mentioned "...suppose that the causes compared either agree or differ by one single point" (Durkheim, 1938/1895, p. 129). In other words, they control or eliminate all adventitious or extraneous elements which might alternatively be adduced to have caused a certain effect. However, in sociology, such control is impossible owing to the great complexity of the phenomena studied:

> As an even approximately complete inventory could not be made of all the facts which coexist within a given society or which have succeeded one another in the course of its history, one can never be even approximately certain that two societies agree or differ in all respects save one. (Durkheim, 1938/1895, p. 130)

Therefore, Durkheim concedes that sociological inquiry must rely on a single scientific method: that of 'concomitant variation' or 'correlation.'

> For this method to be reliable, it is not necessary that all the variables differing from those which we are comparing shall have been strictly excluded. The mere parallelism of the series of values presented by the two phenomena, provided that it has been established in a sufficient number and variety of cases, is proof that a relationship exists between them. (Durkheim, 1938/1895, pp. 130-131)

Nevertheless, it must be remembered that establishing a correlation between two events does not indicate, first, that a causal relationship is present; and second, if one exists, which is the cause and which is the effect.

> It is true that the laws established by this procedure are not always presented at the outset in the form of relations of causality. The concomitance may be due not to the fact that one phenomenon is cause of the other but to the fact that they are both the effects of the same cause, or, again, that there exists between them a third phenomenon, interposed but unperceived, which is the effect of the first and the cause of the second. The results to which this method leads need, therefore, to be interpreted. But what experimental method is there which obtains mechanically a relation of causality without some analysis of the observed data? (Durkheim, 1938/1895, pp. 131-132)

Here, Durkheim seems to be attempting to justify the application of the scientific method to complex social phenomena by indicating that *all* scientific results require some drawing of inferences by researchers--this in spite of the fact that the less certain correlational results of sociological inquiry will require a much higher level of interpretation on the part of the researcher than the causal laws of the natural sciences.[3]

Durkheim (1938/1895) also seems to feel that it is permissible to infer a causal relationship from a correlational result as he asserts (on p. 130) that concomitant variations display 'causal' relationships. Moreover, he states that:

> As soon as one has proved that, in a certain number of cases, two phenomena vary with one another, *one is certain of being in the presence of a law* [italics added]. (Durkheim, 1938/1895, p. 133)

Clearly correlational results cannot be considered to be 'laws' as this

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[3]See the sections which deal with 'the underdetermination of theory by logic' and 'the underdetermination of theory by experience' below for more discussion on the overwhelming importance of human judgment in the conduct of inquiry *and* the interpretation of its results.

designation implies a deterministic relationship between two variables. As Durkheim seems to realize (see the block quote from pp. 131-132 reproduced above), such explanations are incomplete as they indicate some connection between observables, but tell us nothing about causality (see Hempel, 1965). Is he attempting to lend more credibility or certainty to the results of sociological inquiry by referring to them as 'laws' and considering them to demonstrate 'causal' relations? Or does he make consistent category errors? While we may never know the answers to these questions, what is clear is that any causal relationship inferred from a correlational result would be highly uncertain. Notwithstanding this, it is clear that Durkheim held fast to the dream of having "...a definitive, objective science for all society that would eventually produce *the* system of laws" (Smith, 1983, p. 8). At present, it seems likely that the impossibility of exactly determining all of the variables which affect a social system will remain an intractable problem of positivistic sociological inquiry.

What Durkheim discovered in applying the scientific method to social phenomena is that while *some* regularities exist in human nature which are quantifiable and generalizable they are often not stateable in terms of *deterministic* relations of events, or causal laws. This is because social and psychological events are influenced by a vast number of variables which are largely indeterminate; that is to say, each individual and context is itself unique, and is 'influenced' by a distinctive, idiosyncratic, unstable, and often undetermined matrix of environmental factors: e.g., the physical surroundings, the unique interactions between different personalities, social/family/individual norms, values, and meanings, and etc. *Even if* laws--by definition general and abstract--*are* formulated which describe specific regularities of behaviour or of social contexts (e.g., the classroom), they will often prove to have limited usefulness in helping a practitioner deal with his/her practical situations, which rarely, if ever, present themselves in the reduced or simplified form in which the scientific result was obtained. In other words, knowledge obtained by cutting the complexity of social or psychological phenomena in order to ensure the certainty of scientific results has limited usefulness in informing the decision-making of practitioners who must, of necessity, confront the complexity, instability, and uncertainty of their practical situations (see Schön, 1983). As I mentioned in Chapter One, and as we shall rediscover below, this mismatch between the form of representation embraced by scientists (general laws) and the reality of practical contexts (complex, unstable, unique and value-laden) has led to the propagation of new research methods and forms of representation in the sciences of man.

Dewey (1929) agrees that the control and isolation of variables which is possible in the laboratory is not feasible in social settings, such as educational contexts, and that therefore, forms of knowledge other than the general and abstract propositions of scientific inquiry are needed to effect wise practical action.

> The control of conditions demanded by laboratory work leads to a maximum of isolation of a few factors from other conditions. The scientific result is rigidly limited to what is established with these other conditions excluded. In educating individualities, no such exclusion can be had. The number of variables that enter in is enormous. The intelligence of the teacher is dependent upon the extent in which he takes into account the variables that are not

> obviously involved in his immediate special task. Judgment in such matters is of qualitative situations and must itself be qualitative. (Dewey, 1929, p. 65)

Thus the large number of variables present in social contexts, *and* the interdependence of these factors, places limits on the exactness of quantitative measurements in the social sciences: "That which can be measured is the specific, and that which is specific is that which can be isolated" (Dewey, 1929, p. 64). As well, the uniqueness of social and educational events--caused by the distinctive configuration of interacting variables present in each situation--prevents the generalization of a causal law to every case which is supposedly covered by some such universal statement.

> The parent and educator deal with situations that never repeat one another. Exact quantitative determinations are far from meeting the demands of such situations, for they presuppose repetitions and exact uniformities. Exaggeration of their importance tends to cramp judgment, to substitute uniform rules for the free play of thought, and to emphasize the mechanical factors that also exist in schools. (Dewey, 1929, pp. 65-66)

In short, Dewey (1929) suggests that alternative methods of representing the complex phenomena of social contexts are needed in order to reveal potentialities (e.g., the practical wisdom of expert teachers) which are not captured by abstract, nomothetic, deterministic explanations of events.[4]

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[4]As we shall see in Chapter Six, Dewey not only presaged the eventual development of forms of representation in education which capture the essential *qualities* of classrooms (i.e., by qualitative or interpretive research methods), but also inspired a movement for deliberation in action (see Schwab, 1975, 1978c/1970, 1978d/1971, 1978e/1973, 1983), or reflective practice (see Schön, 1983, 1987, 1989), by insisting that *all* thought, undertaken either for theoretical or practical ends, can be considered scientific in nature (see Dewey, 1966/1916).

Morgan (1983), like Dewey, maintains that the social world is a realm which is far less concrete, predictable, and certain than the physical world; indeed, he considers the ontological uncertainty which exists in quantum physics to be a defining feature of everyday social reality:

> ...it is easy to see that social life has an unpredictable rather than certain character, in that the actions of human beings are always drawn from a set of wider possibilities and the most routine situations are frequently disrupted and transformed by unexpected circumstances, since realities typically unfold according to the contingencies present in the way circumstances are constructed or develop. We can also see how the very same surface reality may embody many different meanings, some of which may be complementary and others contradictory, as when an action signifying genuine friendship on one occasion may on another be hollow and perfunctory, and on yet another, be used as a manipulative ploy. As we examine the social world, we can easily see that the kind of uncertainty that modern physicists suggest is a characteristic of the material world has a direct and obvious parallel in the nature of the social world. (Morgan, 1983, p. 388)

This quote suggests two main complexities which distinguish social phenomena from physical phenomena. First, Morgan contends that human behaviour is drawn from, and influenced or transformed by, a wide set of possibilities and unexpected circumstances (i.e., variables). This is synonymous with saying that social systems, composed of many interacting individuals who are influenced by many interacting factors are--to borrow Taylor's (1982/1971) terminology--more 'open' than natural systems; in other words, the number of variables which enter into a potential explanation of a social or psychological event are virtually unlimited and therefore largely undetermined. "...We cannot shield a certain domain of human events, the psychological, economic, political, from external interference; it is impossible to delineate a closed system" (Taylor, 1982/1971, p. 183). (And even if all the interacting variables relevant to an explanation could be determined and isolated in one setting, it is highly unlikely that they would appear in exactly the same configuration in another context, or that the complex of variables present in other circumstances could be controlled by practitioners to an extent that the original scientific result would generalize to that milieu.)

Second, Morgan (1983) intimates that social realities, unlike physical realities (i.e., inanimate, unthinking 'objects'), are *constructed and continually reconstructed* by their constituent members (i.e., 'subjects' with free will and the ability to forge their own meanings); therefore, a single surface behaviour--what Taylor (1982/1971) refers to as a 'brute datum'--can be the incarnation of many possible meanings or intentions. Taylor (1982/1971) refers to the ability of humans in society to create their own meanings as the 'self-defining' nature of man. The fact that human beings have free will and self-consciousness precludes the possibility of exact measurement and prediction of future behaviour, nor is man's behaviour determined by antecedent conditions in a deterministic fashion. In addition to this, "...the conceptual mutations in human history can and frequently do produce conceptual webs which are incommensurable, that is, where the terms can't be defined in relation to a common stratum of expressions" (Taylor, 1982/1971, p. 183). Thus the notion of 'bargaining' can have one meaning in our society and signify something entirely different in certain primitive cultures. This means that a condition of explanation and prediction that exists in natural science--"...that all states of the system, past and future, can be described in the same range of concepts..."--is missing in social science (Taylor, 1982/1971, p. 183).

In short, although a human motivation or intention *can* be inferred from an observable event, e.g. a 'behaviour,' and such an observable behaviour can subsequently be treated as a 'cause' in an explanation (see Hempel, 1965; Skinner, 1982/1953), (a) there exist many different ways that a scientist *could* conceptualize and treat that behaviour, and the choice of the 'correct' version (paradigm) is underdetermined by experience (a source of epistemological uncertainty), and (b) not only is the reality of this social phenomenon a function of how the scientist chooses to engage it, it is also a function of the meaning--of which many are possible--that the acting subject attributes to his or her own behaviour, and of the historical changes in signification that occur in all cultures (sources of ontological uncertainty) (Morgan, 1983; Taylor, 1982/1971).

Morgan (1983) submits that a social reality, e.g. a behaviour, can be subjected to many different 'modes of engagement' (what Kuhn [1970] calls 'paradigms'), which will in turn reveal different potentialities in a single phenomenon. The application of scientific methods to such a phenomenon may reveal that regularities--most often expressed as correlations, and less often represented as causal relations (due to the complexity of social phenomena)--exist between this behaviour and other social or psychological facts. However, as Bredo and Feinberg (1982) point out, this emphasis on the scientist's conceptualization of a surface behaviour and its relation to other brute facts ignores the meaning that the actor attributes to his/her own action, and, beyond that, the meaning that an entire culture ascribes to such an action. The complexity of social reality, and the perceived need to treat actions or behaviours not as unchanging, lifeless things (i.e., as 'objects'), but as the outward manifestations of *sentient subjects who construct (and continually reconstruct) their own meanings alone or in concert*, has led to a new way of conceptualizing and studying this reality--a research paradigm known as 'interpretivism' which is constructed of an entirely different internal logic than is scientific rationalism.

The presuppositions of the interpretivist research paradigm, which uses qualitative methods of inquiry, were first mapped out in Wilhelm Dilthey's (1976a/1894, 1976b/1900, 1976c/1907, 1976d/1910, 1976e/1911, 1988/1923) late 19th and early 20th-century philosophy of hermeneutics. Dilthey (1976c/1907), like Comte, Durkheim and other supporters of the modern presuppositional scaffolding, rejected metaphysical speculation in philosophy because it produces 'universal' knowledge through *a priorism*:

> Its aim is to solve the enigma of the world and of life but to do so in universally valid form. One of its faces is turned towards religion and poetry, the other towards the sciences. But it is neither a science in this sense, nor is it art or religion. It comes into existence on the basis of the presupposition that there is an aspect of the secret of life which is accessible to disciplined thought. (p. 122)

According to Dilthey (1988/1923) such speculation produces a variety of competing, and therefore uncertain, worldviews (or *weltanschauung*) that transcend experience: "It is not possible to demonstrate a definite inner objective structure of reality, such that remaining possible structures are excluded" (p. 319). Dilthey (1976c/1907) welcomed the decline of metaphysics in the modern age and, like Descartes, believed that philosophy should be reduced to epistemology:

> ...philosophy is the *basic discipline* of which the subject-matter is the forms, rules and relationships of all thought processes which aim at valid knowledge. In the form of logic it investigates the conditions for validity inherent in all correct thought processes whatever the sphere of thought. In the form of epistemology it traces the consciousness of the reality of experience and of the objectivity of external perception back to the justification of these presuppositions of knowledge. As such a *theory of knowledge* it is a science. (p. 125)

He also believed that the natural sciences, or *naturwissenschaften*, involved the building of knowledge through passive observation of objectively knowable, unchanging natural phenomena.

> We are able to control the physical world by studying its laws. These can only be discovered if the way we experience nature, our involvement with it, and the living feeling with which we enjoy it, recedes behind the abstract apprehension of the world in terms of space, time, mass and motion. All these factors combine to make man efface himself so that, from his impressions, he can map out this great object, nature, as a structure governed by laws. (Dilthey, 1976d/1910, p. 172)

Dilthey, then, was thoroughly 'modern' in his outlook in that he agreed with the positivists that the uncertain metaphysical constructs of the past must be swept away, and that knowledge of the physical world must begin from the neutral scratch point provided by empirical observation and the logical analysis of theories.

However, there are two main areas where Dilthey's philosophy differed significantly from that of positivist-inclined contemporaries such as Comte and Durkheim (and later Skinner and Hempel): first, he considered the ontology of the human world to be fundamentally different from that of the natural world; and second, as a result of these ontological differences, he felt that an *additional* logic of inquiry would be required in the human sciences (or *geisteswissenschaften*) in order to reveal the true nature of social phenomena. In other words, he was an ***epistemological and methodological pluralist***.

Dilthey (1988/1923), like Dewey and Morgan, understood that any "uniformities we can establish in the sphere of society are far inferior in number, significance, and formal precision to laws we can determine about nature..." (p. 97). This is so because of the intractable complexity of the problem-fields explored by the sciences of society.

> The difficulties in knowing a single psychical entity are multiplied by the great varieties and uniqueness of these entities, by the way they work together in society, by the complexity of natural conditions which bind them together, and by the sum total of mutual influences brought to bear in the succession of many generations, which does not allow us to deduce directly from human nature as we now know it the state of affairs of earlier times or to infer present states of affairs from a general type of human nature...(Dilthey, 1988/1923, pp. 97-98)

Dilthey (1988/1923) understood that human beings are self-conscious, and have free will, motivations, and intentions that distinguish them from matter. As a result, he rejected the notion, later espoused by Hempel (1965), that human motivations can be treated as 'causes' in scientific explanations. In Dilthey's (1976d/1910) view, the mental states and behaviour of a human being are not determined by mechanistic cause-effect relationships, but are instead 'influenced' by 'systems of interactions', by "...such composite structures as communities or cultural systems...", and by "...relation[s] of the whole person to things and people..." (pp. 201-202).

> The individual is an element in interactions, reacting to their impulses with deliberate direction of will and action....The play of what are for us soulless active causes is cut off here from the play of concepts, feelings, and motives. And the uniqueness and wealth of reciprocal

> influences disclosed here are limitless. A waterfall is composed of homogeneous falling water droplets; but a single declaration--which is after all only a breath of air from the mouth--shocks the entire living population of a continent through a play of motives in purely individual human beings: so different is the mutual effect appearing here, namely, motivation arising out of thought, and from every other kind of cause! (Dilthey, 1988/1923, p. 98)

It must be remembered, however, that such influences and interactions are never so powerful that behaviour is entirely determined by them: "...a man finds in his self-consciousness a sovereignty of will, a responsibility for actions, a capacity for subordinating everything to thought and for resisting any foreign element in the citadel of freedom in his person: by these things he distinguishes himself from all of nature" (Dilthey, 1988/1923, p. 79).[5]

According to Dilthey (1976d/1910), the human-social-historical world resists *explanation* using *general laws* because what is universally human cannot be divorced from individuality. A human being by nature is a 'psycho-physical' whole; a mental as well as a physical creature; or a 'structural system of interactions' with things, people, communities and cultural systems *superimposed by thought and free will*. While there are some elements of human experience that are universal, these cannot be

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[5]Cziko (1989) agrees with Dilthey that humans transcend the determinism of the physical world because of their consciousness and free will which regulate behaviour. On this view, it is impossible for a scientist, given knowledge of a set of antecedent conditions, to predict what an individual's behaviour will be. The first reason for this is because inner human consciousness cannot be known by researchers: it is quite impossible to 'read someone's mind' to determine their thought processes and motivations in the past, present, or future. Secondly, human behaviour, regulated by our consciousness, is complex and creative. Our free will transcends attempts to determine or predict our behaviour. For example, if I am aware of attempts by a scientist, armed with an understanding of the structure and activity of my brain, to predict that I will read a book on Tuesday evening, then I can simply decide not to do so to prove him wrong.

separated from the active parts of a person (i.e., the ability to think, to conceptualize, to form attitudes, values and purposes) which are wholly individual and cannot be measured, only estimated. On the macro level, each individual culture is different from all others, and all are constantly evolving with time. This makes it impossible to discover laws like those in the natural sciences which hold in any time or place. Furthermore, while the objects of inquiry in the natural sciences can be counted or measured (directly or indirectly), the inner system of interactions in human beings is laid upon by a level of thought and understanding. As a result of this, scientists limited to observing the outward manifestations of these mental processes (i.e., behaviours) would be required to draw substantial inferences when trying to determine their meaning. Humans--including scientists--experience and understand the system of interactions of which they are a part, and to which they contribute, but this ability to experience and understand the social milieu does not "...present us with homogeneous systems in which laws of change can be discovered" (Dilthey, 1976d/1910, p. 202).

> Understanding reveals common features and resemblances as well as innumerable nuances of differentiation from the great differences between races, tribes and nations to the infinite variety of individuals. This is why, in the mind-constructed world, we are predominantly concerned with the comprehension of individuality--from that of the individual person to mankind as an entity--...(while in the sciences laws of change predominate). (Dilthey, 1976d/1910, p. 202)

Thus, on this view, because of the individuality and uniqueness of each person on a micro level, and each culture on a macro level, humanity is an *indeterminate type* that defies subsumption under general laws (Dilthey, 1976d/1910).[6]

In terms of his epistemology, Dilthey rejected the foundationalism of the natural sciences espoused by the positivists and devoted himself to outlining an additional methodology for the sciences of society. He viewed human beings as *psycho-physical units* who must be studied using methods appropriate for (a) explaining matter (*erklären*), and (b) understanding the mind-constructed world (*verstehen*); in short, he was a methodological pluralist. Although Dilthey (1976d/1910) made distinctions between the physical and the mental, and between the methodology of the natural sciences (*naturwissenschaften*) and that of the social sciences (*geisteswissenschaften* or 'the study of mind'), he did not embrace a dual ontology. He believed a human being to be a psycho-physical unit in which mind and matter are inextricably bound.

> The germinal cell of the historical world is the experience in which the subject discovers himself in a dynamic relationship with his environment. The environment acts on the subject and is acted upon by him. It is composed of the physical and cultural surroundings. In every part of the historical world there exists, therefore, the same dynamic connection between a sequence of mental events and an environment. This is why the influence both nature and his cultural environment have on man must be ascertained and evaluated. (Dilthey, 1976d/1910, p. 203)

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[6]More recently, Cronbach & Snow (cited in Cziko, 1989) have argued that educational treatments interact with individual aptitudes in such a way that an approach which is effective with one type of learner will be ineffective with another type of learner. To further complicate things, these 'aptitude-treatment interactions' (ATI) may also interact with variables such as 'time.' As a result, an approach which was effective with one type of learner may with time become less productive with that type, and more productive with another type. As a result of individual differences, then, "...the most we can ever realistically hope to achieve in educational research is not prediction and control but rather only temporary understanding" (Cziko, 1989, p. 17).

Hence, Dilthey's *geisteswissenschaften* is also correctly interpreted as meaning the 'human sciences': modes of inquiry which include the methods of the natural sciences (see Rickman, 1976). For example, the study of speech involves both the *explanation* of "...the physiology of the speech-organs..." and the *understanding* of "...the semantics of words and sentences" (Dilthey, 1976d/1910, p. 172). In a conversation between two people "...the system of nature...acts as a transit point for influencing another mind....[Here] knowledge of natural sciences blends with that of the human sciences" (Dilthey, 1988/1923, p. 86). Although Dilthey considered the methods of the human and natural sciences to be complementary, he rejected the positivists' subordination of intellectual events to the system of nature. Intellectual processes, imbued with free will, thought, feelings, values and goals, must not be reduced "...to mere characteristics or facets of matter"; in other words, the "...incommensurability of intellectual life with matter" must be respected (Dilthey, 1988/1923, p.82).[7]

In Dilthey's view, the common subject of the human studies is 'human-social-historical reality' or 'the mind-constructed world' and its 'expressions' or 'objectifications of life.' The inner, mental lives of individuals, sub-groups and cultures are objectified through

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[7]See Toulmin (1982) for a discussion of how, in 'postmodern science,' Dilthey's distinction between *Natur* and *Geist*, between the *objectivity* of the physical world which can be passively observed and explained, and the inherent *subjectivity* of human states of mind which require critical interpretation to be understood, has disappeared. The modern ideal of the detached, objective onlooker had been superseded by the postmodern realization that scientists are participant-observers, that "the doctrines of the natural sciences are critical interpretations of their subject matter, no less than those of the humanities" (Toulmin, 1982, p. 95).

expressions which take a variety of forms: "The...human spirit speaks to us from stone, marble, musical compositions, gestures, words and writings, from actions, economic arrangements and constitutions..." (Dilthey, 1976b/1900, p. 248). The mind-constructed world is a system of interactions whose creations--expressions or objectifications of life--form a shared historical or cultural world that is understood by individuals who have been submerged in it from birth.

> I have shown how significant the objective mind is for the possibility of knowledge in the human studies. By this I mean the manifold forms in which what individuals hold in common have objectified themselves in the world of the senses. In this objective mind the past is a permanently enduring present for us. Its realm extends from the style of life and the forms of social intercourse to the system of purposes which society has created for itself and to custom, law, state, religion, art, science and philosophy. For even the work of genius represents ideas, feelings and ideals commonly held in an age and environment. From this world of objective mind the self receives sustenance from earliest childhood. It is the medium in which the understanding of other people and their expressions takes place. For everything in which the mind has objectified itself contains something held in common by the I and the Thou. Every square planted with trees, every room in which seats are arranged, is intelligible to us from our infancy because human planning, arranging and valuing--common to all of us--have assigned a place to every square and every object in the room. The child grows up within the order and customs of the family which it shares with other members and its mother's orders are accepted in this context. Before it learns to talk it is already wholly immersed in that common medium. It learns to understand the gestures and facial expressions, movements and exclamations, words and sentences, only because it encounters them always in the same form and in the same relation to what they mean and express. Thus the individual orientates himself in the world of objective mind. (Dilthey, 1976d/1910, pp. 221-222)

Taylor (1982/1971) uses the term 'intersubjective meanings' to describe this shared world of objective mind. The meanings and norms implicit in cultural or institutional practices exist not only in the minds of individuals, but are also manifested in the external world as

"...intersubjective meanings which are constitutive of the social matrix...", or as "...practices which cannot be conceived as a set of individual actions, but which are essentially modes of social relation, of mutual action" (Taylor, 1982/1971, p. 173). One such practice is 'negotiation' which, in the west, is a process which recognizes the distinctness and autonomy of the negotiating parties, and is adversarial in nature with each party attempting to obtain the best possible terms. Any member of this civilization involved in negotiations is able to relate to his/her adversary because of the shared cultural meanings (i.e., ideas and norms) that are constitutive of this activity (Taylor, 1982/1971).

Dilthey (1976d/1910) recognized the existence of a plurality of cultures and institutions with their own distinctive objectifications of life or intersubjective meanings.

> The objectification of life contains in itself many differentiated systems. From the distinctions of race down to the difference of expressions and customs of a people or, indeed, a country town, there exist natural divisions based on mental differences. Differentiations of another kind arise in the cultural systems; yet others distinguish ages from each other; in short, many lines, which mark out areas of related life from some point of view or other, traverse the world of objective mind and cross in it. The fullness of life expresses itself in innumerable nuances and can only be understood through the recurrence of these differences. (Dilthey, 1976d/1910, pp. 191-192)

One result of this diversity of mind-constructed worlds is that one concept might have different meanings in different societies. For example, Taylor (1982/1971) discusses how the notion of 'negotiation' in traditional Japanese society is quite different from that in the West: Instead of a practice in which parties pursue their own aims and attempt to gain as many concessions as possible from adversaries, traditional

Japanese society viewed negotiation as an exercise in consensus building for the good of society. As a result, a western distinction related to bargaining such as 'negotiating in good or bad faith' would have no place here.

As we saw above, according to Dilthey the aim of the human sciences is *verstehen* or understanding, and not explanation. Verstehen is only possible for each individual by virtue of being immersed in a mind-constructed world; by directly experiencing the inner reality of a culture in all its complexity; or, in other words, by experiencing human states, giving expression to these states, and understanding these expressions (Dilthey, 1976b/1900; 1976d/1910).

> Knowledge of the mind-constructed world originates from the interaction between experience, understanding of other people, the historical comprehension of communities as the subject of historical activity and insight into objective mind. All this ultimately presupposes experience... (Dilthey, 1976d/1910, p. 211)

Human beings, then, must use their own experiences, understandings and empathy as the instruments by which understanding of the experience of others is gained: "Experience and understanding of the objectifications of life disclose the mind-constructed world" (Dilthey, 1976d/1910, p. 195). As an empiricist, Dilthey was interested in studying man and his culture as objectively as possible, hence his concentration on expressions which have been objectified in the external world. However, Dilthey (1988/1923) also understood that it was impossible to know man or society in a purely objective way:

> The starting point of my work is the sum total of research which probes into man, history, and society. I am not beginning with an object, that is, human-social-historical reality, and the relationship of knowledge to this object. These are conceptual abstractions, necessary in their place;

indeed, this reality is only an ideal concept that indicates a goal of knowledge we can never fully reach. The factual element given as the foundation of every theory consists of intellectual efforts which have issued from the purpose of knowing man or history or society or mutual relations among these moments.

> Each of these efforts is defined by the relation of a knowing subject and its historical horizon to a specific group of facts likewise limited in its range by a fixed horizon. For each of them the object exists only from some point of view. Each is thus a definite relative way of seeing and knowing its object. To one who enters into these labors they confront him as a chaos of relativities. *Subjectivity is the modern way of viewing things*. (p. 327)

Hence, on Dilthey's view, while the modern presupposition of passive observation holds for the natural sciences, it is only an ideal in the human studies. The objectifications of life as external manifestations of the human spirit are only understood and interpreted by subjects who, of necessity, use their empathy and understanding of what is being investigated, and their judgment in inquiry. All of these things in cultural interpreters--empathy, understanding, judgment--are "...conditioned by their individuality, the nation to which they belong and the age in which they live" (Dilthey, 1976d/1910, p. 183). So, for Dilthey the empiricist, a major goal for geisteswissenschaften is achieving objectivity in interpretation, or making interpretation as scientific as possible.

Objectivity in studying the mind-constructed world is achieved by *exegesis*, which is the methodical interpretation of recorded expressions: "...understanding only becomes interpretation which achieves validity when confronted with linguistic records" (Dilthey, 1976b/1900, p. 260). On Dilthey's (1976d/1910) view, then, these linguistic records, or recorded expressions are the only complete, exhaustive, and objective records of the life of the mind. The practice of interpreting these records, like the scientific method, grew over time into a rigorous, systematic set of rules: "Exegesis and critical examination have, in the course of history, developed new methodological tools, just as science has constantly refined experiment" (Dilthey, 1976d/1910). This set of rules, known as 'hermeneutics,' had its origins in classical and Christian antiquity, and more recently in post-Renaissance biblical interpretation. However admirable it was of Dilthey to attempt to make the rules of interpretation as systematic as possible (ergo as 'scientific' as possible) by borrowing from interpretive traditions of the past, it proved impossible to achieve an epistemic status for the fruits of hermeneutic inquiry which rivalled that attributed to the scientific method, with its 'neutral scratch point' of 'passive' observation of physical events.

