METHODS IN THE SOCIAL SCIENCES:

A CRITIQUE OF POSITIVISM

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INTRODUCTION

Vaugan, Sjoberg, & Reynolds (1993) observe that sociology has received considerable criticism over the last few decades from the media as well as from within (in particular from the American Sociological Association). It has lost ground in terms of funding and visibility. A great many departments are problem-plagued and some have been downsized considerably or shut down entirely. These authors argue that much of sociology's lost relevance and other problems are rooted in theoretical and methodological orientations.

A new range of social issues are emerging and yet little addressed. This is seen as a consequence of the dominance of a paradigm that claims neutrality to the point of irrelevance. (If sociology implicitly supported the status quo through the dominance of functionalism, the dominance of the positivist methodologies implicitly arrives at irrelevance).

> Our diagnosis is that over the past several decades the discipline's intellectual contribution has been deeply eroded by the growing dominance of the natural science model. Standardized technical research procedures have been employed at the expense of sustained theoretical analysis of empirical phenomena (Vaugan, 1993:5).

The authors note this perspective's emphasis in the

major schools as well as the research journals. They see this as a legitimacy issue. Their criticism is built upon Mills, Gouldner, and Sorokin as well. They add that it is unfortunate that Sorokin "undercut his powerful intellectual argument in, for instance, his Fads and Foibles in Modern Sociology and Related Sciences by the harshness and 'overstatement' of his attacks" (Vaughn, 1993:7).

Vaughan further notes:

> We reason, instead, that the fundamental crises in the field exists in the failure of sociologists to engage in, and make significant contributions to, the discourse concerning the major empirical and moral problems confronting humankind as we approach the end of the twentieth century (Vaughn, 1993:11).

The focus of sociology has been defined progressively by the growing dominance of a specific methodology grounded in an intellectual bias which has a dubious basis in the realm of the hard sciences themselves. This outdated mode of thinking and perceiving has limited the sphere of investigation and the subject matter to be investigated. Whole realms of human experience have been disqualified and these realms appear to be increasingly relevant (if in fact they ever really lost relevance) to modern conceptions of reality.

> Underlying the apparent fragmentation is a pervasive methodological paradigm that stresses the necessity of technical research procedures, primarily quantitative in nature, as the basis for scientific sociology. Technical precision and replicability are the hallmarks of the current methodological hegemony. Yet to achieve these objectives, researchers typically focus on

> narrow, manageable research problems, thereby furthering the fragmentation in the discipline (Vaughn, 1993:11).

This has lead to the deplorable condition which science has traditionally fought against. "Instead of the research problem defining the methods to be employed, research techniques define the sociological problem to be addressed" (Vaughn, 1993:12). Vaughan sees this as a form of intellectual hegemony and as a threat to sociology as "a viable scholarly enterprise" (Vaughn, 1993:12). He notes that the situation has also been acknowledged by two recent past presidents of the American Sociological Association and so is not merely the complaint of a few discontented sociologists.

As a consequence of my analysis in this paper I have concluded that this methodological struggle has its roots in the history of the discipline as well as in the history of the natural sciences. Legitimation appears to play a major role. In the past, science has been closely allied with the voices of authority and power. Any discipline seeking legitimation will need to find it in the arena defined and funded by those with power and authority.

Of particular interest among my findings, is that the practitioners of the natural sciences themselves seem to have tried to ignore the implications of Relativity theory and Quantum mechanics for their own disciplines. Worse still is

how completely it seems to be ignored in sociology. The dominance of a methodology with no basis for legitimacy in philosophy or contemporary science is clearly backward to a fault. Vaughan notes that in their efforts to deal with evaporating funds due to political climate, sociologist seem to have tried harder to win approval by becoming more conservative and rigid in their embracement of positivism. Those who study physics and the epistemology of science know that the paradigm has lost much of its foundations. We appear to be waiting for the old guard to die out as Kuhn (1962) might suggest.

> Our 'natural science sociologists and psychologists' try to build their disciplines according to the model of this Newtonian macrophysics. They seem not to have noticed that the development of the physical science in the twentieth century, especially after the emergence and growth of quantum microphysics, has made this Newtonian physics if not obsolescent then, at least, inadequate (Sorokin, 1956:151).

This paper will be an effort to thoroughly explore from a philosophical and historical perspective the evolution of science and the positivist tradition as it relates to sociology. In the process an effort will be made to identify its shortcomings for sociological research and to explore an alternative route of development that already has a strong history within sociology. This paper seeks to demonstrate that Mead had developed the basis for a sophisticated research perspective with regard to social phenomena. This

perspective and the associated epistemelogical reasoning were derived from his analysis of traditional philosophy as well as Relativity theory in physics. This program of development has never been clearly explicated and yet its implications would appear to be profound.

This paper will also seek to explain why this perspective is better suited to sociology and that it is because it has its foundations in social phenomena itself and was developed specifically for that domain from developments in physics and philosophy. It will be further demonstrated how that perspective continues to be supported by further developmental convergences in several important related sciences as well as philosophy.

More specifically this paper proposes that the early division of the world into primary and secondary qualities by the "new philosophers" (Flew, 1994:18) of science was a fundamental error that was to plague the natural scientists for centuries to come. This distinction would lead to numerous consequences, including the cultural adoption at the cognitive level of a mind-matter duality and a subject-object duality which was continually overlooked in scientific and philosophical debates as a primary problem concerning the natural science model.

This distinction which was so fundamental to the Newtonian model of the universe, also made the natural

science model inappropriate for the universe of Relativity as well. Mead was one of the first to emphasize the error of this primary/secondary distinction and the problems it presented for both the social sciences and physics. His model of the universe attempted to steer clear of both determinism and solipsism (Miller, 1973). It was a perspective grounded in primitive terms of sociality, systems theory, emergence, and a new relation between mind and matter.

Herbert Blumer began to forge a research tradition based on this perspective and it appears from my analysis that he came under attack because of the hegemony of the natural science cognitive bias. The political struggle that resulted left sociology divided and Blumer's program incomplete in light of Mead's theories.

The natural science model apparently adopted by sociology in its search for legitimacy based around the mind/matter cognitive bias continues to dominate the discipline today even though it has little philosophical or other legitimate basis since the advent of Quantum mechanics (other than ignorance).

The fulfillment of Mead's arguments awaits the reconciliation of these two perspectives, quantitative and qualitative, into one integrated model. This paper argues for a sociological paradigm of empirical science based on the

entire domain of phenomena, including primary and secondary qualities, and utilizing the natural science model as a subordinate procedure applicable to a stable domain of data which can be used as a reference system. This reference system would anchor and orient research in a wider ontological domain of data utilizing a variety of methodological and theoretical strata.

Thesis

The scope of this paper is unfortunately but necessarily broad and it is often easy to lose sight of the central arguments in the exploration of all the avenues pertinent to the central issues of consideration. With this in mind I present a core thesis around which the other themes of the dissertation are built.

1. A certain class of social objects are measurable and understandable through the postulate of relative causality. They consist of primary qualities accurately described by scientific method.

2. A certain class of social objects are not measurable and understandable through the postulate of relative causality- they are complementary. They consist of secondary qualities. These objects are understandable through descriptive meaning already general and agreed upon through the process of language. Their accurate description can be achieved through

observation, intuition, deduction, and induction. Phenomena one is a subset of phenomena two. The meaning of all phenomena is through consensus (Brissett & Edgley, 1990).

The agenda here is not to entirely disqualify positivism but to take a critical look at it in depth with respect to sociology. Although other writers have done this, it has not been in light of Mead's contribution. Lincoln & Guba (1985) and perhaps Phillips (1973) have made the most informed effort to date, but they fail to account for some key issues which Mead had brought to light. Consequently, I believe their solutions fall short of the mark. The fact that the traditional positivist perspective continues to dominate sociology demonstrates the unconvincing nature of their arguments. There seems to be a need for a more thorough analysis and documentation which reviews the historical development of thought and the context it emerged from as it relates to science. In this careful, if sometimes tedious, process the truth concerning positivism's weakness and shortcomings should become evident to even the most skeptical.

As I explore the strengths and weaknesses of Symbolic Interactionism (SI), I will present the thesis that the primary existing weaknesses of this perspective are due to two major factors. The first is that its critics have argued against it from the perspective of positivism and that once

this is taken into account many of the arguments fall short of their mark. The second factor is the mysterious failure of Herbert Blumer to include the primitive notion of systems in his method and perspective. This concept was central to Mead's perspective and provides a dimension to SI which answers a majority of its critics with regard to other major criticisms not directly linked to the now discredited positivistic perspective.

The outcome of this analysis will be to present SI as a stronger and more comprehensive theory with respect to social phenomena and a perspective which has developed within the sociological tradition and specifically for the study of social phenomena. SI methods have not been imported from another discipline and they are grounded in a strong contemporary philosophical and scientific perspective. Positivism, on the other hand, has been imported from another scientific domain, dealing with different variables, and based on an outmoded philosophical and experimental tradition.

Method

My methodology is entirely archival. I will draw from several disciplines to establish my theses as sociology alone does not provide the resources for a full analysis. I will draw from philosophical and historical perspectives to

describe the development of positivism. It will also be necessary to utilize concepts from modern physics to explain fully the problems with the positivist perspective and lay the groundwork for understanding the full implications of the perspective of Mead which have never been entirely explicated.

Once this background has been developed it will be possible to better analyze and evaluate the arguments between positivists and interactionists within the sociological tradition. Brief historical accounts of the developments of SI, systems theory, and positivism within sociology will further round out the picture.

Finally I shall be drawing on convergences between SI and developments in cognitive psychology, neurophysiology, and modern physics which cross-validate and support the basic approach of SI as well as further explicate its potential as a meta-perspective within sociology.

In consideration of this task the first chapter will briefly review the development of ideas of early science and the key players. It will focus on the piecemeal fashion in which they emerged and examine how they were aggregated together in several competing perspectives. It will explore the key moment in history when these ideas seemed to be legitimately integrated. It will consider how this legitimation came to be in spite of their contradictory

nature and the weakness of the arguments supporting them.

The second chapter examines the crises that befell science due to the inconsistencies on its conception that were never fully addressed. In this chapter the implications of Relativity theory and Quantum mechanics for traditional science are reviewed. A detailed examination of the efforts of the Logical Positivists and Karl Popper to rescue traditional science and their failure will be made. This brings us to the modern period in which scientists either out of frustration or ignorance have lost track of the debate and continued to use the methods and philosophy of the positivists without regard to their fallacies and shortcomings.

Chapter three explores the process by which positivism became legitimated. Why did the methods of science come to dominate the intellectual life of the west in spite of its inconsistencies and shortcomings. Why does it continue to have such a revered position in academia and why is its use and legitimacy almost unquestioned in sociology.

Chapter four focuses on the problems which have resulted from trying to use positivism in sociology. It reviews the history of positivism within sociology and the arguments of some of its leading proponents such as Lundberg and Zetterberg. It also evaluates their arguments in light of the perspective developed in the preceding chapters including

the problems of induction, causality, prediction, and implicit cultural bias.

Chapter five reviews the history of systems theory within sociology in order to provide a clearer definition of the concept of system. The stereotypes surrounding systems theory in sociology dramatically distort the picture of its development in the sciences in general due to its relation with Parson's perspective. It is necessary to specify the differences between various systems perspectives in order to clearly define Mead's position as well as fully understand that position. Since systems theory is presented in this paper as fundamental to Mead's perspective, this clarification regarding the definition of systems is crucial to the development of my thesis.

Chapter six examines the work of George Herbert Mead as a source of an alternative epistemology of science tailored specifically for social phenomena and grounded in the perspective of Relativity theory. This chapter reviews the cogency with which Mead analyzed the historical shortcomings of the traditional conceptions of knowledge and the relationship of mind and matter. It explicates the disregarded importance of systems as a central concept of Mead's perspective. It establishes Mead as a theorist with a unique intellectual vision that generates a more adequate and fuller conception of social phenomena.

Chapter seven begins to demonstrate some of the ramifications this has for sociology, its methods, and its scientific perspective. It looks at Blumer's methodologies, his political conundrum, and its shortcomings with respect to Mead's vision and sociology in general. It focuses on the implications of reintegrating a systems perspective into SI and the benefits that are derived from this effort.

Chapter eight attempts to outline in sum the overall implications of these findings for sociology. It suggests an ontological perspective that includes both primary and secondary (qualities) data in analysis as well as establishing a solid rational for the synthesis of quantitative and qualitative methodologies within the domain of meaning. It suggests roughly the contours of such a new perspective and outlines future avenues of possible development.

CHAPTER 1

THE EMERGENCE OF SCIENCE

Science is an intellectual product (phenomena) of western civilization and it emerged from Europe primarily in the 15th through the 18th centuries. It was in part a reaction to the confusion and social turmoil arising from increasing warfare between cultures and the expansion of trade (Palmer, 1964). As contradictory ideas flowed into Europe posing as cultural absolutes, writers, scholars, and intellectuals began to succumb to a degree of relativism in their thinking as espoused in the form of the French essayist Montaigne. Skepticism became very popular and it was held by many that all beliefs were only customs as in the writings of Pierre Bayle (Palmer, 1964). Alchemy and astrology included beliefs and practices that today distinguish them from chemistry and astronomy. The authority of the revered scholars and traditional books of the middle ages came under serious scrutiny. The doctrines of the church began to be questioned and Aristotelian scholasticism came under attack as well.

Along with these new ideas rediscovered from ancient times (especially the Greeks) came a new system of math

developed by the Arabs which included rudimentary algebra. A new interest in atomism was revived (Brown, 1996). The internal consistency of Euclid's geometry and Arab mathematics suggested the possibility of a more exact and reliable system of knowledge which might combat the relativism of metaphysics as it was manifesting. Many educated men in the Church (as well as outside) privately dabbled in math in the belief that it held a mystical connection to the fundamental truths about the world (Palmer, 1964). The Ptolemaic model of the solar system, the model accepted by most educated Europeans, was itself a complex mathematical model. It was however getting to be difficult to sustain it due to observations which, as they became more precise, demanded greater detailed accounting which also became more contradictory (Sharratt, 1994).

Such men as Kepler and Copernicus worked privately at developing a mathematical system of knowledge (Scharratt, 1994). Others dabbling in experimentation and observation techniques as well as astronomy such as Tycho Brahe and Galileo sought a more empirical basis for knowledge than mathematics alone (Sharratt, 1994). Still others such as Francis Bacon advocated a strictly experimental and empirical approach to such a new system of knowledge (Faulkner, 1993). This debate over a mathematically based or an empirically based system of criteria raged for several centuries and in fact continues into the present, especially in the field of

sociology.

It cannot be emphasized enough that several approaches to defining a new system of knowledge were competing in the European intellectual currents and that this new effort was stimulated by confusion and the overwhelming desire to establish a new basis for order especially by the Deists (Brown, 1996). In central Europe especially the Thirty Years War and the witchcraft panic generated an atmosphere of confusion and terror among the population. Efforts to establish greater social order, especially for trade purposes, encouraged the development of advances in law (Palmer, 1964).

New forms of government were also emerging alongside of advances in political-social thought. Law was increasingly being based on appeals to reason based on evidence. After the mid 1600s hearsay evidence was no longer valid in English courts (Palmer, 1964) and confession under torture discontinued. Truth was becoming progressively a matter of evidence rather than a matter of faith. Reasonable people, that is the educated authorities, based their decisions on evidence. Since science proposed the same agenda, it was associated with this perspective as well. Doubt, or skepticism became an important component of reasonable analysis of any subject where evidence was lacking (Brown, 1996). The philosopher Rene Descartes in particular popularized the idea of reasoning based on doubt, as the

ancient sophists did, but many other philosophers in dialogue with him at the time were also enamored of its potential (Brown, 1996; Flew, 1991).

The competing perspectives of the time often emphasized mathematics or empirical experimentation as their basis for analysis. Mathematics was championed especially by Descartes and experimental method by championed by Bacon (Brown, 1996). Another perspective, championed by Hobbes, attempted to balance the two approaches of mathematical analysis and empirical experimentation and was inspired by Euclid's geometry and strongly reflected Galileo's ideas (Losee, 1972; Scharratt, 1994; Woolhouse, 1988). This combination of approaches suddenly emerged through the work of Kepler when he established the mathematical correlation of his equations with the empirical observations of Tycho Brahe (Scharratt, 1994). The discovery astounded everyone involved in the debate and suggested that there really was an undeniable connection between math and the physical world.

Galileo shook the world with the development of his new technology for observation, the telescope. This further verified the work of Copernicus and demonstrated the importance of discovery through empirical observation rather than reason alone as Aristotle had suggested (Scharratt, 1994). Galileo argued for an ontology where the world was divided into primary and secondary objects (Brown, 1996). The primary objects would be the focus of investigation for

the new science and Galileo suggested a special method for these investigations (Losee, 1974).

From this point on much of the debate centered on the proper method to be developed for uncovering and verifying new information about how the world worked. A new ontology and epistemology was needed to explain and justify the "new philosophy" of science. The observations of Galileo lead to an inertial theory of falling bodies which was combined with Kepler's calculations by Newton, using the new calculus of Lebinitz, to develop a comprehensive mathematical theory of physics (Brown, 1996; Losee, 1974). This work was defended and justified ontologically and epistemologically in the writing of Locke (Woolhouse, 1983). Thus the efforts of these philosophers culminated in a mathematical perspective which logically measured and ordered empirical observations in such a way as to predict their reoccurrence. The success of this practice suggested that there was an order in the universe that men could gain power over and use to their own ends. In a period of relative social chaos this was a potent balm.

Important themes continue to emerge and be debated as the "new philosophy" developed over the centuries. Woolhouse (1988) agrees with Russell that perhaps the most significant debate is that between the rationalists and the empiricists. This same debate also centers around the division of the world into primary and secondary qualities which would later

be a central focus of Mead. In another form these debates would hold center stage in neo-positivism as well.

Bacon, Hobbes, Descartes, Galileo

The leading figures of the new philosophy of science were clearly Galileo and Newton. It was, however, Newton's Principia which embodied everything the new philosophy was to be. Much of the initial thought leading up to Newton's work in physics seems to revolve around Galileo, Bacon, Hobbes, Descartes, and Gassendi.

Bacon was eager to establish a better system of knowledge through empirical methodologies relating more to experimentation (Woolhouse, 1988). He demonstrated why Aristotle's Organon was wrong and proposed a new (scientific) method (Losee, 1974). He was convinced that great progress was possible once traditional modes of knowing were left behind (Woolhouse, 1988). He advocated the separation of reason and revelation like Galileo and was clearly aware of the advantages this would have for science. He felt humanity's future salvation would lie in technological mastery of nature (Woolhouse, 1988). Faulkner (1993) insists that Bacon's greatest contribution was his influence on the European community with regard to the importance of science. His vision of the utopia it would lead to was

> the first modern or future oriented utopia, that is, the first to rely crucially on products from methodical research, and the first to organize science and society

> to satisfy desires for health, subsistence, security, and compassionate care (Faulkner, 1993:11).

Bacon, who attained great prominence in England, had tremendous influence on scientific thinkers of his time.

Galileo and Hobbes seem to occupy more of a middle ground of the time. Hobbes appears to be a central player as he knew Descartes and Bacon personally (Flew, 1991). He also visited Galileo personally in his pursuit of a science of human behavior based on geometry; a physical mechanics (Woolhouse, 1988). Hobbes also knew Gassendi who was instrumental in rediscovering the work of Democratus concerning atoms (Woolhouse, 1988). Hobbes like Bacon was interested in method.

> Hobbes's references to previous lack of philosophical progress and the disputatious wrangling of the prevailing scholastic philosophy, and his distaste for ideas based solely on the foundation of authority, are all reminiscent of Bacon (Woolhouse, 1988:29).

He admired the resoluto-compositive method of Galileo and Euclid's system of geometry. In the resolutio-compositive method of Galileo analysis involves deconstruction of a matter into its related parts, determining their cause and demonstrating the original cause of the matter through them (Woolhouse, 1988).

Hobbes developed an analytic attitude toward thinking and saw it as a form of adding and subtracting (Woolhouse, 1988). In this sense he anticipated Frege. His perspective was mechanistic with respect to behavior. His mechanical

psychology is very much in harmony with Descartes mechanical man, although he disagreed with him that mind was a separate phenomena (Urmson, 1995). Woolhouse (1988) says in the final analysis he is more interested in theory than method and the new philosophy was more than knowledge about cause and effect derived from senses. His disagreement with Descartes on the mind/matter division and his other publications won him a notoriety which extended into the realm of political philosophy (Brown, 1996; Woolhouse, 1988). Like Locke he extends the ideas of the 'new philosophy' into the realm of politics and influences European thought regarding law.

Galileo was enamored with the mathematical approach with his interest in celestial geometry and clearly empirical observation was of fundamental importance to him (Losee, 1974; Flew, 1991). What is often less mentioned is that he also involved himself with thought experiments (like Einstein) and emphasized the importance of "creative imagination" in his Method of Resolution (Losee, 1974:55). Perhaps his most significant consideration was advocating the division of the world into primary and secondary phenomena in Il Saggiatore (1623). This restricted the domain of scientific investigation which was to be reflected in Descartes dualism and Newton's physics (Losse, 1974).

> In a now famous passage he claimed that whenever he conceived of a material substance he hade to think of it as having certain properties, of being bounded with a distinct shape and size and in some specific place, as being in motion or at rest, as touching or not touching

> other objects and as being one in a number or few or many. These properties, he said, he could not separate from an object by any stretch of the imagination. It was, however, different with other properties such as taste, color, sound, and smell. These are not properties one is compelled to regard an object as having and without our senses we would not have thought of them. So Galileo concluded that the latter qualities are not out there in the world but resided only in our consciousness and without living creatures they would not exist (Brown, 1996:47).

This perspective moved science out of the Aristotelian universe and the realm of metaphysics (Losee, 1974). Descartes, Locke, Boyle, and a host of other thinkers would build around this idea (Mead, 1982; Brown, 1996; Losee, 1974). The arguments about which realm verification should take place in would continue as central to science into the present. Mind would be associated with the realm of secondary qualities, yet problematically, mind was also the realm of mathematics. For this reason Galileo was not entirely empirical in the sense that Locke became (Losee, 1974; Collins, 1967).

> By restricting the subject-matter of physics to primary qualities and their relations, Galileo excluded teleological explanations from the range of permissible discourse of physics (Losee, 1974:52).

Descartes hoped to reduce all knowledge to mathematical laws (Flew, 1991). He thus shared with Galileo the belief that nature was based on mathematics (Sharratt, 1994). His perspective however did not emphasize empirical confirmation (Losee, 1974). Although his conceptualization of dual nature of phenomena was similar to Galileo's, there were important

differences.

In a time of great intellectual confusion they constituted a group of contemporaries in dialogue with each other aimed toward a new system of philosophy that would guarantee certain knowledge of the truth. They were very diverse in their perspectives but they did seem to agree on some basic general points. This "new philosophy" would be atomistic, materialistic, mathematical, and lawful in the logical sense after the manner of geometry. It would be a philosophy apart from theology, metaphysics, and the dogma of the church.

Descartes and the Mathematical Perspective

Descartes influence on scientific thought lay in his emphasis on mind/matter dualism and mathematics (Flew, 1991). Descartes thought that a priori axioms could be developed from which all laws of material matter could be deducted (Cottingham, 1986; Losee, 1974). He was especially interested in physics and physiology and felt that all natural science was to be unified under mathematics which would clearly and distinctly indicate the truth of a belief (Cottingham, 1986, 1991; Losee, 1974). (This was contrary to the hopes of others to use sense data as the basis for experiment and confirmation.) It is not clear how adamant Descartes was on this point as he engaged in experiments himself and according to Cottingham (1986) had a hypothetico-

deductive idea of science.

Descartes proposed that mind (or soul) was immaterial and not subject to physical laws (this could have been to avoid conflict with church doctrine (Brown, 1996) and that the body is a machine (Flew, 1991). In his Passions of the Soul he saw the body as a machine which could be moved by the soul through the pineal gland by agitating "animal spirits" (Flew, 1991).

Descartes supported Galileo's distinction between primary and secondary qualities to some degree in that he felt physical reality was based essentially on extension which was an innate a priori concept (Cottingham, 1986; Losee, 1974). There are "modes" of extension such as size, shape, motion, position, duration, and number which are also innate ideas concerning objects. Other sensations of such things as color, sound, odor, taste, and hardness are not with certainty part of physical objects (Cottingham, 1986). According to this perspective knowledge of physical objects does not come from sensation.

This is an important difference from Locke's later distinction between primary/secondary qualities and the list of qualities differs as well. Descartes did not believe in empty space either and differed with Newton on this point (Losee, 1974). Locke's distinction between primary and secondary qualities was in harmony with this notion because it implied a basic subject-object dichotomy. Primary

qualities included solidity, extension, figure, and mobility. Secondary qualities were due to powers of objects to produce in us ideas of color, taste, and smell (Flew, 1991; Mead, 1982). For Descartes, physics and philosophy were one (Cottingham, 1986). Their premises were metaphysical in character.

Descartes' influence on philosophy is profound, but his influence on science is curious. He sought to derive basic physical laws from metaphysical principles which Newton and others such as Bacon (and Boyle) opposed. There was agreement his idea of mind-matter division, it worked so well with Democritus's vision, but the British Empiricists defined it differently. There is much confusion here. Hobbes also liked the idea of mathematical laws describing physical reality as did Galileo (Woolhouse, 1988). The exact nature of the relation between physical reality and mathematical laws as well as propositions was not clearly understood. As mentioned previously, it is the basis for confusion even into the present debates.