In this sphere of inquiry which aims at understanding rather than explanation there is no definitive truth based on objective observation, there are only greater or lesser levels of understanding. The systematic interpretation of the objectifications of life through the application of hermeneutic methods leads to increased understanding of humankind through an iterative process of examining the part in terms of the whole, and the whole in terms of its parts. This 'hermeneutic circle' is an intrinsic element of all attempts to increase cultural understanding:

> Our knowledge of what is given in experience is extended through the interpretation of the objectifications of life and this interpretation, in turn, is only made possible by plumbing the depths of subjective experience. Similarly, understanding of the particular depends on knowledge of the general which, in turn, presupposes understanding. Finally, a part of the historical course of events can only be understood completely in terms of its relation to the whole

> and a universal-historical survey of the whole presupposes the understanding of the parts united in it. (Dilthey, 1976d/1910, pp. 195-196)

Dilthey (1976b/1900) uses the interpretation of literature as an example of the circular nature of cultural understanding. To gain an understanding of a work of literature one must refer to "...the mentality and development of its author..." and "...the relation of such an individual work to its literary genre" (p. 259). It is only by placing an individual work in context that the interpreter can hope to increase his/her understanding of it. In short, "the work must be understood in terms of its individual parts, individual parts in terms of the whole" (Dilthey, 1976b/1900, p. 262).

Taylor (1982/1971) discusses the trustworthiness and epistemic status of cultural interpretations and concludes that it is impossible to achieve any kind of neutral scratch point and therefore to transcend subjectivity in inquiry. According to Taylor (1982/1971), "a successful interpretation is one which makes clear the meaning originally present in a confused, fragmentary, cloudy form" (p. 155). If an interlocutor disputes the adequacy of an interpretation there is, unfortunately, no appeal to anything beyond common understanding of verbal or written expressions of the language involved. We can only convince an interlocutor of the validity of an interpretation if he/she shares an understanding of the language in question. There is no higher logic or rational argument to appeal to in making our case to the recalcitrant interlocutor, we can only attempt to awaken understanding in him/her. Taylor argues that we cannot break out of the hermeneutic circle, and attempts to do so either through rationalism (i.e., appeal to logical truths) or empiricism (i.e., attempts to go beyond the circle of our own interpretations, to go beyond subjectivity) have led to sterility in the sciences of man. The epistemological monism of social scientists such as Comte, Durkheim, and Skinner is characterized as 'narrow scientism' by Taylor (1982/1971), a tradition which causes human expressions to be treated as 'brute data' "...whose validity cannot be questioned by offering another interpretation or reading..." (p. 157). In the end, this foundational epistemological orientation bounds the practice of inquiry in the social sciences in such a way that our understanding of important dimensions of human life is hindered. However, if we move beyond narrow scientism and consider interpretivist methodology to be 'scientific' in the humanistic sense of the term (i.e., as a systematic structure of thought), then we can achieve a much deeper understanding of the mind-constructed world--albeit one of which we can never be positive. We must accept the epistemological predicament caused by having to operate from within the hermeneutic circle: we must live with uncertainty.

As we saw in Chapter 2, Weber (1978/1922) was one of the first social scientists to acknowledge the true ontological character of the world of mind and to embrace methods which more closely suited this nature. In short, he eschewed the 'narrow scientism' or foundationalism espoused by Comte and Durkheim and embraced Dilthey's dual epistemology. The goal of his 'interpretative sociology' was the interpretation of meanings which humans give to their actions, thereby leading to understanding of the actions themselves. The method to be used in interpreting the meaning of social actions involved both 'observational explanation' and 'interpretative explanation'.

> In the case of 'social systems' (as opposed to 'organisms') we are in a position, not only to formulate functional interrelations and regularities (or 'laws'), but also to achieve something which must lie for ever beyond the reach of all forms of 'natural science' (in the sense of the formulation of causal laws governing events and systems and the explanation of individual events in terms of them). What we can do is to 'understand' the behaviour of the individuals involved, whereas we do *not* 'understand' the behaviour of, say, cells. All we can do in the case of cells is to grasp their behaviour in functional terms and then formulate *laws* governing the way it proceeds. A price has to be paid, admittedly, for these advantages which interpretative explanation has over observational: the results obtained by interpretation are necessarily of a more hypothetical and fragmentary character. Nevertheless, this is precisely what is distinctive about the sociological mode of apprehension. (Weber, 1978/1922, pp. 18-19)

To be acceptable, an explanation must be both causally valid and demonstrate adequacy at the level of meaning. In other words sociological laws concerning social actions must meet the standards of validity laid out for acceptable causal propositions *and* they must demonstrate adequacy in terms of interpretation of what actions mean. Without the latter criterion, "...our generalizations remain mere statements of *statistical* probability, either not intelligible at all or only imperfectly intelligible: this is so no matter how high the probability of outcome (whether in external behaviours or in mental processes) and no matter how precisely calculable in numerical terms it may be" (Weber, 1978/1922, p. 15). By synthesizing scientific and hermeneutic methods, and by applying the trustworthiness criteria of both to the products of sociological inquiry, Weber demonstrated his recognition of the fact that man is not a 'phenomenal object,' but a subject with mind, purpose, feelings and free will (Fletcher, 1971).

In the field of education, the foundationalism of positivists such as Campbell & Stanley and Gage began to be challenged in the early 1970s by researchers who recognized, like Weber, that the complexity, mutability and uniqueness of human problem-fields--although capable of being embodied, on some level, in nomothetic explanations--were not being *adequately* represented by the application of scientific methods alone. Cronbach (1975) was one of the first positivistic social scientists to recognize that the sheer number of variables present in 'open' social systems, and their infinite interactions, limit the validity and generalizability of nomothetic propositions. He advocated adding interpretive methods--more historical than scientific--to the 'Two Disciplines of Scientific Psychology' (i.e., 'experimental control' and 'systematic correlation'). In other words, field researchers should focus on the general *and* the particular when gathering data, they should "...break away from the preoccupation with fixed-condition experiments that seek generalizations" and "...appraise a practice or proposition in [a] setting, observing effects in context" (Cronbach, 1975, pp. 123-124). Thus Cronbach, like Dilthey and Weber, is an epistemological and methodological pluralist, who recognizes that while, on the one hand, the scientific method is valuable in representing those regularities that exist in social phenomena, on the other hand, interpretive methods are worthwhile when one wants to portray the unique factors that exist in a locale.

Schwab (1978c/1970) and Eisner (1991) are two educationists who have rejected narrow scientism, and embraced epistemological and methodological pluralism as a means of obtaining more complete representations of complex, unstable and unique social phenomena. Both recognize that the subject matter of the social sciences embodies both regularities that can be abstracted in the form of laws and theories, and nonuniformities and particularities that will be better illuminated by the choice of different principles of inquiry. There are 'multiple ways of knowing the world' (Eisner, 1991), or 'competing schools' (Schwab, 1978c/1970), that represent phenomena in different ways and, when taken together as an eclectic set, can provide us with a more complete picture of the world. Morgan (1983) mentions two further methodological choices (other than positivism and interpretivism) which have emerged and added to the 'multiple constructed realities' present in human problem-fields. The first approach he labels 'knowledge through action' (also known as 'the Practical' [see Schwab, 1978c/1970, 1978d/1971, 1978e/1973, 1983], 'Reflection-In-Action' [see Schön, 1983, 1987, 1989], and/or 'the Scientific Temper in Practice' [see Dewey, 1929]), which has trustworthiness criteria related to the effectiveness of action in solving practical problems. The second method involves "...generating a liberating form of knowledge...", or "...using research as a means of empowering human beings to take responsibility and control over their lives" (also known as 'Critical Theory' [see Habermas, 1971/1968, 1979/1976, 1982/1970]) (Morgan, 1983, p. 399). These diverse research strategies allow us to bring to light differing aspects of human reality: "Knowledge may serve to explain empirical facts, help us to understand meanings, allow us to act more appropriately, [and] empower in a liberating way..." (Morgan, 1983, p. 403).

Unfortunately, despite the emergence of diverse traditions of research in education--ones which recognize the complexity, uncertainty, and value-laden nature of educational problem-fields--the positivist worldview has maintained its ascendancy in the field. Unlike Eisner and Schwab, many educationists have maintained their foundationalism and failed to embrace pluralism in inquiry as a means of representing complex educational phenomena in multifaceted ways. As Howe (1985) points out, this is readily apparent when one examines the interminable debate in education between proponents of quantitative research and supporters of qualitative methods. More often than not, the discussion has been unwittingly framed using the epistemological assumptions of the dominant, positivistic framework, leading to a forced choice between methods that are characterized as value-laden (qualitative) and those that are considered value-neutral (quantitative). Morgan (1983) agrees that the methodology and knowledge claims of heterodox approaches to social research are often viewed as 'unscientific' because positivist criteria of trustworthiness are applied universally. Howe (1985) asserts that this epistemological monism demonstrates that the field of educational research has been largely unaffected by the emergence of postpositivism, a movement which posits that *all* inquiry is value-laden.

Even more surprising is the stance adopted by Lincoln & Guba (1985), proponents of interpretivist methods of inquiry who have undoubtedly felt the oppression of the positivist hegemony in social science. On the one hand, they reject positivism's assumption of a single, tangible reality and embrace postpositivism's notion of 'multiple constructed realities,' and on the other, they argue that

"...what is needed is a transformation..." to a new, interpretivist paradigm of inquiry, "...not an add-on" (Lincoln & Guba, 1985, p. 33). By promoting a new foundationalism based on interpretivist methods, they clearly demonstrate their misunderstanding of what is meant by 'multiple constructed realities.' According to postpositivistic theory, foundationalism in any form is untenable because it does not mesh well with empirical reality, which presents itself to inquirers in different ways, depending on the theoretical and methodological tools used in different fields of inquiry. Morgan (1983) agrees that foundationalism in any form--e.g. that based on the internal logic of positivism, interpretivism, or any other research program--must be eschewed because first, all of these varied forms of inquiry have their own trustworthiness criteria based on their own internal systems of logic; and second, they illuminate different aspects of the subject matter under investigation.

> Interpretive social science certainly offers a brand of insight that positivism cannot achieve, but on the other hand, positivism can also generate forms of knowledge that elude the interpretive approach. A more relativistic view of the research process encourages us to see these different approaches as doing different things and to attempt to assess their contributions with this in mind. (Morgan, 1983, p. 397)

On this view, we must avoid judging approaches to research using the axioms of one method, as all of these approaches are, in the end, *doxa*, i.e., systems of inquiry that must be taken on faith; none are self-justifying. It is more productive to try to ascertain just what it is that each of these approaches does, and then judge them on how well they accomplish these tasks, or how well these interests mesh with a researcher's own purposes (cf. Donmoyer, 1985; and Toulmin, 1982).

However, as we shall see in Chapter Five, other theorists eschew this relativism as a view that any and all research programs or paradigms are acceptable. They posit that there are still rational bases for choosing between them, i.e., their theoretical and/or empirical progressiveness (see Brown, 1977; and Kuhn, 1970).

In summary, not all researchers in the social sciences have reacted to the distinct ontological nature of human phenomena in the same way. Positivists such as Skinner, Thorndike, Campbell & Stanley, and Gage have maintained their commitment to research programs that employ the scientific method, and "...[aspire] to amass empiricial generalizations, to restructure them into more general laws, and to weld scattered laws into coherent theory" (Cronbach, 1975, p. 125). They have done so in spite of the control problems inherent in studying human beings--alone or in groups--who are sentient, motivated, possess free will, create meanings, and are influenced by a complex matrix of idiosyncratic, unstable, and largely undetermined environmental factors. Others, for example Dilthey, Taylor, Cronbach, Weber, Schwab, Morgan and Eisner, have recognized that the methods of natural science are useful in measuring and quantifying the regularities of human existence that exist, but that they seriously impoverish our understanding of the human world if they dominate inquiry in the social sciences. Consequently, they have championed the addition of heterodox methods of inquiry and representation to the researcher's tool kit. This they have done in order to broaden our understanding of humankind's 'mind-constructed world,' or cultural and historical world: a realm which is unique, ever-changing and value-laden. To paraphrase Dilthey (1988/1923), we must respect the fundamental ontological incommensurability of human and natural phenomena: the former active and unique, and the latter inert and uniform; we must not subordinate social events to the system of nature by applying the methods and truth-criteria of physical science exclusively in human problem fields.

As a final note, it must be stated that the adoption of methodological and epistemological pluralism as a means of more accurately representing the complex ontological nature of social phenomena represents the end of the positivist dream of a unified, systematized body of knowledge obtained by the application of the 'one, true' scientific method. Toulmin (1990) states that "unfortunately, little in human life lends itself fully to the lucid, tidy analysis of Euclid's geometry or Descartes' physics. Aside from these abstract fields of study, the methodology was unrealizable and practically irrelevant..." (p. 200). In the social sciences, empirical reality has led inquirers to embrace a plurality of ways of knowing, each with their own internal logic and unique modes of knowledge representation. While a systematized body of scientific theories and generalizations might be considered by some a more elegant, certain, and valid body of knowledge than that which has been achieved by pluralism, it would be regarded by others as an abstract, generalized representation of human phenomena which is radically incomplete because it failed to reflect the richness of human problem-fields, which are often too complex and idiosyncratic to be adequately embodied in nomothetic explanations.

The Underdetermination of Theory By Logic

$$
\begin{array}{ll}
P_1 & (S_1 \text{ and } S_2 \text{ and } \ldots S_n) \rightarrow E \\
P_2 & \underline{\text{not } E} \\
C & \text{not } (S_1 \text{ and } S_2 \text{ and } \ldots S_n)
\end{array}
$$

Figure 8. A schema demonstrating the underdetermination of theory by logic (Garrison, 1986, p. 13).

Another source of epistemological uncertainty in scientific inquiry is provided by what Garrison (1986) refers to as 'the underdetermination of theory by logic.' According to logical positivism, logical empiricism, and strict falsificationism, if a hypothesis deduced from a higher-order theoretical system is not confirmed by an experimental test or observation, then we must consider the theory to be falsified. However, as Garrison (1986) points out "theories are internally complex..." and "...may be viewed as a conjunction of statements" (p. 13). Referring to the schema reproduced here in Figure 8, Garrison (1986) states that the theory T can be expressed as the combination of statements 'S_1 and S_2 and ... S_n.' By De Morgan's law the logical expression 'not (A and B)' can be considered logically equivalent to the statement 'not A *or* not B.' Therefore the conclusion listed in the schema is logically equivalent to the following statement: 'not S_1 *or* not S_2 *or* ... not S_n.' The point here is that if one or a few of these interrelated statements are refuted by a single experiment, or even a set of experiments, this does not mean, logically, that the entire theory is falsified, as its core may remain intact (Garrison, 1986).

Thus it would seem that the confirmation or disconfirmation of a theory is underdetermined by logic: there is no algorithm which exactly defines the point at which an *entire theory* must be considered disconfirmed owing to the refutation, by experience, of one or a few of its interrelated statements. As mentioned in the previous chapter, Popper (1965/1934) also realized that there was no clear algorithm or logic to guide scientists in deciding whether or not to consider a theory refuted by a falsifying hypothesis. In such a case it might prove more prudent to consider the falsifying hypothesis to be inadequately corroborated, and to continue experimentation to see if it will eventually be falsified (thereby corroborating the theory *for the time being*). As we saw in Chapter Two, Kuhn (1970) was also keenly aware that practising scientists often demonstrate such epistemological conservatism in maintaining their commitment to their accepted theory even when faced with disconfirming evidence. In other words, they make a *human judgment* when they deem it unwise to quickly abandon beliefs which have worked well in the past just because they have not worked well in this one instance (Garrison, 1986). However, should the disconfirming evidence begin to multiply, then scientists, for reasons just as pragmatic as those cited for their initial conservatism, will begin to search for a more viable theory (Garrison, 1986). Clearly, then, formal logic fails to provide a certain methodology to be used in the evaluation of theories; however, this does not leave scientists in the unenviable position of having to base such decisions on purely *irrational* criteria. As we shall see in Chapter Five, a *'logic of practice,'* based on the informed judgment of scientists working within communities exists to aid them in making *reasonable* choices about whether or not to abandon a line of inquiry (See Kuhn, 1970; Brown, 1977).

The Problem of Induction and the Underdetermination of Theory by Experience

A further epistemological uncertainty, the "problem of verification" (see Garrison, 1986) or the 'problem of induction' (see Popper, 1965/1934), was first discussed in Chapter 3. The reader will recall that the problem of induction states that it is impossible to achieve positive confirmation of a theory owing to the impossibility of gathering an infinite number of observations in support of an hypothesis. As we shall discover below, the fact that the number of observations that can be made is necessarily finite causes a second barrier to positive knowledge: the underdetermination of theory *by experience*.

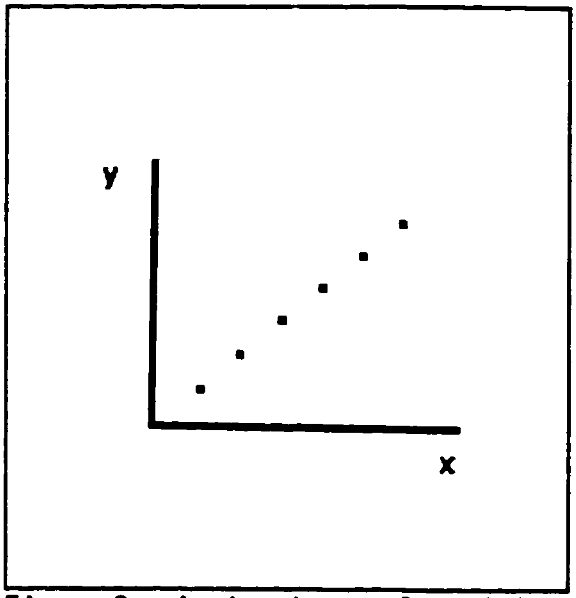

Figure 9. An imaginary plot of data (Garrison, 1986, p. 14).

Garrison (1986) points out that aside from the justification and choice of theories being underdetermined by logic, there is also "...a serious underdetermination of theory by experience, or experiment..." (p. 14). He expresses this underdetermination in the following manner: "...given any finite body of data, an infinite number of theories may be tailored to fit the body" (Garrison, 1986, p. 14). James (1969/1907, p. 142) states it another way: "...sometimes alternative theoretic formulas are equally compatible with all the truths we know." Morgan (1983) provides a practical example from social science:

> I was conducting research on the organization and management of an English new town development corporation...I had just completed a questionnaire survey of staff attitudes toward the organization in which they worked and was reflecting on the statistical results of the data analysis. From one point of view, the statistics provided a very high level of support for the general hypothesis I was interested in testing, *but they were also open to alternative interpretations*...I found that I could use my data to support and refute in some degree most of the rival theories and hypotheses that I cared to generate. (p. 11, italics added)

Garrison (1986) also demonstrates this principle by pointing to the imaginary plot of data contained in Figure 9. In seeking a single-statement mathematical function or theory to account for this data, most people would conclude that the choice *must be* the linear equation $y = mx + b$. But, in reality, we could construct an *infinite* number of equations which would fit these finite facts.

As Garrison (1986) indicates, we can limit the number of theories advanced to explain the data corpus by making further observations which will provide additional data coordinates to be introduced between those already existing. In this way, any proposition not able to account for these new points will be eliminated until, in theory at least, we arrive at the 'correct' representation of this body of facts. However, the 'problem of induction' states that it is impossible to lend conclusive support, by induction, to a general statement using a finite number of observations. Since it is impossible for a scientist to observe every possible instance of a phenomenon (i.e., at all possible places and times), it is not possible for us to irrefutably verify a single theory as 'correct.' Thus, while we can *limit* the number of one-statement theories that explain the data corpus by conducting further observations or experiments, we cannot completely reduce this plurality of possible views to a single 'confirmed' account. In short, "the choice of the 'best' theory, or explanation, remains underdetermined" by experience (Garrison, 1986, p. 14).

The aforementioned wave-particle duality of light--a phenomenon whose features are considered to be highly fixed, or determined--aptly demonstrates the underdetermination of theory by experience. As we saw above, Einstein proved that light was composed of particles call photons without, at the same time, being able to disprove that it was wave-like. In essence, then, the choice of the one 'correct' explanation of the phenomenon of light is underdetermined by empirical experiment. I will further examine how our theoretical and methodological commitments determine what we see in the subsection entitled *Perception Yields Culturally-Determined Facts*.

At this point an important question remains: How are we to choose from among the plurality of theories which exist to explain a certain body of facts when this choice is underdetermined by empirical reality? As with logical underdetermination, pragmatic criteria enter in (Garrison, 1986):

> ...we choose between them for subjective reasons. We choose the kind of theory to which we are already partial; we follow 'elegance' or 'economy'....Truth in science is what gives us the maximum possible sum of satisfactions, taste included, but consistency both with previous truth and with novel fact is always the most imperious claimant. (James, 1969/1907, p. 142)

Hence, scientists are constrained in how they evaluate and select from among a plurality of views by their personal predilections, by background pressures (usually conservative in nature) emanating from the paradigmatic tradition within which they work, and by empirical reality itself (Garrison, 1986). Such decisions must be made in light of the accumulated experience, wisdom and often, intuition, of a practising scientist; they defy the prescriptions of any rule-based formal logic. It is this humanistic view of the conduct of science (i.e., one which recognizes that the level of human judgment cannot be transcended by appeals either to logic or 'objective' experience) which is the key concern of this chapter.

Perception Yields Culturally-Determined Facts

In the previous two subsections we saw that both logic and experience fail to prescribe theory verification (or falsification) and choice. As stated above, both of these limitations to human knowing are 'epistemological uncertainties' (Morgan, 1983). A further such uncertainty is raised by humanist and postpositivist claims about the nature of human perception. The reader will recall that one of the 'neutral scratch-points' of the scientific-rationalistic worldview is that there exists a strict separation between theory and empirical fact; in other words, *perception is immediate and provides pure fact*. On this view, it is this fact-value dichotomy that guarantees the validity and certainty of scientific observations and knowledge. This century, humanists such as the pragmatic philosopher William James, and postpositivists such as Kuhn, Brown, Toulmin, Eisner and Morgan, have instead argued that human perception is, *by necessity*, mediated by cultural meanings.

The following quote by Einstein and Infeld skilfully uses the analogy of the closed watch to express the impossibility of what Eisner (1991), after Newell, calls 'ontological objectivity,' which is the ability "...to experience a state of affairs in a way that reveals its actual features", or, to "...know [things] in their ontological state" (p. 43).

> Physical concepts are free creations of the human mind, and are not, however it may seem, uniquely determined by the external world. In our endeavor to understand reality we are somewhat like a man trying to understand the mechanism of a closed watch. He sees the face and the moving hands, even hears its ticking, but he has no way of opening the case. If he is ingenious he may form some picture of a mechanism which could be responsible for all the things he observes, but he may never be quite sure his picture is the only one which could explain his observations. He will never be able to compare his picture with the real mechanism and he cannot even imagine the possibility of the meaning of such a comparison. (Einstein & Infeld, cited in Zukav, 1979, p. 35)

The following quote from James (1969/1907) also expresses the futility of trying to achieve ontological objectivity:

> It is, as I have already said, *as if* reality were made of ether, atoms or electrons, but we mustn't think so literally. The term 'energy' doesn't even pretend to stand for anything 'objective.' It is only a way of measuring the surface of phenomena so as to string their changes on a simple formula....Yet sometimes alternative theoretic formulas are equally compatible with all the truths we know, and then we choose between them for subjective reasons. (pp. 141-142)

These quotations express a number of things that *practising scientists*--as opposed to *philosophers of science obsessed with the mathematical, logical, or empirical foundations of indubitable knowledge claims*--have come to realize about their chosen enterprise: first, that direct, passive, sensory perception of the experiential world is an impossibility; second, and as we saw above, that the number of theories that can be constructed to explain a phenomenon is unlimited, and that the choice of the 'best' theory is often underdetermined by experience; and third, that the 'real mechanisms' are unknowable, therefore we must settle for a plurality of provisional, constructed, incomplete representations of different empirical realities.

One of the first humanists to recognize that the modern epistemological presupposition of passive perception is beyond the capacity of human rationality was William James (1969/1907). Indeed, James's pragmatist philosophy can be considered a rudimentary version of the postpositivistic philosophy of science that was to emerge in the 1960s and 1970s. Both of these traditions elevate humankind's role in inquiry from that of a passive technician who records objective fact, to that of an active observer who engages and constructs reality.

> Now however fixed....reality may be, we still have a certain freedom in our dealings with [it]. Take our sensations. *That* they are is undoubtedly beyond our control; but *which* we attend to, note, and make emphatic in our conclusions depends on our own interests; and, according as we lay the emphasis here or there, quite different formulations of truth result. We read the same facts differently....Hence even in the field of sensation, our minds exert a certain arbitrary choice. By our inclusions and omissions we trace [a] field's extent; by our emphasis we mark its foreground and its background; by our order we read it in this direction or in that. We receive in short the block of marble, but we carve the statue ourselves. (James, 1969/1907, p. 161)

So James (1969/1907) recognizes, first, that it is our beliefs and prior knowledge about reality that will determine what we observe: "As a matter of fact we can hardly take in an impression at all, in the absence of a preconception of what impressions there may possibly be" (p. 162). In other words, there may exist a reality which is detached from human thinking, but we can never grasp it objectively. Second, he acknowledges that our 'truths' are therefore human constructions: "Human motives sharpen all our questions, human satisfactions lurk in all our answers, all our formulas have a human twist" (p. 159). Finally, because a given reality can be conceived in different ways by human inquirers, and that reality will support these varied conceptions empirically, truth is therefore pluralistic, not monistic: "Man *engenders* truth upon it" *in diverse ways* (James, 1969/1907, p. 165).

As we saw in Chapter Two, postpositivists such as Kuhn (1970), Brown (1977), Eisner (1988, 1991) and Toulmin (1982, 1990) also consider perception to be theory-laden. The latter theorist mentions Locke, who assumed that similar ideas of sense would generate, with repetition, similar ideas of reflection. Unfortunately, Locke did not bother to ask if the acquisition of shared concepts was dependent not only on repeated exposure to sense stimuli, but also shared enculturation and language. In Toulmin's (1990) opinion this is, in fact, the case, as demonstrated by the ethnographic fact of the diversity of colour terminology among different languages and cultures. This fact "...is striking enough to undercut the empiricist choice of neutral 'sense data' as a rational starting point for constructing an intelligible world" (Toulmin, 1990, p. 178).

Kuhn (1970) agrees that sensations are not shared universally by human beings of all places and times: "Individuals raised in different societies behave on some occasions as though they saw different things" (p. 193). Impressions are determined by enculturation, i.e., the inherited ideas of one's culture, and/or *the acquired theories and* *concepts of one's scientific community.*

> To the extent, of course, that individuals belong to the same group and thus share education, language, experience, and culture, we have good reason to suppose that their sensations are the same. How else are we to understand the fullness of their communication and the communality of their behavioral responses to their environment? They must see things, process stimuli, in much the same ways. But where the differentiation and specialization of groups begins, we have no similar evidence for the immutability of sensation. Mere parochialism, I suspect, makes us suppose that the route from stimuli to sensation is the same for the members of all groups. (Kuhn, 1970, p. 193)

Eisner (1988) recognizes that language--common or theoretical--does not simply facilitate the communication of ideas, it shapes what we see: "Our way of dividing the world and classifying its components is significantly influenced by the linguistic system that we have learned to use" (p. 15). Eisner (1991), after Dewey, conceives of the locus of human experience as transactive; in other words, "it is the product of the interaction of two *postulated* entities, the objective and the subjective" (p. 52). What we know about reality, our experience, is the product of a transaction between subjective and objective worlds: we cannot separate the two. Brown (1977) points out that we are mostly unaware that what we perceive is a function of our knowledge and beliefs "...because so much of the information necessary for us to recognize [objects] is learned through the largely unreflective process of growing up in a culture" (p. 82). It makes no difference if we are talking about the lay perception of a common object or the scientific perception of a laboratory event: our preexisting knowledge and beliefs play a central role in determining what we observe. As a corollary of this, two or more observers of a single object or event, each coming into the observation with different prior knowledge, will perceive different things--*all of which are supported by the sense-data (Brown, 1977).* In short, the theory-laden perception of a single event by individuals with different presuppositions reveals *multiple constructed realities.* However, as Brown (1977) stresses, and as I will discuss further in Chapter Five, the fact that objects can be seen in a plurality of ways does not mean that they can be seen *in any way at all.*

The Gestalt Shift

An oft-cited example that demonstrates that a single thing or event can be perceived in different ways, and therefore that truth is plural, is the gestalt shift. One of the forebearers of logical positivism, Wittgenstein, provides us with the example shown in Figure 10. If, when viewing the diagram, one looks first at the corners marked with a's and only glances at the corners marked with b's, then the a's appear to be out in front, and vice versa if one first attends to the b's.

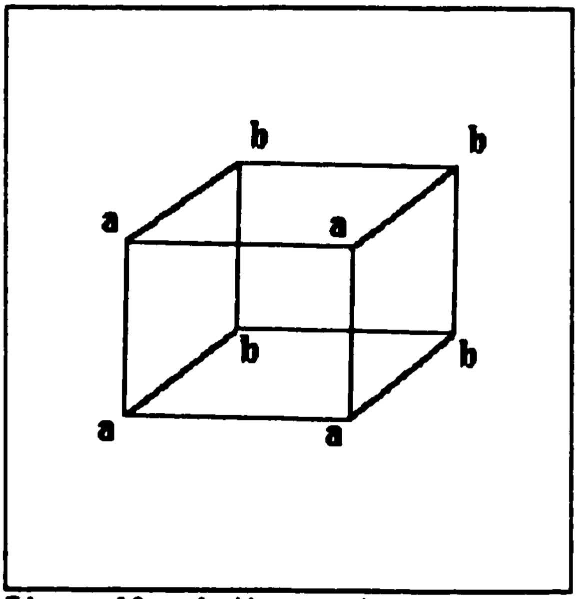

Figure 10. A diagram demonstrating the gestalt shift (Wittgenstein, 1974/1921, p. 54).

Wittgenstein (1974/1921) rejects the claim that the shift is caused by the presuppositions of a 'metaphysical subject.' Instead, he contends that "...there are two possible ways of seeing the figure as a cube..." because "...we really see two different facts," *not* because prior assumptions play a role in perception (p. 54).

The pragmatist-postpositivist worldview repudiates Wittgenstein's positivist interpretation of the gestalt shift. James (1969/1907) discusses the example contained in Figure 11:

> You can treat the adjoined figure as a star, as two big triangles crossing each other, as a hexagon with legs set up on its angles, as six equal triangles hanging together by their tips, etc. All these treatments are true treatments....In all these cases we humanly make an *addition* to some sensible reality, and that reality tolerates the addition. All the additions 'agree' with the reality; they fit it, while they build it out. No one of them is false....In many familiar objects every one will recognize the human element. We conceive a given reality in this way or in that, to suit our purpose, and the reality passively submits to the conception....Which may be treated as the *more* true, depends altogether on the human use of it. (pp. 164-165)

Here James explains that what we perceive about the figure depends on what we first conceived it to be. Brown (1977) refers to this as 'concept-laden perception' (a more accurate term than '*theory*-laden perception' in this case). In all of the versions discussed--the star, two crossed triangles, etc. --the figure or sense data has remained the same, what has changed is the observer's preconception about what s/he will find in the phenomenological field. The reality, the particular pattern of lines depicted in Figure 11, does not provide us with more than one fact as Wittgenstein contends: there is only a single

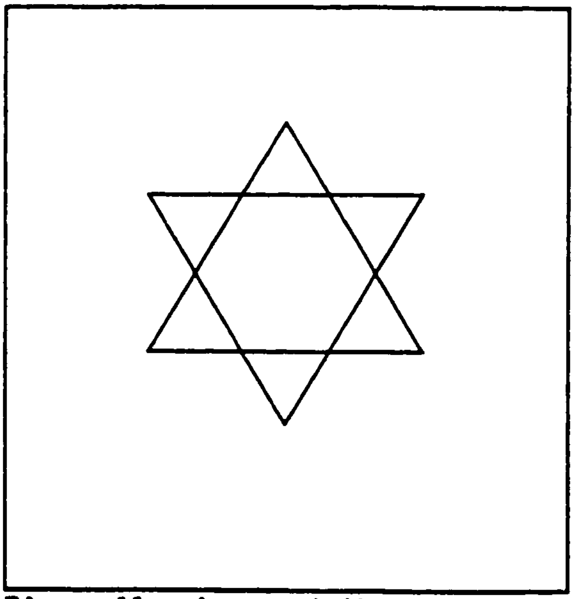

Figure 11. A second diagram demonstrating the gestalt shift (James, 1969/1907, p. 164).

figure depicted. We can, nonetheless, conceive of the sense-data in a plurality of ways: a person who brings one preconception to the observation will see something different from what another person, with a different preconception, will see. As well, a single person can learn to shift his/her viewpoint and thereby see a single figure in a plurality of ways--each supported by the empirical data, each as 'correct' as all the rest.

To summarize: our theories, our learned concepts, the inherited ideas of our culture, all help to determine what we will perceive when we observe a phenomenon. Others, with different theories, learned concepts, or inherited cultural ideas will see the same event differently. To paraphrase James (1969/1907), it is possible for many different treatments to 'agree' with the reality: the truth is pluralistic, not monistic. On this view, the logical positivist fact-theory distinction--commonly supposed to provide 'objective' observations of preexisting, stable phenomena--is a metaphysical concept that does not reflect the reality of human observation.

The Dream of Foundationalism: No Universal, Complete System of Rational Thought Exists

As we saw in Chapter Two, the second neutral scratch line of the program of modernity, after passive perception, was the adoption of a certain, rational starting point for a systematic body of scientific knowledge. For example, Descartes chose Euclidean idealizations of space relations as the perfect, self-validating foundation for a body of positive scientific knowledge. Descartes assumed that Euclid's geometry would provide a foundation of basic concepts or 'clear and distinct' ideas that are shared in all epochs and cultures. This universal, axiomatic system of thought, together with the neutral sensory impressions provided by the method of mathematical physics, would yield a systematic body of knowledge that was certain because it cleaned the slate of all meaningless common sense or metaphysical propositions (Toulmin, 1990). To this point in this chapter, I have shown that a plurality of methods of inquiry have emerged in response to the diversity and complexity of empirical reality and that sensory impressions are never neutral. This constituted the erasure of the first 'neutral scratch line' of modernity. Now I will demonstrate, with the aid of pragmatist and postpositivist theorists, that the second neutral scratch line of a *universal*, *self-justifying*, *tradition-free*, axiomatic system of thought is as much a metaphysical construction as any of the rational syntheses that the positivists attempted to delegitimize as 'nonsensical idealizations.'

James (1969/1907) discusses a 'rationalist-empiricist dichotomy' which will help demonstrate that the second 'neutral' scratch line is far from impartial. He characterizes the 'rationalist temperament' as intellectualistic, idealistic, monistic, religious, dogmatical, and devoted to abstract and eternal principles. Opposed to this is the 'empiricist temperament' which is sensationalistic, materialistic, pluralistic, irreligious, sceptical, and a "...lover of facts in all their crude variety" (James, 1969/1907, p. 20). According to James (1969/1907), the former temper is characteristic of absolutist, metaphysical philosophers who "...make our universe by thinking it..." (p. 26). In the universe of the metaphysician the "...architecture is classic. Principles of reason trace its outlines, logical necessities cement its parts. Purity and dignity are what it most expresses" (James, 1969/1907, p. 27). Rather than conceiving of the universe as something wide open, the rationalists "...make systems, and systems must be closed" (James, 1969/1907, p. 30). Truth, to the rationalist, is certain and universal; in other words, 'true' thoughts must correspond directly with an absolute reality.

> But the great assumption of the intellectualists is that truth means essentially an inert static relation. When you've got your true idea of anything, there's an end of the matter. You're in possession; you *know*; you have fulfilled your thinking destiny. You are where you ought to be mentally; you have obeyed your categorical imperative; and nothing more need follow on that climax of your rational destiny. Epistemologically you are in stable equilibrium. (James, 1969/1907, p. 133)

In contrast, James discusses the empiricist or scientific temper which he claims was firmly entrenched, at the time of writing, as the dominant disposition of western civilization. Empiricists are less interested in pure abstractions, and more interested in the concrete world of facts which is multitudinous, mutable, tangled, muddy, and therefore painful and perplexed. They "...talk about truths in the plural, about their utility and satisfactoriness, about the success with which they 'work,' etc..." (James, 1969/1907, p. 54). Their stance toward inquiry is more skeptical: they are more open to discussion than rationalists. According to James (1969/1907), the pragmatist philosophy is firmly placed in the empiricist camp; notwithstanding this, it does not eschew metaphysical systems of thought so long as they demonstrate utility, or "...definite working-class values in experience" (p. 54).

What is interesting about James's dichotomy is, first, that he describes a 'scientific temper' which is resolutely humanist in nature, i.e., truth is plural, provisional, uncertain, and validated by practical utility. Second, the criticisms he levels at metaphysical philosophy can be applied equally to the versions of scientific rationalism (i.e., logical positivism and logical empiricism)--with their 'quest for certainty'--that would emerge after 1914. In other words, James's version of science--decidedly empiricist and humanist in nature--would soon be replaced by a positivistic conception of science that was unequivocally *rationalistic* in temperament. For example, both metaphysicians *and* positivists view reality as monistic, "...complete and ready-made from all eternity..." and the goal of both is to achieve a positive copy of that reality (James, 1969/1907, p. 147). Metaphysicians achieve their copy of immutable reality through their powers of intellectual abstraction and force their *ā priori* systems of thought on that reality. The scientific rationalists have done the same: the foundation of Descartes's systematic body of scientific knowledge was Euclidean geometry; the logical positivists and logical empiricists chose Russell's and Whitehead's *Principia* logic as their foundation. (However, unlike the metaphysicians, positivists also use the empirical method, which they believe to be immediate and neutral, to represent that reality directly.) Furthermore, both metaphysicians and positivists view truth as monistic, certain, universal and final; both eschew the concrete and the particular; both are prone to abstraction; and both are dogmatic in their beliefs. The irony here is that positivist beliefs about the true nature of reality--and humankind's ability to obtain a systematic body of absolute knowledge about that reality through rational synthesis and passive observation--are just that: *beliefs*. They are human assumptions that are as metaphysical as the religious/philosophical concepts that they eschewed, such as 'God' or 'dialectical process.'

No system of rational thought can transcend human reality and experience by claiming to be, self-evidently, the *only*, *certain*, *universal*, *rational* foundation for knowledge and science. James (1969/1907) points out that, like our sensations, our rational systems of thought are human constructions:

> This applies to the 'eternal' parts of reality as well: we shuffle our perceptions of intrinsic relation and arrange them just as freely. We read them in one serial order or another, class them in this way or that, treat one or the other as more fundamental, until our beliefs about them form those bodies of truth known as logics, geometrics, or arithmetics, in each and all of which the form and order in which the whole is cast is flagrantly man-made. (pp. 161-162)

Rather than being final and universal truths, our rational systems of logical and mathematical ideas undergo, in time, transformations to suit our changing, ever-human needs (James, 1969/1907). Morgan (1983) agrees that no doctrine, despite its internal logical consistency, is self-justifying; all are nothing more than incomplete axiomatic systems which must be taken largely on faith. Eisner (1991) states that there is no certainty in rational synthesis, no higher authority to appeal to in ascertaining truth: truth does not exceed belief. Toulmin (1990) denies the existence of a 'neutral scratch point': there is no objective standpoint beyond human experience--which is necessarily historical and contextual. All we can hope for is that our beliefs "...are rationally warranted, reasonable, or defensible..." in terms of their effect on practical human affairs (Toulmin, 1982, p. 115), or their fruitfulness in explaining or interpreting experience (Morgan, 1983).

> No formalism can interpret itself; No system can validate itself; No theory can exemplify itself; No formal language can predetermine its own meanings; No science can forecast just what technology will prove of human value. In facing problems about the use of new knowledge for human good, we may ignore the ideal of intellectual exactitude, with its idolization of geometrical proof and certainty. Instead, we must try to recapture the practical modesty of the humanists, which let them live free of anxiety, despite uncertainty, ambiguity, and pluralism. (Toulmin, 1990, p. 105)

In short, science and philosophy must not--*cannot*--be removed from humanity: they are human practices which take place in social contexts and which address human problems *(Morgan, 1983). We cannot escape uncertainty through transcendentalism.*

Gödel's undecidability theorems provide us with a principle, stated in relation to mathematics, which effectively discredits the notion that *any* doctrine--mathematical, logical, or otherwise--is final, certain, and self-justifying. It "...states that no axiomatic system of mathematics is able to provide information about both the completeness and consistency of that axiomatic system" (Lincoln & Guba, 1985, p. 27). Stated another way, "...it is fallacious to conclude that the propositions of a system of thought can be proved, disproved, or evaluated on the basis of axioms within that system, since the process becomes self-justifying" (Morgan, 1983, p. 370). This constituted landmark work in 20th-century mathematics because it discredited the notion that mathematics was a completed subject-matter, and it ended nearly a century of attempts, such as that of Whitehead and Russell

(1970/1910), to establish axioms that would give a rigorous basis to mathematics (Frisinger, 1992). On this view, then, the modern hegemony of formal logic and scientific method, as exemplified in the foundationalism of *mathematical physics*, constituted nothing more than a belief in a metaphysical system: a belief which was, nonetheless, functional (i.e., in terms of *explaining* the cosmological order, and *justifying* the socio-political order) in the west, during the 17th, 18th, 19th and early 20th centuries (Toulmin, 1990).

Above, we saw that Descartes considered the 'shared basic concepts' of Euclidean geometry to provide a 'neutral scratch point,' a foundation, for certain knowledge in all cultures of all epochs (Toulmin, 1990). However, on the basis of the analytical argument just presented it is fallacious to assert that the validity and completeness of this rational system of spatial relations can be determined on the basis of its own assumptions. Furthermore, experience has shown that, as a fact of ethnography, not all people in every part of the world at all historical stages describe spatial patterns in ways that conform with Euclidean geometry (Toulmin, 1990). In fact, the mid-19th century mathematician, Bernhard Riemann, built a new system of geometry by questioning one of the so-called 'self-evident truths' of Euclid's pattern--that two parallel lines never meet. Riemann turned this assumption on its head and assumed that two parallel lines do, in fact, meet. The fruitfulness of this unorthodox system of thought was later demonstrated by the success of Einstein's Theory of Relativity, which was largely based on Riemannian--not Euclidian--geometry. The proliferation of new geometries based on different postulates has increased this century, with the addition of six new systems of thought, for a total of eight working geometries (Peck, 1993). Moreover, the number of potential geometries is considered infinite given that each and all of them are cast in forms which are "...flagrantly man-made" (James, 1969/1907, p. 162).

In conclusion, it must be noted that Descartes's choice of Euclidean geometry as the foundation "...of a physics intended to make comprehensive sense of all material nature" was replaced this century, by Whitehead's and Russell's *Principia* logic (Toulmin, 1990, p. 171). Clearly, then, the Vienna Circle of logical positivists did not agree with Descartes that Euclid's geometry had, for all time, provided the one, true foundation for a unified body of knowledge. As James (1969/1907) states, we tend to treat one rational system or the other as *more* fundamental: this choice reflects human beliefs and is, to some extent, an arbitrary decision. In short, the second neutral scratch line of formal logic as a starting point for a comprehensive system of certain theoretical knowledge "...is unfulfillable. *There is no scratch*" (Toulmin, 1990, p. 178). As we shall see in Chapter Five, in the philosophy of science, postpositivism has recently challenged positivism's version of the progress of science. The new philosophers of science have eschewed the logical examination of the products of science as the primary tool of analysis in the field. Instead, they have embraced the historical analysis of the practice of science--i.e., in terms of the significance of meaning structures in particular scientific communities--as a more fruitful approach in explaining the progress of science. This represents another human choice that is neither self-validating, certain, or eternal. Indeed, the modern quest for certainty through rationalism and passive scientific observation could one day recover its former ascendancy.[8]

Conclusion

In this chapter, I have shown that ontological uncertainty exists in physical science, and is a defining characteristic of the human sciences. The positivist assumption that a stable, eternal reality exists 'out there,' and that it can be represented formally, and for all time, in a systematic body of scientific knowledge using the method of mathematical physics, has been shown to be erroneous. In the physical sciences, discoveries this century such as relativity, the dual nature of light energy, the uncertainty principle, and chaos theory have undercut the positivist theories of determinism and causation. There exists some randomness, some level of indeterminacy in physical phenomena, particularly at the sub-atomic level; however, this fact does not preclude the nomothetic explanation of the vast majority of physical events. Indeterminacy and unpredictability have come to be accepted as fundamental characteristics of the social sciences, owing to the consciousness and free will exhibited by human beings, the cultural meanings constructed by them, and the complex patterns of influence found in 'open' social systems. The recognition that human problem fields are fundamentally different from their physical counterparts has led to a proliferation of heterodox research methods and modes of

[8]See Gholson and Barker (1985), who, after Lakatos, believe that a plurality of paradigms can exist concurrently and that these research programs can undergo periods of degeneration and revival.

representation. This pluralism is considered--by humanists and postpositivists[9]--to have provided us with a more complete representation of the human world in all its diversity. It has also destroyed the scientific-rationalist dream of a single, formal body of scientific knowledge explaining all manner of phenomena, human and physical.

I have also demonstrated that the quest for certain knowledge of the world through scientific observation and rationalism is a metaphysical construction that does not mesh well with reality. The intellectual program of modernity, with its attempt at achieving certainty, systematicity, and the clean slate in a perfectly rational system of thought, has sought to transcend the contaminating influence of human traditions and judgments in order to reach this ideal. But as the pragmatist philosopher James (1969/1907) points out, thinking is a human activity and cannot escape human considerations:

> You see how naturally one comes to the humanistic principle: you can't weed out the human contribution. Our nouns and adjectives are all humanized heirlooms, and in the theories we build them into, the inner order and arrangement is wholly dictated by human considerations, intellectual consistency being one of them. Mathematics and logic themselves are fermenting with human rearrangements; physics, astronomy and biology follow massive cues of preference. We plunge forward into the field of fresh experience with the beliefs our ancestors and we have made already; these determine what we notice; what we notice determines what we do; what we do again determines what we experience; so from one thing to another, altho the stubborn fact remains that there *is* a sensible flux, what is *true of it* seems from first to last to be largely a matter of our own creation. (James, 1969/1907, pp. 165-166)

In short, the human imprint is found everywhere in the human acts of

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[9]More specifically: *post-Kuhnian* postpositivists.

theorizing about abstract relations of ideas and observing empirical reality. We cannot escape the inherited traditions--be they sociopolitical (see Toulmin, 1990) or those of a community of scientists (see Kuhn, 1970)--of our historical period and given culture. Our perception is coloured by these traditions, and the same empirical reality is discernable in diverse ways--depending on the presuppositional matrix of the observer. Nor can we transcend cultural or personal idiosyncracies through abstract rationalization. All axiomatic systems are incomplete and cannot be justified as the 'one, certain version of truth' on the basis of their own assumptions.

The modern worldview, in which "...the 'rational' thing to do was to start from scratch,...to insist on the certainty of geometrical inference and the logicality of formal proofs," and to build a systematic body of knowledge through objective scientific observation, has subsided as the dominant epistemology in most scientific fields of inquiry (Toulmin, 1990, p. 199). A more humanistic version of human knowing has arisen. It is one that accepts the uncertainty and plurality of all knowledge *construction*--empirical or rational. It recognizes the preeminent role of human wisdom and intuition in: (a) discovering new theories, (b) deciding which theory best fits a phenomenological field, or (c) deciding when to regard a theory as falsified or verified. It has replaced the strict, abstract, formal, technical 'rationality' of the modern era with the Renaissance supplication that our choices in conducting inquiries--made in context and in the illuminating light provided by our previous experiences--be made for good practical reasons; in other words, that they be 'reasonable.' In the next chapter I will outline the amended view of progress in scientific knowledge provided by postpositivistic theorists, who recognize the centrality of human traditions and decision-making in inquiry, and the importance of making choices that are warranted or wise.

CHAPTER FIVE

THE RECOVERY OF THE HUMANISTIC WORLDVIEW

> Scientific knowledge, like language, is intrinsically the common property of a group or else nothing at all. To understand it we shall need to know the special characteristics of the groups that create and use it.
>
> T. S. Kuhn, The Structure of Scientific Revolutions

Introduction

The reader will recall that in Chapter Two I examined the recontextualization of scientific knowledge provided by Kuhn (1970). There we learned that Kuhn rejected the logical empiricist focus on (a) the logical analysis of the finished *products* of science, and (b) the 'development-by-accumulation' of scientific knowledge since the *one* scientific revolution of the 17th century, in which out-of-date theories are considered 'unscientific' once discarded. Kuhn replaced this with a detailed study of the history of scientific *practice* which stressed: (a) the *necessary* attempts of communities of inquirers to interpret nature in terms of a framework of presuppositions, and (b) the existence of non-cumulative, tradition-shattering episodes in the development of science that change this framework. This chapter will revisit Kuhn's philosophy and explore the controversy that surrounds it concerning the true nature of scientific rationality and progress. What we will discover is that the 'new philosophy of science' inaugurated by Kuhn (and others) has reinstated the humanist conception of rationality that was lost to the 17th century rational revolutions in philosophy and science. Whereas the program of modernity narrowed our conception of reason to include only formal logic and rule-based verificationism (or falsificationism), Kuhn's revolution in the philosophy of science has reinstated practical wisdom or *phronesis* to its rightful place as an integral component of human rationality. In short, there is no escape from fallible human judgment in the conduct of science and, consequently, there is no 'neutral scratch point' which can guarantee the truth of our scientific propositions.

I will begin the chapter by reviewing some of what was covered in Chapter Two, while (in the interest of avoiding repetition) attempting to highlight those areas of Kuhn's thesis that invite multiple interpretations, or invoke controversy about what the *rational basis* is for the discovery, development, comparison and selection of theories. Next, I will explore solutions to, and/or reconceptualizations of, these complex issues offered by various other philosophers of science and social science. Following that, I will outline new trustworthiness criteria for *humanistic science*, i.e., a view of science where a plurality of largely incommensurable paradigms exists, and where no one approach is foundational or capable of being used to judge the knowledge claims of its competitors. Finally, I will indicate my preference for one of five approaches to dealing with plurality and discuss the use of dialectical means to make choices from among competing paradigms more critically intelligent. Overall, what will be described is a version of rationality based on Renaissance humanist values, i.e., one that recognizes that we cannot escape the realm of practical--hopefully wise--human decision making through appeals to formal logic and objective observation.

Kuhnian Philosophy of Science

Presuppositions and Paradigms

While Kuhn (1970) accepts that human perception is concept- or theory-laden, he asserts that *scientific* perception is a function of something more than theory alone. On his view, reality is constructed, nature is 'forced into a box,' by a disciplinary matrix of presuppositions which, in addition to symbolic generalizations (or theoretical laws of nature), includes: (a) exemplars or schemata which illustrate symbolic generalizations for the scientist in practical situations (i.e., Polanyi's [1966] 'tacit knowing,' or Schön's [1989] 'knowing-in-action'); (b) metaphysical beliefs in particular heuristic and/or ontological models which provide scientists with preferred analogies about their subject matter; and (c) values which are used to judge theories (e.g., they should be simple, self-consistent, compatible with other theories, etc.), and define the preferred format for predictions (e.g., that they should be quantitative rather than qualitative). Kuhn labels this constellation of commitments shared by a scientific community a 'paradigm.' This paradigm performs a variety of necessary functions in the conduct of science: it narrows and restricts the focus of the scientist; it determines the problems of the community; it prescribes methodology; it defines what constitutes meaningful and meaningless data; it delimits how facts are to be interpreted; it specifies what counts as valid problem-solutions; it facilitates unanimous judgment and full communication within the community; in *transaction* with the empirical world (see Eisner, 1991), it determines what the scientist sees when s/he views the world of his/her research; it dictates what tools and/or means of measurement are to be used in inquiry; and finally, it leads to the apperception of anomalies of fact which, if recalcitrant, will lead to the replacement of the old paradigm with a new one based on incompatible principles. However, as we shall observe below, not all philosophers of science have accepted Kuhn's concept of a 'paradigm' in this form: e.g., Lakatos has supplanted it with his concept of a 'research program,' and Laudan has replaced it with his notion of a 'research tradition' (both cited in Gholson & Barker, 1985).

Normal Versus Revolutionary Science

According to the main corpus of Kuhn's (1970) work (as opposed to the Postscript written in 1969 and added to the second edition of the book), a field of inquiry only becomes a 'science' when a 'common body of belief' or a paradigmatic state is achieved. The pre-paradigm state of a field of inquiry is characterized by the existence of a multiplicity of competing schools and seemingly interminable debate over--or reiteration of--fundamentals such as the 'correct' methods and/or theoretical representations of the phenomena of study. In a nascent field of inquiry what occurs is that "...different men confronting the same range of phenomena, but not usually all the same particular phenomena, describe and interpret them in different ways" (Kuhn, 1970, p. 17). During these pre-scientific periods, when the basic tenets of the field are at issue, Kuhn (1970) asserts that

"...evidence of progress, except within schools, is very hard to find..." (p. 163). Kuhn (1970) states (on p. viii) that the existence in social science of a concurrent plurality of competing schools led him to the discovery that what distinguished these fields from those of the physical sciences was that the latter had reached a state of 'maturity' in which one competing school managed to develop a more coherent and rigid definition of the field (i.e. a 'paradigm,'), and thereby converted the vast majority of practitioners in the field to that set of presuppositions.

Once a field has achieved paradigm status, it then embarks upon a period of cumulative growth in knowledge known as 'normal science,' in which the members of the scientific community work at solving the specialized puzzles which are set out by their accepted disciplinary matrix. In Kuhn's opinion, then, because the 'incipient' social sciences have groups of inquirers operating from within competing schools of inquiry, and without a dominant set of beliefs guiding the vast majority of practitioners, they fail to achieve the cumulative growth in knowledge that is characteristic of periods of normal science in more *mature* disciplines, e.g. physics, chemistry, and etc. In short, those schools of inquiry dealing with human phenomena are at a pre-scientific stage of development, and those that have reached 'maturity' are led in inquiry by a single, dominant paradigm.

By narrowly bounding the range of anticipated results in the conduct of normal science, a paradigm provides a mechanism, first, for the perception of novelties of fact, and, consequently, for its own demise. If an anomaly proves to be recalcitrant, i.e., it cannot be accounted for by the accepted theory of the scientific field, then it will incite a period of chaos during which the field returns to a condition similar to the pre-paradigmatic state where one or more competing schools battle with the accepted paradigm for ascendancy, and progress is suspended while debate rages over the relative merits (and demerits) of the different views. Eventually, however, the orthodoxy which had defined the previous period of normal science, and failed to explain the anomalous fact, is discarded and replaced by a fresh view that is generally constructed of new, incompatible basic principles which *are* able to account for the novelty. During such a revolution one paradigm necessarily follows the other. Anomalies are initially treated, not as counter-instances, but as research problems to be solved by the accepted theoretical framework. When this framework fails to subsume the novelty, it is only declared invalid when an alternative is ready to take its place. This is so because without a presuppositional matrix to guide the perception of the scientist--indeed to define his/her world of inquiry--there is no 'science' (Kuhn, 1970).