Descartes popularized the atomic theory, the idea of mechanical man, and the mind matter dichotomy it drew its strength from. It was this dichotomy, which was originally established in agreement with Galileo, which created the problem of properly connecting mind and matter so mathematics and logic could serve as a basis for explanation. It left the Church (which so plagued Galileo as everyone was quite

well aware) in charge of metaphysics of theology, but gave over the realm of matter to science. This convenient division may have been political as well as philosophical (Brown, 1996; Sharratt, 1994). It seemed necessary to try to establish truth and certainty in secure and consistent things such as physical objects, logic, and mathematics. They seemed to belong to the same category in this sense and it seemed reasonable to find that they had a fundamental connection which time would reveal.

There were however many fundamental assumptions which were being overlooked at this time which Mead, Wittgenstein, Quine and others would notice. It was up to Newton, Kant, and Locke to develop the models and philosophy that would describe this relationship correctly. Eventually Mill would codify the whole program. A new method for establishing truth about matters physical would be defined along with a detailed but still flawed metaphysics. Other champions such as Frege, Russell, Wittgenstein, and Quine would continue to emerge to patch up the bridge between mind and matter.

Newton and Locke

Isaac Newton, who formalized Galilean mechanics, attacked Descartes for not being empirical and inductive, even though Descartes had paid lip service to observation and experimentation (Brown, 1996; Flew, 1991). Newton was a member of the Royal Society (Brown, 1996). Newton believed

in absolute space and time and this depended on Galileo's division of primary and secondary (Losee, 1974; Brown, 1996). Newton's work, heavily in step with Francis Bacon and building on the work of Galileo and Kepler, had inspired much of Locke's efforts on primary qualities and highly influenced Kant as well (Flew, 1991; Palmer, 1964). In fact it quickly becomes apparent that Newton was the key player in legitimizing this perspective (Brown, 1996). The Principia, when it was delivered to the Royal Society, was a comprehensive detailed mathematical account of all motion in the universe, but it was very different from Descartes conceptualization. (Hawking (1988) notes that it was not by brilliant argument alone that Newton established his hegemony in Physics, it also involved a considerable amount of dubious political intrigue.)

John Locke had established the philosophical groundwork for empiricism (Flew, 1991). Locke adopted and expanded Robert Boyle's notion that physical objects could be best understood in terms of the rearrangement of basic particles of matter (Collins, 1967). He also favored Boyle's approach to methodology (Yolton, 1985). He was skeptical of Descartes program of a natural philosophy and Collins comments:

> Finally, he is able to discover in experience no connection between primary and secondary qualities. Hence no demonstration is possible in regard to the sensible qualities of bodies. This does not put an end to the advance of sciences, but it does force them to

> rely heavily upon the intrinsically less perfect, but humanly more proportionate, means of observation and experiment (Yolton, 1985:38).

Locke's program was to clear the way for the acquisition of scientific knowledge by providing a thorough account of human understanding from an empiricist perspective that also included the latest developments in science (this was later to be Mead's goal when Relativity emerged)(Yolton, 1985). His intention was "in clearing the Ground a little, and removing the Rubbish, that lies in the way to knowledge"(Yolton, 1974:120). Locke was emphatic in his rejection of formal logic as a tool for science (Yolton, 1985) This became the basis for the more refined empirical arguments of Hume and Berkeley.

Locke argued that mens' ideas do not come from innate knowledge, as was widely held at the time, but all ideas in the mind come from experience (experience is composed of ideas of sensation and ideas of reflection)(Brown, 1996; Woolhouse, 1983). Knowledge should come from observation and sense experience (Yolton, 1985). Most important, as mentioned above and which we will take up later as critical, Locke distinguished between primary and secondary qualities as had Galileo, Boyle, and Descartes (Collins, 1967; Woolhouse, 1983). Although Locke drew inspiration for his perspective by arguing against Descartes (Woolhouse, 1983), Locke agreed with Descartes that the fundamental unit of knowledge is intuition but differed by saying we could have

no certain knowledge of general truths about the world (Brown, 1996; Collins, 1967). This is perhaps because "In some passages, Locke held that an unbridgeable epistemological gap separates the 'real world' of atoms and the realm of ideas that constitutes our experience" (Losee, 1972:97). For this reason he insisted that the best science could do was a collection of generalizations which would not satisfy the rationalist idea of truth. In this he lays the groundwork for the neo-positivists later on.

Kant and Hume

Losee (1972) explains that Hume agreed with Locke (and contrary to Descartes)that all ideas were ultimately derived form the senses but disagreed with Locke's ideas regarding truth. Hume believed Euclid was innately true (in agreement with Descartes) and could not be demonstrated true by empirical verification. On the other hand he held that statements about matters of fact must be established through empirical evidence. This lead him to a disagreement with Newton regarding the use of axioms, and interestingly enough a fundamental agreement with Einstein later on regarding the role of mathematics in science. He emphasized that causation cannot be established in any absolute sense and proposed that it was based upon habitual expectation. He also laid the groundwork for Mill's rules concerning cause and effect in his Treatise. He established that the form and content of

scientific laws are derived entirely from sense experience but "Probability is the only defensible claim that can be made for scientific laws and theories" (Losee, 1972:106). Kant believed that Hume was incorrect in believing that the mind merely manipulates ideas copied from sense impressions. He held that the transformation from sense data to ideas was a complex process involving the progressive application of organizing rules of experience innate to the human mind. The idea was that the innate organizing principle of mind were parallel to those in nature. Inconsistent rules were at odds with the "Principle of the Purposiveness of Nature" (Critique of Judgment, 1790). In a sense he was recognizing that sense experience was theory laden, but with innate organizing principles which could be enhanced through reason. He admired Euclid and Newton and saw Newton's axiomatic method as fundamentally correct

Kant's critique was an effort to settle the controversy over the legitimacy of metaphysics in the face of scientific thought. In the Critique of Pure Reason (1781) "Kant rejects both empiricism and rationalism. There are pure ideas of reason but only as regulatory principles in the service of experience" (Hoffe, 1994:34).

> Kant admits that all knowledge begins with experience, but it does not follow, as empiricism assumes, that knowledge originates solely in experience. On the contrary. even empirical knowledge proves impossible without sources independent of experience (Hoffe, 1994:34).

A priori elements exist in all knowledge. In a sense Kant is between Descartes and Locke. Kant generates the analytic synthetic distinctions of a priori and a posteriori knowledge which will in the future be the battleground between Schlick, Wittgenstein, and Carnap, eventually ending in Quine's famous doubting of their value in his Two Dogmas of Empiricism. Kant is interested in a science of metaphysics however and a priori synthetic judgements, not just science proper. Kant finds mathematics and science both containing synthetic a priori elements. "they are still valid only under the assumptions of synthetic principles" (Hoffe, 1994:42) and that "all mathematical judgements are synthetic", not analytic. The Logical Positivists will claim this is nonsense as only logic and experience can be sources of knowledge. These arguments will be developed more fully in chapter two and five.

Wundt, who adopted Kant's position, also attempted to develop a science of mental causality (Hilgard, 1987). Wundt, however, set up a laboratory to conduct his psychological experiments which so later influenced Durkheim (Durkheim, 1972).

Mill

Losee (1974) records the importance of John Herschel's theories of scientific method (and Whewells conclusions about the history of science) a crucial to Mill's formulations. It

was Herschel who developed the idea of the context of discovery and the context of justification as two distinct but important realms in science. He noted that some laws are derived from induction and others by hypothesis and that "A meticulous inductive ascent (in the Baconian sense) and a wild guess are on the same footing if their deductive consequences are confirmed by observation" (Losee, 1974:116). Herschel then defined the several conditions which would constitute justification including various instances of observational forms as well as the crucial experiment the Logical Positivists would utilize this distinction later.

Mill argued against Whewell with regard to the importance of hypothesis and stated that every scientific law had been discovered by one of the five methods of induction which he espoused. Mill's method argued that the context of justification and discovery were both subject to the laws of induction. He also argued for the importance of proof of causation as a primary goal of science and utilized Hume's analyses to devise methods of verifying causation. Mill's arguments are flawed at many points. In spite of Mill's efforts, Losee notes that general agreement among philosophers of science is that "Mill failed to prove his case" (Losee, 1974:157). In spite of this methodologists in many sciences, especially sociology continue to utilize his schema and cite him as a final authority.

It should be clear at this point that there was

considerable disagreement on how math, logic, intuition, and observation fit together. The underlying arguments were never really resolved. It was the general agreement on the components of the new philosophy and a universal distaste for religious dogma and metaphysics that gave an appearance of some unity to these efforts. That these men engaged in similar activities and debated their merits was novel in itself. Most of these men came from a privileged class and their findings had important ramifications associated with improved technologies in trade and war.

Cottingham makes an important point with regard to the assumption that many in the 20th century make that science derives from some organized program of development in the 17th century which everyone had clearly agreed upon:

> A second potentially misleading aspect of the use of words like 'science' and 'scientific' in connection with the seventeenth century is that they suggest an agreed body of standards, procedures and practices in terms of which theories are tested, evidence assessed and experiments conducted. Yet in the seventeenth century there was no such corpus of rules; indeed, part of the achievement of thinkers like Descartes and Bacon was that they envisaged (though in very different ways) the possibility of developing an agreed methodology for the investigation of truth (Cottingham, 1986:2-3).

As we have pointed out in this section, there was no clear program, but rather competing philosophies. It is important to summarize and clarify the ideas that were emerging as they will play a central role as points of controversy later. Central issues concerned causation, subject-object duality, the use of mathematics, intuition vs

observation, and the division of the world into primary and secondary qualities.

Primary and Secondary Qualities

Besides causation, the arguments concerning the division of primary and secondary qualities is of great importance for, as we shall see, it constituted a basic assumption more important to science than causation itself (As Russell was to later point out).

1. This distinction set science apart from philosophy and theology with respect to the subject of its discourse.
2. It allowed for quantification of subjects investigated.
3. It allowed science to develop without threatening the entire domain of the Church.
4. It allowed science to develop a degree of internal consistency and utility which philosophy and theology could not compete with in a world fervently engaged in war and trade.

By limiting the range of its investigation science was able to focus on phenomena that were more consistent over time (Losee, 1972). The subjects of theology and metaphysics often involved events involving secondary characteristics at most or at least confused by them. Consistency was difficult to establish. The subjects of debate varied widely in definition from person to person and definitive analysis had to be based on faith or authority alone. This lead of course

to the relativism of the time.

Events involving the primary qualities could be measured and quantified. This quantification allowed for greater control over the subject of investigation as well as the application procedures of evidence to publicly verify statements and procedures. It offered an independent measure of truth outside the domain of pure authority and faith.

This had also been the domain of the Church and Galileo had felt their pressure to change his opinion based on authority and faith as well as Descartes (Cottingham, 1986). The new philosophers were well aware that the Church would constrict their efforts if it felt threatened. By giving the church the domain of secondary qualities, these philosophers were perhaps able to reduce their political liabilities.

Summary

There was no basis for the assertions of these philosophers other than personal belief. They argued from an ontology that they were unaware and which biased their perspective (although Hume and Kant seemed to be exceptions). All of them had major problems in the details of their systems as a consequence. They did not recognize their ontological limits defined by their culture. They did not have this notion or possibility.

These philosophers, enamored with their own ideas tried to credentialize them by associating them with important

positions and societies of learning. Some ideas in math proved very powerful in terms of applications for war and trade. Technology developed largely independent of theory, academia, and associated laboratories.

It should be clear from the foregoing that what became the positivism of Mill and Comte was initially considered a new form of philosophy engaged in for the purpose of determining truth:

1. These philosophies were only loosely associated and there was considerable difference concerning methods and ontologies.
2. They became amalgamated under Newton, who followed Galileo's lead, into a form of rhetoric involving mathematics, observation, and axiomatic theory.
3. The new approach was able to address only a limited domain of experience and that domain contained only elements which were measurable.
4. The new approach promised to expand its domain and establish truth once and for all.
5. These philosophies provided power and prestige for those associated with them,

There never was any comprehensive and final agreement among philosophers or the emerging scientific community itself regarding fundamental issues. Galileo and Newton had seemed to establish a new legitimate direction for exploration and it was assumed the details would work

themselves out. This assumption gave way to a focus on method and fact that became institutionalized. The truth of this matter became apparent when Relativity theory emerged and was verified through observation.

Science rapidly became a revered institution which was associated with authority and truth. Its struggle with the church is a testimony to this observation. It became separated from philosophy, which had been its original foundation, and declared the realm of metaphysics, epistemology, and ontology irrelevant. The validity of science was in part established by conveniently forgetting that it was not logical and that its founding assumptions were suspect and never fully established.

It is interesting to make note of this because it points to other validating factors at work, social factors. As thinkers focused on the material world more and more, that which was not easily measurable became delegitimized. This focus resulted in a cultural ontology, which in its demand for social order (and a new one at that) and its flight from the church, found science the best rational. It is a dialectic of an emerging cognitive perspective.

Weber has commented on the emerging rationalism of the west and outlined the emergence of bureaucracy. It would seem that science was the penultimate rationale for this new bureaucratic reality. As the Church had defined the ontology of the middle ages, science defined the ontology of the

emerging capitalist industrial world (Foucault, 1973).

CHAPTER 2

LOGICAL POSITIVISM

Science up to the time of Mill and into the late 19th century continued to struggle for a codified program of method based on a strong foundation of logic. As the preceding has made clear, no such consolidation emerged, even though modern positivistic science grew in theory and prestige. Disagreement continued to rage over proper method and the rational behind it (Losee, 1972). In the late 19th and early 20th centuries experiential science began to uncover information that cast doubt on the legitimacy of the entire program within the hard sciences (Frank, 1961; Heisenberg, 1971). Positivism came under serious criticism. Many in the scientific community rushed in to rescue what was left of old rationales and methods.

The effort in this chapter is to review the crises in modern physics and the key thinkers and the ideas associated with them. What follows is a focus on the development of the main problems and solutions associated with the rediscovery of science's fragile foundations. This will set the stage for further discussions concerning the failure of this approach in sociology and alternative conceptualizations of

reality and knowledge emanating from Mead which potentially resolve these conflicts considerably.

Positivism found its most influential and explicit advocate in the person of Auguste Comte in the 19th century, but has its roots in the works of Galileo, Francis Bacon, Hobbes, Sir Isaac Newton and the British Empiricist school of the 17th and 18th centuries (Flew, 1991). It argued optimistically that the only genuine way to obtain knowledge was through scientific methods. John Stuart Mill sharing Comte's enthusiasm, sponsored and supported his hostility toward metaphysics and theology, and himself embracing positivism laid out fundamental rules for experimental inquiry in his own work Logic in 1843. Together they established the what are often still the conventionally accepted methods for uncovering cause and effect.

The outcome of these efforts was a loose alliance of consensus concerning the nature of reality and the best avenues of knowledge. Although they often disagreed on specifics, the focus of effort of these early scientists and philosophers toward knowledge was a program which was causal, mathematical, dualistic, empirical, experimental, and consensus oriented (institutional). J.S. Mill's effort to codify this approach in the 19th century reflects the general belief among leading thinkers of the time that it represented the best approach to knowledge (Lincoln & Guba, 1985).

Unfortunately the propositions of positivism and the

consequent methodology had no unified rational basis in metaphysics or philosophy and became justified on the grounds of its utility (Flew, 1991; Lincoln & Guba, 1985; Mills, 1959). C. Wright Mills in his book The Sociological Imagination ironically quotes a leading positivist, Percy Bridgman (who developed the idea of operationalism), to this effect:

> There is no scientific method as such, but the vital feature of the scientist's procedure has been merely to do his utmost with his mind, no holds barred (Mills, 1959:58).

Although, as Willer noted (1967), this is clearly an overstatement of the matter, it is presently acknowledged by many that there is a profound difference in the professed methods of science and the actual methods (Phillips, 1973).

Science did appear to produce results which could be neatly systematized into formal bodies of knowledge such as physics and biology. Wundt's laboratory in Leipzig promised equally wonderful findings in psychology and inspired Durkheim in his efforts to develop a positivist methodology in sociology (Durkheim, 1972). According to Weber a form of public knowledge which lent itself well to institutionalization in the universities (Bendix, 1970). The weak foundations of this highly effective enterprise, however, became especially apparent with the development of non-Euclidean geometry and the publishing of Einstein's theory of Relativity in 1905.

The emergence of positivism did not reflect a logical and orderly growing consensus over clearly worked out principles but instead displays a rocky history with many failures and disagreements among its proponents. It was an aggregate of philosophical agendas attempting to control the experienced world (then in cultural turmoil) through prediction and explanation. It relied on the very dubious concept of causation. It accepted only what could be conveniently measured and disregarded all other phenomena. At present it still fails to present a unified program and increasingly appears as what can be only described as a rationale and a faith. Heisenberg observes:

> once the main reasoning of classical physics had been accepted as the a priori of physical investigations, the belief arose, through an obvious though false extrapolation, that it was absolute, i.e. valid for all time, and could never be modified as a result of new experiences (Heisenberg, 1971:22).

The advent of Einstein's theory and Quantum mechanics clearly revealed major problems with Newton's physics which had been the anchor for positivism. The Vienna Circle appeared as a last gasp at legitimizing the mathematical reductionist program (Hume, Newton, & Descartes) and obviously disregarded the full implications of the new physics (Lincoln & Guba, 1985). Now not only were there difficulties establishing a new unified theory of positivism (Popper's being the closest) but the very founding assumptions of causation and primary-secondary divisions of

experience so essential to the program were in question.

Once the Michelson-Morely experiments supported Einstein's theory and non-Euclidian geometry became an uncomfortable reality to contend with, traditional positivistic science began to unravel. In the confusion that followed Ernst Mach attempted to redefine the positivist agenda. Philip Frank (1961) explains that the shift in emphasis by Mach from explanation to description was a critical factor in rescuing science. Causal explanation of a mechanistic universe was no longer a viable concept. Mach argued that all explanation was in fact description.

> He maintained that "explanation" by reduction to a system of cherished conceptions is pure illusion. If all the multitude of observable phenomena are reduced to mechanical or organismic phenomena, these special types of phenomena chosen as the basis of explanation are by themselves no more understandable than the phenomena that are to be explained. Mach claimed that there is no essential difference between an "explanation" and a "description"(Frank, 1961:18).

For Mach, who was considered the founding father of the Vienna Circle, a physical law or explanation is merely an economic description of a class of phenomena (Hanfling, 1981). Frank and his associates, lead by Moritz Schlick and basing their ideas on Wittgenstein's Tractatus Logico-Philosophicus (1921), felt that the mathematical relationship between concepts in a scientific theory put them in a different category than ordinary descriptions involving a vague number of connections among vague concepts (Kraft, 1969). They wanted to retain Mach's insight but expand on

it. At the base of this perspective was still the Empiricists' idea that all knowledge can be analyzed into items of sensation, but furthermore as Wittgenstein believed, "The meaning of a proposition is the method of verification" (Hanfling, 1981:7). Their program was to develop a method by which all meaningful statements "could be analyzed into suitable verification-components" (Hanfling, 1981:7). Anything unverifiable was metaphysics.

Einstein's (1954) theory had established that time and space were not absolutes and that measurement itself varied based on the local geometries operating under different gravities. Not only was Euclid's geometry relative, but also mass, extension, velocity, time, space, and all related primary qualities (Einstein, 1954). The consequence of this is that primary qualities could no longer be measured in absolute terms. The basis for Newton's physics was now gone.

To make things worse for positivism, causality itself was called into question by the work of Heisenberg involving Quantum mechanics. Particle positions became a problem of multiple probabilities. His Theory of Indetermanancy undermined forever the idea of absolute causality as Hume had earlier intuited on a different basis. All the basic assumptions of science had been violated. As Frank has commented:

> Two characteristic beliefs of nineteenth century science broke down during its last decades; these were the belief that all phenomena in nature can be reduced

> to the laws of mechanics, and the belief that science will eventually reveal the "truth" about the universe"(Frank, 1961:16).

Frank and his associates, soon to be know as the Vienna Circle, found through Mach a basis in Kant and Locke for their new paradigm, although they believed that Kant was wrong about the immutability of the organizing patterns of the mind (Frank, 1961; Hanfling,1981). Frank explains that non-Euclidian geometry proved this to be true. Much of their perspective however had been summarized earlier by Hume (Hanfling, 1981).

Kant believed that mind can describe natural phenomena by using forms of thinking not provided by physical objects . These forms (patterns of experience) "are provided by the human mind and not by the physical facts, they cannot be changed by the advance of scientific investigations" (Frank, 1961:19). Frank believes this true to an extent, but recognizes that it implies the limits of knowledge are Euclidean geometry and Newton's laws (which Kant maintained are forms of organization intrinsic to the human mind) and lead back to medieval metaphysics. Frank reasoned therefore that the "forms of experience" were changeable.

Poincare' provided the Vienna Circle with a way to reconcile description with postulates, and yet avoid the need for traditional explanation. Poincare' states that objective experience and mind are functions of each other. Together they constitute a system of experience. The symbols of a

logical system such as mathematics are arbitrary in that the "symbols have no meaning in the physical world"(Frank, 1961:24). He recognized however that, "we construct only those systems that can be interpreted in terms of physical facts and that are therefore helpful for the formulation of natural laws" (Frank, 1961:25). The axiomatic system is a product of our free imagination, but it is well chosen when identified with observational conceptions which make it an economical description of observational facts (Kraft, 1969). The symbols can from this perspective be linked to observable facts-through measurement by simple physical methods. Structural systems may be logically arbitrary, but not psychologically arbitrary as people do not usually construct systems that do not relate to the objective world (Frank, 1961).

Drawing on Poincare', the Vienna Circle decided that the 18th century concepts of mass and force "can be interpreted as statements about sense observations"(Frank, 1961:29), they are not about a world behind appearances (ontological realities or metaphysical entities, but are "auxiliary concepts"- economical statements of observation).

The Vienna Circle wished to combine the the work of Mach and Poincare'(Frank, 1961). Kraft explains that this is a radical shift for empiricism. The realm of logic and mathematics becomes dependent on its validity or truth value only in terms of internal coherence. The realm of

observational experience depends on verification by correlation with experienced phenomena. Duhem was a great influence on the Vienna Circle on this point because he also proposed that experimental verification is the culmination of a theory and not its basis:

> A theory of physics is not an explanation; it is a system of mathematical propositions deduced from a small number of principles the aim of which is to represent as simply, as completely, and as exactly as possible, a group of experimental laws (Duhem in Frank, 1961:26).

A body of experiments only confirms the theory. For Duhem an explanation (rather than a description) beyond economical description was an excursion into metaphysics. In this he agreed with Mach. The structural logical system of a theory said nothing about the world of observable physical facts directly in the conventional sense. All that was required was a method of bridging the two. Frank and the others concluded that the laws of science were "arbitrary conventions about how to use some words or expressions" and this allowed Frank to use the concept of causality again but without the implications that it was an aspect of objective phenomena (Frank, 1961:21). Causality becomes an arbitrary logical convention in terms of an axiomatic system and if interpreted physically it becomes a statement of observation and not real in any absolute or physical sense. It is employed because it is an economic description in the sense that Mach had prescribed. A critical aspect of Logical Positivism at this point is to determine the precise way of bridging the gap

between the formal system and physical reality. How should correspondence be established?

In sum, the discourse between these thinkers resulted in the presentation of the view that conventional concepts are to be discarded for a formal system of specially devised concepts, involving logical and primitive terms, whose meanings are derived from that system and are not dependent on physical phenomena for their meaning. The truth value of the formal system would then be established through measurements of sense experience confirming a correspondence. Truth was then not to be derived from sense experience, only confirmed by it. The Logical Positivists recognized two types of meaningful statements which were either verifiable by observation (synthetic) or by analysis of its truth or falseness (analytic) (Kraft, 1969).

Correspondence

Frank considers Einstein an excellent example of logical positivism. He developed his theory from very general abstract principles-it was a neatly developed structural system, the system must be interpreted and predictions of observable facts made: then verification through observation. Newton's theory could be verified with yardsticks and clocks, but Einstein's requires special procedures of interpretation: "The methods of measurement must be developed along with the conclusions from the principles of the theory" (Frank,

1961:30);- it becomes part of the theory. "These descriptions of the operations by which abstract symbols,. . . are connected with observational facts are called today "operational definitions," according to a terminology suggested by P.W. Bridgman (Frank, 1961:31). This constitutes a greater gap between the structural system and experimental confirmation than ever before in science. An economic description of facts that now requires considerable operational definitions (very high level of abstraction related to its generality; also removes it from primary observation).

Schlick's position was fundamental to the positivist perspective and he stated that a cognition is true if the correspondence established between the system of symbols and the facts of the world is unique (Frank, 1961; Hanfling, 1981). Traditional philosophy said truth was behind appearances and discoverable by reason. Schlick emphasized that truth is the establishment of correspondence itself, but since many worlds could fit the same system (underdetermination of theory), the correspondence must be unique although only in the direction from theory to verification.