So on Kuhn's (1970) view, accepted theories are never final truths; instead, they are occasionally found wanting and replaced by more fruitful views during periods of revolutionary science. Once a new paradigm is crowned in the field, a new period of normal science ensues until the next crisis emerges. Truth, then, is plural, but only *serially*, never *concurrently*: only one paradigm is dominant at a time during a period of normal science in a 'mature' field of inquiry.[1] For

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[1]Kuhn's thesis that plurality is serial has led him to the conclusion that the gestalt shift discussed above is a useful, but not entirely accurate, prototype for what happens when a scientist is

this reason, Kuhn originally considered the social 'sciences,' with their multiplicity of competing 'schools,' to be in a 'pre-paradigmatic' state, and therefore at a 'pre-scientific' level of development. He softened his position slightly in the 1969 Postscript by stating that in fields such as the social sciences, in which a plurality of schools compete for supremacy, each of these schools possesses a rudimentary form of paradigm and as such can be considered *scientific* communities. However, such a field will not make the transition to maturity until one of these unformed paradigms develops to a point where it will "...identif[y] challenging puzzles, suppl[y] clues to their solution, and [guarantee] that the truly clever practitioner will succeed" (Kuhn, 1970, p. 179). When this point is reached by one of the schools it will supplant its competitors because it will demonstrate efficiency in solving challenging puzzles, and will thereby usher in a fruitful period of normal scientific research. While this revised interpretation upgrades the status of the social sciences, it still casts them as the somewhat less refined, rural cousins of the more urbane, city-dwelling branch of the family. As we shall see below, post-Kuhnian philosophers such as Schwab (1978a) and Morgan (1983) view the existence of concurrent plurality in the social sciences in a completely different light, i.e., as a sign of the *maturity* and *progressiveness* of such

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converted to a new paradigm, for "scientists do not see something *as* something else; instead, they simply see it"; and "...the scientist does not preserve the gestalt subject's freedom to switch back and forth between ways of seeing" (Kuhn, 1970, p. 85). As we shall see below, one of the main assumptions of Schwab's (1978d/1971) strategy of 'arts of eclectic' (a method to rationally manage theoretical plurality in practical situations) is this ability to switch back and forth between viewpoints.

fields. And while Kuhn (1970) considers the existence of a single, dominant paradigm in the physical sciences to be the defining characteristic of their maturity, philosophers of science such as Lakatos (cited in Gholson & Barker, 1985) consider the simultaneous existence of competing languages of inquiry to be the norm in *all* sciences, social or natural.

The Context of Discovery

An issue raised by Kuhn's amended version of scientific change involves *the context of discovery* in a science faced with a stubborn anomaly. When a practitioner is faced with a situation where his/her accepted theory (and perhaps his/her methodology, exemplars, and/or tools used in inquiry) no longer provides the means to explain an observed event, what is the rational basis for the discovery of a new paradigm that will account for the novel fact? This question arises when you consider that scientific observation, on the postpositivistic view, involves a necessary transaction between the prior experience and knowledge of the observer and the theory-neutral world. If what we know about the world is no longer a function of *passive* perception, then there is a creative, human element in the process of devising a new conceptual framework that will account for an anomalous fact. For those who believe that passive perception is possible (i.e., the positivists) there is no need to talk about a context of discovery: what you know is based upon hypothesis testing through objective empirical observation. Once a hypothesis has passed enough experimental tests, all that remains to do is to use formal logic to make "...clear and explicit the relation of the newly proven theorem to previously accepted theorems and axioms"

(Brown, 1977, p. 131). However, when the spectre of theory-laden perception is raised, leading to a conclusion that discoveries in science are not a product of objective observation, but are instead creative constructions imposed on reality to see if they are fruitful, this leads those who possess a *strict* view of rationality (i.e., it must be logical and rule-based, thereby eliminating imperfect human judgment) to the conclusion that the postpositivists are underpinning scientific progress with a crumbling foundation of irrationality.

Kuhn (1970) does not do much to allay the fears of the positivists (or even the postpositivists) about the rational basis for discovery in his revised version of scientific change. He mentions in two places (pp. 89-90, 122-123) that a new paradigm often emerges in an intuitive flash, usually in the middle of the night. The recipients of these intuitions are usually younger scientists or those who are new to the field; in other words, individuals who have not had the time to become greatly committed to the orthodox framework. In Kuhn's opinion, such flashes of intuition are not irrational, i.e., they are not divine inspirations, they are instead linked to the experience of the scientist.

> Though such intuitions depend upon the experience, both anomalous and congruent, gained with the old paradigm, they are not logically or piecemeal linked to particular items of that experience as an interpretation would be. Instead, they gather up large portions of that experience and transform them to the rather different bundle of experience that will thereafter be linked piecemeal to the new paradigm but not to the old. (Kuhn, 1970, p. 123)

Here Kuhn is stating that the intuition of the creative scientist is based on the experience s/he gained under the old paradigm, but that there is little relation between the way experience is bounded under the old and new regimes. But if the experience provided by the old paradigm is almost completely incommensurable with the experience provided by the new structure, then to what extent can the intuitive or creative act possibly be informed by that previous experience? It seems to me that if two theoretical languages are almost completely foreign to each other, then there is little rational basis for saying that the view of the world provided by a discarded language is the basis for the creation of a replacement. Below, I will outline a logic of discovery provided by Brown (1977) which defuses this problem by positing that competing paradigms are much more commensurable than allowed for in Kuhn's interpretation of the process of discovery.

The Incommensurability Thesis

Kuhn's incommensurability thesis (i.e., the assertion that two competing paradigms are constructed from different values, beliefs and concepts, and are therefore mostly incomparable and incompatible) has been the basis for most of the criticism levelled at him by other philosophers of science. Many of Kuhn's peers consider his beliefs about incommensurability to be too radical, leading his version of scientific change down a slippery slope into relativism and irrationality. In the next few paragraphs I will lay out Kuhn's beliefs concerning this crucial issue, and I will demonstrate how Kuhn clarifies and somewhat moderates his doctrine of incommensurability in places. As well, I will outline the impact that this thesis has had on his beliefs about how scientists choose between competing paradigms, and how science has progressed. In the end, it will be demonstrated that much of the criticism aimed at Kuhn is too strong: he is not the radical relativist that he is often made out to be. Notwithstanding this, I can understand how many were led to label him a radical given that some of his statements concerning incommensurability are more zealous than others.

In the logical empiricist version of the steady accumulation of knowledge since *the* scientific revolution of the 17th century, it is allowed that empirical data will sometimes cause propositions to change; however, the idea of there being conceptual change is seen as impossible since they view reality as singular, immutable, and something that can be observed directly and represented positively. On this account, the meanings of terms are kept separate from theoretical propositions, with the terms remaining constant even if some theoretical revision has occurred (Brown, 1977). Kuhn (1970) replaced this notion of the additive development of science with his rendition, derived from historical inquiry, of periods of normal science--in which progress is cumulative--interspersed with cataclysmic breaks called revolutions, during which the underlying presuppositions of a community of scientists are transformed, leading them into a new age of normal puzzle-solving under a revised set of fundamental beliefs and concepts.

So a 'scientific revolution' in Kuhn's account is an episode that occurs *again and again* in a field of inquiry, and leads to changes in the network of conceptual meanings through which scientists view their objects of inquiry. An example of conceptual change from the revolution Einstein wrought in physics involves the notion of 'mass': "Newtonian mass is conserved; Einsteinian is convertible with energy. Only at low relative velocities may the two be measured in the same way, and even then they must not be conceived to be the same" (Kuhn, 1970, p. 102).

Apart from changes in meaning structures, there are attendant changes in 'paradigm methods and applications,' i.e., amendments in values and beliefs about such things as acceptable methods, definition of problem-field, and standards of solution:

> ...the reception of a new paradigm often necessitates a redefinition of the corresponding science. Some old problems may be relegated to another science or declared entirely "unscientific." Others that were previously non-existent or trivial may, with a new paradigm, become the very archetypes of significant scientific achievement. And as the problems change, so, often, does the standard that distinguishes a real scientific solution from a mere metaphysical speculation, word game, or mathematical play. (Kuhn, 1970, p. 103)

Together, these changes led Kuhn (1970) to the conclusion that "the normal-scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with that which has gone before" (p. 103). On page 6, Kuhn (1970) discusses how scientific revolutions involve "...the rejection of one time-honored scientific theory in favor of another incompatible with it." On page 85, Kuhn (1970) states that during the transition period from one paradigm to another, "...there will be a large but never complete overlap between the problems that can be solved by the old and by the new paradigm." However, once this 'transition period' is over, "...the profession will have changed its view of the field, its methods, and its goals" (Kuhn, 1970, p. 85). On page 149, Kuhn (1970) discusses how a succeeding paradigm will share vocabulary and apparatus with its predecessor, but that they will seldom employ them in the same way: "Within the new paradigm, old terms, concepts, and experiments fall into new relationships one with the other." The result of this is an inevitable "...misunderstanding between the two competing schools"

(Kuhn, 1970, p. 149). On page 150, Kuhn (1970) discusses how a scientist's conversion from an old paradigm to a new one "...is a transition between incommensurables...." Finally, on page 200, Kuhn (1970) discusses the fact that scientists operating from within different paradigms will use the same vocabulary to describe one phenomenon, but will perceive two different things. This is so, he asserts, because they ascribe different meanings to the terms they share: "They speak...from what I have called incommensurable viewpoints" (Kuhn, 1970, p. 200). This is an example which demonstrates Kuhn's contention that competing paradigms constitute scientists' worlds differently: two paradigms yield two incompatible views of nature. In conclusion, it must be stated that these quotations comprise some of Kuhn's more radical pronouncements about the incommensurability thesis. It is easy to understand how after having read such statements one could come to the conclusion that Kuhn discounts the possibility of any translation between competing paradigms.

However, there are also many passages where Kuhn (1970) allows for *partial* understanding between scientists who operate from within different schools. For example, he states on page 148 that the proponents of competing paradigms "...are bound *partly* to talk through each other" and "...must fail to make *complete* contact with each other's viewpoints [italics added]," not to *completely* misunderstand each other, or to *fail altogether* to make *any* contact. Moreover, on page 149 he states that "communication across the revolutionary divide is inevitably *partial* [italics added]," not *impossible*. Another example appears on pages 200-201, where Kuhn (1970) discusses how concepts transfer their subset in a replacement paradigm, causing scientists who once had full communication and understanding to "...suddenly find themselves responding to the same stimulus with incompatible descriptions and generalizations" (p. 201). Notwithstanding this, such breakdowns in communication "...will not be felt in all areas of even their scientific discourse"; that is to say, conversation with partial understanding is still possible, there is some recourse (Kuhn, 1970, p. 201).

> The stimuli that impinge upon them are the same. So is their general neural apparatus, however differently programmed. Furthermore, except in a small, if all-important, area of experience even their neural programming must be very nearly the same, for they share a history, except the immediate past. As a result, both their everyday and most of their scientific world and language are shared. Given that much in common, they should be able to find out a great deal about how they differ. (Kuhn, 1970, p. 201)

Here Kuhn (1970) makes it clear that there exists a definite starting point for proponents of different paradigms to overcome communication breakdowns, to "...recognize each other as members of different language communities and then become translators" (p. 202). We shall see below that despite these moderating statements concerning incommensurability there are some philosophers of science who still consider Kuhn's version to be too radical.

Theory Choice

A first corollary of the incommensurability thesis involves the rational basis for theory choice. In the logical empiricist tradition, the final arbiter in all disputes about the truth of a theoretical account is empirical evidence derived from passive observation. Kuhn's theory of the development of science has downgraded the role of experimental evidence in theory choice because all observations involve a transaction between the inquiring subject--whose perceptions are dependent on his or her accepted disciplinary matrix--and theory-neutral reality. As we saw above, on Kuhn's view, two competing paradigms are constructed from different fundamentals, causing their proponents to see different things when they view one phenomenon and to partially talk through each other. Burgess (1985) discusses how, for critics of postpositivism, this raises the problem of relativism:

> if all observation is theory-laden, as postpositivistic philosophers argue, how can we hope to find a common ground of neutral observational data by which to judge the validity of competing knowledge claims derived from different theories? (p. 21)

To critics of the new philosophy of science this is a relativistic view of science, one where truth is determined by the presuppositions of the scientist (Burgess, 1985). This also raises the question of how the vanguard in a field of inquiry are to convince the entire community to adopt their new paradigm, when attempts to communicate their way of seeing the discipline's subject matter, its problems, its acceptable solutions, and its body of empirical support are hampered by the fact that these fundamentals are framed using a different theoretical language.

In Chapter XII, Kuhn (1970) discusses the resolution of revolutions. He begins by saying that a community of scientists cannot be converted to a new view of their discipline by simply counting up the number of problems solved by the old and new paradigms: "The competition between paradigms is not the sort of battle that can be resolved by proofs" (p. 148). This is so because of "...the incommensurability of the pre- and postrevolutionary normal-scientific traditions..." which leads to misunderstanding between the proponents of the two schools (Kuhn, 1970, p. 148). To allow the advocates of different views to fully communicate, one or the other must shift their paradigm: they must go through a conversion experience in which they shift theoretical lenses and see the world of their discipline in the light provided by the new presuppositional matrix. Such a shift cannot be forced and many scientists resist; they maintain their dogmatic commitment to the old view for quite some time. At this point in Kuhn's argument, he does not really explain how it is possible for the proponents of the new view to convince the advocates of the old view to convert when their theoretical languages are incommensurable (this he does in his Postscript of 1969). But he states that eventually most are persuaded to convert, and for reasons that are *not always rational*.

This leads us to the question of how conversion is induced and resisted in a discipline undergoing a period of revolutionary science. Kuhn (1970) states that there are usually many reasons for the conversion of individual scientists. "Probably the single most prevalent claim advanced by the proponents of a new paradigm is that they can solve the problems that have led the old one to a crisis" (Kuhn, 1970, p. 153). However, comparative ability to solve problems is, by itself, rarely sufficient in persuading scientists to convert to the new view. Another argument that is persuasive is if the new paradigm allows for the prediction of phenomena that were not even suspected to exist under the old paradigm. These are some of the more 'rational' reasons to convert because they are based on the theoretical or empirical progressiveness of the paradigm. Kuhn (1970) states that "to scientists those arguments are ordinarily the most significant and persuasive" (p. 155).

There are, however, other reasons to convert that are considered less 'rational' by critics because they involve the subjective values and professional judgment of the scientist. First, there is "...the individual's sense of the appropriate or the aesthetic...", i.e. the consideration of which theory is 'neater,' 'simpler,' or 'more suitable' (Kuhn, 1970, p. 155). These reasons can be the deciding factor for many scientists because the new paradigm is still nascent: it still has not solved many of its own puzzles, and therefore cannot decisively demonstrate that it is more progressive than the old view. For this reason, the defenders of the old orthodoxy can mount counter-arguments that are equally persuasive:

> Usually the opponents of a new paradigm can legitimately claim that even in the area of crisis it is little superior to its traditional rival. Of course, it handles some problems better, has disclosed some new regularities. But the older paradigm can presumably be articulated to meet these challenges as it has met others before. (Kuhn, 1970, p. 156)

In addition, the defenders of the traditional rival can also usually point to problems that are not solved at all by the new view, or are better solved by the orthodox structure. "Even in the area of crisis, the balance of argument and counterargument can sometimes be very close indeed. And outside that area the balance will often decisively favor the tradition" (Kuhn, 1970, p. 157). This is an earlier statement of Garrison's (1986) thesis that the choice of the best theory to fit a body of data is often underdetermined by experience or experiment. This leads Kuhn (1970) to a conclusion that seems to contradict his statement (see above) that the most persuasive argument in theory choice is the problem solving ability of one theory vis-à-vis another: He asserts that "...paradigm debates *are not really about relative problem-solving ability*" (Kuhn, 1970, p. 157, italics added). The decision to embrace a new paradigm must be made with incomplete information about which view is the more fruitful, and is more a *leap of faith* about the future promise of the new candidate. But there must be a basis for this faith in the new paradigm, for this feeling that it will later prove itself, and Kuhn (1970) admits that "...it need be neither rational or ultimately correct...", that sometimes it will be based on "...personal and inarticulate aesthetic considerations" (p. 158). Curiously, Kuhn (1970) again flip-flops about the relative importance of empirical evidence and subjective considerations in theory choice when he states that: "this is not to suggest that new paradigms triumph ultimately through some mystical aesthetic. On the contrary, very few men desert a tradition for these reasons alone. Often those who do turn out to have been misled" (p. 158). So it is easy to see how some theorists have been led to label Kuhn a relativist and an irrationalist when his arguments vacillate on the relative importance of, on the one hand, empirical and theoretical fruitfulness, and on the other, aesthetic and intuitive considerations, in the selection of a paradigm.

Kuhn (1970) answers those critics who have charged him with relativism and irrationality (as a logical consequence of his 'radical' version of incommensurability) in the 1969 Postscript. He begins by stating that his arguments that (a) communication in debates over theory choice is inevitably partial, and (b) that the superiority of one contender cannot be proven by such discussions, have been seriously misinterpreted by philosophers.

> A number of them...have reported that I believe the following: the proponents of incommensurable theories cannot communicate with each other at all; as a result, in a debate over theory-choice there can be no recourse to *good* reasons; instead theory must be chosen for reasons that are ultimately personal and subjective; some sort of mystical apperception is responsible for the decision actually reached. (Kuhn, 1970, pp. 198-199)

Debates over theory choice are not about resorting to logical or mathematical proofs, because there is no common basis for such proofs: two competing paradigms are constructed from different starting premises and any debate over presuppositions can only be resolved by persuasion (Kuhn, 1970). But this does not mean that there are no good reasons for being persuaded, or that "...the reasons for choice are different from those usually listed by philosophers of science: accuracy, simplicity, fruitfulness, and the like" (Kuhn, 1970, p. 199). If there is a disagreement over the relative progressiveness of two theories, or there is agreement on this, but disagreement concerning the relative importance of fruitfulness, neither proponent of two competing paradigms is being 'irrational' or 'unscientific.' Each axiomatic system or paradigm has its own internal logic: "There is no neutral algorithm for theory-choice, no systematic decision procedure which, properly applied, must lead each individual in the group to the same decision" (Kuhn, 1970, p. 200).

But if there is no neutral language to resort to which is understood by both parties in a paradigm debate, then how can the inventors of a new view persuade the denizens of the house of tradition to convert? As we saw above, Kuhn (1970) solved this problem by rejecting radical incommensurability (or by clarifying his previously blurred position) in the Postscript. In Kuhn's view, both parties in such a debate experience the same stimuli, although they bound them differently; and both share their everyday, and most of their scientific, language and view of the world. As such, there is a starting point for translation which, "...if pursued, allows the participants in a communication breakdown to experience vicariously something of the merits and defects of each other's points of view..." (Kuhn, 1970, p. 202). So translation "...is a potent tool both for persuasion and for conversion" (Kuhn, 1970, p. 202).[2] The vanguard in a revolution begins by translating the few concrete research results achieved by the new paradigm using everyday terms and those disciplinary concepts still shared between the two theoretical languages, thereby demonstrating the (potential) fruitfulness of the new view, and converting a few of the more risk-taking members of the community to their side. As time goes on, they translate more and more research results, allowing more and more scientists to experience the new view vicariously, until such time as the vast majority of community members are persuaded to convert, and a new period of normal science is inaugurated. So relativism is avoided because not just any view of the field is acceptable. As Brown (1977) makes clear, there are two contributors to the impressions we receive when we observe an object: our accepted theory and the action of the external world on our sense organs; therefore, a single object can be seen in different ways, but

[2]Cf. Schwab's (1978d/1971) 'polyfocal perspective,' developed through 'arts of eclectic.'

not in any way at all. Furthermore, the fruitfulness (or 'truth') of theories which compete to explain a single event is not decided by one observer, but by a professional scientific community *for good reasons.* These reasons include the ability of the new paradigm to (a) solve some generally recognized puzzle as no other can, and (b) preserve a large portion of the problem solving ability achieved by its predecessors (although all revolutions incur losses in this area as well--as we shall see below). In conclusion, then, for Kuhn (1970), theory choice has a rational basis, but it is a foundation based in arguments about the practical and theoretical progressiveness of one view vis-à-vis another, not in logical proofs or the ability of a theory to secure a one-to-one representation of objective reality. Below, I will demonstrate that Kuhn's views concerning the rational basis for theory choice are not that different from those philosophers who have accused him of irrationality and radical relativism.

Scientific Progress

A second corollary of Kuhn's incommensurability thesis is his characterization of the progress of science as non-cumulative. This version challenges the logical empiricist account of a single scientific revolution followed by the steady accumulation of knowledge thereafter. Kuhn's historical version of scientific development found revolutions occurring whenever recalcitrant anomalies could not be reconciled with existing theory. Such periods of extraordinary science involve the replacement of one paradigm with another constructed of different concepts, theoretical propositions, methods and applications. In other words, scientific revolutions are occurrences which shatter the continuity of knowledge acquisition in a field because they replace the fundamental presuppositions which structure patterns of inquiry and knowledge representation, and, indeed, the world of the scientist. Moreover, scientific revolutions have losses too: they do not always preserve all of the theoretical gains achieved by displaced paradigms as all theories bound reality in a distinctive fashion and are necessarily incomplete. So if science develops in such a fashion, then in what sense can it be said to *progress*?

Kuhn (1970) begins to answer this question by stating that solving the specialized puzzles laid out by a paradigm during periods of normal science is progress akin to the logical empiricist's version of development by accumulation. However, we must amend our notion of 'progress' when we discuss progress through periods of revolutionary science, when the fundamentals of a field are reconstructed from new, largely incommensurable axioms. On Kuhn's (1970) view, such revolutions close with a total victory for one paradigm in a battle between competing schools.

> Will that group ever say that the result of its victory has been something less than progress? That would be rather like admitting that they had been wrong and their opponents right. To them at least, the outcome of revolution must be progress... (Kuhn, 1970, p. 166)

And while progress through succeeding paradigms aims at preserving the gains achieved during previous periods of normal science, it never does so completely; in other words, while "...new paradigms seldom or never possess all the capabilities of their predecessors, they usually preserve a great deal of the most concrete parts of past achievement and they always permit additional concrete problem-solutions besides" (Kuhn, 1970, p. 169). So by adopting a new paradigm, a scientific community will sustain some losses alongside its gains: some of its old problems will be abandoned as irrelevant or unscientific. A revolution will narrow the scope of inquiry in a field, and increase its specialization. In this sense scientific disciplines will grow in depth and precision. But does science grow in breadth as well? Kuhn's (1970) answer to this question is a reflection of his belief that plurality is serial. If science does grow in breadth it does so through "...the proliferation of scientific specialties, not in the scope of any single specialty alone" (Kuhn, 1970, p. 170). As we shall see below, other theorists--particularly social scientists--disagree with this version of the growth in breadth of science. Instead, they assert that plurality is concurrent in most disciplines and that this is a means by which the field expands its breadth and matures.

Kuhn (1970) concludes, then, that progress in science may not lead us closer and closer to objective truth. The development of science, in his account, is "...a process of evolution *from* primitive beginnings--a process whose successive stages are characterized by an increasingly detailed and refined understanding of nature" (p. 170), but not toward "...some one full, objective, true account of nature..." (p. 171). We should not view progress in science as an 'evolution-from-what-we-do-know,' which is an account of progress which assumes we can achieve positive representations of nature and simply add to this corpus of knowlege in cumulative fashion. Instead, we should view progress as a process of 'evolution-toward-what-we-wish-to-know' in which, with each revolutionary stage, we achieve better provisional exemplars of the phenomena we formally represent, and we increase the articulation and specialization of our scientific disciplines. In other words, scientific progress is a development from theories less adept at solving puzzles to those more able to do so. This we can achieve even without a "...coherent direction of ontological development", "...without benefit of a set goal, a permanent fixed scientific truth..." (Kuhn, 1970, p. 206 and p. 173). Again, below, we shall examine the work of other philosophers of science who disagree with Kuhn, this time on his version of scientific progress. These critics view the development of science to be much more cumulative because they consider succeeding theories to be much more commensurable than what is allowed for in Kuhn's account.

Post-Kuhnian Philosophy of Science

Kuhn's (1970) seminal work on scientific change has been interpreted in different ways by various philosophers, practitioners of science, and social scientists. Gholson and Barker (1985) discuss how many practicing scientists and social scientists have accepted Kuhn's apparently radical version of the incommensurability thesis, and the concomitant assumption that scientific truth is relative. On the other hand, they point out that many philosophers of science also interpreted Kuhn's work the same way and were consequently unable to accept his ideas because of the connected charges of relativism and irrationalism. Gholson and Barker (1985) agree with Kuhn's assertion in his Postscript that his ideas on incommensurability and rationality in science have been misinterpreted. As such, they refer to 'kuhnian ideas' as misinterpretations of his work (i.e., that he supports a radical incommensurability thesis and a relative view of scientific truth) that are attributed to him, as opposed to 'Kuhn's ideas' which do not lead down a slippery slope to irrationality. Although they consider Kuhn's work to have been misinterpreted by many readers, Gholson and Barker (1985) still proceed to outline what they consider to be improvements to his ideas contributed by Lakatos and Laudan. In this vein, I will now also offer additions to, and alterations of, Kuhn's ideas proffered by a variety of philosophers of science, social scientists, and philosophers of education. These ideas and alterations will relate to the crucial issues highlighted above in my treatment of Kuhn's ideas: e.g., the nature of plurality, discovery, theory choice, and progress in scientific practice. In the end I will examine various approaches to dealing with plurality in science and I will discuss a strategy which can be used both by the creators of scientific knowledge and the consumers of their research (i.e. those professional practitioners whose knowledge base is in part or in whole made up of the products of formal inquiry) to ensure that their methodological and theoretical choices are informed ones. Before I continue I must provide notice that although this section is entitled *Post-Kuhnian Philosophy of Science*, I will discuss the works of a few theorists which predate that of Kuhn because the ideas contained in them are thoroughly post-Kuhnian in spirit.

Plurality: Serial or Concurrent?

Kuhn's (1970) account of theoretical plurality is serial. Scientific revolutions finish when one competing school defeats all others, and is adopted as the next normal scientific tradition to be developed by a community of scientists. In other words, only one paradigm exists during a period of normal puzzle-solving, to be replaced with a new view when a recalcitrant anomaly brings the next revolution in the discipline. You will recall, as well, that Kuhn viewed the concurrent existence of a plurality of schools in a field to be a sign of the pre-scientific state of the discipline. The opinion ranged against these assertions comes from a variety of fields, i.e., from philosophers of science such as Gholson and Barker (1985, after Lakatos) and Toulmin (1982); from social scientists such as Eisner (1991), Shulman (1986b), Morgan (1983), and Schwab (1978a); and from the pragmatist philosopher James (1969/1907).

Gholson and Barker (1985) adopt Lakatos's assertion that a plurality of research programs can exist simultaneously in a field of inquiry.[3] Moreover, Lakatos rejects Kuhn's belief that once a paradigm is supplanted, it does not reappear. Instead, on his view, competing research programs can undergo periods of decline and revival in which one approach will prove to be more theoretically and empirically progressive for a time, and will therefore become the more reasonable choice--for the moment--for many scientists within a disciplinary community. However, if a competing program eventually proves to be more fruitful in solving the problems of the discipline, then the first program will stagnate while the second enjoys a period of revival. On Lakatos's view, at any one time it is usually possible to delineate a more 'reasonable' choice of research program by evaluating the fruitfulness of one approach vis-à-vis another. For example, Gholson and Barker (1985) discuss the coexistence of the 'behavioral' program

[3]Lakatos replaced Kuhn's concept of 'paradigm' with his notion of a 'research program.' I will discuss the difference between the two below in the section devoted to incommensurability.

and the 'cognitive' program in the psychology of learning. Both research programs were fairly well developed by the early 1930s when the behavioral program began to bear more theoretical and empirical fruit, leading to the stagnation of the cognitive program for some 20 years. By the late 1950s, because of discoveries in the field, it was no longer clear which program was the more reasonable choice. But soon after, from the 1960s on, the cognitive program revived itself through rapid empirical and theoretical progress, supplanting the behavioral program as the more dynamic choice. However, unlike Lakatos, Schwab (1978a), Toulmin (1982), Shulman (1986b), Eisner (1991), Morgan (1983), and James (1969/1907) do not feel that there is always a clear choice to be made between coexisting programs, i.e., sometimes they bound subject matter in such different ways that the most reasonable thing to do is to consider them *all* progressive in their own way, but to clearly understand the partiality of view afforded by each so that the choice of any of them matches one's own purposes for inquiry or practice.

Schwab (1978a) states that most disciplines are informed by several structures for two reasons: first, they apply different truth or verification criteria to the subject matter; and second, in the asking of different kinds of 'telling questions,' they each "...[give] rise to a different pattern of experiment, different sorts of data, and different ways of interpreting data" (p. 243). Schwab (1978a) also gives an explanation for why Kuhn (1970) might have concluded that the proper pattern for plurality in the harder sciences is a serial one. He begins by outlining how either reliability or validity is emphasized in a science. The criterion of reliability requires that the guiding principles of inquiry be clear and unambiguous, that what constitutes relevant or irrelevant data is precisely determined, and that the "...manipulations and measurements..." used in inquiry "...be precise and repeatable with uniform results" (Schwab, 1978a, p. 243). This approach to science sacrifices the full complexity of the subject matter: "A conception of the subject matter is adopted which ignores whatever is unamenable to existing methods of reliable investigation" (Schwab, 1978a, p. 243).[4] So in order to increase the reliability of research results the complexity of the subject matter is cut, and by doing this, a science necessarily sacrifices some of the *validity* of its research results. Other sciences are instead devoted to validity rather than reliability. The criterion of validity involves choosing "...guiding conceptions of the subject matter which seem to come closest to fitting its complexity" (Schwab, 1978a, p. 244).[5] But because there are many different ways of conceiving of that complexity, the result

> ...is that validity-guided sciences tend to develop and exploit many different principles of inquiry at a given time. By contrast, the reliability-guided sciences tend to show their diverse principles and structures serially instead of concurrently. (Schwab, 1978a, p. 244).

According to Schwab, the harder natural sciences, i.e. mathematics and physics, emphasize reliability, and so physics has only had a few structures that tend to appear serially. By comparison, validity-guided sciences such as the social sciences tend to have a plurality of structures coexisting at once.

[4]Cf. Campbell and Stanley's (1963) notion of 'internal validity.'

[5]Cf. Campbell & Stanley's (1963) concept 'external validity.'

Like Lakatos, Schwab (1978a) believes that structures can undergo periods of stagnation and revival, but that an out-of-fashion view is not usually falsified or proven wrong, it is just considered to be 'less true' for the time being. Furthermore, Schwab (1978a) believes that in a validity-guided science there will be a multiplicity of views that it may be 'reasonable' to choose from (not just a single choice as Lakatos believes) because each of them structures the subject matter of the discipline differently, each of them is bearing fruit on its own terms, and "none of them is so much wrong as incomplete..." (p. 246). Also, because "the problems of any field are usually too diverse to permit their solution by only a single pattern of enquiry," most fields will develop such a plurality of structures which, taken together, will solve *more* disciplinary problems (Schwab, 1978a, p. 239). So rather than being a sign of the backwardness of a discipline as Kuhn believes, the concurrent existence of a plurality of structures is, to Schwab, a sign of its progressiveness. What is important to Schwab is that researchers and consumers of scientific knowledge make rational choices when they choose a methodology or a theory to use in their practice. This they can do by understanding *how* each view is partial: "...what restricted chunk of the world..." is presented by the application of a structure to subject matter (Schwab, 1978a, p. 238). This they can achieve by 'enquiring into enquiries' or learning 'arts of eclectic' to understand how a structure bounds reality, i.e., what it illuminates and obfuscates about a particular field of inquiry. I will return to Schwab's methodology for dealing with plurality in a practically rational fashion below.

Toulmin (1982) agrees that a variety of parallel styles of interpretation can exist in a discipline. As an example, he discusses different ways of interpreting a single biological event, i.e., by emphasizing the event's biochemical aspects, physiological aspects, morphogenetic aspects, or evolutionary aspects. According to Toulmin (1982), all of these points of view contribute complementary interpretations of the subject matter, and help to make our understanding of it more complete. Similar to Schwab, Toulmin feels that the choice of one or many interpretive standpoints is made rational by an understanding of the purposes embedded in such viewpoints. Such purposes cause the field of inquiry to be bounded in one way, rather than in another, and by knowing how they impose borders on reality we can rationally determine which is better in a certain context and for certain purposes (Toulmin, cited in Donmoyer, 1985).

Shulman (1986b) uses Lakatos's concept of a research program rather than Kuhn's 'paradigm' to discuss the nature of plurality in educational research. He rejects Kuhn's argument that the absence of a dominant paradigm in a social science is a sign of "...a developmental disability, a state of preparadigmatic retardation" in a field (Shulman, 1986b, p. 5). Instead, after Merton, Shulman (1986b) asserts that the condition of having a competing set of research programs is superior to the hegemony of a single school in a discipline.

> ...theoretical pluralism encourages development of a variety of research strategies, rather than premature closure of investigation consistent with the problematics of a single paradigm. Different paradigms alert research workers to different phenomena of interest, different conceptions of problem, and different aspects of events likely to be ignored within a single perspective. (p. 5)

To Shulman (1986b), then, "...the coexistence of competing schools of thought is a natural and quite mature state" for social sciences such as education (p. 5). Like Schwab and Toulmin, Shulman (1986b) recognizes that each research program is intrinsically incomplete, but that by understanding the trade-offs among approaches, we can find "...some ways in which those insufficiencies might complement each other" (p. 26). Similar to Schwab, Shulman (1986b) devises a methodology for surfacing the presuppositions of different research programs so that a rational choice between them, or a rational blending of them, can be made.

Morgan (1983), Eisner (1991), and James (1969/1907) also put a positive spin on the thesis that all paradigms are incomplete by recognizing that the flip side of this partiality is that there are *multiple potentialities* present in any object of inquiry which are only surfaced through the application of a plurality of research methods and forms of representation. James (1969/1907) posits that all our laws are incomplete and approximative, none is able to copy some eternal reality directly; instead, laws are man-made languages which "...tolerate much choice of expression and many dialects" such that "...any one of them may from some point of view be useful" (p. 48-49). There are, then, multiple theoretic formulas which are compatible with a world that we can consider to be eternally incomplete, i.e., a world with an *additive constitution* which actually benefits from a proliferation of alternative formulations. Eisner (1991) agrees that there are multiple realities present in most phenomena which are only revealed through the application of multiple forms of representation. By learning to see a situation through the lenses provided by different paradigms, by altering our frame of reference, we can begin to examine situations from different perspectives or viewpoints. When one is able to shift lenses, then one sees that it is possible to perceive many different potentialities in a single event, and that this is many more than would have been revealed by the use of a single procedure. (We shall see below that this ability to shift lenses, to be theoretically eclectic, is something that is achieved by Schwab's method of 'arts of eclectic.') Morgan (1983) also considers the incompleteness of view provided by each contributor to a plurality of research methods to be positive because it provides us with a wealth of possibilities. He sees different research strategies as 'voices' which offer diverse interpretations of events or "...different arguments in favor of understanding a phenomenon in a particular way" (Morgan, 1983, p. 374). Again, the goal in choosing from among this plurality of voices is to be rational by understanding "...the taken-for-granted premises..." in each approach to research so that we use methods that suit "...the issues being studied or the problem being solved" (Morgan, 1983, pp. 378-379).

A Logic of Discovery

Kuhn (1970) claims that there is a rational basis for the discovery of a new paradigm to replace one unable to explain a novelty of fact. On his view, a scientist will often come up with a replacement paradigm in a 'flash of intuition.' Such an event is rational, in Kuhn's opinion, because it is based on the experience gained by the scientist under the old paradigm. However, he also asserts that the new creation will be, for the most part, incommensurable with the view that it replaced. If the old and new views are that incompatible, then it does not seem likely that the former led to the latter in some rational fashion. In other words, in this version it seems as though discoveries are somehow intuited or divined, rather than being the rational result of a blend of past experience and new creativity. Unlike Kuhn, Brown (1977) succeeds in placing the context of discovery on a rational footing by demonstrating how the creation of a new paradigm is based on a *logic of discovery*. This is a logic which is different from that used by the logical empiricists in the context of justification, but which can be considered 'rational' just the same.

Brown (1977) outlines how the logical empiricists equate 'rationality' with the application of formal logic, mechanical rules, or algorithms: "...any departure from mechanical rules is a step into the realm of the irrational, or more accurately, the 'arational'..." (p. 132). Brown (1977) labels this "...a strange concept of rationality..." since rule-based decision making would not seem to require any thought at all, let alone *rational* thought (p. 132). In contrast, an ambiguous practical situation in which mechanical rules are of no help whatsoever is, to Brown, a paradigm case of a situation requiring *reason*, but we will need to conceive of 'reason' as being a wider category than just 'formal logic.' Brown's (1977) logic of discovery is an attempt to demonstrate that theory creation has an intelligible structure of rational thinking, "...even though this may not consist of necessary relations" (p. 132).

Brown (1977) chooses Platonic dialectic as a rationality which can inform practical decision-making because it is a content logic, whereas the deductive logic of the logical empiricists is one concerned only with formal relations between scientific propositions. In order to understand the context of scientific discovery, one needs a rational tool that is capable of analyzing the content and structure of successive paradigms in terms of their historical context. In Brown's (1977) view the Platonic dialogue provides us with a useful exemplar of practical reasoning that involves the level of presuppositions.

In a Platonic dialogue there are attempts to answer questions, but these questions arise in the context of a set of presuppositions. Within the boundaries set out by these presuppositions there are many possible answers to a question. Although the presuppositional structure will suggest a direction, it will not prescribe the answer that ought to be put forth at a given time. As well, the presuppositional matrix will rule out many self-consistent responses. So if a point is reached in the discussion where an answer to a question is inconsistent with the presuppositions framing the discussion, there are two ways to proceed: first, the response can be reframed to fit the bounds created by the accepted presuppositions; or second, attempts can be made to alter these priori assumptions and thereby change the course of the dialogue. If the latter path is chosen by a discussant, this does not lead to a total break in the development of the conversation, for

> [w]ere he to hold no presuppositions at all in common with the others, rational debate would become impossible; it is the presuppositions the protagonists hold in common that provide the touchstone for debate. (Brown, 1977, p. 134)

Like a participant in a Platonic dialogue, a creative scientist faced with the task of accounting for anomalous data is involved in a dialectical process that involves the level of presuppositions.

When faced with a conflict between accepted theory and novel fact, the first response of the scientist is to offer an hypothesis which is in agreement with the conventional paradigm. When this maneuver fails to achieve the desired result, i.e., an explanation of the anomaly, the scientist will begin to change her/his presuppositions in an attempt to account for it. However, in Brown's (1977) opinion, this change in presuppositions does not constitute a complete break with tradition:

> ...the revolutionary thinker [begins] with a problem generated by an existing research tradition, [takes] his guidance from this tradition, and [moves] away from it only with the greatest caution and in small steps. (p. 137)

Brown (1977) provides a useful example in his discussion of Kepler's discovery that Mars's orbit is an ellipse. The conventional view of planetary orbits at the time was that they were circular. Kepler found that this assumption did not fit the data he had collected in his study of the Martian orbit. Rather than jettisoning this theory when he encountered a counter-instance, Kepler assumed something was wrong with his methodology. "Only very slowly and reluctantly did he give up the circle" (Brown, 1977, p. 137). His first step away from the circle is characterized by Brown (1977) as "...a reasonable next step...", not, on the one hand, as something that follows directly from having rejected the circle, nor, on the other hand, as "...an arational guess..." (p. 137). This is so because Kepler, in his search for a replacement figure, first chose an ovoid: a shape that had some things in common with a circular orbit and was therefore not much of a conceptual stretch. Because Kepler's use of an ellipse first arose out of computational difficulties associated with the use of the ovoid, Brown concludes that, although creative, this discovery had a footing in the scientist's previous experience. In this way, then, Brown (1977) has demonstrated "...that there is an intelligible basis in the existing tradition for even the most revolutionary proposals..." and that "...this in no way detracts from the originality of the creative scientist" (p. 136). This is, without a doubt, a much more convincing argument that scientific discoveries have a rational basis than that provided by Kuhn, whose arguments are less persuasive because of his earlier, more radical version of the incommensurability thesis.

Incommensurability, Theory Choice, and Scientific Progress

Above, I demonstrated that Kuhn's (1970) statements concerning incommensurability and the rational basis of theory choice vacillated between versions that were more radical (and therefore more 'relative' and 'irrational' to some critics) and others which were less extreme. However, despite Kuhn's moderation of some of his more drastic statements, there are still philosophers of science who consider his positions on incommensurability, theory choice, and progress in science to be too extreme still. For example, philosophers of science such as Gholson and Barker (after Lakatos and Laudan) are uncomfortable even with Kuhn's moderation of the incommensurability thesis and go to great pains to demonstrate that (a) competing theories share a great deal conceptually and can therefore be compared to determine which one is clearly more progressive, and (b) that the progress of science remains cumulative. Although they accept that presuppositions play a role in scientific practice (although a lesser role than that described by Kuhn), they are concerned that the relativism and irrationalism that (in their opinion) this position leads to be greatly limited. In short, they are on a quest for *more* certainty than is allowed for in Kuhn's account. Still other philosophers of science, such as Brown (1977), seem comfortable with Kuhn's 'middle-of-the-road' beliefs that: (a) competing theories are *partially* incommensurable, (b) there remains a rational basis for theory choice because translation between different theoretical languages is possible, and (c) although science does not build up final truths cumulatively, it does progress serially from paradigms less adept at puzzle solving to those more proficient at it. Finally, there is a third group of theorists, consisting of Toulmin (1982), Morgan (1983), Eisner (1991), Schwab (1978a), and Shulman (1986b), who are comfortable with a more radical version of the incommensurability thesis and with a greater degree of relativism. They do not feel that this leads to irrationalism, but to a different form of rationality based on making informed decisions amongst a plurality of paradigms that reveal different potentialities in a subject matter. On their view, a field (particularly in the social sciences) can be informed by a concurrent plurality of paradigms each with their own internal logic. As a result, it is not possible to compare them to choose the one version that is more 'correct,' or 'true.' While some frameworks may be more progressive than others, all are fruitful to a degree, and all are incomplete in some way. Given this situation, the rational thing to do is to understand how these diverse theoretical languages bound reality differently and to choose those that suit your own purposes as a researcher or a professional practitioner. This section will explore these three positions, which are based on different degrees of acceptance of the incommensurability thesis and relativism in a field of inquiry.

Gholson and Barker: Preserving The Quest For Certainty

Although Gholson and Barker (1985) say that they are criticizing wrongheaded interpretations of Kuhn's ideas (i.e., that incommensurability is radical and scientific truth is relativistic), they also state that Kuhn himself is responsible for problems associated with incommensurability, i.e., that he has removed empirical evidence as the final, rational arbiter between competing paradigms because there is no neutral language with which to compare them. Gholson and Barker (1985) use the accounts of scientific change provided by Lakatos and Laudan to assert that competing scientific programs or traditions are much more comparable than allowed for by Kuhn, and, as a consequence of this assertion, to show that empirical evidence retains its crucial role in making theory choice rational. They also claim that scientific progress is much more cumulative than allowed for in Kuhn's account. In short, they have tried to reestablish two crucial elements of the scaffolding of modernity: the certainty of scientific truth (which is based on empirical evidence) and its cumulative pattern of growth. However, I will demonstrate how Gholson and Barker have paid more attention to Kuhn's more radical statements concerning the incommensurability thesis, rather than his more moderate versions of this idea, which allows for a different form of rationality in theory choice (i.e., one based on communal standards).

Gholson and Barker (1985) begin by stating that Kuhn's concept of a 'paradigm' as something so fundamental that it is immune from empirical testing has placed scientific knowledge on a foundation of irrationality. They attribute to Kuhn a belief that each paradigm has its own internal logic, causing proponents of different frameworks to talk past one another, and eliminating the possibility of a neutral standpoint from which to claim the objective superiority of one viewpoint or another. In other words, they see Kuhn as a proponent of radical incommensurability, which means that there is no rational basis for theory choice. As well, they consider Kuhn's thesis that the most important type of scientific change (i.e., when one paradigm is replaced by another) is not rule-governed (but is instead based upon the consensus of a scientific community) to allow the entry of irrational human judgment into his version of scientific progress.

Gholson and Barker (1985) believe that Kuhn's version of the development of science is corrected by accounts outlined by Lakatos and Laudan. Lakatos replaces Kuhn's notion of a 'paradigm' with his own concept of a 'research program.' A research program is a succession of theories that are linked by a 'hard core' of shared commitments. While succeeding theories are modified, 'hard core' assumptions will remain intact. A research program is either 'progressive' or 'degenerating.' It is progressive if each successive theory accommodates the achievements of preceding theories, accounts for the novelty of fact that brought the previous view into question, and leads to new predictions that are verified experimentally. If a program is not progressive, then it is degenerating: If anomalous data are not explained by succeeding theories, or if new theories raise more problems than they solve, then the program can be said to be in a state of decline. At this point, the majority of scientists within a scientific community will choose a new research program on the basis that it has proven to be more theoretically and empirically progressive. And often the choice a scientist must make is not to abandon a *degenerating* research program, but to leave a program that is progressive, but less so than a rival. So Lakatos's conception of a research program allows for the cumulative growth of scientific fact through a succession of theories that are linked by a hard core of shared commitments. This is a much more continuous view of scientific change than that provided by Kuhn, who asserts that: (a) theoretical views are a large part of what makes up a 'paradigm'; (b) a change in theory to accommodate a novelty of fact is, in fact, a paradigm shift; (c) such shifts are changes in language that reconstruct a scientific community's view of the field from a new, largely (but not completely) incommensurable, set of axioms; and (d) because of this incommensurability, and because the replacement paradigm will not preserve all of the successes of its predecessor, the development of science cannot be considered cumulative.

Another fundamental difference between Lakatos's 'research program' and Kuhn's 'paradigm' is that the choice of a new, more progressive research program (with a different hard core of shared commitments) is not always a final decision: such programs can coexist and undergo periods of degeneration and revival. In contrast, Kuhn saw the replacement within a scientific community of one paradigm with a largely incompatible rival as a final choice. Additionally, the choice among coexisting rivals, on Lakatos's view, is one that can be made *rationally*, because the replacement of one program with an *incompatible* one is not what happens in the vast majority of cases. In other words, in Lakatos's opinion, Kuhn's incommensurability thesis (or the radical version of it) does not stand up to scrutiny. He found instead that rival research programs contribute fruitfully to each other, which would not occur under the incommensurability thesis. For example, in examining the revival of the cognitive program at the expense of the behavioral program in the early 1960s, Gholson and Barker (1985) found that cognitive theorists had incorporated an important element of the behavioural program (i.e., Markov models) into their account of the psychology of learning. This refutation of the incommensurability thesis therefore paved the way to a rational basis for comparing and appraising rival programs: if they contributed elements to each other then there must be a common core of understanding which could then be used to make a relative assessment of the theoretical and empirical progressiveness of each approach. Scientists could then have a *reasoned preference* for one program or another: the experimental evidence garnered by each approach could now be compared to determine the more rational choice.

At this point I must comment that Lakatos's account of rationality in theory choice does not seem all that different from the less radical version Kuhn provided in his 1969 Postscript. Like Lakatos, Kuhn assumes that the proponents of different paradigms share everyday and scientific concepts, and that this provides enough common understanding to form a rational basis for theory choice. However, their versions diverge from here: On the one hand, Lakatos seems to feel that there exists enough of a common conceptual base between research programs to reinstate empirical evidence as the final arbiter of scientific truth, for rival research programs will not bound reality *all that differently.* On the other hand, Kuhn concludes that although the unique view provided by a paradigm (and its theoretical and empirical successes) can be translated into terms somewhat understandable by outsiders, and will therefore provide a series of short, vicarious glimpses of the world through new theoretical lenses, this is still a process of *persuasion*, not one where the objective empirical superiority of a rival structure will be immediately obvious to an observer. And so scientists are persuaded to adopt a new paradigm for good reasons, and the consensus of the scientific community will determine what constitutes a provisional truth. In the end, the progressiveness of a paradigm or research program provides a good reason for adopting it on either account, i.e., whether you can clearly see that a view is more fruitful, or whether--bit by bit--you must be persuaded (by immersion) that a language which is mostly foreign to you is more accurate than your own in portraying reality.

Gholson and Barker (1985) consider Laudan's view of scientific change to be superior to that of Lakatos on a number of accounts. Two of these differences are relevant to my discussion. First, Laudan replaces Lakatos's concept of a research program "...with a super-theoretical entity called a 'research tradition'" (Gholson and Barker, 1985, p. 761). A research tradition is made up of a family of theories that share common presuppositions which are concerned only with ontology and methodology, *not* theoretical propositions as in Lakatos's 'hard core' commitments. For example, these metaphysical propositions perform functions such as articulating an ontology to which theories in the tradition must conform. However, in his account, these propositions are not *functionally* metaphysical because Laudan considered this core of shared commitments to be something that was malleable: i.e. they changed as the research tradition developed through consecutive theories. Although Laudan saw the need for a research tradition to provide *some* continuity in inquiry, he felt that there was no aspect of such a tradition that was so essential that it resisted change when faced with new evidence. In short, Laudan has provided another sense in which the progress of science can be said to be cumulative: malleable core commitments are seen to evolve rather than being replaced through violent revolution.

A second criticism of Lakatos's work that is relevant to my purposes is Laudan's belief that, while theoretical and empirical progressiveness are important factors in theory appraisal, they are not the only considerations. Laudan posits that there are also important conceptual factors that play a role in the acceptance or rejection of a theory. For example, a major defect which would lead to a tradition not being accepted in a scientific community is if a concept in that theory conflicted with an established notion of truth. Gholson and Barker (1985) demonstrate this by discussing Maxwell's theory of light, which, although progressive, was not accepted for some time because it included a concept of force that conflicted with the orthodox (Newtonian) view of the day. To me, this addition of 'conceptual considerations' to the list of factors involved in theory appraisal is reminiscent of the logical empiricist's application of formal logic to verified theoretical propositions to ensure that they will fit the accumulated body of accepted scientific facts.

With their refutation of Kuhn's incommensurability thesis, Gholson and Barker (1985) conclude that empirical evidence has been restored to a key position in accounts of scientific progress, eliminating the relativism and irrationality of Kuhn's account. A clear sense of progress emerges when scientific change is conceived of as a process in which it is possible to compare one research program with another, and both with empirical reality, to determine which one is more theoretically and empirically progressive in explaining the phenomena of interest in a field of inquiry. Furthermore, because research programs fruitfully contribute concepts, methods, and etc. to other programs, this provides another sense in which the progress of science can be said to be cumulative. Finally, Laudan's conception of core metaphysical commitments that change in response to empirical testing is a view of scientific change that is more evolutionary than revolutionary.

Again, I must point out that this account of scientific progress has aspects that are similar to Kuhn's version. While Kuhn would not consider succeeding theories to share as much conceptually as Lakatos and Laudan do, there is in his account a sense in which new propositions and concepts are built upon old ones. Moreover, Kuhn agrees that science progresses through a series of theories which are more precise and fruitful than their predecessors. However, this does not mean that these consecutive theories are leading us closer and closer to an objective account of nature. In contrast, Laudan seems to preserve the positivist dream of passive perception of objective reality by his removal of theoretical propositions from his malleable core of commitments. And Lakatos posits that although proponents from competing research programs will not share all concepts, there seems to be a large overlap in their descriptions of nature, leading to the conclusion that empirical observations have, to some degree, a universal basis. So it seems as though Lakatos and Laudan are attempting to reinstate the positivist presuppositions of passive perception and positive truth (or at least *more certain* truth) in the philosophy of science. Another crucial difference in Kuhn's account of scientific progress is that he believes that succeeding theories, because they are built on different premises, do not preserve all of the achievements of replaced accounts. By comparison, Lakatos considers this ability to preserve explanations provided by predecessors to be a sign of the 'progressiveness' of a theory. Clearly, then, Kuhn rejects such a cumulative account of theory change.

In the end, Gholson and Barker (1985) point out that Lakatos and Laudan have refuted all of Kuhn's claims concerning incommensurability, theory change, and the progress of science, *save one*. Kuhn's sole contribution to philosophy of science, on their view, is his identification of metaphysical commitments that determine the scientist's ontology and methodology in research practice. And even these are not so much 'metaphysical' (i.e., beliefs beyond empirical verification or refutation), nor are they very strong 'commitments,' for in Laudan's account they will change in light of new experimental evidence. So Lakatos and Laudan have banished irrationality and relativism from scientific accounts of the world by refuting the incommensurability thesis and by reinstating empirical evidence as the final judge of what constitutes scientific truth. It would seem that they have reinstated most of the key presuppositions of the positivist tradition, and that they consider this to be the more progressive account of the development of science.

So are Gholson, Barker, Lakatos and Laudan the progenitors of a new paradigm, or research program, or research tradition, in the philosophy of science? Or are they defenders of the positivist faith, trying to account for the anomaly Kuhn discovered (i.e., the existence of protected metaphysical beliefs) within the bounds provided by orthodox presuppositions? I have shown how they have rescued the field from relativism by demonstrating that competing theories can be compared on the basis of their empirical support. As well, I have shown how they have recovered the positivist sense of cumulative progress in science by refuting Kuhn's incommensurability thesis. This they have done by changing Kuhn's conception of a 'paradigm,' and refuting his version of scientific change, while explaining the anomaly which led to the crisis in the field, *all within the confines provided by positivist presuppositions*. In short, they have preserved the certainty of scientific truth, and in the process have demonstrated their commitment to a protected set of presuppositions. They have modified the positivist account of the development of science to explain the existence of protected propositions while preserving its main ā *priori* assumptions, and have even managed to locate evidence to support their account in the data corpus of the field (i.e., the history of science). However, as Garrison (1986) points out, "...given any finite body of data, an infinite number of theories may be tailored to fit the body"

(p. 14). Perhaps multiple realities can be constructed out of the history of science, each supported by events in this history. It is not inconceivable that Kuhn's and Brown's version of scientific change, and/or the account of philosophers (such as Toulmin) and social scientists who accept a greater degree of incommensurability and relativism, are just as progressive in explaining the progress of natural and social science as that provided by Gholson and Barker. In the end, maybe none of these viewpoints has solved enough 'normal scientific puzzles' to allow us to make a rational choice between them. Or possibly they are such different accounts--based on incompatible axioms--that we should not even bother to try to choose based on appraisals of their relative fruitfulness; instead, perhaps we should try to delineate what they include and leave out of their account of scientific progress.

Kuhn and Brown: Partial Incommensurability and a New Rationality

As stated above in the introduction to this section, Brown (1977) supports Kuhn's theses concerning incommensurability, theory choice, and scientific progress, while further developing them to demonstrate how they are based on a new conception of rationality which is as valid as that of the logical empiricists. Brown (1977) improves on Kuhn's thesis of partial incommensurability by providing a metaphor for a theory that demonstrates how concepts and propositions change when one paradigm replaces another.

> A scientific concept is a knot in a web; the strands in the web are the propositions that make up a theory; the meaning of a concept is its location in the web. Thus the meaning of a concept is determined by the strands that come into this knot, by the other knots the one in question is

> connected to, and by the further connections of these other knots. (Brown, 1977, p. 120)

When a scientific revolution is precipitated in a discipline by a recalcitrant anomaly, some creative scientist is provoked to formulate a new theory to account for the novelty of fact, and the following conceptual changes occur:

> Some of the strands which come into a particular knot are removed, others are redirected, and some new strands are introduced. The concept retains some of its old characteristics since some of the old strands are left intact, but it also loses some old relations and acquires new ones, thus we acquire a new version of an old concept. (Brown, 1977, p. 120)

So a scientific revolution is an event in which a scientific community's view of its field is reconstructed with fundamentally different presuppositions and concepts: new theories are not deduced from old ones. But is there any fundamental logical relationship between successive theories or are they completely incommensurable? And, perhaps more importantly, is there any rational basis for comparing competing theories so that the choice of one or the other is a reasonable one?