Reichenbach's perspective was sympathetic to this approach and saw geometry as a good example of such a system as Schlick advocated (Frank, 1961). It had axioms and a description of measurement- a system of axioms of

coordination (recall Hobbes). With this type of system every theory with nonobservational concepts must contain relations (implicit or explicit rules for) between these abstract concepts and observational concepts. The prescription for experimental verification was contained within the system of postulates. Together Reichenbach and Schlick formulated a verification theory which is based on Wittgenstein's interpretation of meaning. This theory defines the meaning of a proposition as derived from its method of verification (Hanfling, 1981). The focus of these contributions is consistently away from dependency in any way on sense data for anything other than verification.

Schlick believed Wittgenstein could provide logical positivism with a sound symbolic logic system drawn from ideas of Russell and Whitehead's ideas concerning logic (Hanfling, 1981). Wittgenstein claimed problems of traditional philosophy were verbal problems. Ordinary language was not set up to deal with philosophical problems. (Frank claims James and Peirce also had the same conception of meaning as Schlick, Reinbach, Mach) As we have seen the Logical Positivists were already working in this general area. Carnap also joined the group to help with this effort (Kraft, 1969).

Frege had defined meaning as reference (the same starting point as Russell and Wittgenstein) and truth value lay within the accuracy of reference (Flew, 1991). This

contributed to the idea of unique correspondence. Unfortunately this disqualified many mathematical statements (propositions) and indicated that they were neither true nor false, but meaningless. Wittgenstein avoided this problem as he did not base his perspective on a true-false dichotomy (Grayling, 1988). He was still interested in the limitations of factual discourse and made the distinction that some categories of statements were either true or false and others were neither and therefore meaningless (this still equates meaning with true/false dichotomy) (Ayer, 1985). Wittgenstein was interested in flagging the boundary between these two types of propositions as they were the source of major confusion (Anderson, 1986). In this manner he could indicate the domain of science and conveniently define other domains as meaningless. Therefore science is the only meaningful discourse.

Carnap, in light of Wittgenstein's pronouncements, offered an example of Schlick and Reichenbach's "true cognition" with a system of symbols that indicated the world of facts uniquely in The Logical Structure of The World (Frank, 1961). It was also the final integration of Mach and Poincare' (Kraft, 1969). Carnap was interested in a criteria that would determine the verity of a statement by analysis of its symbols' form and the syntactic rules it employed (Kraft, 1969). He also wanted to develop formal protocol statements into a meta-language that was internally consistent. Along

with Neurath he developed the "coherence theory" of truth where-in the truth of a statement is a matter of its coherence with other statements (Hanfling, 1981). Schlick felt that this went too far in disregarding the importance of verification through observation. Wittgenstein also felt that Carnap's program was impossible because any meta-language would have to have its own meta-language and so on ad infinitum (Kraft, 1969). Carnap answered this by saying his formal language was a sub-language described by general language itself (Kraft, 1969). Meaning was dependent on logical verification and truth on empirical verification. Meaningless statement are not verifiable in either case.

Carnap declared metaphysics was meaningless because it could not meet the requirements of his system. Metaphysics has apparent logical relations, but synthetic elements which are unverifiable, even in any potential circumstance. It was not even verifiable in principle.

The Vienna Circle felt through Carnap's work that:

> They had demonstrated logically that no scientific metaphysics is possible because metaphysical statements do not fit into the pattern that statements must have in order to be called true or false (Frank, 1961:44).

This was a further refinement of Locke's effort to separate metaphysics and science and was in harmony with Wittgenstein's refinement of the correspondence program he and Frege were developing.

> Instead of building up the system of human knowledge upon concepts like "red spot" or "feeling of warmth,"

> one should use elementary symbols expressing concepts like "rock" or "table," and define "redness," or "warmth" as derived concepts" (Frank, 1961:45).

(This is division of primary and secondary qualities on a higher level of abstraction) Carnap had attempted to work out the new language of exact correspondence, but innumerable problems plagued his efforts. The consequence of his system was that math and logic were meaningless. He also found that many important propositions become invalid under this criteria. Worse yet, two synthetic sentences could combine to become meaningless.

Frank is careful to emphasize that there are no primary philosophical propositions, philosophic activity is to clarify statements of the special sciences- it is auxiliary. The new physics had forced the positivists back into philosophical speculation concerning the unfinished business over assumptions, but they kept it at arms length. It was to remain in its early projected role of edifying scientific discovery.

The Position of Logical Positivism: A Summary

Much of the discussion Frank and others offer concerning the development of logical positivism demonstrates the eclectic and patchwork nature of its evolution. It draws support from many diverse authors. Although many of the important problems of classical physics are finally addressed, many of the solutions still present major

problems. Much of the discussion is confusing because many important issues are glossed over. Often more questions are raised than answered. The received position of Logical Positivism will consequently be summarized to clarify the approximate implications of all these contributors and their complex arguments. Although the implications of Einstein's theories eroded the concepts of absolute time and space and called into question causality, the logical positivists continued to pursue a program to support causality. They continued to define the world in terms of subject and object. They continued to find a means to justify experimental designs through a new form of operationalism. In spite of their repeated glaring failures and the lack of experimental support for their assumptions they continue to this day in the form of social scientists those who are not aware of their failure such as Zetterberg, Lazarsfeld, and Stinchcombe.

The Logical Positivists focused on the procedures of justification or the "context of justification" as Reichenbach (1966) refers to it. It is the measurable realm of primary qualities which Locke and the traditional positivists had focused on. Although they acknowledged the importance of the context of discovery (where Einstein did his thought experiments), they regarded it as a domain which could not be analyzed and so they would not analyze it (Bechtel, 1988). The realm of secondary qualities is ignored

once again. There appear to be three critical features to their perspective (Bechtel, 1988): The verifiable theory of meaning, the deductive-nomological method of explanation(which is related to the hypothetico-deductive model of justification), and the axiomatic view of theories.

The Verifiability Theory of Meaning

As we have seen the Logical Positivists felt that the conventional meanings of words were imprecise and lead to meaningless questions. They discarded the classical notion that ideas were causal products of experience and replaced it with the notion that sentences and words were vehicles of meaning. This meaning was dependent on the set of conditions that demonstrated its truth and thus verified its meaning. Although many sentences could be verified by direct observational experience, it was necessary to formulate rules for verifying more abstract sentences with these "observational sentences" and this was to be done with symbolic logic and often in the form of "if, then" statements. This results in analytic statements not dependent on experience for verification. Unfortunately it was quickly discovered not possible to translate all concepts into these terms and despite valiant attempts by Carnap the problem was never fully resolved.

The Deductive Nomological Model of Explanation

Statements of scientific laws and antecedently known empirical facts are deductively employed to arrive at a description of an event which serves as an explanation for it. There is a symmetry between explanation and prediction such that their identity is determined solely by whether they are stated before or after the event. These deterministic explanations provide a specific consequence every time initial conditions satisfy the law statements. Hempel proposed that this procedure could be applied to statistical relationships to allow for inductive-statistical explanations (this only works with events whose probability of occurrence exceeds .50) (Bechtel, 1988).

The procedure for deriving scientific laws was termed the "hypothetico-deductive method." In this instance a hypothesis would be proposed and operationalized in the "context of justification." If the predicted outcome was verified, then the hypothesis was determined to be confirmed providing further support for a general theory or as Willer argues establishing the hypothesis as a specific theory. One of the difficulties here is that the definition of scientific law is hard to establish in formal terms. "If, then" statements often cannot contain enough information to provide a satisfactory law. Another difficulty is Hume's assertion that an inductively derived statement has no guarantee of continued support in the physical universe. The "Raven

Paradox" suggests that "if, then" statements are tautological in that they often only seek to examine what is proposed.

The Axiomatic Account of Theories

A theory is a "structured network of statements from which one could derive specific laws" (Bechtel, 1988:28). The model for theories as can be seen from Franks arguments is Geometry (recall Hobbes). Laws would be axioms derived from assumptions and postulates. All good theories could be axiomized and this would lead to larger general theories in science

The Alternative of Karl Popper

Karl Popper was at first considered sympathetic by the Logical Positivists, but as he observed their line of development he became aware that the difficulties their program embroiled them in were insurmountable (Hanfling, 1981). Science was supposedly inductive, involving empirical observations made through experimentation which are logically analyzed. Unfortunately it was clear to those who carefully considered the matter that no extension of observations of empirical phenomena can result (is logically sufficient to establish) in an absolute induction (an unfortunate by-product of Hume's efforts) (Flew, 1991; Lincoln & Guba, 1985). Science, ironically, had to survive by faith in the uniformity of nature.

Popper advocated the acceptance of this paradox and stated that all generalizations were tentative and subject to falsification and that theory was always subject to revision (Kraft, 1969; Bechtel, 1988). Looking on the brighter side he rationalized science as, therefore, a democratic approach to knowledge in which theories are constantly subject to test and question (Bechtel, 1988). Science can only be a loose body of theories. He proposed his famous hypothetico-deductive approach as the solution (Anderson et al, 1986).

This solution was more effective than the Vienna Circle's. They attempted to add the mathematical developments of 20th century (especially Russell and Wittgenstein) to systemize empiricism in support of Hume's perspective. Mathematics and logic are considered meaningful though tautological (Flew, 1991). They explicate the meanings of things, but they do not say anything about how things in the world are specifically. Frege and Russell failed to show that mathematical truth was a part of logic and therefore as certain as our knowledge of logical truth (Bechtel, 1988). Arguments continue as to whether mathematical objects exist independently of thought and so mathematical truth is intuitively known (self-evident) as Kant proposed (Anderson et al, 1986). In summation of this condition Heisenberg explains:

> Yet, having said this, we probably understand now, better than before, that there exists apart from the phenomena of life, still other aspects of reality, i.e.

> consciousness and, finally, mental processes. We cannot expect that there should be a direct link between our understanding of the movement of bodies in time and space, and of the processes of the mind, since we have learnt from science that our mental approach to reality takes place, at first, on separate levels which link up, so to speak, only behind the phenomena in an abstract space (Heisenberg, 1971:93).

It is instructive to note that this analysis is all within the confines of a belief system and its related cognitive sensory set that makes a strong distinction between knower and known, between subject and object, and between mind and matter. It is an attempt to redefine the boundaries that had initially set science off as a consistent and reliable program of knowledge apart from the relative world of secondary qualities and the metaphysics that dominated it. In fact metaphysics has become a part of modern physics once again. Hawking (1992) describes the distinction between quarks as metaphysical in nature. The Vienna Circle had hoped to maintain some of the traditional cartesian boundaries and build new connections, bridges, between the two realms.

(It overlooks Mead's distinction that mind and matter coemerge in an ongoing dialectic as social reality once mind itself emerges from the physical universe (the world that is there and pushes back). Social objects (which are always the limits of human knowledge) are as much mind as matter.)

Strangely enough the work of Wittgenstein later in his life and the work of Quine begin to touch upon this primary

secondary problem, but continue to overlook it. The cognitivists such as Simon, Hanson, Lakatos and Laudan, however, are acutely aware of the problem (Bechtel, 1988). They perhaps to fail to utilize it to their advantage because of their apparent lack of knowledge concerning physics.

Later Wittgenstein

The importance of the contributions of Wittgenstein and Quine are in terms of sociology of knowledge. These philosophers and the congnitivists wrestle with the epistemological and ontological ramifications of the issues that undermined the Logical Positivists in the final analysis (Bechtel, 1988). It is one of the theses of this work that this failure and the problems and solutions of Wittgenstein and Quine point to a more fundamental problem; the division of primary and secondary phenomena. Their struggle not only defines the failures of Logical Positivism, but points out the weakness of assuming this division and the related Cartesian dualism.

The main distinction between early and later Wittgenstein is that the distinction between sense and nonsense statements is no longer based on correspondence but on conventions of usage (Grayling, 1988). This approach becomes central to his Investigations:

> It is this that makes Wittgenstein's later work look as if it might be much closer to sociology than some other philosophies because it places major emphasis on the

> institutional and collective nature of language (Anderson et al, 1986:191).

Wittgenstein explains that generalization and explanation are not the same thing or "Indissolubly associated" (Anderson et al, 1986:181). All intellectual questions are not necessarily resolved by generalization and language is used in social interaction for other purposes (Grayling, 1988). Craving for generality leads to an attitude about science- that it is the only legitimate form of explanation (recall Carnap). "All that people think they want from a generalization can be obtained from the careful description of particular cases" (Anderson et al, 1986:181).

Wittgenstein feels that instinctive use of language is the correct employment of it (compare this with Frank), but when we reflect on our use of words we often lose our way because it is primarily a nonconsciouss or natural process (Grayling, 1988). We distort it through self conscious efforts. "It is the problems of philosophy that originate in confusions about language" (Anderson et al, 1986:183). They have little to do with matters of fact.

> When we are making "instinctive" use of the language we are right in our employment of it. Trouble begins when we start to reflect on language, to reflect on the use of words, for then we become self-conscious about something that is properly done in unself-conscious fashion (Anderson et al, 1981:182).

The mature thought of Wittgenstein saw correspondence as an erroneous basis for knowledge and an incorrect formulation for explaining the way language works (Grayling, 1988). The

basis for Logical Positivism is conceived as erroneous. This new perspective moves him close to Mead in that it places an emphasis on usage (interaction) as the basis for meaning. It also moves him closer to Quine's work which focuses on language as a whole as the source of meaning. Truth and meaning become a function social interaction rather than scientific investigation. This relativism mirrors the failure of absolutes in physics. Heisenberg (1971:25) notes:

> Thus contemporary science, today much more than at any previous time, has been forced by nature herself to pose again the old question of the possibility of comprehending reality by mental processes,. . . (Heisenberg, 1971:25).

The problem of correspondence is the re-emergence of the problem of dualism introduced by Descartes and by Galileo's primary secondary distinctions. This reflects the breakdown of subject-object dichotomies in Quantum mechanics as well. As the domain of science expanded, the boundaries of its infancy, primary characteristics, dissolved. Quantum mechanics operates in a realm of the abstract. The illegitimacy of secondary characteristics as the domain of science becomes questionable. The program of science is no longer truth and meaning becomes a function of human interaction.

Quine

Hard line empiricism expects all knowledge to be gained directly from experience; the overriding constraint on them

is to "eradicate the role of subjective interpretation in the acquisition of knowledge" (Anderson et al, 1986:143).

> For our knowledge to be independent of our theories we must be able to estimate the fit between the theory and the world, which is only possible if the world actually does have an independent existence and organization which we can come to know about (Anderson et al, 1986:143).

True description would capture that existence. He still champions science as the best system we have; a better language could make it more precise.

Is language interpretive? Sociologies of knowledge suggest that knowledge systems are culturally anchored and relative. Theories, categories, methods are a consequence of socio-historical events. This relativism challenges Quine to redefine empiricism and realism. Quine's empiricism focused on the logical structure of language. He begins with the assumption that there can be no philosophy "which is fundamental to science" (Anderson et al, 1986:146).

Quine was much influenced by Pragmatism and the idea that the value and truth of ideas finds their ultimate test in their utility in assisting us in our relations to the world (Bechtel, 1988). Economy of conceptualization is therefore of considerable importance to him and he pursues simplicity. Ontologies with fewest entities is the best (Bechtel, 1988). Quine is a relativist- there is no single right answer to the question "what is there" (Anderson et al, 1986). Meanings are not things. There are only physical

phenomena and no mental phenomena.

Quine saw the task of traditional epistemology as futile and felt that psychology could better address it- but a behaviorist type psychology.

> The essential problem is to understand how, from very minimal input of stimulation that the human organism receives from the physical environment, it is possible for us to construct our very rich and complex theories of that world (Anderson et al, 1986:153).

Evidence for theories must come through the senses, but: "Physical objects are conceptually imported into the situation as convenient intermediaries" (Quine, 1953:44). He denies that certainty can be sought from the two key dogmas of empiricism- there is no distinction between questions of fact and questions of meaning (Quine, 1953). In contrast to Frege, Quine believes that meaning resides in whole languages and not in sentences i.e.in its entire ontology (Quine, 1953). Some beliefs require few changes in the web of meaning, and some require re-spinning the whole web (such as 2+2 doesn't equal 4)(Anderson et al, 1956). The difference between analytic and synthetic is one of degree only (recall primary secondary qualities) and "it is becomes folly to seek a boundary between synthetic statements, which hold contingently on experience, and analytic statements, which hold come what may" (Quine, 1953:43). There are no statements immune to revision (how about objects?). We tend to adhere to ontologies rather than facts. The difference between the analytic and the synthetic is often the degree to

which we are willing to revise our language and the range of that revision (Quine, 1953). The analytic is often more difficult to alter. We can accommodate the facts by manipulations of our theory. Translating ontologies is nearly impossible (and argues against the progression of knowledge in science and favors Kuhn's theory of revolutions) (Anderson et al, 1986). We often can't distinguish the true in fact from the true in meaning.

According to Quine theories are underdetermined- the facts may fit many theories. We must however judge the adequacies of other ontologies from our own which is invariably scientific. Although Quine was admirable in his effort to have science face its inadequacies, he did not encourage us to inspect and question them from an ontological perspective. Whether he felt that we could not is another question beyond our present scope. The cognitivists clearly disagree with Quine (if only implicitly) by the very nature of their research which like Quantum mechanics points the objective empirical finger back at us. Science altered the cognitive bias of our Euro-American culture once with Newton and there is no reason to doubt that we can do it again in a manner congruent with our scientific investigations. Science is trying to transform itself. (Quine believed social science were unreal because they are unverifiable (Bechtel, 1988).

Heisenberg's insight is an interesting commentary on the work of Wittgenstein and Quine:

> On the one hand the experimental questions which we ask of nature are always formulated with the help of the plain concepts of time and space. For indeed we possess only a form of speech adapted to the objects of our daily environment and capable of describing for instance the structure of some apparatus of measurement. Our experiences, too, can only be made in time and space. On the other hand, the mathematical expressions suitable for the representation of experimental reality are wave functions in multi-dimensional configuration spaces which allow of no easily comprehensible interpretation. (Heisenberg, 1971:15).

Summary: Old Dogs and New Physics

> In the 1920's the science of physics, assaulted on all sides by phenomena it could not explain, was in a state of disarray. The models of the atom that had been devised by the best physicists of the time failed to provide an adequate accounting for the simultaneous wave-particle nature of radiation, including visible light. . . .At the international Soloway Conference held in 1927, Heisenburg provided proof of the proposition that it is impossible to determine both the mass and the momentum of a particle other immediately and forever indeterminate (Lincoln & Guba, 1985:97).

This flew in the face of logic and counter to many of the fundamental assumptions of the methodologies employed in positivism. The Logical Positivists attempted to adjust for these findings but confronted innumerable barriers. Diesing observes:

> By the mid-1950's, all the original distinctions of the Vienna Circle had become unclear, and all the original certainties had collapsed. The distinctions between logical and empirical, analytic and synthetic, theory terms and observation terms, meaningful and meaningless, even science and metaphysics, had become differences of degree, circumstance, interpretation (Diesing, 1991:19).

Eventually Carnap died and so did the logical positivist program (Diesing, 1991). Popper abandoned most of this

program and presented a new version of cartesian doubt which is also popular today (Diesing, 1991). It is instructive to note that classical physics remains alive and well-employed to this day. The ramifications of Einstein's and Heisenburg's works have never been fully acknowledged by the scientific community in general (Lincoln & Guba, 1985). Palmer (1964) remarks that it took only fifty years for the public to absorb the Newtonian perspective, yet the inconsistencies in positivism remain begrudgingly acknowledged on a limited scale and unresolved (Lincoln & Guba, 1985).

> The thing that is remarkable about scientific theories is that the inconsistencies are walled off and do not appear to affect the theory other than very locally. . .(Wimsatt in Lincoln & Guba, 1985:17).

Lincoln and Guba (1985), as well as others, have codified many of the weaknesses of positivism exposed above: The problems with axiomatic arguments, the arbitrary division between observer and observed, the weak assumptions behind cause and effect, the limited validity of generalization. They have reviewed in detail the underdetermination of theory and the unavoidable theory-ladeness of facts. There may be some truth in the fact that, as Kant asserted, that positivism reflects innate categorical biases and that these biases stubbornly resist efforts to transcend them. Physicists, however, have developed mathematical systems to investigate events occurring beyond the realm of these

categorical limits. Heisenburg acknowledges that these systems must be imported back into social dimensions to be meaningful. Frank was correct in this sense to attempt to move theory beyond meaning in the conventional sense.

In addition to the inherent weaknesses in Logical Positivism Godel's Theorem demonstrates the fruitlessness of trying to reduce any mathematical analysis to one consistent set of propositions (Flew, 1991). Bell's Theorem rules out the assumption that spatially separated events can be considered independent (Lincoln & Guba, 1985). Werner Heisenberg's Uncertainty Principle struck at the heart of subject-object dualism as well as causality (Heisenburg, 1971).

The method of science has resulted in conclusions that call into questions many aspects of those same methods and the perspective that claims to justify them. What kind of rational method denies validity of its own findings? Is this much different than the problem Galileo met with? When rational men repudiate and ignore their own evidence it seems likely that strong cultural beliefs supporting powerful political agendas are at work. In considering the pedigree of positivism, it should be obvious that it is a perspective with considerable investment behind it. To abandon it might imply cultural revolution. Scientific revolutions as slow as they may be in coming may have to await cultural shifts in belief systems (Kuhn, 1962).

After several centuries of working its way into our language and culture, many investigators believe positivism has come to dominate the way we see our world both physically and conceptually (Focault,1970; Leyotard, 1991; Lincoln & Guba, 1985; Alexander, 1982; Phillips, 1973). Cartesian dualism lies at the heart of our Euro-American cultural perspective (Damasio, 1994; Flew, 1991; Mead, 1982). It informs our symbols, our dialogue, and our acts (Leyotard, 1991; Alexander, 1982). It generates a certain style of meaning (Mills, 1959; Burke, 1965). Connected with the way we process information is the fact that it is the perspective of authority (Leyotard, 1991). It validates and legitimates forms and channels of information (Bendix, 1970). It is the source of our military technological power. To abandon it threatens not only our self, but perhaps our security as a nation and a society. How are we to extricate ourselves and confront what our own methods are telling us about reality? Gergen (1991) suggests that our technology may not provide us with a choice and is in fact forcing us to alter our fundamental cultural definition of self.

One aspect of our problem is often overlooked, however. As Gergen and others have noted, Newtonian physics, though invalid at the quantum level of analysis, was used to calculate trajectories for the moon shots. This is no small statement. As a society we entrusted human life to these equations, even though their validity is in fundamental

question. Although we could have used Einstein's equations to perform the more precise calculations, the specialists explain that it would be like using a sledge hammer to kill a fly (Gergen, 1991). At our everyday level of physical operations Newtonian physics are sufficiently accurate and simple to use for most tasks, and Einstein (1954:231) comments that the resulting differences in calculations are almost too minuscule to acknowledge in many instances:

> The new theory of gravitation diverges considerably, as regards principles, from Newton's theory. But its practical results agree so nearly with those of Newton's theory that it is difficult to find criteria for distinguishing them which are accessible to experience (Einstein, 1954).

Efficiency inspires the retention of Newtonian mechanics in the engineers repertoire. We should not throw the baby out with the bathwater. Positivism has a place, but not in the driver's seat.

The next chapter will seek to explore in more detail these assertions that positivism is a cultural bias linked to political and economic concerns for power.

CHAPTER 3

POSITIVISM, TECHNOLOGY, AND CULTURAL DOMIANTION

Basic assumptions or metaphysical beliefs underlie our system of ideas and they, "represent the ultimate benchmarks against which everything else is tested. . ." (Lincoln & Guba, 1985:15). Over the centuries the arguments of the great philosophers forged these assumptions and they found consensus in a perspective grounded in the scientific community. Locke's arguments for empiricism and a "public knowledge", Descartes' mathematics, Newton's physics, Comte's positivist perspective, and Mill's logic laid the foundations for a methodology which reflected these assumptions. The assumptions and the methodology became known as the positivist paradigm and more popularly as science.

Part of legitimacy of this perspective was perhaps derived from its ability to supply a technology and a rationale for that technology through the associated philosophies of realism (Palmer, 1964). Those economic or political entities that employed this technology and the associated mathematics (especially Newton's calculus for deployment of accurate mortar and cannon) gained the upperhand in trade and war, and at the same time they found a

justification for exploitation of resources in the rational behind the technology (Bendix, 1956; Braverman, 1974; Marx,1988; Marglin, 1982; Thompson, 1982; Weber, 1965).

As trade grew in Europe during the 14th and 15th centuries, the European community competed voraciously to expand their trading partners and colonial acquisitions (Boorstin, 1983). Navigational charts and all information regarding coastlines and ship routes were frequently considered a state secret and divulging it was treason (Boorstin, 1983). Explorers like daGamma, Balboa, and Columbus traveled between kingdoms seeking support for their ventures and their services frequently went to the highest bidders. Central to their efforts was navigation. Navigation relied upon a coordinate system of latitude and longitude developed by the Greek cartographer Ptolemy and had been revived in the 14th century (Hale, 1971). The primary difficulty for European navigators in utilizing this system was a lack of mathematical expertise, good navigational equipment, and a clock that would operate on board ships (Hale, 1971; Brown, 1996). Without the clock, it was difficult to compute with any accuracy longitudinal positions. On incident almost cost the English their entire fleet in 14 and inspired a national competition to devise a ship worthy clock (Boorstin, 1983).

The Dutch on the other hand had taken to utilizing mathematicians as teachers aboard all their ships in an

effort to improve navigation (Boorstin, 1983). Frequently as not, it was as navigators that many mathematicians made their livelihood. Mathematics became increasingly associated with expanding trade routes and consequently with state concerns for security and power (Hale,1971). Navigation was also critical to successful naval campaigns by the reigning powers (Boorstin, 1983). As the discipline of mathematics gained status and importance in circles of authority so apparently did the growing legal concept of evidence.

The idea of objective observation became increasingly popular and the Italian Machiavelli, enamored with the idea, attempted to observe the behavior of successful rulers in his writing of The Prince (Palmer, 1964). In his effort to be scientific, objective, and non-moral he was perceived to be immoral (Treasure, 1985). The idea of natural law and natural rights began to absorb Europe (Brown, 1996). Locke was highly influential and his "Essay Concerning the Human Understanding" 1690 encouraged the idea that certain knowledge or truth was derived from experience and not innate ideas as Descartes had argued (Yolton, 1985). He wrote other political treatises that were highly influential as well. Natural law came to be used to justify diverse political situations. Hobbes was highly influential in this arena with his work Leviathan in 1651 (Maland, 1983). Brown (1996) explains that the Enlightenment in general appealed to natural law rather than divine law as the guide to human

behavior.

English law began to emphasize evidence near the end of the 17th century as judges lost their free discretion and rules of evidence became equally applied to all cases. As early as 1650 hearsay evidence was no longer allowed in courts and confessions under torture disqualified. Observation and natural law became a part of legal systems (Treasure, 1985). Too a growing educated middle class the ideas of social contract, evidence, and natural law seemed to harmonize with their interests.

With respect to evidence, math, and military power Palmer maintains:

> The fact that knowledge could be used for practical purposes became a sign or proof that it was true knowledge. For example, the fact that men could aim their cannon and hit their targets more accurately in the seventeenth century became proof of the theory of ballistics which had been scientifically worked out (Palmer, 1964:264).

Palmer adds that calculus "which allowed an exact treatment of curves and trajectories, reinforced by technical discoveries in the working of metals, led to an increased use of artillery" (Palmer, 1964:270). He comments that armies in 1750 used twice as many cannon per soldier as armies in 1650. This naval and military technology gave Europeans a considerable advantage over other peoples of the world (Treasure, 1985; Palmer, 1964).

It is important to note however that science was also a

popular notion and it did not in fact contribute that much to technology. Maland notes:

> Technological progress continued to depend upon the development of empirical methods by practical men, and science probably gained more from technology than she gave in return, at least before 1750 (Maland, 1983:11).

This is significant because technology is so often associated with scientific theory, yet in reality it usually precedes theory in its early development and may even do so today. It is sobering to consider that some of the major inventions of the 20th century were developed through trial and error efforts of men of little education such as Edison and Marconi.

In the 15th century it was the Church that inspired Spanish explorers to spread the gospel and bring wealth to their kings (Treasure, 1985). Later in the 19th century Social Darwinist philosophy, as well as scientific realism, which grew out of this perspective, provided a further justification for the existence and implementation of this imperialistic agenda in spite of the best efforts of the church to halt its encroachment on its own authority (Palmer, 1964). At the same time the Church, captive to its own ambivalence, enjoyed its own form of religious hegemony while riding on the same wave of technological superiority as the countries it thrived in colonized the planet (Palmer, 1964).

Social Darwinism is one of the most powerful perspectives in the 19th and 20th century Euro-American

industrial cultures. Survival of the fittest was seen as grounded in the scientific theory of evolution and it justified employing the technical might of Europe in subjugating other people in its pursuit of power and resources (Palmer,1964). "Faith in 'modern civilization' had become a kind of substitute religion. Imperialism was its crusade" (Palmer, 1964:622). It spawned the Eugenics movement in England and America and culminated in the Nazi rational for the final solution (Palmer, 1964; Hughes, 1961). It was the basic rational for the exploitation of English and American entrepreneurialists (Bendix, 1956). It is instructive to note that Carnegie referred to Spencer as "dear master" in his letters to him (Ritzer, 1992). The positivist ideology under the rubric of science became a fundamental cultural perspective which persecuted those who did not adopt a scientific perspective (Leyotard, 1991). The subject-object division implicit in this perspective became the cognitive basis for perception and consequently all investigation.

It seems clear from the study of history that this positivism gained pre-eminence because of its association with power and authority (Bendix, 1956; Braverman, 1974; Marcuse, 1971). Its strongest proponents were in the industrial revolution's fastest growing centers, especially England and Germany (Palmer, 1964). Its legitimacy appears to have been grounded in this industrial power as much as was

the Church's in the political power of Rome and Constantine's decrees.

Royal Societies were attended by the landed gentry as well as funded by governments in order to support the growth of this precious resource (Treasure, 1985; Maland, 1983). The association between science and power and authority was a continuous evolution that brought them ever closer together in the public mind (Boorstin, 1983). Eventually, science won greater legitimacy with regard to power and authority than the Church itself. The ideas of science were themselves invested with the power and authority of Pope and King. The tenants of positivism and their implications by association became the hallmarks of learning, power, and authority. Justification for political action became couched in scientific terms of evidence. Its most perverted manifestation took the form of scientifically justified genocide performed by doctors in Nazi Germany (Hughes, 1961). Emotion, sentiment, and morality had little authority within this arena (Comte, 1988).

Comte and the Politics of Positivism

Comte and Mill were two of the most outspoken advocates of Positivism as a philosophy and a social perspective (Swingewood, 1984). They were highly influential and their writings well know. Swingewood (1984) explains that Comte built his positivist perspective on the failure of the

empiricists of the Enlightenment to develop a conception of society that transcended the idea of aggregate of atomistic individuals. He sees Saint-Simon as crucial to this process with his concepts concerning civil industrial society and class hierarchies. He also finds the work of deBonald and deMaistre with their emphasis on society as an organic harmonious whole as an important contribution. Of Comte he notes:

> J.S. MIll, who corresponded with Comte, argued that his influence in the development of social science was greater than his actual achievements and that while not creating sociology as a science Comte's work nevertheless made it possible (Swingewood, 1984:41).

Comte's attempt to explain social phenomena through empirical positivist perspective and the rejection of metaphysics places him at the beginning of the story of positivism in sociology and the founder of its positivist movement. He was enthusiastic over Condorcet's ideas of social evolution as a consequence of natural law and saw positivism as a philosophy that rejected the Enlightenment but embraced science as the solution to mans evils.

> All competent thinkers agree with Bacon that there can be no real knowledge except that which rests upon observed facts (Comte, 1988:4).

Unlike the empiricists such as Locke, he did not believe sense data alone was sufficient for building knowledge:

> For if, on the one hand, every positive theory must necessarily be founded upon observations, it is, on the other hand, no less true that, in order to observe, our mind has need of some theory or other (Comte, 1988:5).

In this statement Comte is moving beyond the empiricists in recognizing the theory ladenness of facts to some extent. In his repudiation of empiricism and the idea that knowledge cannot be based on collecting facts he also supports the idea that we must include "laws which connect all social phenomena"(Swingewood, 1984:46). He insists on the importance

> that all our knowledge must be founded upon observation, that we must proceed sometimes from facts to principles, at other times from principles to facts. . .(Comte, 1988:23).

The emphasis on this pattern will be restated by Durkeheim and all positivists up into present time. Yet no clear program will ever emerge. Comte was at the time was optimistic concerning his program and he believed natural static and dynamic laws of social development were shaping history. They needed to be uncovered and codified, but first investigators required a guiding framework:

> We have seen that the fundamental character of the positivist philosophy is to consider all phenomena as subject to invariable natural laws. The exact discovery of these laws and their reduction to the least possible number constitute the goal of all our efforts;(Comte, 1988:8).

He cites Newton as a good example of such an effort. Comte felt that mankind was entering a new phase of history where all knowledge would be unified as sciences under the positive philosophy. He selects the era of Bacon, Descartes, and Galileo as the period when positivism (mental revolution) began: "It was then that the spirit of the positive

philosophy began to assert itself in the world" (Comte, 1988:11). Inevitably it would lead to a new order that was based in logical and scientific rationality:

> We may look upon the positive philosophy as constituting the only solid basis of the social reorganization that must terminate the crisis in which the most civilized nations have found themselves for so long (Comte, 1988:28).

Comte's vision, like so many other sociologist's to follow, was one of a social science elegantly ordered by mathematics.

> It is also evident that, in thus placing mathematical science at the head of positive philosophy, we are making only a further application of the same principles of classification (Comte, 1988:66).

Mill was highly influenced by Comte and agreed with his

> basic sociological principle, the theory of stages, the distinction between dynamics and statics, the historical method of analysis, and the concept of consensus (Swingewood, 1984:51).

He also agreed there was no fundamental difference between methods in the social and physical sciences.

> Mill argued that social science consisted of the empirical laws of sociology, demonstrated in statistical studies and surveys, the laws of psychology, derived less from empirical studies than philosophical reflection and finally, linking the sociology and the psychology, the laws of ethology, the fundamental laws governing human nature:(Swingewood, 1984:52).

Spencer, on the other hand attempted to distance himself from Comte's positivism (Swingewood, 1984).

The idea of objective rules of procedure and scientific efficiency accompanied and promoted the development of bureaucracy (Weber, 1960). Rationalism and the scientific perspective both derive from the enthronement of reason over

emotion in the Enlightenment. Europe was recovering from plague, centuries of war, and witchcraft trials. Rationalism and science seemed to offer salvation from the excesses that preceded this period (Palmer, 1964). At the same time growth of trade and industry encouraged specialization and technical skills (Weber, 1960). Governments became large and unwieldy. Weber (1960) describes the evolution of bureaucracy under these conditions and its progressive association with power and authority. As industry developed in the nineteenth century into concerns like Standard Oil exploiting mass economies of scale and the huge rail systems that serviced them, managerial bureaucracy grew also (Chandler, 1992). Science invaded business management in the form of Taylorism (Taylor, 1967) and bureaucracy became the primary form of industrial management in the twentieth century (Bendix, 1956). Science and academia were absorbed into the bureaucratic organization of society. The perspective of positivism and its peculiar brand of rationalism not only pervaded academic thought but also the organization of everyday life in society at large (Marcuse, 1971).

Education

Brown (1996) tells us that the growth of protestantism resulted in a strong anti-clerical movements among the laity. The laity was encourage to learn to read and think for themselves. A wide variety of religious parties and sects

developed especially in England and some became associated with political radicalism. Groups like the Quakers developed their own sophisticated metaphysics. Lay philosophers emerged who were not clergymen and not trained at the universities.

> The training of clergy (as well as doctors and lawyers) was a major part of the business of universities throughout this period and, for some of them, through much of the nineteenth century as well (Brown, 1996:5).

The idea of academic freedom was unknown in the 17th century. Places like Oxford and Cambridge were not usually associated with free thinking and students were required to subscribe to the Thirty Nine Articles, the doctrines of the Anglican church.

Brown (1996) further reports that private houses, clubs, and salons or even coffee-houses and taverns were the place for the exchange of free ideas. Universities were both theologically correct and intellectually conservative places. The French led by Descartes also began to publish works of philosophy in the vernacular. French became the vehicle of philosophy in Europe. These works were subject to censorship laws however, but in England these laws lapsed in 1695 and were never renewed (Brown, 1996). Still philosophy became part of everyday life and the great philosophers of the period were widely read and discussed in native tongues.

As Weber (1965) makes clear in his work on Protestantism, it provided a perspective for work and profit which directly influenced people but also lead to the need to

educate lawyers to support a stable legal system for the purposes of trade. The universities provided this service and not the service of the development of new ideas. In the future they will expand this service to include scientists.

Weber addressed the problem of the relation of science to academic perspective in his work Science as Vocation (Bendix, 1970). He saw it as the endeavor of an intellectual aristocracy and an attempt by western man to eliminate magic. "Scholarship has become part of increasingly large academic and research enterprises" (Bendix, 1970:90). Scholars are progressively influenced by other institutions they do not control and specialization has separated them from the means of their own production.

> Engineering, medicine, and other technical professions base themselves on scientific results, but are directly concerned with purposes extraneous to science. Politicians often resort to the testimony of scientists and professionals in the hope that political decisions can be based entirely on knowledge and thus be greatly facilitated (Bendix, 1970:91).

Inquiry is often driven by concerns extraneous to it.

Parelius and Parelius (1978) document the growing relationship between corporate concerns and education since the late 1800s. Progressively the concerns of educators have been primarily to meet the labor requirements of industry. The early close collusion, behind the official political veneer, of business and industry in the early formation of the American educational system in Massachusetts is clearly documented by Bowles and Gintis (1977). Bureaucratic

criteria for accountability and evidence drives its demands for outcome evaluations with respect to academic funding of studies. The outcomes criteria it utilizes is based on scientific perspective. As the following will show, business expects educational outcomes to be measurable in traditional scientific terms and expects courses to be designed to meet the needs of a scientifically oriented managerial bureaucracy. Political bureaucracy is organized along similar lines. Any endeavor not justifiable from this perspective is of questionable validity and definitely not worth funding. Utilitarian standards are used to evaluate academic work. A research projects value with regard to profit (rather than truth) becomes the final criteria for funding.

Research

Blalock (1982) has acknowledged the bias that funding organizations can insert into the consideration of research projects by social scientists:

> a strong emphasis on quantification may result in an overemphasis on one set of substantive problems at the expense of others; for instance, we may be financed to study those whose behaviors the sponsoring agent would like to control (Blalock, 1982:18).

In 1917 the United States Public Health Service was one of the first organizations to act as such a sponsoring agent. Their study comparing performance and fatigue in a Ford plant was carried out by Philip Sargent Florence, not technically a

sociologist and later a professor of commerce (Madge, 1962). Industrial psychology was growing in England at the time and Henderson and Mayo were beginning their experiments at Harvard. It is interesting to note that what drove these studies was practical interests in industry. The Hawthorne Studies (1927-1939) involving Elton Mayo must also be considered in this respect, as they centered around a General Electric Plant.

The research behind the renowned work An American Dilemma was sponsored by the Carnegie Corporation and was one of the first large scale fundings of research in sociology specifically (Madge, 1962). The work behind An American Soldier by Stouffer was funded by the Army, and although manned initially by Army personnel, employed civilian advisors. These advisors included Stouffer of Harvard, Likert from the Department of Agriculture, and McNemar, from Stamford. Stouffer managed to gain considerable autonomy for the researchers and his "Research Branch" grew quite large and influential (Madge, 1962). The social psychologists Dollard, Guttman, Cantril and others were brought into the project (Madge, 1962). They completed almost three hundred surveys over a five year period. The Carnegie Corporation again funded the Social Science Research Council to support Stouffer in codifying and publishing the material. Madge reports that

> the Research Branch was determined to quantify its results, and the decision was therefore taken to develop simple type questionnaire which could, as a general rule, be filled in by the soldiers themselves (Madge, 1962:296).

This had less to do with Stouffer than with the precedent which had its origins in pencil and paper IQ tests that had been used extensively in the Army since World War I (Bowles & Gintis, 1977). It was a method easily employed by the Army as a bureaucracy and it was familiar, non-threatening, and convincing. It was particularly bureaucratic in terms of efficiency because a small staff could carry out a large program (Madge, 1962).

Madge (1962) reports that they used Market researcher techniques of the 30's in their questionnaires. Kendall, Lazarsfeld and Merton used this data to further develop their own methodological perspectives. Guttman perfected his design of scales and began using Spearman-Thrustone type factor analysis while working for the Army (Madge, 1962). Hyman in his enthusiasm concluded that the

> explanatory survey follows the model of the laboratory experiment with the fundamental difference that it attempts to represent this design in a natural setting (Madge, 1962:301).

Lazarsfeld became a consultant in the last years of the war (Madge, 1962). Rather than Scalogram analysis he developed latent-structure analysis which bears a close relation to statistical theory of factor analysis. In 1950 he published Measurement and Prediction.

Some critics, The New Republic in particular, observed that science was being used "to sort out and control men for purposes not of their own willing" (Madge, 1962:320). Alfred M. Lee commented:

> If managerial problems for industry and the military are to continue to dominate the research of the leading social psychologists and sociologists, the value orientation of the managerial technician rather than the value orientation of the social science educator will dominate what evolves and is called social science (Madge, 1962:320).

What is overlooked here is that this not only influences research projects, but shapes methodologies which come to dominate academic institutions and as Mills, Philips and many others have commented it drives research selection.

Kinsey's research, he was a Zoologist was sponsored by Indiana University at first. It was then then sponsored by the National Research Council's Committee for Research on Problems of Sex and largely funded by the Rockefeller Foundation (Madge, 1962). He utilized a behaviorist perspective and a taxonomist approach as his previous major work was on gall wasps. This type of approach assumes the importance of a statistically well developed sample size and rigorous statistical method.

From the historical record there appears to be an obvious pattern of relationship between government and corporate interests and the major researchers and methodologists in the field of sociology. Other researchers have specifically focused on the relationship between

academic professionals and these sponsoring organizations.

Silva & Slaughter's (1984:40) analysis of academic experts and their associations finds

> Leading academics forge links with political economic elites able to deliver resources for the institutionalization of social science within the higher education system. We see academics as able to procure resources by using their expertise outside the university and demonstrating to possible clienteles the uses of social knowledge. The way social scientists served the powers-that-be included their work on the trust and labor problem, their work on municipal reform and imperialism, and their work with foundations. We argue that academics as experts linked the economy and the state, and in return for this mediation received resources, on the one hand, and responsible positions with some power, on the other hand (Silva & Slaughter, 1984:40).

Silva & Slaughter (1984) review the history of the ASA and other social science professional organizations and their relation to state and private funding institutions. In their investigation of foundations such as Carnegie and Rockefeller they noted that the expectation of foundation managers was rooted in their

> experiences organizing capitalist production and distribution" and they "assumed that idea-workers could predictably create their commodities in much the same way that other skilled crafts could (Silva & Slaughter, 1984:248).

The Social Science Research Council, as it turns out, was mandated to coordinate research expectations between funders and grantees. Lazarsfeld in <u>Mathematical Thinking in the Social Sciences</u> enthusiastically reports:

> The Social Science Research Council, with the help of the Ford Foundation, has inaugurated a series of

> training seminars, with the double purpose of giving mathematical instruction to social scientists and injecting social science materials into college courses in mathematics (Lazarsfeld, 1954:4).

Lazarsfeld, however, nowhere states their purpose. The implicit assumption is that math is good and that the foundation is rescuing an ailing science. From Silva & Slaughter (1984) we know that the Social Science Research Council was created to assure the foundations interest in getting the type and form of information it wanted. The foundation

> used its resources to organize and rationalize existing knowledge-production groups already approaching the issues of the day from the ideological position favorable to their shared elite interests (Silva & Slaughter, 1984:250).

The graduate students of the training seminars may as well have been navigators on board the Dutch ships being indoctrinated by state financed mathematicians.

Statisticians and economists of international repute early in the 1900's were called in to develop guidelines for form and content of projects to be developed for foundations such as the Carnegie and Sage foundations (Silva & Slaughter, 1984). The Rockefeller Foundation, especially famous for having its administrators closely allied with active industrial management, wanted a bureau to report on social problems relating to rail rates, labor disputes, tariffs, etc (Silva & Slaughter, 1984). Although Silva and Slaughter focus on ideological issues, it is clear from their report

that foundation officials were from disciplines that depended on quantitative reports and surveys for analysis related to industrial concerns. Thus the overriding format expected of research reports was quantitative in nature and reflected bureaucratic rational as Weber pointed out.

Marcuse summarizes this process in terms of Weber's perspective quite well:

> the specifically Western idea of reason is reified in a system of material and intellectual culture (economics, technology, way of life, science, art), which is fully developed in industrial capitalism, and this system tends towards a specific type of domination which has become the destiny of the present epoch-totalitarian bureaucracy (Marcuse, 1971:135).

In this instance "reason" is positivism in the sense that Comte argued above. Since science in the form of positivism has become the "religion of modern times" we find:

> (1) Progressive mathematization of experience and knowledge, which, arising from the natural sciences and their brilliant success, affect the other sciences and the way of life itself; (2) insistence on the necessity for rational experiment and rational proof in the organization of science and way of life; and (3) the result of this organization, which according to Weber is the birth and establishment of a universal organization of expertly trained officials: this organization becomes "an absolute clamp on our whole existence" (Marcuse, 1971:135).

It would seem that trade (and industry), rationalism, and science are so intertwined that Weber is probably correct in finding, at bottom, rationalism as the defining issue since it stands in contrast to dogmaticism and scholasticism of medieval Europe. But the rationalism at work here is that of the empiricists and the positivists that has grown up in the

service of trade and industry as defined by state interests.

Conclusion

What I have tried to outline is fairly rough in form, but it is a further explication of Weber's theses regarding rationalism. Positivism, under the rubric of science, is a specific type of extreme rationalism that has informed all institutions of Euro-American culture. It has generated a cultural cognitive bias that has oriented trade, industry, state politics, and science in a concerted program of domination and control of all realms of the human universe. As Comte predicted, it has become a major organizing principle of human endeavor. Alexander, citing Koyre', reports that this bias is likely rooted in our language itself and derives from the Greek language (Alexander, 1982). Taking this into account we might argue that positivism is an overemphasis on this aspect of our language, as European languages did not focus on this division prior to the enlightenment. Between the institutionalization of this perspective and its pervasiveness in our very language we perhaps have an explanation for the continued favored position of the positivist perspective in sociology.

CHAPTER 4

POSITIVISM IN SOCIOLOGY

Having reviewed the history of positivism and established its general shortcomings one is lead to the question concerning its continued use. Chapter three attempted to establish an explanation for this in terms of a social phenomena rather than a psychological or purely philosophical argument. This chapter returns to the specific shortcomings of positivism as they relate in particular to the field of sociology. The chapter will examine the claims of Vaughn, Sjoberg, and Reynolds in light of this new information and corroborate their accusations in greater depth.

It is useful at this point to recapitulate what we have established so far and add clarification from other sources. Positivism has divided experience into two realms of primary and secondary qualities and assigned the phenomena of mind to one and the phenomena of sense objects to the other. It proposes initially that causality is a fundamental aspect of physical reality and later attempts to assign it to the mathematical realm, retaining it through reification. It assumes a correspondence exists between mathematics and the

physical order. It assumes that basic patterns of the physical universe are eternally repetitive and can be described by concepts codifiable into mathematical form. It asserts that these laws can be operationalized and inductively verified through observation. This verification process proves correspondence between concepts and physical events exists and that a unique pattern of descriptors corresponds to a unique pattern of events. This verification process provides evidence of proof that the postulate tested is true.

It has also come to light as a consequence of modern physics that causality is questionable in any absolute sense, that Heisenberg's Uncertainty Principle reveals the division of observer and event observed is an arbitrary distinction and consequently so is the primary/secondary distinction of experience. Shrodinger has provided powerful arguments to support this position as well (Lincoln & Guba, 1985). Hume long ago provided serious doubt concerning induction, Quine argued that theories are underdetermined and facts are value laden. Hesse's discourse on positivism has established that the naive realism and correspondence theory of truth are unrealistic and an examination of positivism has revealed the weakness of nomological deductive methods (Raven's Paradox). Godel's theorem indicates the limitations of axiomatic arguments and formal languages with respect to coherence. There is more of this damaging criticism but this should

suffice since any one of them seriously disqualifies the program. The most damaging consequences come from within physics itself, which Heisenberg attempted to make very clear early in the century.

Sociological Implications of Positivism

Bailey (1987) argues that the major division in sociology has its two-fold roots in the perspectives of Weber and Durkheim. Sjoberg and Nett (1968) notes a similar division and relate it to the relationship between assumptions concerning theory and methods used on data. Fundamental to this division is the relationship between the researcher and his/her data. Without explicit awareness of Galileo and the British Empiricists role in this (as well as Descartes') they have acknowledged a primary theme of this dissertation. I will develop this distinction in greater detail in a later chapter, but intrinsic to it is the idea that Verstehen (direct understanding) makes it possible and necessary for the scientist to derive information differently in social science than in the hard sciences. I will not focus on Weber at this point as my concern is with the positivists.

Durkheim and Method

Durkheim espoused a method and perspective consonant with positivists and consequently "is often taken to have been a positivist" (Hammersley, 1995). He saw little

difference between natural and social science, as did Comte, with regard to method and believed "that the methods of science applicable in the field of the natural sciences are, nevertheless, valid within the social field" (Catlin, 1950:xiii). Unlike Comte, Durkheim applied his ideas of positivism in a method, and in this sense he is the first positivist methodologist in sociology. It is presently Durkheim who is most honored for his scientific approach, especially his rudimentary statistical analysis of suicide (Catlin, 1950; Madge, 1962). Durkheim set a precedent which has been imitated to the present and it is frequently claimed the greatest precedent in sociology (Catlin, 1950). Some critics have suggested that sociology has focused on the statistical aspect, ignoring Durkheim's scholarship and use of that scholarship (Sorokin, 1956).

Durkheim felt universal social laws were behind social phenomena or social facts in the same way as universal physical laws were behind physical phenomena (Durkheim, 1950). Like Comte, he advocated an intellectual hygienic approach to research in which all previous notions and biases were to be eradicated before approaching the research problem. "All preconceptions must be eradicated"(Durkheim, 1950:31) to scientifically develop concepts as Descartes and Bacon agreed must be done. As with all other positivists who follow in his path defining phenomena "is the first and most indispensable condition of all proofs and verifications"

(Durkheim, 1950:34). Observation is a crucial part of the research method but

> sensation may easily be subjective. It is a rule in the natural sciences to discard those data as sensation that are too subjective, in order to retain exclusively those presenting a sufficient degree of objectivity (Durkheim, 1950:44).