Brown (1977) answers these questions by returning to the notion of dialectic that he developed when he examined the context of discovery. The conceptual change which occurs during a scientific revolution is similar to the decision in a Platonic dialogue to continue discussion under a new set of presuppositions. There is no complete break in the development of the argument: some presuppositions are held in common otherwise rational discussion would be impossible. Similarly, the relationship between an old theory and a new one is also dialectic:

> A dialectical change from one theory to another is a reorganization of the strands of the theoretical web, along with the removal of some strands and the addition of others; this reorganization accounts for the changes in the meaning of scientific concepts and observations associated with a scientific revolution, since both the concepts and the data of a theory derive their meaning from their location in the theoretical web. But theory change takes place within definite problem situations, within which the strands retained in the new theory provide the continuity of development and the grounds for comparison even though these strands take on a different meaning in the new theoretical structure. (Brown, 1977, pp. 139-140)

So there are rational grounds for comparing two drastically different theories: First, the proponents of two different theories should be able to translate meanings for each other by using any unchanged propositional strands in a theoretical web as a common basis for building understanding. Second,

> [t]he new theory is continuous with the older one, for it grows out of the failures of the older theory to solve its own problems and to account for the entire range of phenomena that the latter itself selects as relevant, and it takes over many of the obervations, techniques and principles of the older theory while it changes their meaning. (Brown, 1977, p. 144)

Finally, there are a few specific phenomena that proponents of both paradigms can agree are relevant to a choice about which theory to embrace. These can also serve as a rational basis for discussion and persuasion. However, the choice of relevant phenomena, and the criteria to be applied in assessing competing theories are not determined by appeals to some logical algorithm or some theory-neutral set of observations; instead, they are determined by a scientific community's critical examination of their own specific problem situation (i.e., a recalcitrant novelty of fact), and the theories offered as solutions to this puzzle (Brown, 1977). The final arbiter in theory choice is the community of scientists who make up a discipline: they make what are hopefully wise judgments "...on the basis of information and experience, but without the benefit of necessary truths or algorithmic procedures which can guarantee a decision immune from being overturned by future research" (Brown, 1977, p. 150). In the end, "...the best that science can hope to attain is tentative rational consensus on the basis of available evidence" about which paradigm is more progressive or fruitful (Brown, 1977, p. 152).

Brown (1977), offers Aristotle's 'man of practical wisdom' as his new model of rationality in science, and thus further develops Kuhn's amended notion of scientific reason. In the new philosophy of science, it is the community of scientists who are the final adjudicators of what constitutes truth and knowledge. Again, there is no appeal to some set of logical rules or to purely objective observations when faced with a choice between two paradigms which are constructed of different axioms, and which therefore constitute different languages needing translation. As Brown (1977) points out, "...theories provide the only access we have to reality" (p. 153). As a result, different scientists can construct two different theoretical solutions to a single disciplinary puzzle that finish with conflicting conclusions *without either being irrational*. However, even though both are arrived at rationally and have some empirical support, this does not mean that both constitute scientific 'truth' or 'knowledge.' A theory becomes scientific knowledge when it is accepted as such by the relevant community of scientific practitioners. They must deliberate, weigh information, and make *rational human judgments* in the absence of any first principles (e.g., a set of rules to follow) or necessary knowledge (e.g., that provided by 'theory-neutral' observations). We must rely on their collective expertise and intricate knowledge of their problem-field to lead us out of the desert when novelties of fact cast doubt on the adequacy of previously accepted truths.

> It is the ability to decide how an exceptional case should be handled that is characteristic of rationality. That logic and experiment alone cannot decide the fate of theories does not imply that these decisions are irrational. It implies that they require judgments in which the results of logic and experiment are taken into account along with all that the scientist knows about the current state of his discipline.

However, practical rationality must not be considered perfect: group decisions during paradigm debates about which competing theory is more empirically and theoretically progressive are not infallible. (But they can be considered superior to individual decisions.) Sometimes a scientific community does not recognize that the theory they have accepted as true is, in fact, inadequate. But, as Brown (1977) points out, the community of scientists has been remarkably self-correcting. As Kuhn (1970) notes, this is probably because of the effectiveness demonstrated by paradigm-based normal science in surfacing those areas where the accepted theory does not mesh with reality. Eventually all paradigms will be replaced for this reason, but if a scientific community has chosen unwisely, this will no doubt lead the discipline into crisis much faster than is normally the case.

Brown (1977) has also added conceptually to Kuhn's (1970) version of the progress of science. He begins by stating that the new philosophy of science has destroyed any "...grounds for maintaining that any scientific claims are infallible..."; in other words, it has eschewed the logical empiricists' 'absolutist epistemology' (Brown, 1977, p. 151). In postpositivist epistemology, science does not develop by accumulating indubitable, final truths. Instead, all scientific truths are provisional accounts that attain the status of 'knowledge' through the rational consensus of a scientific community. On Brown's (1977) account, once we accept the presuppositions that all perception is theory-laden and that obtaining final truths is impossible it becomes pointless to deride this new version of scientific epistemology as 'relativism.' Brown considers it more fruitful to redefine the concept of 'truth' to allow for a plurality of provisional theories.

Brown (1977) begins by distinguishing between truth_1 and truth_2. Truth_1 refers to the 'absolute,' or 'ultimate,' or 'correct' description of a phenomenon. Scientists may strive for such an account, but it is impossible to achieve it as our only access to ontological reality is through a theoretical lens which bounds that reality in some partial way. In contrast, truth_2 refers to any *provisional* proposition which has been admitted into the body of knowledge in a field of inquiry by the community of scientists working in that discipline. Brown (1977) also delineates two concepts related to falsity: "...a proposition is false_1 if and only if it does not provide an adequate description of reality; it is false_2 when it is rejected by the current consensus" (p. 153). So now it is possible that a theory that is true_2 at the present moment was false_2 in the past, and vice-versa. Brown (1977) provides an example from the science of geology:

> The theory of continental drift...was overwhelmingly rejected by geologists in the 1920's and was thus false_2; during the past ten years it has been incorporated into the widely accepted theory of plate tectonics and is now true_2;

whether it is $true_1$ remains an open question. (p. 153)

During a period of revolutionary science when one school is trying to overtake another and become $true_2$, there may not yet be enough information concerning the fruitfulness of the theory for scientists to decide to accept or reject it. In this case we can consider it to be an hypothesis that is neither $true_2$ nor $false_2$.

Brown (1977), like Lakatos, has found that it is possible for multiple paradigms to coexist during a period of normal science (see p. 153). In this instance, we can say that one theory is $true_2$ for one group, and another is $true_2$ for a competing group. Like Lakatos, Brown (1977) seems to feel that one coexisting theory must be more progressive than the other, i.e., a closer representation of objective reality. He demonstrates this belief when he states that "[a]t most one of the competing theories can, of course, be $true_1$. As we shall see below, philosophers such as Toulmin (1982), Morgan (1983) and Eisner (1991) believe that each member of a coexisting plurality of paradigms will represent some different aspect of $truth_1$, and when taken together, these theoretical views will achieve a more complete view of that reality than is possible under a single theory.

In conclusion, Brown (1977), like Kuhn, posits that we can never know objective reality directly (i.e., $truth_1$). There are no permanent scientific truths, and no cumulative body of knowledge. "The only permanent aspect of science is research" (Brown, 1977, p. 166). The structure of disciplines and the scientist's view of the world are reinvented again and again through a sequence of paradigms made up of new presuppositions, but this succession will never lead us to $truth_1$.

However, this does not mean that there is no continuity in science, for revolutions are not complete breaks with tradition: some concepts are eliminated, others are gained, but the majority of old concepts are retained in an altered form. There is thus some basis for rational debate in deliberations about theory choice. And although revolutions structure the reality of the scientist in new, radically different ways, s/he is not thrust into the position of being a total 'stranger in a strange land': a new paradigm is in some way continuous with its historical antecedents.

Multiple Viewpoints Yield Multiple Potentialities

I have demonstrated above (in the section on plurality) how Schwab (1978a), Toulmin (1982), Eisner (1991), Morgan (1983), and Shulman (1986b) are comfortable with the notion of radical incommensurability, and do not seem so concerned with whether or not there is a common basis of understanding between coexisting paradigms. Instead of attempting to reinstate 'rationality' (i.e., in a verificationist sense) in theory choice by refuting the incommensurability thesis, or by demonstrating that common grounds for comparison remain despite partial incommensurability, these theorists have been concerned with delineating what I believe is a different form of practical rationality than that provided by Kuhn and Brown: one based in eclecticism.

Schwab (1978a), Toulmin (1982), Eisner (1991), Morgan (1983), and Shulman (1986b) all view incommensurability and concurrent plurality in disciplines as *opportunities* to increase our stock of knowledge (and thereby *progress*) rather than as problems to be overcome. A plurality of research methods and forms of representation will, on their view, reveal multiple potentialities in a single phenomenon because each approach will bound the reality differently (although incompletely) and, when taken together, will reveal more about it than is uncovered by the application of a single view. As Eisner (1991) points out, the world can be known in multiple ways depending on the approach (i.e., paradigm) used by the inquiring subject, which will determine the nature of the transaction made with nature. In other words, "...knowledge is made, not simply discovered" (Eisner, 1991, p. 7). By increasing our range of paradigms, "...of ways we describe, interpret, and evaluate..." the world, we will make inquiry in a discipline "...more complete and informative" (Eisner, 1991, p. 8). This does not mean, however, that the choice of *any* view of a phenomenon can be considered rational, or that the products of such an approach can be deemed to be 'knowledge' (e.g., an astrological explanation of human behaviour). A theoretical representation must be accepted as 'knowledge' by a group of professional scientists operating within a paradigm in a discipline, it must comply with the trustworthiness criteria laid out by that paradigm, and it must in some way correspond with empirical reality (even though it can never represent it perfectly).[6]

In the rigorous application of the methodological and theoretical standards of a paradigm in inquiry, a scientist behaves rationally. He or she can also demonstrate rationality during periods of revolutionary science by locating areas of linguistic commonality between competing paradigms, attempting to shift lenses to understand the new perspective

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[6]I will elaborate on new trustworthiness criteria for the new philosophy of science below.

provided by the heterodox view, and ensuring that his/her choice is based, as far as possible, on the relative problem-solving ability of the competitors (see Kuhn, 1970; and Brown, 1977). But perhaps Kuhn and Brown should be less concerned with demonstrating that progress in science involves "...the perfection of consensus...", and be more willing to see it as "...a refinement of debate" (Geertz, 1973, p. 29). For those who take the latter view, who consider an eclectic set of paradigms to be a sign of the maturity and progressiveness of a discipline, there is a third sense of 'rationality.' Like attempts to 'perfect consensus,' this is another form of rationality which is *beyond method* (see Morgan, 1983), i.e., the reasoning that is demonstrated when a scientist wishes to research a particular phenomenon, and is faced with making a choice from among an eclectic set of accepted methods and forms of representation which will all bound that reality differently. To make a rational choice among methods that reveal different potentialities in the subject matter, this scientist will need to know how the various approaches frame inquiry and structure reality differently. What does each method reveal about the subject matter? What does it obscure (Schwab, 1978a)? What are the purposes embedded in each approach to inquiry (Toulmin, cited in Donmoyer, 1985)? Would it be better to utilize a single approach in inquiry, or to formulate some useful synthesis of two or more methodologies (Shulman, 1986b)?

Toulmin (1982; cited in Donmoyer, 1985) provides a useful discussion of this new form of rationality. As we saw above, Toulmin (1982) acknowledges that within most disciplines there exists "...alternative interpretive standpoints...", "...each with its own scope and justification" (102). These coexisting theoretical languages are a sign of the progressiveness of a field of inquiry.

> A variety of parallel interpretations does not reflect the personal preferences or characteristics...of the individual scientists who adopt them. It reflects, rather, the fact that alternative investigative postures and interpretive standpoints *have been found* productive in dealing with different aspects of the discipline's subject matter. (Toulmin, 1982, p. 102)

Different interpretive standpoints will yield different propositions that constitute opposing conclusions, but which still seem valid because they are all supported by empirical reality. So it would seem that the adequacy of one position over another cannot be determined by empirical evidence because all such data is seen in the light provided by a selected paradigm (Toulmin, cited in Donmoyer, 1985). But does this mean, then, that the choice of an interpretive standpoint to use in inquiry is a *subjective* one, i.e. one that is *irrational*? Toulmin (1982) thinks not: "On the contrary, the question, 'Which style of interpretation would it be appropriate to adopt in dealing with this particular kind of subject matter?' itself calls for an answer based on actual experience--and so an objective answer" (p. 100). So a means of rationally assessing the relative worth of conflicting knowledge claims exists; in short, *there is a rescue from relativism* (Donmoyer, 1985).

Toulmin (cited in Donmoyer, 1985) feels that the scientist, when making a rational choice between competing interpretive standpoints, should consider above all else his or her *purposes for engaging in inquiry*. Each discipline has its own purposes in inquiry. If disagreements arise over these purposes (e.g., quantitative researchers in education who are interested in explanation and prediction of human behaviour versus qualitative practitioners who prefer interpretations of meaning), then a discipline will split. When this occurs, scientists are faced with choices between different language communities. However, we cannot objectively assess the validity--the truth or falsity--of a language, be it an everyday language or a theoretical one: e.g., English is not *truer* than French. Instead, we can only speak of one language or another being more or less appropriate--more or less adequate--*for some specific purpose*. For example, a bilingual member of an Inuit community who writes poetry both in English and in Inuktitut would probably choose the latter to describe snow because of the existence in that language of a large vocabulary describing this phenomenon. This would be the more reasonable choice. In contrast, Inuktitut would probably not be the more appropriate choice for the purposes of describing computer technologies. The same holds true for theoretical languages: "Data cannot speak for themselves. For data to speak they must be translated into a language, and languages are inventions..." which use different--often untranslatable--terms to describe reality (Donmoyer, 1985, p. 17). Different theoretical languages can construct the meaning of a single phenomenon in diverse ways. A scientist faced with a choice between diverse theoretical languages to use in researching a phenomenon, like the poet choosing a dialect to describe snow, must ask him/herself: 'Which approach will lend itself the best to this context, and to these purposes?' This leads us to another important question, how can a researcher surface the underlying presuppositions of an interpretive standpoint or paradigm in order to make an informed or reasonable decision?

Schwab (1978c/1970, 1978d/1971), Morgan (1983), and Shulman (1986b) all discuss the use of dialectic as a means for making reasonable choices between competing paradigms. The goal is to instil in researchers the critical ability to shift paradigmatic lenses, i.e. to see their disciplinary subject matter from the different viewpoints provided by a plurality of theoretical languages. While Kuhn (1970) did not think it possible to shift back and forth between different viewpoints, Schwab, Morgan and Shulman consider it not only possible, but essential in order to maintain a rational basis for theory choice. Schwab (1978d/1971) has named this ability to shift paradigms a 'polyfocal perspective,' and considers it an essential skill for any researcher or practitioner who wishes to make his or her choice from among an eclectic set of paradigms a *reflective* one. The process of dialectic involves an analysis of: first, the discipline to uncover any commonplaces (e.g., the crucial problems and phenomena of the field); and second, each paradigm which deals with that problem-field in order to uncover its underlying presuppositions, which will bound that subject matter and address those problems in a particular, and incomplete, way. Once a researcher understands how a paradigm treats and truncates its subject matter, s/he is in a better position to determine rationally which approach will best suit her/his own purposes in inquiry. I will give a more complete description of this process of dialectic below.

Donmoyer (1985) points out that even though a form of practical rationality exists which addresses the problem of relativism in theory choice, there remain three ways that researchers can make mistakes. His 'first order mistake' is one which involves the first type of rationality I mentioned above, i.e., that used when the theoretical and methodological apparatus of a paradigm are rigorously applied in solving disciplinary puzzles. This mistake occurs when the theoretical propositions derived from a paradigm are not adequately supported by empirical evidence. For example, this problem can arise when a quantitative research design has an inadequate sample size. Donmoyer's (1985) 'second order mistake' is one which is hopefully eliminated by the application of the dialectical methods outlined in the previous paragraph. Such a mistake occurs when there is a mismatch between the theoretical language employed in inquiry and the particular purposes for conducting the research. For example, MacMillan and Garrison (1984) point out that the main purpose of the process-product paradigm is to measure the behaviour of teachers and determine how this impacts on student learning. They consider this plan misguided because it ignores the importance of the content of the lessons: "Methods of research that can combine the elements of content and behavior would solve problems left in the air by process-product research" (Macmillan and Garrison, 1984, p. 17). Finally, Donmoyer's (1985) 'third order mistake' relates to the adequacy of purposes themselves. For example, MacMillan and Garrison (1984) discuss how, in viewing teaching as "...the causation of student achievement by teacher behavior...", the process-product paradigm has pursued purposes that are unworthy of such a complex human activity (p. 17). In their opinion, a more appropriate purpose for inquiry would be to investigate the underlying intentions, moods, aspirations, goals, and beliefs of students and teachers, and how these impact on the educational undertaking. In short, then, postpositivistic philosophy extends the range of methodological mistakes that can be made by a researcher from those related to the suitable application of a single research method, to considerations that are beyond a single method involving the match between method and purposes, and the adequacy of purposes themselves. The world of the researcher just got a lot more complicated, but fortunately this complexity is accompanied by the potential for improved research designs and more adequate theoretical propositions.

In conclusion, we must consider how science progresses for those who accept the thesis of radical incommensurability, and view this not as unchecked relativism, but as an opportunity to demonstrate an alternative practical rationality, one where a scientist chooses *reflectively* from among an eclectic set of paradigms that each bound subject matter differently and partially. We saw above that Kuhn and Brown are interested in securing more certainty in knowledge construction by building a larger consensus within a community of scientists about which view is more true$_2$. According to Kuhn (1970) scientific revolutions end when this consensus is reached, and a new period of normal science follows until the next crisis surfaces. In his account, the development of a science from one accepted paradigm to another is mostly non-cumulative. It involves fundamental shifts in the axioms which define the conduct of research in a discipline, while maintaining only some of the problem-solutions and conceptual apparatus of previous orthodox views. In this way, then, scientific disciplines grow in precision and depth, and demonstrate progress in their problem solving ability, but do not get any closer to achieving objective truth.

In terms of growth in breadth, Kuhn (1970), because of his belief in serial plurality, does not consider this to occur within a single discipline, although it may follow through the creation of new scientific specialties. In contrast, Schwab (1978a), Toulmin (1982), Eisner (1991), Morgan (1983), and Shulman (1986b), feel that disciplines do grow in breadth and have reinstated the notion of cumulative progress in science, but not in the sense employed by the logical empiricists. Scientific progress, in the logical empiricist account, involves the cumulative growth of knowledge in a discipline through the application of the foundational method of mathematical physics. By comparison, Schwab et al. view progress in a discipline to involve the accumulation of an eclectic array of interpretive structures that each reveal different potentialities in the subject matter. In this way, each new paradigm reveals something new about the disciplinary problem-field and adds cumulatively to the body of accepted knowledge obtained about these multi-faceted phenomena. So by rejecting Kuhn's (1970) serial version of plurality and his concomitant conclusion that a discipline does not grow in breadth, Schwab (1978a), Toulmin (1982), Eisner (1991), Morgan (1983), and Shulman (1986b) have established a new sense in which a discipline can be said to progress cumulatively.

New Trustworthiness Criteria

The postpositivistic philosophy of science has destroyed the positivist dream of creating a certain foundation for knowledge based on either formal logic or passive perception. If we reject the positivists' absolutist epistemology with its doctrine of infallible truth, does this mean that there is no longer any basis for saying that we 'know' something to be true rather than just 'believing' it to be so (Brown, 1977)? Does this mean, then, that there are no grounds for saying that some (or *a great deal of*) 'objectivity' remains in the conduct of scientific research? Can we be rescued from subjectivity, relativism, and irrationalism? Are there alternative criteria that can *reasonably* assure us--even if they can never guarantee--that our theoretical representations of the world are trustworthy? Although most of these questions have already been answered in some way above, I consider it useful to draw all of this information together into a definitive statement on trustworthiness. I turn to Eisner (1991) and Toulmin (1982) to provide us with a framework for doing this.

Toulmin (1982) argues that with the advent of postpositivistic philosophy Dilthey's distinction between *Natur* and *Geist*, between natural science based on objective observation and human science based on hermeneutic interpretation, is dissolved. All sciences, natural and social, are recognized to use interpretive frameworks in inquiry. Research, then, necessarily involves transactions between inquiring subjects whose view is theory-laden and an objective reality that can be experienced in no other way than through the lense provided by an interpretive standpoint (Eisner, 1991). But, "[i]f we cannot have a purely objective view and refuse to trust a subjective one, how do we know when to trust a transactive account" of a phenomenon (Eisner, 1991, p. 53). If we are faced with an eclectic set of methods and forms of representation all dealing with the same subject matter, how will we know which are trustworthy and which are not? In the postpositivistic age, when many consider it no longer acceptable to apply the truth criteria of one paradigm (e.g. the scientific method of natural science) in a foundational way to all other structures, are there 'meta-criteria' that can be used to assess the trustworthiness of incompatible transactive accounts? Eisner (1991) provides three criteria for appraising the trustworthiness of qualitative accounts of educational phenomena: coherence, consensus, and intrumental utility. However, he also insinuates that these features can be used to assess *any* transactive account of a phenomenon. As Toulmin (1982) has pointed out, the class of representations that can be considered 'transactive' or 'interpretive' has recently been expanded to include the products of scientific method, and so I will use and adapt Eisner's (1991) three categories to outline trustworthiness criteria for *humanistic* science (i.e., a widening of the concept of 'science' that includes *any* knowledge that is organized or systematized or structured by a paradigm).

Coherence

According to Eisner (1991) the 'coherence' trustworthiness criterion relates to the 'tightness of argument' present in a qualitative narrative. In other words, does the narrative 'cohere,' does it 'make sense' or 'ring true'? Or instead does it contain 'lapses of logic,' 'inconsistencies' or elements that 'don't seem to fit'? The equivalent 'meta-criterion' for humanistic science relates to the adequacy of an account of reality in terms of the unique internal logic provided by the paradigm which framed that account. In other words, does the scientific result meet the standards of trustworthiness laid out by the paradigm? As Garrison (1986) points out, before turning to practical judgment (embedded in the trustworthiness criteria of 'consensus' and 'instrumental utility') to appraise a knowledge claim, we should first exhaust "...the principles of inductive and deductive logic..." (p. 17). Brown (1977) agrees with Garrison that the results of logic and experiment must be considered first in deciding the fate of theories, but that these factors alone are not enough: such decisions ultimately rest on the application of practical wisdom. Garrison and Brown were obviously talking about the trustworthiness criteria of the scientific method here, but their point applies to any internally consistent method of inquiry. In short, any research result must first demonstrate that it is an example of the *rigorous application* of a certain method, then it will be judged in terms of practical considerations that go *beyond method*, or beyond the internal logic of a single paradigm.

Consensus

Eisner (1991) views the 'consensus' trustworthiness criterion as "...the condition in which investigators or readers of a work concur that the findings and/or interpretations reported by the investigator are consistent with their own experience or with the evidence presented" (p. 56). This consensus or agreement (or lack thereof) is ultimately a function of *persuasion*: "What some find persuasive others may find unpersuasive" (Eisner, 1991, p. 56). The corresponding meta-criterion in humanistic science also involves consensus building within a community of scientists. Both Kuhn (1970) and Brown (1977) consider the final arbiter in a debate about theory choice to be the community of scientists who have the expertise and experience to help them choose wisely. This is necessary because the new philosophy of science has demonstrated that an appeal to either objective observation or formal logic to guarantee the certainty of knowledge was always too much to ask for. Scientists will be persuaded to adopt a new paradigm for a variety of reasons, including the following: it preserves some of the important gains of its historical antecedents; it solves the problem(s) that cast doubt on a previous structure; it predicts the presence of previously unsuspected phenomena; its ontology, epistemology or methodology comforms to some individual sense of the appropriate or the aesthetic; and etc. (Kuhn, 1970). As we saw above, both Kuhn and Brown view succeeding paradigms as having enough in common that they can be rationally compared to determine which viewpoint is the more progressive or fruitful one. In other words, they are interested in building consensus to a point where we can say one theoretical account is clearly the truth$_2$. Other theorists (e.g., Schwab, 1978a; Toulmin, 1982; Eisner, 1991; Morgan, 1983; and Shulman, 1986b) who accept that both radical incommensurability and concurrent plurality exist in most disciplines (particularly in social science), assume that not all of the members of a discipline will be convinced of the superiority of a single paradigm. As a result, there will exist in most disciplines *many factions each with their own consensus.*[7]

[7]See Garrison (1986) for a convincing argument that the scope of practical deliberations should be enlarged to include the consumers of the products of research: i.e., those professional practitioners whose knowledge base includes theoretical propositions.

Instrumental Utility

Eisner's (1991) final criterion for the trustworthiness of a qualitative interpretation of the social world is that it must in some way be *useful*. For example, an interpretation can be considered useful because it helps us "...understand a situation that would otherwise be enigmatic or confusing," or because it allows us to anticipate the future: i.e. it points us toward an "...experience [that] we otherwise might have missed..." without the benefit of the qualitative study (Eisner, 1991, pp. 58-59). This criterion parallels the postpositivists' resolution that an account of nature or society can be considered true (but only $true_2$, never $true_1$) if it is theoretically and empirically progressive. In other words, it must prove itself useful in dealing with the problems or phenomena of interest in a field of inquiry. For philosophers of science such as Kuhn (1970) and Brown (1977) who believe that one paradigm is superior to all others, this means that a structure will: (a) explain anomalous data better than its competitors, (b) preserve as much of the problem-solving ability of its predecessors as possible, and (c) if possible, allow the prediction of new, previously unimaginable, phenomena. For theorists who support the notion of a concurrent plurality of incommensurable paradigms (e.g., Schwab, 1978a; Toulmin, 1982; Eisner, 1991; Morgan, 1983; and Shulman, 1986b), an interpretive structure will prove useful if it reveals some potentiality in a phenomenon that was not brought to light by its competitors. Finally, the pragmatist philosopher James (1969/1907) (the most radical relativist of all) goes so far as to consider a religious or metaphysical speculation to be a valid knowledge form provided it proves its instrumental utility. For example, if a belief in the concept of the 'afterlife' provides comfort for grieving family members following the death of a relative, then it can be considered a 'true' proposition for their purposes. And if an educator finds Whitehead's (1959) metaphysical notion of the cyclical nature of education to be a useful heuristic in informing curriculum planning, then it too can be considered 'truthful' on some level. Indeed, these examples show how the category of those competent to judge the utility of a knowledge claim might be expanded in some situations: the former instance demonstrates how common folk might help decide the 'truth' of an assertion, and the latter case illustrates how professional practitioners can perform this function, as Garrison (1986) encouraged. However, most scientists would refuse to trust such subjective accounts of what constitutes truth on the basis that just about any version could be justified in some way.

Beyond the Internal Logic of Paradigms: Enquiry into Enquiries

Now that postpositivism has superseded logical empiricism, along with its foundational method and its absolutist epistemology, how can we deal rationally with the theoretical and methodological plurality that has resulted? What are some different ways of viewing this plurality? What intellectual tools do we have to enable us to make reasonable, reflective choices from amongst the eclectic set of paradigms that exist in most disciplines? This section will begin by exploring five approaches to dealing with diverse research methodologies. Next, I will demonstrate my commitment to one of these approaches by exploring the use of Platonic dialectic to help researchers develop what Schwab terms a 'polyfocal perspective' on their field of inquiry. This dialectical method is a means of enabling researchers to go 'beyond method' (see Morgan, 1983), or to conduct 'enquiry into enquiries' (see Schwab, 1978d/1971), in order to surface the implicit presuppositions of different paradigms, to determine the exact way in which each bounds its subject matter, and to thereby make choices from amongst these interpretive structures more intelligent or more rational. As Morgan (1983) points out, most practitioners of science are fastidiously rational in terms of the designs used in inquiry and the level of rigour demonstrated in their implementation. In spite of this, many are also woefully *irrational* in that they either consider their approach to research to be complete and therefore foundational, or they recognize the imperfections of their chosen paradigm, but fail nonetheless to surface and critique its axiomatic foundations. Hopefully in the future more and more scientific communities will recognize the benefits of having a polyfocal perspective on their discipline and will train their scientists in dialectical methods so that (a) they are able to see how different viewpoints are fruitful, (b) they will choose to be a part of more than one 'consensus' or community, and (c) they will be able to select the viewpoint(s) that best suit(s) their purposes.

Five Approaches to Dealing With Plurality

Morgan (1983) outlines five approaches to dealing with plurality that range from foundational approaches to complete 'anything goes' relativism. The first approach, labelled 'supremacy,' calls for some evaluation or test for determining which paradigm is undoubtedly superior to all others. As we saw above, the dominant view of modernity has been that a supreme and certain foundation of knowledge is provided by objective scientific observations and rational synthesis (i.e., Euclidean geometry or *Principia* logic). Postpositivism has refuted this foundational view of knowledge by positing that a plurality of research strategies exists each with their own internally consistent logic. It is therefore not possible to determine which paradigm is superior because there is no neutral language to use in evaluating competing claims.

Schwab (1978a) labels such a doctrinaire or foundational approach to pluralism 'self-guaranteeingness.' On his view, each paradigm or structure in an eclectic set, although supported by empirical data, is incomplete because it only asks a limited number of questions about the subject matter, thus cutting its complexity. It is easy to be self-deluded concerning the foundational nature of one's preferred theory or method, for "...as long as practitioners of the discipline ask no other kinds of questions, they are unlikely to have doubts about the singular propriety of their favored structure" (Schwab, 1978a, p. 257). For example, Shulman (1986) points out that supporters of the process-product research paradigm have assumed since 1965 that their approach in framing the questions of educational research, and their trustworthiness criteria, are foundational, giving them the right to critique all competitors using their self-justified presuppositions about the nature of 'rigorous' inquiry. So it would seem that arguing for the supremacy of a single method constitutes a form of self-justification or 'self-guaranteeingness' that is unwarranted because in the end all paradigms are based on *ā priori* assumptions that are nothing more than metaphysical beliefs (although some belief systems may prove to be more fruitful than others).

Morgan's (1983) second response to plurality is also foundational in that like 'supremacy' it is also "...concerned with finding an optimal way of conducting research" (p. 378). 'Synthesis' is the search for an integrated approach to research that limits the weaknesses of each individual paradigm, while maximizing their strengths.

> Attempts to find an all-embracing paradigm or metaphor for framing enquiry, to translate different strategies into a common language, or to find ways of overcoming traditional dichotomies, provide good examples of such integrative effort. (Morgan, 1983, p. 378)

An example of an attempt at synthesis is provided by Habermas's (1971/1968, 1979/1976, 1982/1970) critical theory, which begins by criticizing positivist and interpretivist self-understanding for each considering their partial knowledge interests to be complete. For example, 'empirical-analytic' science has only a technical knowledge interest, while historical-hermeneutic science has only a practical knowledge interest. Neither has the interest supplied by the other, nor do either of them possess an *emancipatory* knowledge interest; therefore, both are incomplete. Critical theory integrates positivist and interpretivist methods in the greater interest of revealing how communications in mass society are systematically distorted for ideological reasons. In so doing, these methods transcend their own narrow, incomplete interests and are employed in the service of a higher purpose: the emancipation or empowerment of the people. Critical theory is, then, a foundational view in that it seeks an optimal way of conducting research through the synthesis of different methods.

Morgan's (1983) third approach to plurality, based on pragmatist philosophy, is 'contingency', i.e., the belief that knowledge should be judged according to its practical usefulness. This is not a foundational view of plurality because those who hold this belief do not consider there to be an optimal approach to research; instead, they view the choice of method to be contingent on the particular problem to be solved. In other words, they consider it appropriate to vary your choice of paradigm depending on what your purposes are in inquiry. To be sure you are implementing the most appropriate contingency, it is most important to know the presuppositions of the approaches you are considering in order to understand the form of knowledge that each will yield. In this way you will be able to match the interests embedded in the paradigm with your own practical interests in conducting an inquiry. I will outline below a method (i.e., Schwab's 'arts of eclectic' or 'reflective eclectic' approach) based on Platonic dialectic which is designed to make manifest the implicit structures of different interpretive standpoints so that researchers can make intelligent choices between them. This is obviously the approach taken by those theorists (i.e., Schwab, Toulmin, Shulman, Eisner, & Morgan) who consider plurality an opportunity to discover multiple potentialities in subject matter. Although Kuhn and Brown also believe that the trustworthiness of knowledge is dependent on its instrumental utility, they are not interested in picking and choosing from among a set of paradigms to achieve a match with their own interests in inquiry. Instead, they are more interested in finding out which approach amongst a set of competing schools is the 'better' or more progressive one.

(However, Kuhn's and Brown's approach is *not* foundationalism, because each theory that is accepted as knowledge only attains this status on a provisional basis.)

Morgan's (1983) fourth approach to plurality is 'dialectic': *Hegelian* dialectic to be exact. Those who take this approach, like the supporters of 'contingency,' accept that plurality is an inevitable part of the researcher's life. However, rather than using Platonic dialectic to help them choose the best approach for certain purposes, those who support this approach employ Hegelian dialectic in "...attempts to use the differences among competing perspectives as a means of constructing new modes of understanding" (Morgan, 1983, p. 379). This is done by counterposing the insights gained by one presuppositional matrix (thesis) with those obtained by a different viewpoint (antithesis) "...in the hope that a completely new mode of understanding will emerge from the debate generated by this opposition" (synthesis) (Morgan, 1983, p. 379). In short, this approach attempts to use plurality constructively by subjecting different approaches to conflict and debate so that new forms of understanding can emerge which will hopefully go beyond the original formulations.

Morgan's (1983) final approach to plurality is 'anything goes'; in other words, *complete relativism*. On this view, every research strategy has something to offer and, taken to its extreme, even ancient or absurd ideas might in some way improve our stock of knowledge. This approach parallels James's (1969/1907) radical relativism, which even includes metaphysical or religious speculation in the category of those propositions which might prove useful and thus 'truthful.' A proponent of this approach to plurality, such as James, would consider the *individual*, be s/he a researcher, a practitioner, or a layperson, to be the final judge of what propositions or ideologies are useful in guiding practical action. In short, what counts as truth is completely relative to the subjective appraisal and interests of any individual who wishes to be guided in action.

Morgan (1983) correctly points out that as with the first two foundational approaches, 'supremacy' and 'synthesis,' (which attempt to justify their foundational nature on the basis of their own 'obviously superior' premises) there is no objective viewpoint from which to appraise the adequacy of all of the five responses to plurality. The first response, supremacy, has been the dominant approach of the modern age. More recently, the modern scaffolding of *ā priori* assumptions has been supplanted by a new philosophy of science which has shown that the positivist principles of certainty, systematicity, and the clean slate are nothing more than metaphysical doctrines themselves; they must be *believed in*, they can never be *known for certain*. The postpositivists have embraced new beliefs in theory-laden perception, plurality, and a form of rationality that is practical rather than formal. There is no objective standpoint from which to judge the adequacy of these doctrines either.

And so I will embrace Morgan's (1983) third approach, contingency, for reasons that are both practically rational and personal. First, if reality presents itself to us in diverse ways, depending on the conceptual and methodological apparatus applied to it, then it seems *reasonable* to me that we should look at phenomena from different viewpoints to obtain as many potentialities from them as possible. In this way, the knowledge base in a discipline will progress cumulatively by growing in breadth as new paradigms are added. Furthermore, if all paradigms bound and truncate reality differently, then it seems reasonable that we should understand how each approach does so, so that we might intelligently match the implicit interests of a viewpoint to our own interests in inquiry. Aside from these appraisals of the practical utility of a contingency approach, there are personal aesthetic considerations that enter into my choice. As part of my upbringing I was always taught that the best path in all things was the *moderate* approach. On this basis it seems *more balanced* to accept an approach based on dialectical means--i.e., reasoned dialogue--rather than the dogmatic extremes embodied in either a foundational or an 'anything goes' approach. The former constitutes a monologue, and the latter is akin to the unintelligible cacophony of a mass crowd. I now turn to a description of Schwab's 'arts of eclectic' as a means of making reflective choices in inquiry.

Arts of Eclectic

Schwab's 'arts of eclectic' were originally conceived of as a means of preparing theories for use in curriculum writing. Schwab views curriculum as a practical field requiring a rationality which is content- and context-based rather than formal, and which can aid in deliberation and persuasion in complex practical situations where rule-governed decision making fails. As we saw above, researchers must also employ practical rationality when choosing from among an eclectic set of paradigms to be used in inquiry. There is no longer a foundational method which *must be used* if scientists are to be considered 'rational'; instead, they must engage in practical deliberations to help them reach a *reasonable* decision. According to Kuhn and Brown, the most reasonable choice will be the paradigm which has proven itself to be the most theoretically and empirically fruitful alternative *for the time being*. In contrast, Schwab, Toulmin, Shulman, Eisner, and Morgan would consider the most reasonable choice to be the paradigm which best suits the researcher's interests or purposes in inquiry. Schwab's arts of eclectic employ dialectical means which are appropriate to either group's ends. For Kuhn and Brown, arts of eclectic are a tool which can be used to locate areas of commonality between old and new theoretical languages (which are largely incommensurable), thereby facilitating translation, and ultimately turning the new doctrine into a 'view' so that scientists can appraise the relative progressiveness of competing schools during periods of revolutionary science. For those theorists who consider a state of concurrent plurality to be progressive, arts of eclectic will allow scientists to "...bring into clear view the particular truncation of subject characteristic of a given theory and bring to light the partiality of its view" (Schwab, 1978c/1970, p. 297). When this is done to all paradigms which contribute knowledge to a discipline, the researcher will be able to *intelligently* choose that approach which suits his/her purposes best. Or, as Shulman (1986b) suggests, perhaps the most reasonable choice for some purposes is the combined use of different paradigms. Schwab's (1978c/1970) 'eclectic operations' permit such "...conjoint utilization of two or more theories on practical problems" (p. 297).

Achieving Polyfocal Perspective

The aim of Schwab's 'arts of eclectic' is to instil a polyfocal perspective in those trained to use these dialectical methods. Knowledge of the eclectic arts will allow a scientist to choose from among the plurality of paradigms in his/her field in a sophisticated manner. To achieve an understanding of plurality, Schwab advocates a program of "...sophisticated enquiry into enquiries" (Schwab 1978d/1971, 340). This is basically an education in postpositivistic philosophy dealing with scientific enquiry. The process involves three basic steps: (a) dialectical analysis, (b) reflection on the partiality of view afforded by theories or paradigms through the use of commonplaces, and (c) the use of cases studies to turn doctrines into views.

Dialectical Analysis

I must begin by stating that Schwab envisioned his eclectic arts being applied to *theories*, and not *paradigms*. As we saw above, a paradigm is a much wider category than a theory, as it is a presuppositional matrix that--in addition to theory--includes metaphysical commitments concerning methodology, epistemology, and ontology. As a result, a dialectical analysis must go beyond theory to include these other important commitments.

Dialectical analysis involves the identification of the terms and distinctions which make up the skeleton or structure of a theory (paradigm) and the relations between them (Schwab 1978c/1970). This is essentially an inquiry into the presuppositions of the theory (paradigm) and how they bound, cut, or frame the subject matter. Schwab (1978d/1971) recommends the schematization of "... the order and relation of the terms and distinctions, to one another and to the questions they generate ..." (p. 341). One method of schematizing a paradigm is to engage in 'map-making' or to create a 'web diagram.' Such a process should be repeated for every paradigm in a discipline so that the maps of different strategies can be compared later. This approach is reminiscent of Brown's (1977) web metaphor in which the knots in the web represent concepts and the strands in the web are the propositions that make up a theory. It is the pattern of concepts or knots in the web, delimited by the propositions or strands connecting them, that determines the character of a theory. Novak and Gowan (1984) describe how 'concept mapping' can be useful in externalizing such a 'web' or schematization:

> A concept map is a schematic device for representing a set of concept meanings embedded in a framework of propositions . . . Propositions are two or more concept labels linked by words in a semantic unit. (p. 15)

It must be pointed out that Brown (1977) and Novak & Gowan (1984) only discuss the use of concept maps or webs to delimit the skeletons of *theories*. However, for our purposes, such webs or maps must be expanded to include the other metaphysical commitments that are implicit in a paradigm.

Morgan (1983) recognizes that map-making need not be limited to schematizations of theoretical concepts and propositions alone: such maps can also outline premises concerning methodology, ontology, and epistemology. As well, Morgan (1983) demonstrates that we need not use maps to make the assumptions of different paradigms evident. His volume, ***Beyond Method***, is an edited anthology of chapters written by researchers with commitments to diverse research methods; each chapter can be considered a schematization of a paradigm. Morgan (1983) instructed each contributing author to present their favoured method in such a way that their implicit presuppositions would be visible "...and thus open to examination and critique..." in a reflective conversation about research methods in social science (p. 378). Regardless of the approach taken in completing dialectical analyses, the ultimate aim is to surface the underlying presuppositions of each paradigm in an eclectic set so that an overview of the entire discipline is achieved, and so that each approach can later be analyzed and compared in terms of how they deal with the commonplaces of the field.

Reflection on The Partiality of View Afforded by Paradigms Through The Use of Commonplaces

Once the presuppositions of each paradigm have been exteriorized to determine the distinctiveness of each 'view,' it is then time to compare and contrast the various structures. The process of comparison involves the examination of the skeletal structure of the competing paradigms to discover *commonplaces* which can be used in comparing them. Schwab (1978d/1971) defines commonplaces as a set of factors derived from "...systematic comparison of the principles, premises, methods, and selections used by and in each..." paradigm in a field of enquiry (p. 339).

> These commonplaces represent, in effect, the *whole* subject matter of the whole plurality of enquiries in which each member-theory reveals only one facade at best....An adequate set of commonplaces, then, provides a map on which each member of a plurality can be located relative to its fellow members. (Schwab, 1978d/1971, p. 339)

Such a map "...makes it possible to discriminate and relate the biases of each theory: what its terms illuminate, what light cast by others' terms it fails to shed, what aspects of the subject it brings to the foreground, and what it thrusts into the background (Schwab 1978c/1970, p. 298).

At this point, another tool emerges to aid us in schematizing different paradigmatic biases: the matrix. In such a chart, the commonplaces representing the whole subject matter are placed in the column headings and the names of the different theoretical positions are placed in the row headings. Then, the cells are filled in, where appropriate. Now it is a relatively simple task to determine how different theories deal with each of the diverse commonplaces of the whole subject matter, if they deal with them at all. It is simply a matter of comparing the different cells of the matrix. Shulman (1986b) takes a different approach in schematizing the commonplaces of a discipline. In his dialectical analysis of different paradigms in the study of teaching, Shulman (1986b) began by examining a variety of paradigms to discover the commonplaces of this field of inquiry. He then used these commonplaces to construct a 'synoptic map' of the field, i.e., "...a map of the full domain of research on teaching..." (Shulman, 1986b, p. 7). This map diagrams as many of the field's commonplaces as possible in the following areas: actors involved in teaching and learning, their attributes, contexts in which teaching activities occur, content and its organization, the agenda or ends of education, and research perspectives (methodologies). The aim is to create "...a map sufficiently broad and encompassing that we can locate upon it not only the particular sections of terrain well captured by particular programs but also those left out" (Shulman, 1986b, p. 7).

Turning a Doctrine into a View

The reader will recall that Kuhn (1970) considers a paradigm to be much more than just a set of presuppositions that guide inquiry: in fact, they constitute the world of the scientist to such a degree that they will determine what s/he sees when observing phenomena. Schwab concurs on this point; theories are much more than doctrines, they are *viewpoints*. In order to achieve a polyfocal perspective on a field of inquiry, one must learn to turn doctrines into views so that the world can be experienced from inside different standpoints. Unlike Kuhn, Schwab considers it possible to train someone to shift back and forth between viewpoints. Indeed, the professional education of scientists in the postpositivist era should include training in arts of eclectic to achieve the ability to shift paradigmatic lenses at will so that a reflective, eclectic set of paradigms can be applied in solving disciplinary puzzles.

Training someone to turn doctrines into views can be done in one of two ways. This first way is treated by Knitter (1985) and involves a dialectical analysis of doctrines using a text-based problem or case. For example, he cites two resources that can be used to bring the principles of diverse paradigms to a single case and thereby turn different doctrines into views: (a) "...Smith and Keith's Anatomy of Educational Innovation and Lighthall's essay review of that work", and (b) "... Graham Allison's Essence of Decision: Explaining the Cuban Missile Crisis" (cited in Knitter 1985, p. 393). Schwab (1978d/1971), on the other hand, suggests that this process of 'turning a doctrine into a view' or a 'revealing lens' can be accomplished using visual observations in the form of video-taped cases. The process would involve a first cycle and a number of cumulative cycles which, together, would add up to the total number of theories being treated.

In the first cycle, a theory would first be mastered through the process of dialectical analysis described above. Then, a videotaped case would be presented and students would be expected to bring the doctrine they have just learned to bear as a revealing lens on the concrete case. In the cumulative cycles, competing theories (paradigms) are, again, mastered through dialectical analysis, but also undergo a period of scrutiny in order to clarify the differences between successive standpoints. This can be readily achieved through careful questioning regarding the treatment of the commonplaces of the subject matter by the various theories (paradigms). Each successive standpoint will then be transformed into a "view" by the presentation of a different case. However, to ensure that students can apply more than one "view" to a case, the presentation and interpretation of the second case (or third, etc.) is to be immediately followed up by the unexpected presentation of the previous case study in order to have students attempt to escape the tyranny of the preceding doctrine (Schwab 1978d/1971).

In conclusion, I must reiterate how a reflective eclectic approach, achieved through the application of dialectical means, is more rational than the choice of one or another dogmatic extreme. The first extreme, the uninformed or foundational choice of a preferred paradigm, impoverishes the scientist's perception of his/her phenomena of interest for two reasons. First, all theoretical treatments of empirical facts provide incomplete views of those realities. And second, in eschewing the use of competing paradigms, the proponent of foundationalism has limited his/her access to the multiple potentialities present in each phenomenon, and has thereby limited the scope of knowledge that could be discovered about that event. The second extreme, 'conspectus' or 'unreflective eclectic,' leaves researchers "...quite unprepared to recognize the character of theoretic pluralism, much less cope with it" (Schwab, 1978d/1971, p. 335). In other words, without a dialectical treatment of plurality, the scientist will choose his or her paradigm blindly: s/he will have no way of knowing if the interests embedded in a particular approach match in any way with her/his purposes for conducting research. So in using either a conspectus or foundational approach, a researcher may very well avoid Donmoyer's (1985) 'first order mistake' and demonstrate rationality in applying a paradigm in a rigorous fashion, but s/he will very likely demonstrate irrationality by committing his 'second order mistake,' i.e., using a theoretical language that is inappropriate for particular purposes.

Conclusion: Return to a Humanist Conception of Rationality and Science

The dominant ideology of modernity has been the quest for certainty in knowledge acquisition. As Brown (1977) points out, this quest can be divided into two subproblems: (a) the search for a positive starting point for knowledge (i.e., passive perception), and (b) the search for a completely reliable means of formal reasoning (i.e., a set of rules or an algorithm which will tell us when a hypothesis is confirmed or falsified). As human judgment is fallible, the main goal of scientific rationalism in the modern era has been to make formal inquiry 'scientist-proof'; in other words, "...to remove the scientist from the decision making process and replace him with a set of algorithms" when trying to determine the truth value of theoretical propositions (Brown, 1977, p. 146). In Chapter Three I outlined three 20th-century versions of this quest to reduce rationality to a formal set of rules that would eliminate the need for the practicing scientist to exercise judgment in inquiry (i.e., the strict verificationism of the logical positivists, the moderated verificationism of the logical empiricists, and finally Popper's falsificationism). Reid (1978), in discussing the competing traditions of 'rationalism' and 'humanism,' points out that in the modern era science has been claimed by rationalists such as the logical empiricists, and that this claim has been widely considered to be legitimate; however, in his opinion, this does not necessarily mean that science belongs to this camp. This chapter has been devoted, on the one hand, to refuting the scientific rationalist quest for certain knowledge, and on the other, to outlining the *humanist* conception of scientific practice provided by the new philosophy of science, in which human history, communities, languages and judgments are crucial elements in the development of inquiries. In short, according to the humanistic version of scientific practice and progress that emerged in the 1960s, *the human imprint is found everywhere in the conduct of research.*

The first human imprint is found at the starting point of knowledge: a scientist does not enter into an observation of an event as a blank slate which receives an exact impression of that which has been perceived. Instead, all of the scientist's knowledge and past experience plays a role in determining what is observed, and there is a necessary transaction between subject and object. It is impossible to clean the slate prior to observing a phenomenon; indeed, prior knowledge is needed for a scientist to be able to distinguish between sense data that are relevant to his/her inquiry and those that are not. Beyond this, scientists work in specialized language communities that share commitments to a certain view of their field of inquiry. These shared commitments define such things as the problem-field of interest, the theory to be applied to that problem-field, acceptable problem solutions, and acceptable methodology for the community. Sometimes a crisis occurs in a scientific community when a discrepancy arises between accepted theory and observed reality. When this occurs there are no rules for the scientist to follow in deciding how to respond. As Garrison (1986) points out, the verification or falsification of a theory is underdetermined by logic: there is no rule to tell the scientist when to consider a theory falsified if one or a few of its propositions is disconfirmed by empirical observation. Should the scientist try to find a solution to the problem using the accepted language of the community, or is it time to create a new language to deal with the novelty of fact? Decisions such as these concerning exceptional cases encountered in practice require reasoned deliberations and human judgment: they cannot be informed by appeals to formal logic or experimental evidence.

The human imprint is also found in the context of discovery, i.e., when a new theoretical language is formulated to account for a recalcitrant anomaly. The scientist takes all he knows about his discipline, its phenomena, and the orthodox view of these events, and moves away from these known premises in small, creative steps. Eventually a new theoretical language emerges which is based on different axioms and which provides a new view of the discipline's problem-field. In short, knowledge is a human construction, not something that is discovered passively by a neutral observer.

Another area where practical deliberation and wisdom enters into the conduct of science is derived from the postpositivist assertion that a plurality of mostly incompatible interpretive standpoints can be constructed to explain a single phenomenon, with each being supported in some way by empirical evidence. Under the old logical empiricist philosophy of science, a scientist behaved rationally if s/he arrived at some conclusion through the rigorous application of the scientific method used in physics. Under the new philosophy of science, different methods, each with their own internally consistent logic, can be applied to a problem by different scientific communities, yielding results that are equally valid, and at the same time *discordant*. Despite this, all of these communities can be said to have behaved rationally provided they have followed the internal logic (e.g., they met the trustworthiness criteria) of their chosen method. This is the application of a set of algorithms designed to limit human error, similar to what was accomplished under the positivist hegemony of scientific method, except that now competing heterodox methods have emerged.

The advent of a plurality of paradigms in most disciplines has led to a situation where, if the narrow definition of rationality as rule-based decision making is followed, scientists could find themselves behaving in decidedly irrational ways. Under postpositivism, the definition of 'rationality' must be expanded beyond formal logic to include practical deliberation and wisdom. There is no algorithm to follow when making a decision about which theoretical language to apply to a disciplinary problem. The scientist, as a 'wo/man of practical wisdom' must exercise her/his professional judgment in choosing a paradigm. Although some subjective considerations (e.g., the 'elegance' of an explanation) might enter into such a decision, empirical reality still plays a decisive role--even though there is no longer a *single* reality that is objectively extant. Instead, there are multiple constructed realities that must be analyzed and compared so that a scientist can make an informed decision based on practical considerations such as the relative fruitfulness of the competing viewpoints, or the match between his/her purposes in inquiry and the interests embedded in an approach. When faced with plurality it is not enough to stringently follow the rules of a foundational approach, one must go beyond method and weigh the practical consequences of applying one paradigm or another to a disciplinary problem. In short, if a scientist ignores one form of rationality or the other--i.e. formal reason or practical wisdom--then s/he has behaved irrationally.

Brown (1977) states that "...it is the scientists, not the rules they wield, that provide the locus of scientific rationality" (p. 149). I disagree with this statement. It is *both* the scientists as 'wo/men of practical wisdom' *and* the methodological rules they ply that form this locus. We must remember that *all* of Donmoyer's (1985) mistakes are important: our purposes in inquiry must be adequate, the language chosen must match these purposes, and the language must be applied in rigorous fashion. Under logical empiricism, only the last of these mistakes can be made because the scientific method of mathematical physics is considered to be the superior approach in all cases. This philosophy has served to downgrade the status of practicing scientists to that of a technician involved in problem solving which is made rigorous by the application of a set of methodological rules. In other words, on this view, the only human acts which are rational are those which are guided by algorithms; ironically, such computations "...could in principle be carried out without the presence of a human being" (Brown, 1977, p. 148). In contrast, the new philosophy of science views the scientist as a critically intelligent person who must not only articulate an accepted theory, but also deal reflectively and wisely with the uncertainty that ensues when competing schools emerge in a discipline. Furthermore, this view of scientific progress casts scientists as *artists* who must create new languages when old ones have failed to describe the experiential world in an adequate fashion. Perhaps, then, it is this view of the scientist and his/her competencies that has caused working scientists to wholeheartedly embrace Kuhnian philosophy of science, as pointed out by Gholson and Barker (1985), who seem incredulous that such an *irrational* epistemology of science could have gained such wide acceptance.

CHAPTER SIX

A PLURALITY OF WAYS OF KNOWING IN EDUCATION

> The sources of educational science are any portions of ascertained knowledge that enter into the heart, head and hands of educators, and which, by entering in, render the performance of the educational function more enlightened, more humane, more truly educational than it was before.
>
> John Dewey, The Sources of a Science of Education

Summary: Positivism Gives Way to Plurality

The dominant theme of this thesis has been the modern, rationalist quest for certainty: an unattainable dream that has been so seductive and compelling in the history of western thought that it has shaped our views on reason and scientific inquiry for over four centuries. In Chapter One, I outlined a recent manifestation of this quest for certain knowing, namely, the hegemony of positivist methods of inquiry in educational research, and the dominance of technicist modes of teacher education and practice. In other words, educational research and practice have been captured for most of this century by positivist assumptions about: (a) which research methods and trustworthiness criteria are legitimate, (b) what modes of knowledge representation are appropriate, and (c) what the most reliable source of erudition is for the professional knowledge base for teaching.

The scientific method was first applied in the field of education at the end of the 19th century as an attempt to duplicate the technical successes of other practical fields such as engineering and medicine. True to the program of modernity, educational positivists began by 'cleaning the slate,' i.e., they discarded the traditional knowledge base concerning the nature of children and pedagogy which was derived from metaphysical speculation and the accumulated practical wisdom of teachers. Such knowledge, although based on common sense or rational representations of lived experiences, was considered untrustworthy because it was not gathered using the objective scientific method. This century, positivist research paradigms in education, such as the process-product program, have applied scientific (i.e., quantitative) methods to the discipline's problem-field and have managed to assemble a modest body of formal knowledge pertaining to pedagogy. The forms of knowledge derived from the application of scientific methods are nomothetic or statistical laws. These are formal propositions which are gathered by ignoring what is unique about each setting and concentrating on what can be generalized across contexts. It is subsequently assumed that these generalizations can be applied as rules of practice in most classrooms. Technicist teacher training, then, is concerned with developing the practitioner's ability to recognize classroom events as instances where certain rules apply, and then giving them practice in the proper application of these algorithms.

Since the 1970s the foundationalism of positivist research methods (and technicist modes of teaching practice) has come under fire from proponents of heterodox (i.e., qualitative, critical, deliberative) modes of inquiry and teaching practice. Although most supporters of these competing methods will admit that there are indeed some functions of teaching which are adequately informed by generally applicable rules, they point out that education is a practical field where the most common and crucial problems are *value-laden, complex, uncertain, and unstable*--the very things that are cut by the application of the scientific method. In other words, there is a fundamental mismatch between the form of knowledge derived from quantitative inquiries and the nature of teaching practice. As a result, the hegemony of positivism in research (and technicism in teacher education) has failed to provide practitioners with an adequate body of knowledge for rational action, and has instead promoted the widespread miseducation and deskilling of teachers. According to these critics, what is needed is a variety of research methods that will represent not only the regularities present in educational contexts, but also those particularities which define the educational enterprise, and the practical wisdom demonstrated by expert teachers in dealing with these grey areas of practice.

Despite the emergence of competing research paradigms which together provide more complete representations of educational phenomena, the field still seems to be dominated by positivist assumptions. For example, many educationists still insist on applying the trustworthiness criteria of the quantitative paradigm to all competing methods, which are inevitably found wanting when criticized on these terms rather than on their own. Others talk about 'rigorous educational inquiry,' which usually means the application of scientific methods which are considered 'objective' because they limit 'fallible human judgment.' Still others reveal their foundationalism through assertions to the effect that qualitative inquiries are useful because they provide 'scientific' researchers (as opposed to 'unscientific' interpretivists) with new variables for quantitative studies.