Durkheim, then, subscribes to the subject object division that Descartes suggests:

> Social life consists of free currents perpetually in the process of transformation and incapable of being mentally fixed by the observer, and the scholar cannot approach the study of social reality from this angle (Durkheim, 1950:45).

These currents crystalize in the form of rules and moral regulations which may be objectively studied. "Scientific propositions..." should be accompanied by reasons to explain them and not considered sufficient in themselves" (Durkheim, 1950:60). Method cannot rely on casual observation and

> the true experimental method tends rather to substitute for common sense facts-decisive or crucial facts, which, by themselves and independently of their number, have scientific value and interest, as Bacon has pointed out (Durkheim, 1950:79).

Social facts depend on causality for their value according to Durkheim regardless of what Hume argued. "Since the law of causality has been verified in the other realms of nature. . ." then ". . .we are equally justified in claiming that it is equally true in the social world:" (Durkheim, 1950:141). To explain social phenomena "we must seek separately the efficient cause which produces it and the function it fulfills" (Durkheim, 1950:95). Unlike modern

sociologists his perspective discounts multiple causation as a valid concept: "A given effect has always a single corresponding cause" (Durkheim, 1950:128). He is definitely against multivariate analysis and with regard to experimentation insists on "the impossibility of all artificial experiments" (Durkheim, 1950:130).

Most of Durkheim's positions will be reiterated again and again by positivist oriented methodologists up to the present and this gives it a timeless quality. Given the lack of progress based on this method, we should however wonder why it has not been more critically reviewed.

At present, the majority of quantitative methods which dominate the discipline of sociology are derived from positivism. Mills (1959:57) notes "What they have done, in brief, is to embrace one philosophy of science which they now suppose to be The Scientific Method." Mills (1959:58) quotes Bridgman to the effect that "there is no scientific method and that scientific procedure is to do the utmost with the mind and no holds barred." Mills consequently develops a detailed argument against Lazarsfeld's notion of sociologists as methodologists who do not require traditional scholarly knowledge of the area they plan to inquire. Mills (1959:57) argues that this approach to method not only drives and confines the line of inquiry but is itself not derivative of "the classic lines of social science work." Sjoberg and Nett (1968) echo these findings. Phillips (1973) emphasizes the

role method plays in defining research problems as well as Bendix (1970), Weber (1960), and many others. Sociology has minimized the value of a great deal of other types of research as well as research approaches for the positivistic method largely modeled on Durkheim's lead.

Willer (1967) supports Mill's analysis, but with a different emphasis. Willer notes that during a major portion of this century the methods which have tried to mimic positivism have disregarded key problems involving induction, deduction, and the nature of scientific laws. It is ironic that the strongest advocates of this perspective write widely used texts including Lundberg, Zetterberg, Willer, Cook, Stanley, Campbell, Bailey, Babie, and Blalock. Their texts cite Frank, Reichenbach, Duem, Carnap, Bridgeman, Mills, etc. It is a confounding and ironic fact that sociology embraces a this methodology and perspective which has a limited basis in modern science.

Historical Review of Social Science Methods

One can find innumerable books on the history of theory, but they contain little discussion on the theory of method. Method is presented as a given in method books as if it is derived from some non historical source. There is little on the history of method and little concern for the theoretical consequences of method, except for Blumer. Phillips (1973) comments that he is not concerned with where the rules come

from even though he critiques them. In this discourse we are interested in where the rules come from. At present, although they are presented as God given in graduate schools, as Phillips (1973:84) notes, "These rules are not God-given, but represent some degree of consensus within the sociological community." Their justification seems grounded in their apparent sanctioning by the hard sciences.

In reviewing the history of methods it is apparent that major positivists played a minor role in social theory, but a major role in method development. Some of the most important of the positivist methods were developed during the World War II (McKinney, 1957; Madge, 1962). Positivists are not even considered in most theory books and when they are it is always Comte and Durkheim. The methods of Coleman, Lazarsfeld, Guttman, Likert, Bogardus, and others are as positivist in rhetoric as the writings of Lundberg, and more persuasively effective because they are congruent with the existing cultural bias regarding science and progress. These are the methods that dominate the discipline today (Phillips, 1973). The perspective that dominates in present sociology emerges from a type of research and research report format which has become institutionalized. McKinney (1957) reports that in 1931 the Social Science Research Council brought out Methods in Social Science which was considered a milestone in methods books. As we noted in the previous chapter, this council was mandated by industrial concerns.

Much of perspective derives from method (Phillips, 1973). It is legitimated by method. The method defines sociology as a science (Lundberg, 1961). It is the undiscussed background of ideas behind method, which is positivism, which defines method. This is why our focus has been on positivism as a cultural bias. Many methodologists apparently are unaware of where their ideas come from, and many seem reluctant to discuss it (see Coleman or Lazarsfeld). Positivism and intellectual hygiene make it irrelevant to them as it is not an empirical process.

In reviewing the positivist role in sociology I hope to identify the main constituents and their chronology. In the sections after I will further clarify some of their key ideas and the role they play in defining sociology. This is important because it is easy to disregard the role implicit positivist assumptions play in the positions of those who may be overlooked as positivists because of the myopia of cultural bias. This can lead to a misconception of the range of positivist influence. Specifically I propose to show that it is through method especially that the positivist perspective has most been promoted. By contrasting this analysis with the background developed so far, it is hoped that the subtleties of the positivist bias will be further explicated with respect to sociology.

McKinney (1957) argues that the major methodological trend can be described along several continuum including

empiricism-rationalism, neo-positivism-anti-positivism, induction-deduction, quantitative-qualitative, and nomothetic-idiographic. The empirical-rational division, as maybe recalled, was a central division in philosophy according to Russell and Woolhouse, but the positivist perspective seeks to overcome this division and so it is not a category of concern for us (Woolhouse, 1988). Likewise induction-deduction are a point of controversy within positivism (Losee, 1972). What is of interest is the overt controversy over quantitative-qualitative and positivism-verstehen. Modern sociological divisions seem to form around these lines with the nomothetic perspective including the positivistic and quantitative categories (Lincoln & Guba, 1985).

McKinney (1957) observes that neo-positivism formally enters into sociology with the definitive volume produced by Lundberg entitled Foundations of Sociology in 1939. McKinney explains that it was contested by many because of its advocation of the use of causality and the procedures of the hard sciences in sociology, but supported by others like McIver in his book entitled Social Causation (1942). Apparently its greatest opponents were the verstehen theorists who included Blumer, Hughes, Loomis, MacIver, Merton, Parsons, Redfield, Sorokin, Becker, and Znaniecki (McKinney, 1957). Since many of these individuals utilize the concept of causation, it was clearly not on this basis

that they rejected Lundberg, but because, according to McKinney, they emphasized an ideographic perspective. It appears that McKinney's analysis confuses the issue with regard to positivism. As will become clear, Merton, McIver, Parsons, and Loomis were working within the positivist paradigm to some degree. It is important to observe that other researchers such as Stouffer and Merton spoke out against such an extreme position in the 40s, but nevertheless continued to build a positivist methodology.

McKinney (1957) claims that the extreme position on quantification in the 20s and 30s has died down and that it is considered an essential technique of research on par with conceptual equipment. In the 90s this is obviously not the case, but the received position of mid-century sociology is well articulated in his following statement:

> The success of natural science may be attributed to the objective character of its data and the quantitative treatment of its results. Hence, if sociology is to emulate that success, it must change its ways of getting and handling data. This means that sociology must develop techniques that secure objective social data suitable for quantitative treatment by statistical means (McKinney, 1957:201).

Many reacted against this by pointing out the differences in sociological data, esp. Sorokin, MacIver, Willer, and Znaniecki. Others refused to go along with an "either or" approach such as Ogburn, Thomas, Thurstone, and Stouffer. Becker, Blumer, Hughes, Parsons, and Wirth were among those who denied that the behavior of men could ever be

quantified. Quantification could not help in the understanding of society (McKinney, 1957).

The middle-range group, Angell, Becker, Guttman, Lazarsfeld, Loomis, Merton, Stouffer, and Suchman, saw these extremes as based on false assumptions. They saw quantitative to qualitative as a research continuum and not a dichotomy.

Becker's account is instructive in that it offers not only a good account of the introduction of positivism into modern sociology, but also the confusions regarding it are implicit in his perspective. Less explicit influences of positivism come in the form of another perspective emerging from psychology and which I will consider next.

As we have seen from Comte, positivism advocates a mathematical approach to analysis. One of the early avenues which positivism covertly manifests itself prior to Lundberg and his close associate Dodd is through the use of mathematics in statistical analysis and through sociometry. McKinney (1957:201) observes: "Conspicuous in the methodological thought of the twenties and thirties was the belief that sociology could become a natural science through statistical procedure."

Around World War I the use of statistics in sociology was confined almost entirely to descriptive forms (Larson, 1968). However, during the 20s and 30s generalizing forms of statistics became increasingly popular as advances were made

in that field (Campbell & Stanley, 1963). It became extended to more qualitative subject matters such as Dorothy S. Thomas's Social Aspects of the Business Cycle (1925) which involved longitudinal analysis (McKinney, 1957). Inferential statistics made it possible to generalize about entire universes of phenomena through the use of sampling theory and probability logic. Campbell and Stanley (1963) note in particular the important work of How To Experiment in Education by W.A. McCall in 1923. It foreshadowed the more explicit advances presented by Fischer's Statistical Methods for Research Workers (1925) in which he proposes pre-experimental equation of groups through randomization (Campbell & Stanley, 1963). Accidental or volunteer sampling was the norm in the 20s and 30s (McKinney, 1957). Summarizing measures of association such as coefficients of contingency, coefficients of correlation, and linear and curvilinear correlations began to be used in the 20s as well (McKinney, 1957:216). Most of the research was one-variable-at-a-time and true multivariate techniques would not appear until after World War II (Campbell & Stanley, 1963).

In the 30s, due to depression, the government began funding large demographic studies which began to use more systematic and random designs (Sjoberg & Nett, 1968; Larson, 1968). Stratified sampling also evolved in the 30s in particular under studies conducted by departments of Agriculture, Labor, Commerce, and the Census Bureau (Larson,

1968; Lundberg, 1961; McKinney, 1957). Quota sampling in opinion polls and market research became popular in 30s and 40s (Lundberg, 1961). World War II in particular focused statistical research on the military (Mills, 1959; Sjoberg & Nett, 1968; Madge, 1962). The departments of Agriculture, Labor, and Commerce also encouraged the development and use of systematic and random sampling techniques in the 30's (McKinney, 1957).

In the 40s, R.A. Fisher introduced analysis of variance and covariance which have come to be used extensively with ordinal data and surveys (McKinney, 1957). Fisher advocated focusing on the rejection of the null hypothesis based on probability which is in line with Popper's arguments (Sjoberg & Nett, 1968) but did not advocate the logico-deductive method (Sjoberg & Nett, 1968:278). These developments also led to regression analysis which has dominated research for several decades, especially since the advent of the PC.

Sociometry was also an important positivist approach introduced by Moreno in the 30s (Larson, 1968). Larson (1968) says it opened up to objective investigation the area of inter-personal relationships. These developments reflect a growing interest in the use of statistics to do research and the major methods texts such as Lundberg's Social Research and Odum and Jocher's An Introduction to Social Research reflected this interest (McKinney, 1957). Larson as editor of Sociometry was in continual contact with Stouffer,

Lazarsfeld, Dodd, Moreno, Loomis, Cottrell and many others, and they represented a concerted effort to develop positivist techniques (Larson, 1968).

The development of scales was also of critical interests to positivists because it allowed them to treat attitudes in an objective manner. Bogardus presented the first successful efforts followed by Thurstone's ranking scales in "Attitudes Can Be Measured" in 1928. By the late thirties Likert would contribute major advances in this area and would join Guttman in further refinements through research funded by the Army. It is important to note the role of large government agencies in the development of these particular types of research methods as they are most useful to large bureaucracies as evidence for policy decisions. Madge (1962) places most of the major development in positivist methods around World War II and research related to the war effort.

Although there was much research of major methodological importance to sociological method in the 20s such as the work of Thomas and Znaniecki, it was not quantitative in the positivist sense and did not interest the government (Madge, 1962). The work of the Chicago school in the 30's also involved some use of statistics and census tracts, but it also was not positivist in orientation (Madge, 1962). The majority of the methods books have not however been written by these researchers, except for Znaniecki, but by those who are interested in causality, the objective nature of social

phenomena, mathematics and logic as organizing principles, and the use of statistics. The authors include Lazarsfeld, Rosenberg, Festinger, Blalock, etc. It is also these types of texts that are used to train graduate students.

Another covert entry point of the positivist perspective is through theorists. Of special interest is the theoretical perspective of Talcott Parsons who McKinney and many others would not place in the positivist camp. It is important to recall, however, that Parsons was highly influenced by Pareto (not to mention Durkheim) and drew heavily from his ideas. Pareto conceived of sociology as a logico-experimental science to be governed by scientific cannons and derive propositions through classic positivist procedures (Schermerhorn & Boskoff, 1957). Parsons was heavily invested in perspectives drawing on positivist assumptions. Most contemporary theorists agree that his perspective dominated sociology for a major portion of the twentieth century (Ritzer, 1992). If this is so, then positivist assumptions dominated sociology as well in theoretical areas.

It is of particular interest is that Becker's (1957) text overlooks the fact that all parties mentioned were drawing from the positivist tradition which had already infiltrated science in general and sociology in particular through Comte and Durkheim. We have already noted in chapter two that positivism was an ongoing effort to integrate the mathematics of the rationalist perspective without succumbing

to the idea of innate knowledge. The Logical-Positivists had consequently accepted the context of discovery as irrational and part of the scientific process in general, as had Whewell, and acknowledged the innate nature of knowledged to some degree. They had planned on using logic and deductive verification as the main realm of activity for science in the context of discovery. The point here is that McKinney's categorizations betrays his limited knowledge of the subject and explains why this categorical distinction is so weak. Almost all of the above researchers were drawing on the positivist model which accommodates both rationalism and empiricism. This is why I proposed that the positivist verstehen dichotomy was more realistic. Weber and Blumer both reject the positivist perspective both in theory and in method and they both are acutely aware of the important relationship between method and theory (Weber, 1960; Blumer, 1969; Sciulli, 1988).

As mentioned above, interpretive or verstehen sociology was the leading source of opposition to neo-positivists. They made a distinction between the nomothetic and ideographic which culminated in Weber's ideal types as a substitution for mechanical laws. The focus was on understanding through knowledge of subjective motivation and causality was imputed only on the level of meaning (Mckinney, 1957). The foremost representatives of this perspective according to McKinney are Blumer, Hughes, Loomis, MacIver,

Merton, Parsons, Redfield, Sorokin, Becker, and Znaniecki. Their emphasis is on theory, the concept of system (Blumer turns in his grave), structural functional perspective, motivation and a means to test it, and an emphasis on qualitative research. This may be somewhat true of Merton, McIver and Parsons, but they are all committed methodologically and/or in terms of many basic assumptions to positivism. This after all was part of Blumer's argument against Parson's determinism (Blumer, 1969).

> The tendency of sociologists is to regard these complexes as entities operating in their own right with their own dynamics. Each is usually seen as a system, composed of given parts in interdependent arrangement and subject to the play of mechanisms that belong to the system as such. Structural functionalism, which is so popular today, is a good example (although only one example) of this view (Blumer, 1969:57).

Becker's divisions, although instructive of an older perspective in the discipline, do not accurately describe the situation or provide really mutually exclusive categories. Bailey (1987), and many others it seems, is really more accurate when he suggests the neo-positivist/verstehen controversy is most prominent in the present qualitative/quantitative division which emerged from this dichotomy and continues today. Sjoberg and Nett are in agreement. With the present emphasis on methods at most universities, the empirical vs rationalist divisions in theory and their influence on the history of method are obfuscated.

In more recent times the work of Zetterberg has been heralded as the most exhaustive and definitive work on positivism (Phillips, 1973; Alexander, 1982). It must also be noted that Willer has often been overlooked. Zetterberg's work though self-avowedly neo-positivist is still often less technical and thorough than Stinchcombe (1968) under the general editorship of Merton. Interestingly enough he is only briefly mentioned in Alexander's (1982) critique of positivism. Other volumes by Lazarsfeld and Blalock also slip by unnoticed as primers in positivism. They rarely cite the sources of their ideas concerning methods.

Behaviorism

Behaviorism is clearly positivistic in the extreme and was in fact so extreme that it proved unproductive in sociology in spite of optimism in the 20s and 30s with regards to experiments (Ritzer, 1992; Mckinney, 1957). Behaviorism is interesting in that it represents an implicitly positivistic view. Drawing on the experimental tradition in biology and the laboratory psychology of Wundt and Pavlov, it was later refined by Watson, Thorndike, Hull, Skinner, Toleman, and others (Hilgard, 1987). Miller and Dollard represent its highest expression (Hall & Lindzay, 1985).

Operant conditioning theory of Skinner in particular views the subject of its experiments as a "black box" which

cannot be looked into and which may be evaluated only through its overt physical responses (Hall & Lindzay, 1958). It operationalizes all its variables and builds axioms based on a cause and effect model of stimulus-response (Domjan & Burckhard, 1986). Its ultimate focus is general prediction and control of behavior in a deterministic sense (Hall & Lindzey, 1985). Consequently it makes most of the key assumptions about measurement and causality that are central to the positivist paradigm. It attracts those researchers committed to these assumptions and who produce research reports based on the rhetoric of positivism and outlined in the APA writing manual. Because behaviorism is centered around the experiment, it is through its theory of method that it is grounded in positivism. Thus it introduces the theory of positivism in an implicit fashion into the research of early social psychologists and indicates their positivist bias.

The influence of positivism through behaviorism enters into sociology through social-psychology starting at the turn of the century with Triplett concerning "Social Facilitation Effects." Allport followed in his wake in the 20's in the same area of interest. Sherif did experiments in the 30s on conformity, followed by Asch, Lewin on autocratic and democratic atmospheres, Hovland & Levin on persuasion in the 40s and 50s, and Milgram in the 60s (Lippa, 1990). Mayos experiments in the 30's were more natural in nature and not

laboratory experiments in the same way as Allport's or Asch's (Scott, 1992).

These experiments were highly influential in the field of sociology and gave experimentation an air of respectability, but there were too many subjects which could not be approached by experiment and Hyman eventually concluded that the survey is the sociological experiment (Madge, 1962). The logic of the experiment was applied to the survey situation. As the survey method developed and began to dominate the discipline, the logic of the experiment became a common rhetoric in methods books (McKinney, 1957).

The work of Homans also brought the positivist behavioristic perspective into sociology. Ritzer (1992) reports Homans was an avid follower of Skinner's work, knew him personally, and employed his operant conditioning to develop his basic propositions. These six propositions were axioms which defined the basic laws of human social behavior and were codified and defended in his work Social Behavior: Its Elementary Forms(1961).

McKinney (1957) claims that Mead represented a modest type of behaviorism and Ritzer (1992) places him with "radical behaviorists." This designation is profoundly incorrect and consistent with Becker's improper analysis and categorization of this perspective if we are to believe Miller, Cook, Blumer, and others specializing in this area.

The Emergence of Positivist Methods In Sociology

Positivist methodology is associated with a theory and a philosophical perspective which we have explored in chapters one and two. Experimental design is a direct outgrowth of this perspective and its accommodation in sociology reflects the migration of positivism into the field. Chapter three attempted to explain the legitimating influence this had on sociology. Lundberg's work, Can Science Save Us (1961), clearly reflects the concern many sociologists had regarding sociology's legitimacy as a discipline, its value to society, and the importance of having the public discern sociology as a science in the traditional positivist sense.

The effort to import the techniques of the hard sciences into sociology has resulted in many controversies and problems too numerous to cover here, however, I shall attempt to review some of the more outstanding problems and issues. I shall also briefly explore some of the key ideas given emphasis by different authors with respect to positivism in sociology. Much of it sounds like a recapitulation of Durkheim.

The authors are ordered into that categorical aspect of positivism that I see them most representing. The categories cover issues of statistics, experimentation, mathematics, causality, and the social variable. In each area I attempt to outline the major problems which positivism encounters through these authors.

Statistics

The main problem with statistics according to many methodologists is one of measuring variables (Blalock, 1982; Sjoberg & Nett, 1968; Bailey, 1987). Scaling of variables along a continuum became a preoccupation since the 20s and five general types emerged by the 50s (McKinney, 1957). In chronological order they are social-distance scales, rating scales, ranking scales, internal-consistency scales, and latent structure scales. The Guttman attitude scale led to work in the 40s by Wallin, Schuessler & Strauss, Riley, and others which paved the way for the wide spread use of attitude surveys today (McKinney, 1957).

Statistics became so much a basis for research that by the 50s Sorokin was sounding the alarm that a new sociological phenomena was appearing in the discipline and degenerating scholarship- the "statistical omnibus researcher"(Sorokin, 1956:17).

> The new and growing belief is that when one masters routine statistical method, he becomes competent to do research on any problem in any field, including the fields which he has not studied at all. The 'omnibus-researcher' has already become an institution in psychosocial research and teaching and is widely used by government, business, and research institutions (Sorokin, 1956:17).

As recently as the 1980's some universities have encouraged their sociology Phd candidates to first get a masters in statistics! (Vaughn, Sjoberg, & Reynolds, 1993). The general

knowledge of graduate students in the fifties generally did not go beyond standard textbooks, statistics, and basic research procedures (Sorokin, 1956). Sorokin (1956) noted that 37% of references in 129 introductory texts of the period were from other introductory texts.

With regard to hypothesis testing, Selvin (1957) persuasively argued that conditions in social research are not suitable to employ statistical tests. Surveys attempting to sample populations may give variations in results as high as .30 as a result of question arrangement alone (Warde, 1995). Returns on survey are rarely above 50% when statistical validity demands a 95% return rate (Warde, 1995). Sorokin (1956) has in detail demonstrated the dubious value of correlation in sociological studies. Sjoberg and Nett (1968) emphasize the difficulty in being sure that indicators are accurate and Bailey (1987) explains that there is no way to verify this except by argument.

Sorokin (1956) documents the dubious application of the idea in the works of L.M.Termsn, J. Bernard, E.W. Burgess, and L.S. Cottrell with respect to survey questionnaires. He notes that Stouffer's techniques often involved asking questions without resort to any other process of verification.

> Their acceptance of the combat performance variable is an "operation" of pure, unadulterated faith in the infallibility of evaluations made by largely unknown army authorities (Sorokin, 1956:39).

Sampling is another major problem in statistical analysis. Sample sizes are not large enough to account for all the influence of all uncontrolled variables, all relevant variables may not have been included in the design, and some variables are too intertwined with others to be controlled. Kish (1968) has dismissed Selvin's critique as unrealistically idealistic stating that he overlooked the fact that not all hypothesis require the control of all variables (Sjoberg & Nett, 1968). Willer (1967) has argued that sampling in survey research is a dubious enterprise since it is impossible to be sure you have given all items in the sampling universe an equal chance of being selected in social research. Further, in sociology we are sampling finite populations:

> Its fault is that it results in population parameters, not conditional universals, and thus results in knowledge which cannot be replicated, is isolated in that it is limited to the population, and therefore cannot be added to other knowledge to form general body of empirical knowledge (Willer 98-99)."

If, as Peirce (1967) says, that a random sample can only be drawn from a finite collection, then universal laws cannot be established in sociology because they will be based on finite sets. Willer (1967:99) further argues that surveys are not capable of generating a body of consistent findings through replication:

> Since population surveys are concerned with parameters which are constantly changing over time (attitudes, statuses, etc.), changing in directions not wholly determinable and sometimes not at all determinable by

> the results of the survey, any replication would yield different results because it would require sampling a different population (Willer, 1967:99).

Finally, over dependence on statistics often leads to "data dredging" in which investigators just look for interesting patterns in their data without any theoretical purpose or position to drive their research (Phillips,1973; Zetterberg, 1965). Zetterberg notes quoting Churchman:

> One cannot take a set of data, make certain distribution hypotheses about their populations, and proceed to a statistical test; one cannot do so and expect a meaningful answer will result. To paraphrase Kant, statistical teats without theory are blind: no general results can be asserted, no predictions made unless one assumes that the statistical hypotheses are consequences of a general theory within which predictions can be made independent of specialized restrictions. . .(Zetterberg, 1965:139)

A general framework of meaning is required to drive the tautological procedures of statistics.

> the current lack of consensus that exists within the social sciences, as well as the frustrating slowness of genuine knowledge accumulation in these fields, stems from our failure to face up to some very difficult and fundamental issues that are inherent in the scientific method (Blalock, 1982:9).

Blalock also stresses the idea that statistics is not sufficient for knowledge without close attention to theory and assumptions. He too sees measurement problems as fundamental to sociology. At the heart of this dilemma lies the problem of variable definition and he acknowledges the advantage of the physical sciences in dealing with homogeneous and stable variables with consistent attributes across contexts. Multicollinearity generates a major

problem. Measurement conceptualization problems lead to over readiness of investigators to select "whatever remotely connected indicators he or she can locate and then merely announces that these will serve as measures of some highly abstract theoretical construct" (Blalock, 1982:19). Such efforts are difficult to criticize constructively.

Blalock (1982) maintains that too much emphasis on quantification can lead one to avoid certain research problems, especially if they require funding from sponsoring agencies. Individuals may evade theory questions which are difficult to operationalize. This leads to inconsistent and biased investigation of theories. Blalock (1982:24) agrees with Lundberg and Zetterberg that a major problem "is the question of how one goes about formulating reasonable general propositions, which contain concepts or variables that are appropriate to a wide variety of circumstances." This is the same issue that Durkheim wrestled with a century before him. The lack of progress should hint at deeper problems.

To a degree Blalock recognizes this in his acknowledgement of the problem of auxiliary measurement theories. It is difficult to devise reliable strategies for connecting the right indicators to the associated postulates. Blumer (1969) has made a strong point of this as well. Blalock summarizes this problem for his approach in the following:

if we want our theories to be generalizable across a

> variety of settings or with respect to a variety of phenomena, then we obviously need to conceptualize our variables in such a way that propositions that contain these variables can be applied across such settings and diverse phenomena (Blalock, 1969:29).

In establishing indicators for concepts under investigation researchers can never be sure they have a legitimate indicator.

Stinchcombe (1968) confidently presents a positivist perspective involving definitions of causality, crucial experiment, facts, and a heavy dose of statistical theory. He presents formal languages and axiomatic transformations of the positivist type (if x, then y). Stinchcombe (1968:31) is heavily invested in causality which he defines:

> A causal law is a statement or proposition in a theory which says that the exist environments in which a change in the value of one variable is associated with the change into the value of another variable and can produce this change without any change in other variables in the environment (Stinchcombe, 1968:31).

Stinchcombe's presentation is every bit as detailed as Zetterberg's with regard to theory and postulate derivations as well verification procedures. He goes into far more depth with formal language, outlines appropriate statistical procedures and gives detailed examples of how various research projects interpret sociological variables objectively. His work is based around Logical Positivist concepts but he is reluctant to discuss the issues. Concerning problems with his methodological theories he says,

> we have tried to leave aside philosophical and epistemological problems whenever we could. Our purpose

> has not been to outline the ultimate justification for scientific belief, but to outline how scientific belief systems operate in practical fact, so we can use this knowledge in constructing social theories (Stinchcombe, 1968:56).

Apparently his method is a statement of faith and one wonders just where he does justify his approach.

Experimentation

Experimentation was optimistically seen as a possibility for future sociologists in the 20s and 30s (Campbell & Stanley, 1963; McKinney, 1957). This helps to explain the popular nature of positivism. Many attempts at experimentation were conducted with dubious results. A review by Odum & Jocher (1929) and also by Brearly (1931) revealed a great deal of confusion in the field at the time. Eventually Hyman (1961) states the logic of the experiment was considered applicable even if the experiment wasn't and Stouffer agrees:

> . . . the heart of our problem lies in the study design in advance, such that the evidence is not capable of a dozen alternative interpretations. . . Basically, I think it is essential that we always keep in mind the model of a controlled experiment, even if in practice we may have to deviate from the ideal model (Stouffer, 1950 in McKinney, 1957:224).

This of course assumes the validity of the experimental method and its underlying assumptions as well as a unified approach to phenomena outside the field of sociology.

Sorokin (1956) protests that the weaknesses of Bridgeman's operationalism, especially with respect to social

phenomena, are irrelevant and meaningless unless performed for the sake of disproving certain ideas. It often narrows the field of inquiry to absurdity:

> Experimental verification of the same hypotheses by the use of different operations yields equally fragmentary results and a different set of notion. As a result, in a study of the same problem there would be as many different results and concepts as there are different operations. None of them can give a general formula, concept, or uniformity valid for all the different operational manipulations used. Thus, the concept of the pressure of gas, operationally measured by the ordinary U-tube, is different from the concept of the pressure of gas as measured by an ionization gauge, since the operations are quite different. The concept of temperature would be different, if defined only through operational measurement by different thermometers (Sorokin, 1956:34).

Experimentation for the Logical positivists involved the crucial step of defining variables in terms of how they are measured. Bridgeman in the US has developed a similar approach and it became known as operationalism (Mills, 1959; McKinney, 1957). Sorokin (1956) comments that operationalism would require abandoning many of the elements of traditional scientific research that have lead to physics.

Experimental method deals with a few crucial cases as opposed to statistical methods. Ideally only two cases are required. "Like a small aggregation of atoms or particles in quantum mechanics, a small aggregation of experimental psychosocial phenomena may be "lawless"(Sorokin, 1956:185). Sorokin notes that extrapolation beyond cases studied into assumptions of universal uniformity cannot be done. Replication is required.

The failure of sociology to implement the positivist experimental design has lead to the concept of the constructed type and the quasi-experimental design (Stanley and Campbell, 1963). Willer argues along with innumerable others from Durkheim to Zetterberg that " experimental manipulations are only possible in contrived and artificial conditions" that are meaningful only for the non-social sciences. He nicely states the problem:

> For ethical as well as economic reasons, the range of behavior which can be created in the social experiment is limited (by size of group, strength of sanction, time, etc.), so limited that experimentation must be classified as a very specialized method which cannot perform alone the same function for the social sciences as it has for some physical sciences (Willer, 1967:3).

Scaling

Sorokin (1956:122) complains scaling is "unrestrained quantification of qualitative data." Sorokin's argument is that much of Guttman's scales applies to phenomena which do not have quantified qualities ie they are not measurable in a physical sense (primary qualities). Thus subjective ranking is the result. It is more likely to introduce error in the form of bias based on researcher opinion. He observes that Guttman's measures of fear (used to justify his use of scales) involves categories of physiological response which are neither scalar in intensity or in the time order of their appearance. Lazarsfeld is likewise chastised for assuming latent continuum structures or classes which are inferred

from data on survey:

> Lazarsfeld has neither mathematical, nor logical, nor empirical grounds for his postulation that all or many manifestly nonscalar items represent in reality a scalar continuum, and that when all the latent classes of this continuum are considered, the apparently discontinuous or nonscalar items become continuous and scalar (Sorokin, 1956:128).

Bidgeman's operationalism though of dubious value with respect to experimentation in sociology, was promoted by Lundberg in Logical Positivist form. Lundberg adopted the Logical Positivist attitude toward imprecision in concepts and considered sociology on a disastrous course because of this imprecision (Lundberg, 1961). However many have noted that operationalism limited the use of concepts useful to sociology severely, especially Mills and Sorokin. In the following section we will address this issue in more detail.

Mutability of Variables

The social variable has been difficult to control, measure, and define in traditional positivist terms. The problem is compounded by the nature of sociological phenomena. The relationship between the subjects of sociological investigation are not simple as with atomic particles and molecules. The connections between sociological variables are far more complex. Bailey acknowledges this as mutability and hopes to control for it with systems theory and further developments in statistics. Network theorists attempt to use computer simulated models

(Knottnerus, 1994). For Lundberg, Zetterberg, Blalock, and many others in the positivist tradition this is the major stumbling block. To account for and attempt to control all the variables involved seems to many highly unreasonable (Durkheim, 1950; Mills, 1959; Shibutani, 1986 to name a few). This is recognized as an absurdity in physics and has been comment on by Hawking in his Cambridge Lectures (1994) as an unreasonable endeavor mathematically and probabalistically.

The definition of variables alters rapidly over short periods of time and may vary from observer to observer radically as even Lundberg (1961) notes. This variation in definition means that the object's relationships to whole categories of phenomena may alter instantaneously at a moments notice. This mutability of social object makes control an unmanageable proposition, and according to Willer (1967:99) means "that for survey work replication is impossible".

With respect to survey work variable mutability frequently invokes the rational of operationalism. Sorokin exclaims that it is ironic that Dodd imports concepts, definitions, and formulae from physical sciences and does not operationally derive them from procedures of his own.

> When Dodd, Lundberg, Burgess, Stouffer, and others, turn to a study of human values, they do not show any trace of using operational method for either their classification of values, or the construction of their definitions or for discovering the characteristics and interrelationships of their subject matter. Instead, they use, often in rather unskilled fashion, traditional

> Aristotelian logic and other common methods of scientific investigation with a special penchant for the questionnaire-interview technique and statistical "measurements" (Sorokin, 1956:49).

Sorokin uses examples from the 30s, such as F.S. Chapin, E. Greenwood, and draws on examples of Hovland, Lumsdaine, and Sheffield in the 50s in his critique of surveys. He explores the works on group dynamics form the book by Cartwright and Zander. Sorokin (1956) observes that many sociological experiments consist of a matched comparison of an experimental with a control group or observation of the same group before and after treatment. It is clear that in comparing experimental with control groups the two groups are never really matched. He argues that none of the designs meets Mill's criteria of "inductive inference according to the methods of agreement or difference or concomitant variation, etc" (Sorokin, 1956:176).

Even if matched along many of the standard demographic characteristics, such as age, sex, race, they still remain very different with respect to innumerable other variables such as "ethical and legal convictions, aesthetic values, scientific preferences, philosophical outlook, temperament, emotionality, prevailing moods, favorite sports, food and drinks,. . ."(Sorokin, 1956:177). Each individual defines his catholicism or race differently and these differences are not accounted for. Gross similarities cannot account for the profound differences within these similarities.

Alexander (1982:7) characterizes Zetterberg's effort as the "most elaborate attempt" at inductive theorizing in sociology. Zetterberg (1965) utilizes a logical positivist scheme for developing sociological terms. Following Carnap he divides propositions and axioms into primitive and operational terms and manipulates them according to the familiar "if x, then y" rules. Interestingly he also advocates procedures for rejecting the null hypothesis in terms that agree with Fischer and Popper. Yet Popper's approach was based on a rejection of Logical Positivism. Zetterberg (1965:52) does acknowledge that terms cannot be borrowed from physics and biology for sociology as Lundberg advocates and argues "The mainstream of sociological thinking on this issue" agrees with this position.

The primitive terms (extra-logical) Zetterberg (1965:45) wishes to use "represent a combination of observable human beings and their actions" and reflect aggregate rather than individual behavior. Terms such as social beliefs and social norms will become building blocks of theory in conjunctions with formal operators of logic. He cites Lundberg's suggestions on categories for social properties.

Zetterberg is eclectic in his approach, and does not reflect on the contradictions in perspectives he wishes to combine. Furthermore if he was aware of Kant's arguments regarding a priori and a posteriori he would realize that his conception of data versus concept is highly dubious. As

Alexander notes:

> The sharpness of this perceived separation is demonstrated in practice by Zetterberg's insensitivity to the impact of nonempirical elements in the very examples which he chooses to illustrate his argument (Alexander, 1982:8).

Zetterberg disregards the entire history of the controversy between Whewell and Mill concerning the context of discovery and attempts to inductively derive all hypothesis. Alexander (1982:8) also observes this (one wonders if Alexander knows the history of these arguments) "only propositions that have been inductively derived from observation can lead to a real theoretical explanation." Here we seem to have strong evidence for Mills's argument that many proponents of this approach are ignorant of its background and history.

Lundberg's Foundations of Sociology spends a great deal of time emphasizing the objective nature of language. At times he sounds like Durkheim. In fact much of Lundberg's conceptualizations agree with Mead, and Lundberg is well aware of this agreement. It is a prerequisite in order for him to propose objective measures in the form of surveys. Adler (1968:37) noticed that Lundberg confuses his audience by referring "on the one hand emphasized that words were just words and nothing but words. On the other hand, he asserted, that words or 'symbols . . are the data of sociological science'."

His most important program is to argue for a technical language of terms in sociology which can be manipulated in

the Logical Positivist sense. In this he shares much in common with Zetterberg. His conceptualization begins however with the assumption that a problematic dichotomy exists to begin with. This is the cartesian bias he carries like so many others. Like Carnap and Frank he plans to begin with experience and sees words as indicators corresponding to experience. It is a correspondence model of the universe. This is not Meadian in conception, but cartesian.

Lundberg also supports Carnap's and Wittgenstein's hypotheses regarding the meaninglessness of certain types of analytic statements. In spite of his interest in this area, he never reviews Carnap's or Wittgenstein's ideas and the problems the logical positivists encountered in developing them. In this sense, as Sorokin has argued along with Mills, we begin reinventing the wheel (only this time a flat tire) due to academic amnesia. We know the Logical Positivist program is full of inconsistencies and contradictions that by their own rules invalidate the system. This was Godel's Theorem. A coherence theory of truth cannot be true.

Lundberg (1961:7) feels words like will, feeling, motives, values, etc "are the phlogiston of the social sciences." He would used "operationally defined terms of such character that all qualified observers would independently make the same analysis and predict the behavior under the given circumstances" (Lundberg, 1961:9). This is in response to McIver's program of social terms. He argues

that in human discourse "all data are known to us through human responses and we infer both the existence and the characteristics of any phenomena from these responses" (Lundberg, 1961:13). In his effort to define the objective aspect of symbolic events the he states

> The alleged greater tangibility of certain physical events resides not in the events, but in our more highly objectified methods of responding to them (Lundberg, 1961:13-14).

Lundberg is quite specific concerning the symbol/event and says that "It is our response which gives it meaning" (Lundberg, 1961:18).

The correct response for a positivist is to collect evidence. The sociologist merely demands sensory evidence of thoughts and imaginings; only better technic is needed to study these phenomena. Having said that objectivity lay in the response to symbols Lundberg (1961:14) explains "to the extent that numbers of individuals use the same word to designate similar behavior phenomena it is conventional to designate the phenomena to which they respond as objective." Consensus confers objectivity: "Both an iron fence and a taboo will keep men from touching an object. . ." (Lundberg, 1961:17). This set of arguments, however, leaves doubt as to whether objectivity is in the method or in consensus. One senses he is attempting to reconcile Mead and operationalism and Adler considers this a major error on his part. It sounds like a veiled argument that the objective method

should be ratified through consensus, without explaining why.

Lundberg argues we need to explicate postulates and axioms which we use implicitly in everyday life and verify them as we do naturally. He proposes judging their value through "their self-consistency, the possibility of logically deducing from them theorems capable of empirical verification, and their compatibility with the general framework of science" (Lundberg, 1961:22). This is obviously the application of coherence and verification theory from logical-positivism. He believes they should be organized into "if,then" propositions. He argues that we need to select significant categories of human behavior and define them in terms "that lend themselves to operational representations of relationships" (Lundberg, 1961:57).

To Lundberg human behavior is movement (a system of energy) in a social field of force and he wants to provide a mathematical framework to describe that space:

> the time is ripe for the systemization of the whole field of general sociology in quantitative symbols...which can be manipulated according to the already established and tested rules of mathematics (Lundberg, 1961:125).

By the fifties positivism had dominated sociology, but hidden under the theoretical umbrella of Structural Functionalism. Lundberg acknowledges this dominance for which he has worked so hard. He quotes Timasheff (1950) "the school dominating present day sociology at least in America is the neo-positivist one." and "It is best represented in G.

Lundberg Foundations of Sociology (1939). . ."(Lundberg, 1961:83). In the best tradition of positivists who believe that intellectual hygiene is possible and that positivism is objective and value neutral he declares "Science as such is non-moral" (Lundberg, 1961:28).

Mathematics

Early in the century Lundberg's close associate, Dodd, attempted to establish a fully mathematical basis for sociology. Dodd's Dimensions of Society was his formalistic attempt to accomplish this feat. Unfortunately a notable mathematician, E.T. Bell, reviewed his work and found that for the most part there was no mathematics in the book, only a translation to "an esoteric set of symbols had been accomplished" (McKinney, 1957:203). Dodd's S-theory reduced concepts to a symbolic shorthand employing mathematical symbols. After his review, Bell commented, "There is no more pathetic misapprehension of the nature and function of mathematics than the trite cliche' that mathematics is a shorthand. . ." (Sorokin, 1956:107). Kurt Lewin had excited many of the mathematically naive but easily impressed associates of his field when he utilized the same approach by trying to import geometry into psychological terms (McKinney, 1957).

These shorthands are not in fact even formal languages because they do not follow the rules of logic. Sorokin

(1956) notes that Dodd's formula was derived form Lundberg's who appears to indulge in the same pseudo-mathematics. Sorokin finds similar problems with Zipf's arbitrary categories for ranking cities to derive patterns of uniformities which harmonize with his theory. He complains that the juggling of figures in this fashion is becoming a time honored practice.

By the 1950s Lazarsfeld (1954:3) was still announcing "Even the most ardent optimist would not claim that mathematics has yet led to important discoveries in the behavioral sciences." He continues to argue that no one has presented a valid argument why they shouldn't. Citing Thurstone's efforts as well as Guttman he is extremely optimistic that it will prove as valuable to sociology as to the physical sciences.

Quite incorrectly and in apparent ignorance of Wittgenstein, Russell, and Carnap's difficulties Lazarsfeld (1954:4) states "There is no idea or proposition in this field which cannot be put into mathematical language, . . ." It seems obvious that Lazarsfeld has more homework to do. His idea is to explore the thinking in behavioral science and find effective ways to translate it into mathematics, yet he appears to consider previous attempts as unworthy of detailed review. Although Lazarsfeld's program is self-admittedly exploratory and hopeful, forty years later little has emerged from it. In spite of this, Lazarsfeld has had a profound

influence on sociology through his development of statistical methodology involving multivariate analysis.

Coleman (1964:3) complains one of the reason there "has never been such a simple correspondence between mathematical structures and the structures of relations between elements in most of social science." has been that "no generally useful and easily measurable set of elements (or concepts) has been posited in most social science." This is a reiteration of Lundberg's thesis in the 20s. He further states that verbal theories are "so vaguely stated or so weak that it is difficult to translate them to mathematical language, and once translated they fail to show an isomorphism with powerful parts of mathematics" (Coleman, 1964:3). Zetterberg could not agree with him more. Quite correctly, Coleman has noted this problem is largely due to the fact that "sociology has kept to the richness-and ambiguity-of ordinary language. . .(Coleman, 1964:3). He does not want to contemplate the fact that these ambiguities may be crucial to social theory and that social theories and formal language may be incompatible (Fuzzy sets do not work with binary logic, and we shall pursue this later).

Coleman (1964) sees five areas for math to be of important service in sociology: the combination of a number of observations to provide a measure for some hypothetical construct (scale analysis of an attitude), quantitative empirical generalizations which are related to results in a

law, a language for theory, and development of predictive models (not theories) to efficiently utilize data in applied research. Coleman develops ideas for each of these substantive areas of proposed application. Much of it reflects the work of Logical Positivism.

The mathematics presented in most Logical Positivist proponent's repertoire involve elementary operations of formal logic to explain the pattern of development of deductive and inductive inferences made from observation or between axioms and postulates. The presentations in both Willer and Zetterberg are idealistic constructions of how the process should work. They do not reflect any practical reality, and Carnap has already demonstrated their limitations. The authors consistently seem unaware, or worse utilize them out of wishful thinking.

Mathematics in sociology is employed more in the form of statistical analysis in sociology at present. As discussed above, it is a tautological system which is a valuable tool when used in limited domains with expert training. Too often, as Blalock has noted, it is misused and abused by those who know only enough to employ it.

Problems with Causality

Many authors have pointed out that the complexity of causes in any one given social event makes it impossible to explicate any clear sequence of events, there is almost an

infinite regression of causal sequences associated with any one event.

> Any event that is explained in this manner is being lifted out of a very complicated context. If all the antecedent events are provided in sufficient fullness to make possible some kind of calculation of consequences, they become so numerous that they are unlikely ever to recur in that combination (Shibutani, 1986:28).

Blalock refers to this as "multicollinearity problem" (1982:15)

Willer (1967) accepts causality unquestioningly, as does Blalock (1982). Lundberg (1961) uses it unabashedly as does Zetterberg (1965). Stanley and Campbell (1963) at least acknowledge its limitations and attempt to justify it, albeit on very shaky arguments. Sjoberg and Nett (1968:27) find that it is "often well-nigh impossible, to avoid the assumption that some kind of cause-and-effect relationship does exist." Most of these methodologists believe it is fundamental to science itself. To their credit they attempt to justify its use with a rationale and acknowledge that it has been questioned. They note a huge body of literature exists on the subject but frequently refer the reader to Carnap who is profoundly biased on the subject as a leading exponent of Logical Positivism. It is interesting to note how many recognize its weakness in light of the new physics, yet adopt the logical positivist analytic convention position. Causality is then reified in all following contexts.

McIver states,

> We have sought to show that the concept of causation is derived from experience, the primary experience of living in an environment. It is the concept of primary relationship, so that, even if we regard it as illusory, we cannot analyze the concept itself into any simpler one (McIver, 1942:68).

McIver's book is a 400 page defense of causation that says nothing new with regard to preceding arguments other than causation is a useful idea and we are justified in using it based on that alone.

Sociological methods text frequently do not mention the controversy even within sociology over the validity of the concept of causality. Bailey (1987) in a standard methods text, like others, cites Hume, but instead of justifying the validity of causation he recognizes its failings and suggests that it is a useful convention rather than a reality (Bailey sees causality as residing in empirical phenomena and "not strictly a logical concept" however as we previously noted the logical positivists had abandoned the use of causality as a physical phenomena and attempted to use it as a logical device for theory. They arrive at a similar attitude however in using it as a convention.)

To sample everyone's position and rationale with respect to causation and methods is beyond the scope of this work, but those who have done so report that causation is very alive and well in the social sciences (Vaughan, Sjoberg, and Reynolds, 1993). The main point to be made here is that most

programs of research methodology feel that causation is crucial to the concept of science, even though physics denies its validity. The rationales developed by even the most informed methodologists writing textbooks ignore the problem because it is crucial to positivist methodology. As Lincoln and Guba (1985) have noted it is compartmentalized and walled off. McKinney's (1957:195) claim that the outcome of early argument concerning causation was the "the virtual elimination of the 'cause as force' notion and the substitution of a very broadly expanded version of causality" appears to be accurate today.

Conclusion

> Partly as a reaction against Sorokin's effort to write grand theory, one of his students, Robert K. Merton, formulated a strategy that has become widely accepted by contemporary sociologists. Merton entered a plea for "theories of the middle-rang"(Zetterberg, 1965:17).

This became the main theoretical defense of methodologists who wished to pursue positivist methodologies without deeper examination. By ignoring that the emperor has no cloths, they have managed to deeply institutionalize the process of social research in the positivist tradition. Today this program continues with unabated optimism.

It would seem important to at least make a cursory evaluation of this programs results. Zetterberg (1965) remarks that Merton's program of theories of the middle range had resulted in Berelson and Steiner's <u>An Inventory of Human</u>

Behavior (1964) which lists of the findings of all the research to that point in the form of 1045 propositions. He mentions that only between five and fifty of these propositions would constitute a law from the positivist perspective. The perfect empirical collection of unrelated postulates.

Another advantage of Merton's program is that it has kept the theorists out of the way with regard to methodology. It is this separation which has allowed methodologists to pursue their program without deeper critical analysis. Hill remarks:

> Despite the score of years that has elapsed since the first edition of Merton's analysis, there continues to be less relationship between theory and research than most sociologists believe to be necessary to a respectable science (Hill, 1970:13).

Blalock as late as the 80s acknowledges the sterility of the deployment of positivist methods;

> A very common stance that may be taken-one that is certainly based on a degree of realism about the current state of our knowledge- is that our theories are at present so tentative and our research so exploratory that it is premature to pay too much attention to careful conceptualization or precise measurement until we have discovered a reasonably small set of explanatory variables on which we may pin our hopes (Blalock, 1982:13).

The list of researchers who find the progress of scientific sociology disappointing includes a long list that extends from Zetterberg to Phillips. In light of the foregoing it is difficult to comprehend why any researcher

would want to devote time to this methodology or attempt to apply it to sociology, yet it is the dominant approach. We must again fall back on explanations involving cultural bias and legitimation.

It is useful to mention at this point that the combinations and permutations of logical positivism, Popperism, and Millism seem endless and each methods book has its own formula. What seems universal about them is a distinct lack of awareness with regards to the failings of each of these perspectives and the history of science in general. Mills (1959) is well supported in his allegations in this regard.

CHAPTER 5

THE CONCEPT OF SYSTEMS

Having reviewed the impact of positivism in sociology I find it necessary at this juncture to turn to an entirely different topic which will be further integrated in the next chapter. The topic now at hand to be considered is systems theory. There is much confusion and disinformation within sociology regarding this concept and it requires considerable analysis to clarify how this condition arose. Bailey (1994) in particular has commented on the confusions regarding systems in sociology and has done an excellent job in contributing to clarification on many issues.

Since I intend to review Mead's use of the concept of systems in the next chapter, it is important to provide a brief review of the history of systems theory in sociology. This will provide a solid background for better understanding this review of Mead and enhance the definition of his position with regard to other theorists of the past.

The Concept of Systems

The concept of system has been used extensively in sociology since its inception in the works of Comte. Herbert

Spencer is probably the person most responsible for developing the concept fully and in retrospect very thorough in his treatment of it (Turner, 1985). Although modern systems theory is far more sophisticated, the fundamental concepts remain the same (Bailey, 1994).

Systems theory, known today as General Systems Theory (GST), and the concept of system are not the same thing and often confused in scientific literature (Buckley, 1967; Collins, 1988; Bailey, 1994; Berrien, 1968). System as a concept is generic and can be applied in almost all contexts (and has been). Its full fruition as an evolving concept resides in GST (Bailey, 1994). Most sociologists appear to have used the concept or theory at least partially and fused either into their own perspective (Collins, 1988; Buckley, 1967).