In teacher education, the dominant epistemology of practice continues to be technicism. Teachers continue to be 'trained' to apply 'rules' or 'techniques' in practice. One panacea for what ails education follows another. If only teachers could be trained to implement cooperative learning structures in their classrooms, prosocial behaviours would ensue, and this would improve the learning of content. Or perhaps training teachers in the use of direct teaching approaches, e.g., the socratic method, will best improve educational outcomes as measured by standardized tests. But wait, perhaps students are failing to achieve to expected levels because they are bored; what we need is a motivational approach, e.g., activity-centred learning. While all of these techniques are useful--indeed, *truthful*--in that they emphasize some important aspect of pedagogy, all are *incomplete*. However, the technicist epistemology of practice, like the positivist epistemology of research, assumes that there is *one truth*, one best answer for achieving agreed upon ends for education. As a result, we in the business of educating children are faced with a non-stop barrage of new approaches, each a 'silver bullet' which will solve all our problems provided we can be *trained* to implement them properly. The problem is that we work in ever-changing, unique, value-laden contexts where it is not always obvious what the 'reasonable' course of action is, and where the ends of education are rarely agreed upon. Those who work in such settings require not only knowledge of how to apply technical rules or methods, but also *practical wisdom* and *creative action* for their practice to be truly 'rational.' If this is the case, then why does the field of education cling to a positivist view of research and practice which eschews practical wisdom, and thereby narrows our view of human rationality?

According to Toulmin (1990), the reason for this foundationalism and this narrow view of human reason lies beyond the field of education; it is a symptom of the dominant intellectual scaffolding of modernity, i.e., that provided by the scientific-rationalistic quest for certainty. As we saw in Chapter Two, Toulmin argues that the beginning of modernity predates the 17th-century rationalist revolutions in philosophy and science, and can be traced to Renaissance humanist thinkers such as Montaigne and Rabelais, who broke with medieval modes of thought and embraced Aristotle's dual epistemology (i.e., the view that knowledge involves both theoretical *and* practical arts). For Toulmin, the logical empiricist starting point for modernity--i.e., the 17th-century revolutions in philosophy (inaugurated by Descartes) and science (initiated by Newton)--actually constitutes the second phase of the modern era, not the first. And rather than viewing these revolutions as the *birth* of a rationality which would guarantee the certainty of knowledge, Toulmin views them as the narrowing of reason and the systematic denial of the true, indeterminate nature of reality.

The intellectual scaffolding born of the 17th century is constituted of three interconnected presuppositions designed to secure positive theoretical representations of empirical reality. The first presupposition is that all inquiry should begin by 'cleaning the slate,' i.e., by jettisoning all fallible knowledge claims derived from metaphysical speculation or common sense experience. Second, certainty in knowledge was to be guaranteed by starting from two neutral scratch points which eliminated fallible human judgment in inquiry, i.e., pure logical reasoning and neutral scientific observations. Finally, a unified, systematic body of certain scientific knowledge was to be assembled by the application of the foundational method of mathematical physics. These connected *ā priori* assumptions have penetrated the consciousness of modern western civilization to such a degree that most educated people, and most people in education, will not consider something to be true until it is verified by scientific methods of testing.

While Toulmin agrees with Kuhn's version of the development of science (i.e., the existence of language communities that construct knowledge during periods of normal science, interspersed with revolutions which fundamentally alter the idiom of a field), he feels that it fails to explain the fervent manner in which the modern worldview was adopted as the dominant ideology of modernity. He points out that the most extreme versions of the program of modernity appeared during periods of profound cosmopolitical crisis in Europe, and were born of intellectuals living in catastrophic times who were attempting to reason their way out of uncertainty. The first such period occurred in the 17th century, involved simultaneous crises in politics (i.e., the Thirty Years' War) and cosmology (i.e., the emergence of Newtonian physics), and led to the rational-scientific revolution. The second period of cosmopolitical crisis happened in the 20th century, involved two consecutive world wars and the appearance of Einstein's physics, and led to the strict logical positivist philosophy of science. On Toulmin's view, it was more these upheavals and the rational solutions to *uncertainty* that emerged from them, than the explanatory progressiveness of rational-scientific methods, which led to the almost *religious* ardour with which positivist assumptions were accepted by philosophers and scientists. As the socio-political climate of the west improved following the two periods of crisis, the narrow versions of rationality and science born of these periods (i.e., 17th-century rationalism and 20th-century logical positivism which both advocated the strict application of scientific verification and formal logic), gradually gave way to more moderate versions of philosophy and science. On the one hand, philosophy recovered its humanist content by reinstating practical reason, and on the other, science recognized the sometimes ambiguous nature of reality and allowed the emergence of diverse research practices which better suited that nature, but which deviated from the foundational method of mathematical physics.

In Chapters Three and Four I outlined how the modern presuppositional scaffolding was first moderated by, and then gradually crumbled under, the weight of logical and empirical obstacles to certain knowing. Chapter Three outlined how two 20th-century traditions *which accepted the program of modernity* were forced by logical inconsistency and experience to temper the strict formal reason of 17th-century rationality and that of its 20th-century version, logical positivism. The logical empiricist philosophy of science was forced to downgrade the certainty and systematicity of knowledge contained in the logical positivist account of science in a number of ways. First, the logical positivists distinguished scientific from unscientific propositions by stating that the former class of statements were verified by direct, passive observation. The problem with this was that many phenomena of interest to scientists could not be observed directly. To solve this problem, the logical empiricists allowed a much less stringent standard of verification, i.e., the confirmation of scientific propositions through *experimental testing*. Second, the logical positivists believed that empirical observations provided *positive* verification of hypotheses. Unfortunately, it was soon recognized that a finite number of scientific observations or tests could never, by induction, categorically verify the truth of a scientific proposition. In response to this problem, the logical empiricists allowed claims to be considered trustworthy and 'scientific' if they had *a high degree of inductive support* from direct observations, or indirect scientific tests.

A third difficulty with the logical positivist program was caused by their belief that *all* classes of phenomena could and should be subjected to scientific modes of inquiry. Unfortunately, experience soon proved that not all events (especially in social science) could be explained using deterministic, cause-effect laws *which applied in all cases*. To deal with this problem, the logical empiricists expanded the class of propositions considered 'scientific' to include less certain statistical-probabilistic explanations of events. In this way, then, the more complex and dynamic phenomena of the social sciences could be studied using the method of the natural sciences, and could yield a 'scientific' body of knowledge. The final problem with the logical positivist version of the development of science involved the systematicity of the body of formal knowledge. The logical empiricists soon recognized that a unified body of scientific knowledge was a unrealizable dream. They demonstrated that the subsumption of narrower theories under wider theories was an inexact process, and therefore had to accept that the assembly of a 'pure theoretical structure' built on a foundation of mathematical physics could not be achieved.

The second 20th-century tradition forced by logic and experience to moderate the dogmatic claims of the modern program was Karl Popper's falsificationism. Brown (1977) considers this a transitional view of science which leads away from the strict rational quest for certainty, and points us toward the new version of scientific rationality and progress that would emerge in the 1960s and 1970s. In contrast with logical positivists and logical empiricists who considered *verification*, or *a high degree of verification*, to be the defining characteristic of *scientific* propositions, Popper considers a statement to be scientific if it is capable of being *falsified*. In other words, in his opinion there is no such thing as a final scientific truth, verified for all time by inductive support. There are only scientific laws which have passed experimental tests *for the time being*, and which will most assuredly be falsified at some point in the future. Therefore, scientific knowledge is, on this account, *highly uncertain*. In addition, one interpretation of Popper's thesis views the decision of when to consider an hypothesis falsified to involve something more than the following of rules, i.e., it might occasionally involve the professional judgment of a community of scientists. In sum, while both logical empiricists and falsificationists have retained modern ideas such as the use of formal logic to analyze formal knowledge, the passivity of perception, and abstract, general forms of representation, it is also quite clear that they have significantly reduced the certainty of scientific truths.

In Chapter Four I outlined a series of epistemological and ontological uncertainties in both natural and social science which suggest that the conduct of science can not be informed solely by formal rules of inquiry, but also require the application of practical reasoning and judgment. In addition, I demonstrated how these uncertainties have led to a proliferation of competing theories, methods of inquiry and forms of knowledge representation in most disciplines--even those concerned with explaining physical events. Finally, I discussed how, as a consequence of the propagation of incompatible modes of knowledge representation in response to uncertainty, the rationalist dream of a unified body of certain scientific knowledge has been destroyed.

In natural science the positivist assumption that a stable, eternal reality can be explained using deterministic, cause-effect laws has been undercut by a number of findings (i.e., relativity, the dual nature of light energy, chaos theory and the uncertainty principle) that demonstrate that some indeterminacy exists in natural phenomena, particularly at the subatomic level. This indeterminacy changes the traditional view of scientific inquiry in a number of ways. First, empirical reality reveals itself to human observers in different ways, depending on the viewpoint taken in inquiry. For example, Einstein's theory of relativity proved that what you observe is relative to your motion and position. Also, light energy reveals itself to observers in two different ways, i.e., as a wave or a collection of particles, depending on the scientific experiment applied to the phenomenon. Second, the human act of observation sometimes interferes with what is observed. For example, according to Heisenberg, the reason that we can never know the simultaneous position and momentum of a subatomic particle is because by observing one we inevitably alter the other. Finally, some natural phenomena resist exact, deterministic explanations and predictions. For example, the path of an individual photon in a double slit screen experiment cannot be predicted with any certainty. And according to chaos theory, there exist some physical systems which demonstrate sensitivity to initial conditions. Therefore, because it is impossible to take infinitely precise measurements of the state of a system at one point in time, it is impossible to predict the future states of such systems. Although some randomness has been found to exist at the subatomic level, this discovery should not be generalized; in other words, the vast majority of natural phenomena continue to lend themselves well to causal explanation, *even if more than one theoretical formulation can be fruitfully applied to a single event.*

In social science, indeterminacy and unpredictability are viewed as the hallmarks of human reality. This is because human beings have consciousness and free will, construct their own subjective and intersubjective meanings, exist in 'open' systems, and are influenced at any one time by a complex pattern of environmental factors, both physical and cultural. In social sciences such as sociology and education, many researchers and practitioners have come to the conclusion that scientific methods of inquiry reduce the complexity found in most social problem-fields to such a degree that the general laws obtained by them are *practically* meaningless. As a result, a plurality of heterodox methods has arisen in the human sciences, all with their own internally consistent logic and trustworthiness criteria. Most of these alternatives to the scientific method have recognized that in order to gain an understanding of a cultural reality, a researcher cannot remain neutral. S/he must assume the role of a participant-observer and exercise critical judgment in inquiry in order to avoid misrepresenting the deep meanings found in such a context.

Chapter Four also demonstrated that all knowledge--empirical or rational--is constructed, not discovered. In other words, the acts of theorizing about abstract relations of ideas or observing empirical reality are shaped from beginning to end by human considerations. We cannot transcend fallible human judgment in knowledge acquisition by appeals to formal systems of thought or algorithmic procedures of verification. For example, the decision of when to consider an hypothesis or theory falsified by a counter-instance is underdetermined by logic (i.e., by a set of rules ostensibly designed to guide practice). A scientist must exercise his/her own judgment in making such a decision. Furthermore, a finite data set will lend inductive support to a plurality of theoretical explanations, and the choice of which one is correct is underdetermined by scientific observations or tests. Moreover, human perception is theory-laden, not passive. A single event can be seen in different ways, depending on the prior experience and knowledge of the observer. Finally, no system of rational thought is self-justifying; all suggest *human* preferences for one structure or another. For example, whereas Descartes chose Euclidian geometry as the foundation of his unified system of rational knowledge, the logical empiricists preferred that Whitehead's and Russell's *Principia* logic fulfil this function.

Finally, in Chapter Five I outlined a postpositivistic justification for a *plurality* of epistemologies or ways of knowing in all fields of inquiry. In addition, this 'new philosophy of science' has reinstated the humanist content lost to the 17th-century rational-scientific revolution. On this view, the 'neutral' scratch point of scientific observation is no longer considered to be disinterested. The conduct of science is no longer seen as a rule-governed practice that eliminates the need for human judgment. Scientific progress is no longer viewed as the accumulation of certain knowledge of a single form (i.e., nomothetic laws) through the application of a foundational method. Instead, scientific observation is considered to be theory-laden, scientists are viewed as members of language communities whose world is constituted by a set of disciplinary presuppositions, practical judgments are considered to enter into inquiry at a number of crucial points, scientific truth is considered to be plural, and scientific progress is seen as a development from theories less adept at solving puzzles to those more adept at doing so (or as the accumulation of an eclectic set of theories which each reveals some new potentiality in a phenomenon).

The view of the scientist and the competencies s/he wields in practice have been elevated by the new philosophy of science. The scientist is no longer a technician who follows the rules of inquiry laid out by the *one true foundational method of mathematical physics*. S/he is a member of a scientific community which shares a paradigm, or a set of underlying assumptions about such things as proper methodology, the problem-field to be studied, the theory to be applied to that problem-field, what counts as an adequate puzzle solution, etc. In short, a community's paradigm defines its epistemology, ontology and methodology. These presuppositions constitute the world of the scientist to such a degree that they determine what s/he sees when observing a phenomenon; in other words, perception is transactive or theory-laden. A different scientist, operating from within a competing paradigm, will see something different when observing the same event. Therefore, a plurality of standpoints can exist concurrently in a discipline, all supported by empirical evidence, all 'truthful,' and yet all incomplete because they are each based on different presuppositions that bound, and truncate, the subject matter of the field in different ways. The choice of the more veridical account can remain underdetermined by experience.

Occasionally, a community's paradigm will fail to account for a novelty of fact. At this point, the practicing scientist has no rules to follow in deciding how to treat this anomaly. The first response is usually to treat the counter-instance as a problem to be dealt with under the existing theory. However, there is nothing to tell the scientist when to abandon orthodox theoretical presuppositions and to begin constructing a new view of the field. S/he must make a judgment as to whether or not it is possible to extend the existing structure to somehow account for the novelty. If it is decided that the most fruitful course of action is to construct new fundamental axioms to deal with the problem, then again the professional judgment and creativity of the scientist come into play. S/he will move away from the orthodox presuppositional matrix in a series of small, creative steps until the counter-instance is accounted for. The new paradigm that is created will hopefully preserve most of the successes of the previous standpoint, and, if lucky, will bring heretofore unsuspected phenomena into view.

The scientist faced with a choice from among a plurality of competing paradigms must exercise a form of rationality that is beyond the rigorous application of a preferred method. The scientist who considers his or her paradigm to be foundational can behave rationally by carefully applying the techniques and rules which are prescribed by it for the acquisition of trustworthy knowledge. However, such a scientist, when faced with a plurality of largely incommensurable standpoints existing simultaneously in a discipline, each in some way fruitful, and all incomplete, behaves irrationally when s/he chooses a foundational method simply because s/he believes it to be superior. There might be a competing school which is more theoretically and empirically progressive. Or if there is no clear choice between paradigms, perhaps the treatment of subject matter provided by one of the standpoints more closely matches the researcher's purposes in inquiry. Again, there are no rules to help the researcher decide. S/he must listen to the rhetoric concerning the relative fruitfulness of competing paradigms which emanates from the communities existing in a discipline, and in the end exercise his/her professional judgment in choosing. Or if there is no clear winner, a scientist can use dialectical methods to surface the underlying presuppositions of different paradigms, turn each structure into a view, and decide among them based on a complex understanding of how they each bound the subject matter of the field differently. In the end, however, a scientist cannot choose just any view of the field. Each of the contenders must have the support of a community of scientists who have relied on their combined practical expertise to appraise the match between their accepted paradigm and empirical reality. If they agree that the former sheds light on the latter in a useful fashion, then we are in the presence of a provisional scientific truth.

So part of the scientist's practice is informed by rules, and part of it involves the application of practical wisdom and creativity. When solving the puzzles laid out by a paradigm, scientific practice is black and white: it is enough to follow the methodological rules laid out by an accepted structure. During the onslaught of a crisis in the field, when accepted theory no longer meshes with empirical reality, the scientist is thrust into a grey zone where the old rules no longer apply and uncertainty rules. It is at this point that the critical practical judgment and creativity of the scientist must come into play. While not perfect, practical decision making based on sound reasons is as rational as formal logic; indeed, it is *more rational* than continuing to employ a foundational method when clearly it is no longer fruitful to do so.
In sum, the practical part of Aristotle's dual epistemology which was lost in the 17th century has been recovered in this postpositivistic

view of scientific practice and progress.

So now my philosophical justification for a plurality of paradigms in educational research, and a plurality of ways of knowing in teaching practice, has come full circle. Quantitative methods of research and the technicist view of teaching practice have been foundational in education not so much because research paradigms such as the process-product program have been so outstandingly progressive in explaining educational phenomena[1], but perhaps more so because the belief in the superiority of the scientific method has been so engrained in the consciousness of the west. The postpositivistic philosophy of science has demonstrated that a plurality of progressive paradigms can coexist in a discipline such as education, each contributing a different form of knowledge, and each providing a different way of viewing the crucial phenomena of the field. These multiple constructed realities provide practitioners such as teachers with a plurality of ways of knowing. No longer is rational teaching practice limited to applying the general rules provided by quantitative inquiries in a technical manner.

The new view of scientific practice provided by postpositivism is also instructive for teachers. Like the scientist who must act in those grey zones where the rules of his/her chosen paradigm no longer apply, teachers daily face situations where values are in conflict, where a unique situation arises, or where complex patterns of influence create a classroom environment that impedes learning. In these indeterminate zones of teaching practice, the generally applicable rules provided by quantitative research methods are also inadequate. The teacher, like

[1]Cf. MacMillan & Garrison (1984).

the scientist, must exercise his/her professional judgment in such situations. This does not mean that theoretical views are useless in such circumstances. Indeed, they can inform the perception of the teacher in practice and help to make decision making more critically intelligent. And because different paradigms in education will reveal different potentialities in educational phenomena, a teacher who has the ability to shift theoretical lenses and view a practical problem from different standpoints will increase his/her possible courses of action, and make his/her response more reflective. I now turn to a brief outline of three research paradigms other than the scientific method which could contribute ways of knowing to teaching practice.

The Professional Knowledge Base For Teaching: A Plurality of Paradigms

As indicated in Chapter Four, since the 1970s three new research paradigms have emerged in the field of education. This section will briefly outline some of the underlying presuppositions of each approach. As well, a brief sketch of the type of knowledge that each approach can contribute to the professional knowledge base for teaching will be given. In the end, the purpose of this section is to demonstrate that there exist forms of knowledge ready to be added to the teachers college curriculum which could serve to widen the perspective of the neophyte teacher on his or her chosen profession. It is also suggested that educational practitioners, like researchers, be educated in arts of eclectic so that they can choose reflectively from among an eclectic set of paradigms. Finally, teachers should be trained to employ reflective, deliberative or scientific modes of thought in their practice, which are more systematic than--and therefore superior to--common sense approaches to problem solving.

The first class of research methods to be added to the professional knowledge base for teaching are qualitative, or interpretive, modes of inquiry. The products of such inquiries are case studies which include (a) 'thick descriptions' (Geertz, 1973), and (b) interpretations (or emergent theoretical analyses), of individual educational contexts (see Lincoln & Guba, 1985; Erickson, 1986; Eisner, 1977, 1991). Unlike scientific propositions and techniques, the assembled case study literature of teaching, describing and interpreting both exemplary and lackluster practice, provide teachers with *vicarious* means of knowing 'what teachers know and do' in a variety of unique pedagogical situations.

> While process-product researchers view classrooms as reducible to discrete events and behaviours which can be noted, counted, and aggregated for purposes of generalization across settings and individuals, interpretive scholars view classrooms as socially and culturally organized environments. Individual participants in those environments contribute to the organization and to the definition of meanings....Those personal meanings become the focal point for inquiry in contrast to the behaviours that focus the effort of the process-product scholars. (Shulman, 1986b, p. 20)

Indeed, case studies can expose student-teachers to more exemplars, or paradigms, of professional practice in unique, and often problematic, situations than they could ever experience in their brief practice teaching rounds. Such exemplars will re-educate their perception (Eisner, 1991), and help them to interpret their own circumstances when they teach by providing them with a repertoire of familiar examples or themes (Schön, 1983).

The second category of research methods to be appended are critical modes of inquiry. Critical theorists begin their inquiries--which use quantitative and/or qualitative methods--with a dominant value or set of values, such as undistorted communications (see Habermas, 1971/1968, 1979/1976, 1982/1970); or democracy, equality, and justice (see Giroux & McLaren, 1986); or caring (see Noddings, 1988). The outcomes of critical inquiries are usually twofold: (1) a determination of how and where schools as institutions are undemocratic (uncaring, etc.), or reflective of the biases of the dominant culture in society; and (2) a list of suggested alternatives to the present system (Tom & Valli, 1990). The knowledge yielded from such studies differs from scientific generalizations in that no attempt is made to be 'objective,' and differs from qualitative interpretation in that no attempt is made to balance subjectivity and objectivity in inquiry. Such knowledge, in "...establishing that current practices are ineffective for realizing democratic values and...generating new practices that are more consistent with these values" (Tom & Valli, 1990, p. 384), provides neophyte teachers with a basis for emancipatory practice. In fact, teachers, as 'transformative intellectuals,' are encouraged to formulate their own critical analyses of current educational institutions and to recast practices in emancipatory terms (Giroux & McLaren, 1986), thereby integrating their belief system with their practice (i.e., praxis).

The third, and final, addendum to the knowledge base for teaching is the wisdom of practice.[2] This epistemological paradigm is variously known as: (a) 'reflection-in-action' (Schön, 1983, 1987, 1989), (b) the

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[2]Also known as *phronesis*, an Aristotelian term.

'experimental or reflective method of thought' (Dewey, 1966/1916), (c) 'inquiry-oriented practice' (Zeichner, 1983; Garrison, 1988), (d) 'expertise as deliberate action' (Kennedy, 1987), (e) the 'art and craft of teaching' (Eisner, 1983), (f) the 'logic of knowledge application' (Fenstermacher, 1986), (g) the 'deliberative orientation' (Zumwalt, 1982), (h) the 'logic of practice' (Reid, 1978), and (i) the 'practical' (Schwab, 1978c/1970, 1978d/1971, 1978e/1973, 1983). Rather than emphasizing the use of scientific methods for knowledge codification, this paradigm underscores the importance of using scientific modes of thinking in practical deliberations to foster wise decision-making and action in often complex, uncertain, unique, unstable, and value-laden practical situations.

The practical, or deliberative, paradigm is *theoretical* in two senses. First, practical deliberations make use of all pertinent codified knowledge: scientific laws and systems, interpretive case studies, and philosophical (metaphysical or moral) doctrines (*doxa*). Scientific propositions and techniques are used *not as rules to be followed in unreflective, instrumental decision-making*, but in a *generative* or *interpretive* manner, as (a) 'assertions' about events, states, or phenomena (Fenstermacher, 1986), (b) 'intellectual instrumentalities' (Dewey, 1929), (c) 'schemata' (Fenstermacher, 1982), (d) 'views' (Schwab, 1978d/1971), (e) 'rules of thumb' (Eisner, 1983), or (f) 'exemplars', or 'ways of seeing' (Kuhn, 1970), which can improve a teacher's practical deliberations. In short, rather than *prescribing* practices, theories are to *guide* thinking practitioners.

Interpretive understandings, rendered in case studies, are an important source of formal knowledge for practitioners. They provide formal representations, using artistic language, of the expert practitioner's (a) knowing-in-action (Schön, 1983, 1989), (b) tacit knowing (Polanyi, 1966), (c) know-how (Ryle, 1949), (d) embedded knowledge (Pratte & Rury, 1991), or (e) craft knowledge/theory (Eisner, 1983; Battersby, 1987). Again, such knowledge functions in a generative or interpretive fashion, helping to re-educate the perception of the practitioner so that he/she can distinguish between the significant and irrelevant qualities of his/her own practical situations (Eisner, 1991). Or, stated another way, such a repertoire of exemplars will improve the analogical reasoning of the practitioner, helping him/her to *see* his/her present situation *as* like one experienced in the past (i.e., Schön's [1983] 'seeing-as').

In those areas of professional practice where the teacher must choose among conflicting ends (i.e., the value-laden areas of practice) knowledge of philosophical or moral doctrines will provide them with the language and concepts needed to reflect on their own beliefs concerning education, and to choose among conflicting ends in a *rational fashion*; that is, by having good *reasons* for choosing one path over another. Again, such metaphysical knowledge is interpretive, serving to generate alternative ways of seeing a practical dilemma.

This paradigm descended from pragmatist philosophy, and so it embraces the practical use of a plurality of theories, interpretations, and philosophies (see James [1969/1907] on his doctrine of 'epistemological pluralism'); however, to avoid radical relativism or subjectivity, these views are not to be used in an unreflective manner. A teacher must *critically* apply these knowledge claims in practical arguments in order to be considered to be acting rationally. Some individual elements of an eclectic set of theories and doctrines will prove more fruitful than others in informing a practitioner's thinking regarding a problematic situation. This is because the presuppositions of different researchers operating from within competing paradigms will cause them to explore certain elements of phenomena, and ignore others. As a result of this, it becomes essential for practitioners to have the ability to discern the theoretical and methodological commitments behind different research findings in order to know which knowledge claim(s) will best apply to their practical problem. Schwab's (1978d/1971) 'arts of eclectic' provide us with a means of (a) making the substantive and syntactical structures of theories manifest, and (b) undertaking 'enquiries into enquiries' (Schwab, 1978a) which will allow practitioners to make rational choices from among an eclectic set of necessarily partial theories.

The second sense in which the practical paradigm is theoretical is in its use of scientific modes of thinking by practitioners to generate and test their own theories in those practical situations where formal, general knowledge breaks down. Harris (1993), in summarizing Schön's (1983, 1987, 1989) 'reflection-in-action,' provides an excellent synopsis of this complex process of practical rationality:

> Specifically, ...some of the most important problems in practice are characterized by complexity, uniqueness, uncertainty, and conflicting values. The goal of practice is wise action. Wise action may involve the use of specialized knowledge, but central to it is judgment in specific situations, with conflicting values about which

> problems need to be solved and how to solve them. An essential genre of knowledge used in practice is practical knowledge--"knowing how"--which is embedded in practical reasoning. [Practical reasoning] involves knowing-in-action, reflection-in-action, and reflection-about-action, using repertoires of examples, images, and understandings learned through experience (pp. 26-27).

Knowledge-in-action is Polanyi's (1966) tacit knowing, or knowing more than we can articulate. Such knowledge is transmitted to novices through both case studies and apprenticeships with expert teachers. Reflection-in-action, which occurs during practice, and reflection-about-action, which occurs following practice, are systematic forms of thought--indeed *scientific forms of hypothesis formulation and testing in and following practice*--by which a teacher attempts to (a) understand troublesome or puzzling practical situations, and (b) effect wise action (Schön, 1983, 1989).

A New System of Teacher Education

In conclusion, the paradigm just discussed, i.e., the wisdom of practice, suggests a two-part system of teacher *education* rather than training. The first part, lasting a year, would be spent in the academy learning Schwab's 'arts of eclectic' in order for neophyte educators to have a polyfocal perspective on the professional knowledge base for teaching. This would allow them first, to see practical situations from a variety of viewpoints, and second, to reflect about which form of knowledge would best apply to a practical problem. Furthermore, beginning teachers should be educated to have a polyfocal perspective on the subject matter content that they are going to teach. In this way teachers will more easily acquire what Shulman (1986a) refers to as the 'missing knowledge base for teaching,' i.e. *pedagogical content* *knowledge* or know-how in representing subject matter knowledge in a way which is easily learned by students. By achieving a polyfocal perspective on the discipline they are to teach, pedagogues will understand the different paradigms or structures that exist in the field, and will be able to point out where controversies exist, where knowledge claims are inconclusive, etc. They will understand how their subject matter was arrived at, and should therefore be able to structure its presentation in such as way as to be easily digestible by their students.

The second part of this new system of teacher education, also lasting a year, would be spent as an internship in the field among a community of expert teachers dedicated to reflective practice. Special teaching schools, like teaching hospitals, could be set up where master teachers are allocated time to educate both students and student-teachers. In this way they would have the time to demonstrate expert teaching, and scientific modes of thinking in practice (i.e., reflection-in-action) and following practice (i.e., reflection-about-practice) in round-table discussions about teaching. In other words, they could truly mentor the beginning teachers assigned to them. And the intern teachers could spend a year observing a variety of expert teachers plying their trade, teaching part-time, and reflecting with peers and mentors about the science, craft, and art of pedagogy. In this way perhaps future generations of beginning teachers can avoid the 'practice shock' I suffered upon beginning my teaching career, in which I struggled to figure out when and how to apply the rules of practice that I was taught in teachers college. In short, we must avoid training beginning teachers to be techicians, and educate them to be critically reflective in their practice. Our students deserve nothing less.

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