There have been combinations and permutations of systems theory as applied by many influential and leading theorists of sociology over its history since Comte but to a large extent it has become highly associated with functionalism. Having fallen into considerable disfavor, functionalism seems to have taken the concept of systems with it (Buckley, 1967). This has also alienated it from symbolic interactionists who draw heavily from Mead, who based his perspective on the idea of systems in his Philosophy of the Present (Miller, 1973). It is hoped that further clarification will resolve some of the confusion and restore the value of the concept somewhat

in the eyes of sociologists in general.

Social Physics

According to Buckley (1967) the term system came into use in the study of social physics in the 17th century and its appearance is dated at around the first decade of that century in the English language. Society was seen as a system analogous to an astronomical system in which people were objects that orbited in interrelations in balanced opposition to each other. There were forces of mutual attractions and repulsions in this conceptualization and the entire mechanistic system was based on natural forces that could be measured and calculated. This model contained definitions of moral space, position in social space, attraction and inertia, social pressures, and a system in equilibrium:

> hence arose "social static" or a theory of social equilibrium analogous to statics in physical mechanics, and "social dynamics" involving motion or change as a function of time and space expressible by various mathematical curves (Buckley, 1968:8).

Buckley observes that the terms utilized in astronomical mechanics were originally imported from their social settings and everyday experience and then ironically re-imported back as social theory with their new form of respectability.

It was clearly this social physics which inspired Comte and this astronomical metaphor was also utilized in Spencer's re-exploration (re-invention of the wheel) of systems theory.

Sorokin (1928), commenting on this pattern of rediscovery, notes that the revival of this perspective in the late half of the nineteenth century did not acknowledge (or perhaps know directly about) the 17th century version. Buckley (1967) refers to this as the mechanical systems model and portrays Pareto, Homans, and Parsons as the inheritors of this perspective.

Comte: A Trend Setter in Organismic Systems

Comte developed his social physics in 1822 (Ritzer, 1992) and it is clear he was attempting to model sociology after the hard sciences. This was a trend that was to continue into present times and that was to generate many problems for sociology as it sought to become a legitimate science (Sorokin, 1956). Perhaps the greatest confusion would develop over the term "system," leading to the rise and fall of one of the most influential and misleading theories in sociology known as the functional systems theory of Talcott Parsons.

It is interesting to note that Comte is the first sociologist to employ the term "system" and it is likely a term he imported from his readings in the physics and biology of the era. Not only did Comte consider society in terms of statics and dynamics, but he stressed the "systemic character of society - the links among and between the various components of society" (Ritzer, 1992:16). This approach was

very compatible with his focus on the larger units of analysis such as the family as basic units of society rather than the individual.

Comte developed an organismic perspective of society that was to be highly influential in later sociology. According to Ritzer he employed his metaphor to identify correlates to biological phenomena such as cells and circulation. Ritzer (1992:4) notes organicism was his "most influential concept". Society was viewed by Comte as an organismic system that functioned in a manner similar to biological organisms. This analogy was applied by Comte as rigorously as Goffman appears to have applied the dramaturgic analogy in recent times. Comte's system theory was mechanistically organismic reflecting Descartes perspective of *man the machine*.

Spencer & Durkheim

Herbert Spencer's self-training in biology inspired him to adopt the organic perspective (Turner, 1985). Durkheim incorporated much of this perspective as well in his arguments against Spencer. Spencer's Social Statics, published in 1852, reflects his roots in Comte, although he argued that Comte's evolutionary theory did not deal enough with the real world (Ritzer, 1992). Spencer made a philosophy of science out of the organic analogy, the culmination of which is presented in his First Principles,

published in 1862. The Synthetic Philosophy, published later, "was a general systems approach to social reality" (Turner, 1985:31).

Spencer developed the concept of "equilibration" and discussed different forms of structures as they manifested around his conception of the first law of thermodynamics. His third form of equilibration is described using the solar system as an example and sounds very similar to social physics.

> Any system of bodies exhibiting, like those of the Solar System, a combination of balanced rhythms, has this peculiarity; -that though the constituents of the system have relative movements, the system as a whole has no movement. The centre of gravity of the entire group remains fixed. Whatever quantity of motion any member of it has in any direction, is from moment to moment counter-balanced by an equivalent motion in some other part of the group in an opposite direction and so the aggregate matter of the group is in a state of rest (Spencer (1892) in Bailey, 1994:95).

Bailey notes that this conception of equilibrium system is also very similar to Le Chatlier's conception in thermodynamic theory as it evolved around 1888.

Spencer was informed by a specialist in the field at a dinner party that his "ultimate equilibration" was in fact a description of maximum entropy (system disintegration) according to the second law of thermodynamics (Bailey, 1994). Apparently this problem was never resolved by Spencer and remained a serious flaw in his First Principles (Turner, 1985; Bailey, 1984).

This error concerning entropy was partly due to the fact

that Spencer was not formally a trained scholar in the traditional sense and derived most of his knowledge of science from conversations at English men's clubs where "he listened to and questioned some of the leading scientists of his time in his daily afternoon visits to various clubs in London" while spending time with researchers and professors who engaged him in conversation (Turner, 1985:12).

Spencer's concept of system was as highly developed as any modern perspective in the sciences represented by those such as James G. Miller (Turner, 1985;1991). It could be characterized as mechanistic and closed systems in its conceptualization in First Principles, but Turner argues that Spencer was sensitive in its application to the emergent nature of social systems. Buckley (1967) notes that Spencer's perspective was not specifically organismic, but organic:

> Here let it once more be distinctively asserted that there exist no analogies between the body politic and a living body, save those necessitated by that mutual dependence of parts which they display in common ... The social organism, discrete instead of concrete, asymmetrical instead of symmetrical, sensitive in all its units instead of having a single sensitive centre, is not comparable to a particular type of individual organism, animal or vegetal (Spencer in Buckley, 1967:11).

Turner (1985) echoes this analysis in his argument that Spencer was more concerned with structure than function. His systems perspective along with his error concerning equilibrium was to be later adopted by Pareto and eventually

Parsons, but his emphasis on structure places him in the organic systems model (Buckley, 1967).

Since much of Durkheim's ideas were in reaction to Spencer, he used many of Spencer's concepts of structure(developed in his First Principles, 1862 and Principles of Sociology, 1874-1896), function, and social wholes as well as Spencer's idea of system (Turner, 1985; 1991; Ritzer, 1992). Because Durkheim read Spencer, it is clear that he was aware of Spencer's Synthetic Philosophy as it applied to sociology in his work Principles of Sociology (1855). Spencer, as we have mentioned confused equilibrium and entropy. Durkheim and others seem to have consistently overlooked this critical point, but Durkheim's overall perspective reflected a more organismic aspect than Spencer's and did not depend on an idea of equilibrium (Lukes, 1972). However, Durkheim saw the concept of emergence as crucial to his perspective and it must be acknowledged that this brings him very close to the process systems perspective of Mead and Cooley (Ritzer, 1992; Buckley, 1968). Parson's (1948) and Wolff (1960) both commented on the similarity between his later work and Mead's, especially because of his focus on collective representations.

Pareto

Pareto's version of systems theory was partly borrowed from his flawed understanding of thermodynamics, particularly

as espoused by Gibbs around 1874, who focused on equilibrium (Bailey, 1994). However, Gibb's theory was a model for generating theory for empirical confirmation and equilibrium was used as a mathematical concept. Pareto mistakenly assumed it was an empirical reality and applied it to social events in his effort to develop a sociology like the physical sciences. This is interesting in that according to Buckley (1967) Pareto was a trained engineer.

Pareto used an isolated system model to represent an open-system society, assumed a reversal of process that did not exist in physical systems, and he confused empirical and hypothetical definitions of boundary openings. Bailey (1994:100) finds that Pareto altered classical theory concerning thermodynamic equilibrium in at least six ways that invalidated its application. Pareto clearly attempted to develop a mechanistic closed systems theory that he could apply to social process.

Parsons and the Harvard Crowd

The idea of society as a system of interrelated parts, with boundaries and equilibrium was "explicitly entertained" by N. Bukharin, P. Sorokin, F. Znaniecki, and K. Lewin (Buckley, 1967). These perspectives likely came from either Pareto and or Spencer as the concept of equilibrium is especially peculiar to their perspectives.

Parsons, Homans, and Miller were highly influenced by

Harvard psychologist L. J. Henderson, who read Pareto and admired his works. Henderson assumed Pareto understood equilibrium, became a "true believer," and promoted the notion as a given around Harvard beginning in the 1930s. (Bailey, 1994; Parsons, 1981). He also wrote a book entitled Pareto's General Sociology . Paul Samuelson, an economist at Harvard, also tried to implement Henderson's agenda but failed to develop a workable mathematical model.

Parson's functionalism was based on the notion of equilibrium generated by Gibb and Pareto. Pareto and Parsons both "considered equilibrium to exist empirically" (Bailey, 1994:89). Alexander (1990) also notes that Parsons "conflated" the concept and reified it. For Parsons equilibrium was synonymous with social order and integration. Bailey (1994:92) explains that it was "never meant to be applied to open systems such as social systems" by those in thermodynamics who used it as a mathematically abstract criterion such as absolute zero on isolated systems. Parsons also misunderstood the meaning of the concept, as did Spencer, and assumed it meant homeostasis rather than a state of total entropy (which is its real definition) (Bailey, 1990; 1994).

Buckley (1968) points out that homeostasis in organisms operates within very narrow limits, whereas societies do not. Parsons adaptation of this organismic version of equilibrium implicitly supports the notion that the existing order is

life supporting and beneficial and tends to "overemphasize the more stable, overdetermined, and supported normative aspects of the social system at the expense of other, equally important aspects without which dynamic analysis is impossible" (Buckley, 1968:15). Functional prerequisites is also an outcome of the organismic aspect of this model (influencing Parsons by way of anthropology) and allows teleological assignment of values to social objects which they would not otherwise have.

Radcliff-Brown drew his system perspective from Durkheim. Turner (1991) explains that Radcliff-Brown believed Durkheim originally developed this perspective and Radcliff-Brown saw social systems in terms of how they met integration needs in the same way Durkheim did. However he attempted to avoid teleological problems by replacing integration with "necessary condition of existence" (Turner, 1991:43). All requisites were reducible to one criterion. But he next reintroduced integration as a contingency for survival(organismic functionalism).

Malinowski and other anthropologists had used the concept of systems but in a more tentative and abstract manner emphasizing the whole-parts aspect of systems theory. Malinowski (Turner, 1991) reintroduced Spencer and had a more sophisticated requisite levels approach than Radcliff-Brown. Turner (1991:47) notes, "Malinowski drew the rough contours for modern sociological functionalism." His criterion for

system functioning was survival, which is in fact closer to the notion of equilibrium and the complete entropy it implies. Malinowski introduced a hierarchy of systems level from the biologic to the symbolic and gave different systems independent identities at each level with only constraint powers to other levels (Turner, 1991). This is more of a general systems approach, but still violates it with requisite needs at each system level.

Parsons had the support of these theorists who were employed in social analysis at the same time. Parsons also enjoyed the atmosphere of growing enthusiasm of Von Bertalanfy and other scientists who were concurrently developing General Systems Theory. Von Bertalanfy (1968) (a German biologist) was widely influential in the development of this interdisciplinary perspective and published his formal work General Systems Theory in 1968, even though he had been working on it since the 1930's. James G.Miller coined the name "systems theory" in 1952 and established the Society for General Systems Research in 1954 (Bailey, 1994). Pioneering articles were presented by Miller in 1955 and Bertalanfy in 1962 (Von Bertalanfy, 1968).

In view of the foregoing it should be clear that equilibrium theorists predate systems theorists in the modern formal sense of GST and Parsons is really a member of this former category. His theory is not a systems theory, but an equilibrium theory stressing functionalism. Parsons utilizes

the systems concept, but synthesizes it with other concepts which are inapplicable from a classical physics GST perspective. Bailey (1994:116) notes that the functionalists associated with Parsons such as Merton, Aberle, and Davis were not system theorists either. Parsons failed to develop clear concepts for a systems theory, show satisfactory empirical analysis, or develop a consistent mathematical model (Buckley, 1967; Bailey, 1994). It appears that Parsons was more interested in adopting a concept from the hard sciences to support his theories.

Homans initially used equilibrium after studying with Henderson (as did Miller), but both dropped the concept of equilibrium from their perspectives early on. Although Bailey wishes to discredit Parsons as a true systems theorist, it must be kept in mind that Parsons' ideas were similar to Spencer, by way of Pareto, and Turner makes a convincing argument that Spencer was the first systems theorist as we identify the concept today. It was Parsons total misapplication of systems theory that discredits his standing and his theory.

Parsons could be considered a mechanistic closed ended system theorist initially but the inclusion of cybernetics later on began to edge him toward a more emergent perspective (Bailey, 1990). Norbert Wiener, who had published Cybernetics in 1948, influenced Parsons with an analogue of a homeostatic biological systems model for control engineering.

Turner (1991) notes that at this point in his theoretical development Parsons reintroduced the evolutionary aspects of Spencer and Durkheim which he had earlier dismissed, resulting in a grand perspective very similar to Spencer.

This synthesis of evolution and systems concepts would seem to place Spencer, Mead, and Parsons in the same analytical arena. It should not be surprising then that Rose found functionalism and interactionism to be identical except for a difference in emphasis on process (Reynolds, 1993). It should be recognized, however, that Mead came to a very different conclusion than Spencer and Parsons because of his familiarity with the new physics and the epistemology of science (Miller, 1973). Parsons was familiar with the work of Whitehead and Mead, but overlooked their fundamental shift in perspective away from a classical physics perspective (Parsons, 1968). It may be that a careful review of Mead's analysis may have saved Parsons from well founded critiques accusing him of teleology, tautology, determinism, and atemporality.

In the last analysis much of Parson's problems come from some of his fundamentally false assumptions concerning physics and science.

> In sum, it becomes increasingly clear that mechanical and socio-cultural system are very different types of systems with basically different organizing principles and dynamics (Buckley, 1968:11).

Clearly the examples of Spencer, Parsons, and Pareto shows

the dangers of borrowing conceptual schemas from other disciplines without really understanding those disciplines. Comte was the first with organicism, Spencer with equilibration, Pareto with conflation, and Parsons with confusion of entropy, homeostasis, and equilibrium.

Niklas Luhmann's General Systems

Luhmann's approach is based around a systems environment model which strives toward complexity reduction. However in his model complexity reduction attains the status of a functional requisite if only implicitly. Turner (1991:95) notes that the "basic functional requisite in Luhmann's analysis is thus 'the need to reduce the complexity of the environment in relation to a system of interrelated actions." The teleologies that emerge from a functional requisite approach is wedded to a system format that is still very much like Parson's. Hence it is not a pure, or general systems approach. It is a functional approach to structure. This again reflects a fundamental confusion concerning the implications of the concept of system and the historical problems related to systems theories.

Mead, Cooley, and the Loyal Opposition

Buckley (1968) has categorized this group as process models of systems theory. He also includes Albion W. Small, R.E. Park, and E. W. Burgess. Buckley sees their version of

systems theory as anticipative of Cybernetics as opposed to the organic and organismic models. He observes that systems as a model is not explicitly developed among these theorists, however an analysis of Cooley's writings tends to cast doubt on this statement with respect to Cooley in particular.

Cooley in his work Human Nature and the Social Order explicitly uses the concept of system and in a manner which suggests he has an understanding which is very similar to Mead's. Cooley discusses social life as a canvas made up of square inches:

> but if you should look at these one at a time, covering the others, until you had seen them all, you would still not have seen the picture. There may, in all such cases, be a system or organization in the whole that is not apparent in the parts. In this sense, and in no other, is there a difference between society and the individuals of which it is composed; a difference not residing in the facts themselves but existing to the observer on account of the limits of his perception (Cooley, 1956:38).

In review, Cooley's perspective would fulfill most of the criterion of a closed-ended organismic system of an emergent nature based on symbols and information.

Mead based his emergent systems perspective on the latest findings in physics and especially with respect to Relativity theory (Miller, 1973). Although stated in the more formal scientific rhetoric of his time, the following quote, in terms of systems perspective, compares favorably with the previous one by Cooley:

> The social act is not explained by building it up out of stimulus plus response; it must be taken as a dynamic

> whole - as something going on - no part of which can be considered or understood by itself - a complex organic process implied by each individual stimulus and response involved in it (Mead, 1934:7).

Mead, like Cooley, tends to focus on the informational aspect of human relations, but for Mead language is a real as objects (Miller, 1973; Mead, 1934).

Elements of systems theories in both Cooley and Mead are: organic whole, dynamic whole, each part dependent on the other, (part implies boundary as does whole), and a process or something going on. Mead addresses systems and emergence specifically as fundamental to his perspective and Miller argues that it is at the very foundation of his entire perspective (Miller, 1973). For Mead a totally deterministic and predictable universe was impossible and emergence was a fundamental phenomena which allowed change and evolution to manifest.

> However, I have defined emergence as the presence of things in two or more different systems, in such a fashion that its presence in the later system changes its character in the earlier system to which it belongs (Mead in Miller, 1973:43).

According to Miller (1973), Mead was very familiar with Spencer. Mead's lectures (from 1914) include discussions of Spencer and Cooley both. Mead was clearly familiar with both Spencer's and Cooley's conception of system and did not argue against them in this respect. Mead was very occupied with evolution as was Spencer. Mead, however, does not explicitly employ systems theory in a structural analysis, since his

main concern is with the emergence of mind, self, society, and the implications for epistemology and ontology. However, it is clear that it is the basis for his entire ontology.

Systems and New Systems

Collins (1988:46) has to date presented the simplest definition of system: "A system is anything that has parts which are connected to each other." He is quite accurate in his assertion that system is a general concept of considerable value in all areas of analysis. We can find this term employed to denote social processes by most major theorists. Several important social theorists have used the concept as a sort of metatheoretical basis. Most of these efforts represent a subdomain or aspect of General Systems Theory (GST) as it has evolved and been defined by modern systems theorists such as Von Bertalanfy and Miller.

GST as a perspective is more comprehensive and more accurately grounded in classical physics than most specific sociological variants. GST provides a detailed analysis of systems as entities with boundary relations or interface, inputs and outputs, feedback and feedforward processes, information relationships, tension levels, open and closed characteristics, morphostasis and morphogenesis or emergence, and finally entropy and synergy (Buckley, 1994; Bailey, 1967; Berrien, 1968). Bailey identifies as many as ten types of systems.

It is instructive to review Buckley's definition of a system as it reflects quite well GST and its position with respect to positivism:

> The kind of system we are interested in may be described generally as a complex of elements or components directly or indirectly related in a causal network, such that each component is related to at least some others in a more or less stable way within any particular period of time (Buckley, 1967:41).

Note that he includes "causal network" in the definition. This places him squarely in the same tradition as Bailey.

Conceptually, different theorists view the basis of systems theory from different perspectives. Parsons clearly overemphasized homeostasis as both Buckley and Collins point out. Spencer tended to focus on evolution and morphogenesis (Collins, 1988). To be more in harmony with modern theoretical developments, process should be emphasized. Mead's definition focused on emergent pattern and feedback *involving an observer* (Miller, 1973). Positivists tend to disregard the observer in contradistinction to this (Lincoln & Guba, 1985). In this respect Mead anticipated quantum mechanics, but this is probably because he was familiar with relativity which laid the groundwork for it.

General Systems Theory attempts to account for all categorical variations of systems analysis and integrate them in all scientific fields of endeavor at all levels of analysis (Bailey, 1990; Turner, 1991). This is very similar to Spencer's agenda and although GST may be more

sophisticated in their terminological distinctions, Turner (1985) would argue that conceptually they do not differ dramatically.

Some of the most recent efforts to explicitly develop systems theory in sociology have been pursued by Buckley (1968), Berrien (1968), and especially Bailey (1990; 1994). Bailey refers to these as "New Systems Theory." These efforts tend to align themselves with the General Systems theorists. Bailey makes it clear that Parson's functional concepts of system utilized the old physics and presented problems to him as theorist. Bailey's social entropy theory was a response to this dilemma. However, Bailey's recognition of the problem only goes so far as to trade equilibrium for entropy. He becomes caught in the classic Cartesian dualism which haunted the old physics. He chooses sides in Miller's (1978) distinction that systems are primarily of two kinds: abstract and concrete (recall the Spencer-Comte debate). Bailey (1994) sides with concrete systems. He then proceeds to develop a mathematical social systems model and attempts to reconcile GST with Alexander, Collins, and Giddens. With the advent of Chaos theory and computer analysis the potential for Bailey's theory to transcend the arbitrary dichotomies of classical physics may prove very fruitful. Such a theory no doubt would be forced to reconcile itself with Mead's analysis.

It should be clear at this point that Mead's conception

of emergent systems is a step beyond the positivist approach to systems theory. Mead had the advantage of Einsteins's theories, but we must remember that Parson's and others since him have had this advantage as well. They failed to take into account the significance of Mead's work or the implications of quantum mechanics. Parsons and most of his contemporaries were still working with a Newtonian conceptualization of reality. Their efforts were essentially reformulations of Spencer's work, as Turner urges us to consider (Turner, 1985). The theories of Von Bertalanfy and Miller are also based on simple causality and the older models of physics. Mead's system theory was integrated with the new physics.

Conclusion

In reviewing some of the major social systems theories we have reviewed the distinctions which were presented by Buckley. The mechanical models, the organic models, the process models, and the functional models all represent partial aspects of a fully developed systems theory. Other distinctions can be made such as between organismic and organic models, but these constitute fine points for which there is no room in this paper. For Buckley and Bailey these are all precursors to GST.

There are several other themes which emerge. The first is that the concept system and system theory are two

different things. The concept has been used widely, but systems theory has only partially been employed by most theorists with many variations. The second is that the systems theory of classical physics was a limited systems theory and incorrectly employed by several important theorists such as Spencer, Pareto, and Parsons. The third point is that Mead was more precise in his application of systems theory and its relation to the new physics. The fourth, that GST reflects the old physics even though it is considered the definitive systems theory by sociologists explicitly employing systems theory today. A proper use of systems theory would need to reconcile itself to the new physics, and only Mead appears to have done such an analysis so far. The fifth point is that there is a development of organismic/organic emergent systems theory within the sociological tradition which revolves especially around Spencer, Durkheim, Mead, and Parsons. These men appeared to build upon each others work with respect to this theme. It is a mystery why Parsons disregarded so much of Mead's analysis even though he was familiar enough with it to write a paper for Kurt Wolff on the similarities between Durkheim and Mead (Parsons, 1948). In the same manner Parsons had access to Spencer's work but chose to focus on Pareto (Parsons, 1968).

A final important point which becomes clear is the danger of applying this concept without a full understanding

of its history, combinations, and permutations. Sociological theory is full of well intentioned efforts based on a misunderstanding of physics, mathematics, and the epistemology of science (Sorokin, 1956). These efforts often gain wide audiences of the uninformed who are guided more by bureaucratic titles and credentials than sound research and scholarship. This research suggests that we can now add systems theory to this list of misunderstandings. Hopefully we shall remember our mistakes.

CHAPTER 6

MEAD AND SYSTEMS

In reviewing the early history of positivism several points have become clear. Science, contrary to popular opinion, was not a universally agreed upon enterprise. Its definition has varied from practitioner to practitioner, the rationale supporting its practice varied greatly, and was riddled with undiscussed assumptions. The "new philosophy's" exact form varied from the mathematical idealism of Descartes to the hard experimental empiricism advocated by Bacon and Boyle. In between was Newton's Principia which was favored by Locke and Kant.

Science became a varied aggregate of mathematical postulates, theoretical laws, and various experimental practices. The enthusiasm of Comte and Mill to codify and extend these practices to all domains of knowledge was admirable but flawed. Mill's arguments are riddled with problems (Losee, 1972). The efforts of the logical positivists to rescue traditional concepts of science has been demonstrably flawed as well (Bechtel, 1988). The mathematical and technological developments that occurred contemporaneously with scientific efforts gave the illusion

that science somehow was their source. These technologies gave power to those who had the resources to use them. Science, technology, math, the novel, industry, medicine, bureaucracy, and a host of other developments emerged in concert with science and largely as a consequence of rationalization. It has never been entirely clear exactly what science is! Like good poetry, art, or cooking it is a host of practices that are categorically related.

With regard to sociology in particular we have traced the influence of positivism on academia and research. We have explored how the method and rhetoric of research are defined by the industrial and trade sectors and legitimated through funding. We have established that a cognitive cultural bias generates a context which has consistently encouraged a positivist methodology regardless of theoretical innovation. We have also discussed the underlying weakness of the theory behind that methodology as it has been presented by various leading methodologists within sociology. This of course begs the question concerning what are the alternatives.

The qualitative methodological tradition has provided a strong alternative which has continued to grow in sophistication in the later half of this century (Cook, Fine, & House, 1995). Unfortunately some of these traditions are based on the cognitive cultural bias of positivism as well. Glasser and Struass (1967) represents one such important

effort. Glasser and Strauss (1967), however, concede their position is compromised by limitations of their method with regard to verification processes. Herbert Blumer on the other hand presented a research program grounded in the epistemology of science of George H. Mead. This small but enduring tradition in sociology continues to produce powerful social research not dependent on positivistic conceptualizations.

My thesis in this section is that Mead and Blumer's program is the best present alternative existing in sociology. I propose that most of the criticism that has been marshalled against this perspective is political in nature or grounded in the positivist cognitive cultural bias. This is not to say that this perspective has no failings outside these criticisms. There are problems with the present form of Interactionist theory and method which have legitimately been criticized and are not due to this bias. What follows is a review of these two categories of criticism. Following that will be the proposition that the weaknesses in SI are derived from a failure on Blumer's part to fully explicate the systems aspect of Mead's theories.

Systems theory, as I propose it, would allow us to extend and refine Blumer's method and perspective in a way that would include critical theory and a limited version of positivist methodology. Although some, such as Stover (1977) have commented on convergences between systems theory and SI,

none except Buckley (1967) have proposed that Mead had an explicit systems perspective. Consequently I have reviewed the history of systems theory within sociology to provide a background and perspective for better understanding what Mead proposes and how it is similar or different to previous uses of the concept of systems. I will review Mead's writings in some depth to explicate and clarify his position in this regard. I will reserve the impact of these issues with regard to Blumer for chapter seven.

The Positivist Bias as Source and Weakness of Critiques

Symbolic Interactionism has continued to survive as a perspective in sociology since Mead laid the groundwork for Blumer's perspective and method. It has been consistently criticized for different failings but continues to thrive. Denzin (1992) recognizes three basic waves of criticism beginning in 1963. He divides these criticisms into five categories of: theory and method, the astructural bias, politics, the neglect of emotions, and textuality. His economical analysis reflects a similar but more comprehensive and in-depth analysis by Reynolds (1990).

Denzin (1992) notes that one block of critiques argue that SI avoids historical, economic, institutional, and political issues, citing Block (1973), D. L. Smith (1973), Ropers (1973), Wagner (1964), Day and Day (1977), A. Rose (ed.) (1962), and Zeitlin (1973). Reynolds (1990:137) argues

that SI is apolitical, ahistorical, noneconomic, and that it is "depicting (or constructing) a social world that is overly quaint or exotic." Kanter (1972) complains that it cannot deal with real issues of power. Shaskolsky (1970) argues that interactionists have bought into the American myths of freedom and democracy. Huber (1973) charges it is subjectivistic and commonsense (not science) sociology and that it could only operate in liberal climates. Lichtman (1970) perceives it as an idealist perspective that ignores how oppressive institutions shape behavior. Gouldner (1970) complains it is overinvolved with the powerless and fringe elements and that it fails to study the powerful. Mills (1966) observes that it is a perspective which is infatuated with science, technology, and biologically oriented models of action which ignore critical perspective. Prendergast & Knottnerus (1993) argue that it answers what it considers an inadequate objectivism with subjectivism, idealism and humanism.

Denzin (1992) presents simple and compelling evidence to the contrary with respect to criticisms regarding text and emotions. However, the other critiques are more sophisticated and complex than his response to them, and continue to haunt Interactionism. These critiques in general appear to focus on a failure to address the external and coercive nature of social experience and a lack of objective predictive method. This critical body is further supported

by other critics with prescriptive agendas to cure SI of its problems. Collins (1992) suggests adding a Durkheimian dimension of solidarity as well as disregarding Mead's polemics against Newton and determinism as standing in the way of science. McPhail and Rexroat (1979) claimed to have uncovered a "detailed and explicit" positivist methodology in Mead's writing. Their agenda upon scrutiny becomes, as Denzin (1992) notes, another form of positivist critique along the lines of Huber (1973).

Within SI Stryker probably speaks for the more positivist oriented research tradition in general by expressing his frustration at the idea of accepting Blumer's advocacy of indeterminacy in social life and complains:

> Thus along with denying the possibility of explanatory sociological theory, Blumer severely restricts the legitimate range of investigatory (data gathering) techniques as well as analytic methods (Stryker, 1992:186).

He further argues that,

> to accept a principled indeterminacy in social life does not demand that we reject the aspiration to generalized theoretical knowledge based on the "fit" of empirical evidence to prior theoretical arguments, that we reject "conventional" science as an appropriate model for the work that we do (Stryker, 1992:187).

The aspects he finds illegitimate and intolerable in Blumer's approach are that (metatheoretical level) general predictive social theory is impossible, (methodological consequences) investigation without a priori theory is considered bias, and that the method fails to address structure adequately.

This complaint regarding structure agrees with most other major theorists above and has become known as the astructural bias (Reynolds, 1993; Prendergast and Knottnerus, 1993). The complaints about predictive theory and method are clearly positivistic in nature. They seek explanation and causality as methodological approaches and are grounded in Newtonian positivist perspectives. The critique more central to sociology here, disregarding positivism, is the concept of social constraint and Stryker (1992:188) is quite eloquent on this point:

> Somehow we need to come to grips with the full implications of the recognition that it is interaction that shapes the self but it is social structure that constrains, and so within limits shapes, interaction (Stryker, 1992:188).

There is considerable support in Blumer's (1969) writings to the effect that he never invalidated the techniques of positivism except in their exclusive use. He acknowledged social structure as a reality, but only part of reality. In fact Blumer acknowledges the constraining aspect of society:

> The differences do not mean, incidentally, that Mead's view rejects the existence of structure in human society. Such a position would be ridiculous. There are such matters as social roles, status positions, rank orders, bureaucratic organizations, relations between institutions, differential authority arrangements, social codes, norms, values, and the like. And they are very important (Blumer, 1969:75).

Mead's perspective, as this chapter will show, invalidates positivism as a <u>fundamental</u> perspective. Blumer's position

apparently had to be radical to overcome the cultural bias (behaviorism) threatening to overwhelm SI. Stryker (1992) and others see it as only an effort to hold off Structural Functionalism. Mead and Blumer did not construct a perspective over and against positivism and structuralism, but one that was meant to encompassed and transcended the limitations of perspectives bounded by those concepts.

Although these criticisms appear to represent a rich diversity of failings, as well as confusion, it is also possible to view them as the facets of two underlying issues. One is the positivist cultural bias of SI's critics and the other is an important omission in the explication of Blumer's development of Mead's perspective. This omission is the systems aspect of Mead's perspective. These two themes are intimately related and can account for a large measure of the shortcomings of SI. This chapter will seek to develop these two themes.

Implications of Mead's Position

There are many themes which run through the history of science, but it is postulated here that one of the most critical of these is the primary/secondary division early advocated by Descartes, Bacon, Locke, Newton, and in his own way, by Kant. We have discussed its advantages for establishing control, finding consistent patterns, and offering a stable reference in the storm of emerging European

culture. Some historians have questioned whether Europe might not have gone into decline like India without this stable body of knowledge (Palmer, 1964).

Science became closely associated with war technology, trade technology, bureaucracy, and those who controlled the rising capitalist bureaucratic system. As previously discussed it was the ultimate bureaucratic form of knowledge; simple, refined, precise, consistent, reliable, economical, accurate, and powerful. Science became the voice of authority, the uncontestable rationale for doing. Facts became the bureaucrat's weapon, the lawyer's weapon, the military's weapon.

Science has become a part of our culture. It has a mythology. As Burke (1965) noted it is a form of rhetoric. It can exist only in a certain cultural context and is supported by the grand narratives of progress (Lyotard, 1991). It has its canons, its martyrs, and its saints. Comte attempted to make it explicitly a religion. After several hundred years it has profoundly adjusted our cognitive filters as Bechtel, Gergen, Hubner, Hesse, Foucalt, Kuhn, Lyotard, and an army of other scholars and scientists can testify.

We see the material dichotomies the way we have learned to see them from science (Lyotard, 1991). The relatively new knowledge, that we are looking at a universe of various energy quanta in shifting probability, fields boggles our

concretely material minds. We want to continue looking at the safe, ordered, and explained world of Newton. We want to practice science the way we thought it was practiced. Like the primitive cultures we have invaded and crippled, we want to return to the old way, to reformulate the maizeway of the grandfathers (Newton et al), as Anthony Wallace (1979) suggested.

The details regarding the weaknesses of science have been explored: the underdetermination of theory, value-ladeness of facts, the problems with induction, Heisenburg's Indeterminancy, etc (Science is more of a club or a culture). We have taken special note of the constant effort to separate mind and matter and build the proper connection between them. In the face Heisenberg's warnings and despite his efforts the Vienna Circle, Popper, and a throng of others have tried to save the old science; despite the danger of pouring new wine in old bottles, Frank and his followers only put new labels on old product. The mind-matter division is still a cultural and cognitive problem in the scientific community; especially in sociology.

George Herbert Mead was acutely aware of the source of these problems and attempted to establish a new direction. His success, of course, was ignored by those enamored to the old ways. Academia has its politics, its networks, and its biases. Mead did not publish a great deal, but his ideas often surfaced in the writings of others like Cooley and

Dewey (Cook, 1993).

Although Mead did not specifically address all of the problems with philosophy and science in the way in which they are currently discussed, most of these difficulties are resolved through the implementation of his perspective. As formerly mentioned, Mead was familiar with Relativity theory and taught courses on the subject at Chicago (Miller, 1973). It was his program, like Locke in his time, to develop a philosophy grounded in the most recent advances of science. Miller (1982:8) says, "Mead wants to furnish a theory of the self consistent with all of the latest developments in the physical sciences."

Many of the mainstream philosophers of science touched upon or arrived at similar conclusions to Mead's, but were unaware of his work and failed to fully develop the implications of these conclusions as Mead had done (Miller, 1973). It is the overall aim of this investigation to demonstrate that within this tradition of sociology is a solution to many of its difficulties as an emerging science.

Primary and Secondary Qualities

In the previous sections much emphasis has been placed on the division of experience into realms of primary and secondary qualities. This emphasis is derived from Mead's observations. He frequently commented that this step taken by early philosophers of science was a fundamental error in

their development of a system of knowledge. In attempting to explain the world in terms of mass and motion, individuals relegated all unexplainable factors to the realm of "mind." It was Meads thesis that scientists continued to divide the world into what can be controlled and called it objective reality and relegated what its method cannot deal with to the realm of mind and non-reality. It tended to drive analysis to extremes of solipsism or determinism. Miller (1973) argues that Mead was able to steer a course between these two extremes. Mead argues against cartesian dualism and Lock's empiricism as well as the monism of Berkeley (subjectivism).

The consequence of this division was stated by Mead in this manner:

> Scientific psychology tends to divide the psychic situation into two parts, bifurcating states of consciousness and the causes of these states, thus setting up a sort of parallelism. This pushes all of consciousness into one field of observation and leads to the idea of the conscious world as opposed to the physical world, that of electrons and protons (Mead, 1982:109).

Mead was apparently aware of Heisenburg's work and develops this consequence to its logical conclusion:

> But the electrons happen to be determined by the relationship between the intellect of the physicist and the proton; so the organism of the scientist also determines what the object out there is (Mead, 1982:115).

Mead draws the obvious conclusion from the quantum interpretation of events: "Mind and body are not to be separated on the basis of our present physical science"

(Mead, 1982:167).

He goes on to elaborate about the excommunication of secondary qualities to what he calls the realm of "imagery" and how this leads to a tautological relationship between observer and observed that defines events causally:

> Physical science has no place for imagery or the meaning of things and so far as physical science is concerned, all that is left over from its account of physical objects is disregarded. It interests itself in what can be controlled, stated in terms of mechanical control, which implies a direct causal relation throughout (Mead, 1982:110-111).

But he insists that this is an error:

> Imagery is not a structure or conscious stuff different from the physical world. It is not a stuff than enables us to tell the difference between outside and inside, as the bifurcation of the world implies. We must consider inside and outside together, and the world cannot be divided into inside and outside (Mead, 1982:107).

Mead proposes that mind is a complex emergent phenomena which cannot be separated from physical phenomena and that is co-emergent in an on-going basis. He explains that once we are free of this dichotomy we should also see the fallacy that mind emerges mechanistically from material structures. In his interest to explain the genesis of self Mead (1982:107) sought to be "free of the assumption that the self is built from the physical world and conscious states." He saw behaviorists as following this error in analysis.

Mead understands mind as emerging from its material context through a complex history of action over time. His Philosophy of the Present proposes a systems theory in which

the present is recreating itself, as well as the past and the future, in an on-going basis. The act is the manifestation of the phenomena of "sociality" on a higher systems level than material sociality. This sociality reflects an intelligence which is fundamental to matter itself. "This tendency is what marks intelligence. We find it in all stages, perhaps even below life levels, in crystals" (Mead, 1982:109).

Behind the "act" is intelligence and it cannot be explained in mechanistic terms. To try to build organizations out of individual acts is trying to explain intelligence of organizations-the social whole- in terms of mechanics. The emergent solution in harmony with Mead is another level or order of intelligence. "Mechanical explanation does not make room for this selecting process or act" (Mead, 1982:109). "Life systems" express this impulse or tendency (Mead, 1982:109). Living systems are not mechanical and neither is behavior.

According to Mead consciousness, mind, and self, cannot be separated from action (1982:19). Mead's position is that mind, the self, and the symbolic process, "though dependent upon a physical environment as well as a social environment, and necessarily functionally related to the latter, are also real and objective" (1982:20). (not epiphenomenal and reactive- this is pluralism) Miller says Mead is opposed to Durkheim's transcendent group mind. The difference between

mind and matter is not substantive. but functional.

Mead is proposing that intelligence is a fundamental force of the universe and is related to sociality (develop sociality). The form intelligence takes is a reflection of the activity of an organism, its potential range within the environment, and the history of its interaction.

This intelligent action comes in the form of the selection of stimuli for the purpose of interaction. "The selection of stimuli is the intelligence of the form" (Mead, 1982:116). For Mead intelligence does not have to be conscious. The form can be a crystal, an organism, or an organization.

In sum, Mead is proposing that intelligence emerges from the ongoing interaction of evolving complex forms of material reality (now various densities based on vibrational frequencies of energy in quantum probability fields) based on the phenomena of sociality- or the interactional nature of all phenomena. As we shall further explain; mind, self, and society cannot arbitrarily be separated from each other or from action itself. They are all facets of the phenomena of sociality or society system as it emerges in more complex forms and higher system levels.

This picture is very consistent with modern physics. It also has profound implications for ontology and epistemology, which were not lost upon Mead at all as he developed his philosophy of knowledge. This picture also develops the

implications of a non-dualistic perspective (primary/secondary) with respect to mind and matter. Mead has explored the implications of the new physics in a very different program than his contemporaries. Wittgenstein and Quine continued to develop their philosophies based on the Cartesian dualism (although they qualify their dualism considerably) and struggled with the frustrating consequences as did those who ignored the implications of the new physics in other domains of science.

Mind, Self, Society, and Language

Mead is fundamentally concerned with the concepts of the whole and its relation to the parts. Rather than engage in a mathematical analysis as Russell did with set theory, Mead sought to employ a larger conceptual domain of philosophy and scientific discovery to construct his picture of epistemology and ontology. His concept of sociality plays with the relationship of parts of the universe to its theoretical whole. In the same manner Mead plays with the idea that this is an important principle in the process of the emergence of the phenomena of mind. In his review of some of the examples of sociality as it manifests across the philo-genetic scale, he notes that termites are a community with no mind (1982:136). He observes a similar relationship at the organismic level:

The unity that transcends the organism does not enter

> into the life of the separate cells of the organism. There is unity of the organism, but it does not get into the separate units (Mead, 1982:165).

He notes that human society is an exception: "With humans, who have significant symbols, the whole process enters into the life of the separate organism" (1982:165).

Mead explains that interaction is problematic for humans and that grasping for objects seen at a distance has resulted in gesturing between humans. This gesture has evolved into a complex language (the depth of which Blumer fails to recognize, but Goffman makes a career out of). Grasping action, language, and the mind co-evolve. Language emerges in growing complexity over time through interaction. Words/symbols are anchored in physical gesture and scripts, or collapsed acts. "The percept is thus a collapsed act which gets its full meaning only in the social structure of the group" (1982:133). The percept, or perception itself, is conditioned by the script.

Here Mead deviates from modern cognitivists (still working with implicit Cartesian dualism) because he locates the act as an organism/environmental system:

> The act is not to be located inside the brain; it belongs to the organism in its environment. Hammerness is in the hammer. This comes about through social mechanisms within a social structure (Mead, 1982:133).

The social structure is part of mans environment and imagery is not a conscious stuff different from physical events. His social structure emerges at the same time as mind and its

perceived environment. "Language is an all important medium of the social process that completes the act" (Mead, 1982:142). The language of the society provides the ontological reality it perceives, the theater of action (see Goffman).

> An individual without a social structure has a different object from that of an individual in a social structure. A hammer is not a hammer to a Gorilla (Mead, 1982:133).

Mead perceives the group as evolving an expanding world of social objects growing out of the complex dialectics of language, environment, and action. "The social medium represents a certain type of activity; we are adjusting ourselves to the activities of other beings" (Mead, 1982:141). Finally Mead comes back to the relation of the part to the whole with the emergence of the self: "The group is essential for the completion of the normal act of the individual; all completion of acts occurs through this structure" (Mead, 1982:144). The combined activity of the organism within the group becomes the collapsed act which is the self:

> The unity that makes up the self is the unity of a social organization that makes one feel part of the social process, where one is ready to put oneself in the position of others (Mead, 1982:164).

With the emergence of the individuated self comes self-consciousness. "Consciousness is the entrance into the life activity of the individual of the organization of the larger whole to which he belongs" (1982:166).

Consciousness then is a function of the relationship of the individual to the whole group through the aggregate of collapsed acts, derived over time and symbolized in gesture. This concept parallels Quine's with regard to meaning and the entire language system. The web of self-consciousness for Mead is similar to process as the Web of Meaning for Quine. One philosopher relates to a social system, the other to a language system. The meaning of self or identity is derived from the group in the same way as the meaning of a word is derived form a language.

Self is a process of taking the role of other, of objects, and of the inside objects; the world that pushes back. The world at an instant, however, as in math, does not exist.

Implications for Knowledge

Language for Mead is not a metaphysical given, a world apart from the physical, derived from a world of absolute Platonic forms. Language is a social event emerging through interaction with the environment. Co-emergent with language and society is mind and self. The self is both object and subject in society. This reflects the reality of the non-social world of animals. But for Mead the social world is the real world. It cannot be separated. This requires us to take the role of the other. The other is always there. We can only occupy one place at a time. We know of the

objective world because it pushes back upon us, independent of our program. But social objects are a combination of mind and matter. They are real and highly mutable because of this. Trees cannot fall in the woods if no one is there. There are no trees without human community because there is no social mind. There are only differentiated energy fields unfolding.

Mead would agree with Quine that different ontologies occupy different social worlds and that both can be true at the same time. This is in harmony with Einstein's conception of two systems defining the same event differently. The ontologies of our gravity field define the same physical object differently than the ontologies of a black hole. Wittgenstein found a limit to language that corresponds to Mead's conceptualization. Many questions are meaningless when related to physical systems but not to social systems. Meads conceptualization transcends and contains Quine and Wittgenstein because it resolves the dualism they are based on from the start.

What does this have to do with science? As you recall Locke was interested in public knowledge and consensus. Language is public by nature in Mead's system. To objectify an object which is already objective is redundant. To strip it of its secondary qualities in fact makes it esoteric, not public and objective. Cultures develop public ontologies which are quite objective. They are not stable or static

however. Social objects change or mutate between contexts to some degree. They must in order to maintain their complex of meaning. Social objects are by nature highly mutable as Bailey (1994) notes. Atoms and molecules are very distant social objects and highly abstract. It was this fact that inspired Blumer to develop his methodology.

Cartesian dualism and the division of Galileo are not legitimate based on the findings of modern physics and Mead developed a perspective that was congruent with these new findings. It was this perspective that was the basis for Blumer's method (Blumer, 1969). Mead also conceptualized his new perspective in terms of systems. This was not utilized by Blumer explicitly in his method. What follows will discuss how Mead envisioned the role of systems in the emergence of social phenomena.

Mead and the Idea of System

I shall rely heavily on Miller for support as he is one of the leading authorities on Mead as well as trained in the discipline of philosophy. Mead, however, also is quite clear on these matters in his collected lectures in the Philosophy of the Present (1980). Miller notes that

> Sometimes Mead refers to an order as a system, sometimes as a perspective. In The Philosophy of the Present he generally uses the word system, but in many places the two other terms would be equally appropriate (Miller, 1973:204).

Mead's meaning of system according to Miller is as follows:

> A system consists of a set of entities or objects that are interrelated in such a way that the significance of any one entity or object depends upon its relation to other entities or objects of that set, and by virtue of their interdependence they constitute a coherent whole as over against a mere aggregate. This applies to systems of facts or happenings in nature and to formal systems such as logic, mathematics, and language systems (Miller, 1973:189).
>
> For an element to belong to a system means that it is to be understood from a certain point of view or perspective, and it is to be interpreted by use of a certain categorical scheme and that what it is or how it functions is determined by its relation to other members of the system (Miller, 1980:189).

This transcends objectivist/realist definitions of system, which makes it a more unique definition. Mead is speaking in terms of what positivists might call a mind/matter synthesis. A systems identity is established by the constraint it places on its members. This constraint arises from their unique pattern of "sociality" or relationship. In physical systems this would be unique physical features which functionally relate an object to other objects- such as a piston to an engine (Bailey, 1994). This also extends for Mead to the realm of meaning. Non-physical objects can have unique features which define them as part of a system (Buckley, 1967). Thus we have thought systems as well as biological systems. This is very similar to the way in which Turner (1985) tells us Spencer uses system, however Spencer's physical version was wrong and confused entropy with equilibrium.

No object, and for Mead all objects are social objects, can have an identity without relationship in a system. Miller notes,

> If anything is comprehensible it is because of its relation to some other entity or entities in a system. Without presupposing a system, nothing could be thought about; nothing would have significance, since nothing by itself (such as a particle) has significance (Miller, 1973:189).

Objects within a system gain their identity from the system by their relation to other elements in the system ie through sociality:

> these relations must be such that certain combinations of them will make the individual entities in the system intelligible or understandable (Miller, 1973:190).

Socialty is therefore fundamental to system and object identity or meaning. As we shall recall McPhail & Rexroat (1979) criticize Blumer for not having a clear definition for meaning. This exclusion helps to explain why.

In the philosophy of the present Mead speaks quite clearly for himself:

> The other dimension of sociality, where this term expresses the determination of the nature of an object by the natures of other objects belonging to the same system, is evident in the conception of energy systems, in the development of multicellular forms in which the life of the whole system is the integrated life of the differentiated cells that make it up, in the social systems involved in the propagation of the species and in the integration of societies, from those in which at first balance is reached between reproduction and the consumption of one form by another, up to those in which a social process is mediated by differentiation of individuals. In all these the nature of the individual is in varying degrees the expression of the natures of other members of the system or society (Mead, 1980:77).

For Mead a society is explicitly a system,

> A society is a systematic order of individuals in which each has a more or less differentiated activity. The structure is really there in nature, whether we find it in the society of bees or that of human beings. And it is in varying degrees reflected in each individual (Mead, 1980:87).

Mead further argues for the generic concept of specialization: "A society is a systematic order of individuals in which each has a more or less differentiated activity" (Mead, 1980:86-87).

For Mead mind is an emergent from a system:

> "that it is a natural development within the world of living organisms and their environment. Its first characteristic is consciousness, that emergent which arises when the animal passes from the system in which it formerly existed to an environment that arises through the selectiveness of its own sensitivity, and thus to a new system within which parts of its own organism and its reactions to these parts becomes parts of its environment (Mead, 1980:84).

More direct to the point he states, "I have wished to present mind as an evolution in nature, in which culminates that sociality which is the principle and the form of emergence" (Mead, 1980:85).

> The past experience of the organism becomes part of its environment in the form of its new interaction. The animal comes to respond to an environment consisting largely of possible futures of its own delayed reactions, and this inevitably emphasizes its own past responses in the form of acquired habits. . .These pass into the environment as the conditions of his acts (Mead, 1980:84).

Eventually in higher order primates,

> These characters of the environment constitute the stuff out of which values and meanings later arise, when these characters can be isolated through gestures in

> communication (Mead, 1980:84).

Finally,

> The systems to which I have referred are in all cases interrelations between the organism and the world that reveals itself as environment, determined by its relationship to the organism (Mead, 1980:84).

We can see that Mead is referring to different systems emerging at different levels of organization. The principles of sociality and emergence hold consistently in this process. Emergents are in the form of mind and consciousness. This is an open systems perspective on the phenomenal world. Mead ends up where Heisenburg (1971:85) does, "Any essential change in the organism brings with it a corresponding change in the environment."

> Animals evolved toward bringing more and more of the activity of the animal within the environment to which it responds, by the growth of the nervous system through which it could respond to its sense processes and also its response to these, in its whole life activity (Mead, 1980:85).

The animal could not transcend its differentiated nature and become "an object to itself as a whole until it could enter into a larger system within which it could play various roles, . . ."(Mead, 1980:85). Mead (1980:85) explains what this larger system is in the next line, "It is this development that a society whose life process is mediated by communication has made possible." For Mead (1980:85) this "is the realm of continual emergence."

In fact as we have noted, Mead did have a systems perspective, but it is not objectivist in nature because that

would have contradicted physics. The question arises as to how such a concept be reintegrated with Blumer. A close reading of Blumer and Mead reveals that Blumer is for the most part very true to Mead's ideas, although he leaves a great deal out. McPhail and Rexroat (1979) attempted to make much out of this, but clearly did not understand Mead very well (Denzin, 1992; Johnson & Schifflett, 1992). Miller and Collins, as discussed, point out that Blumer's omissions are due to the fact that Mead is not interested in sociology, but in philosophical problems. Mind, Self, and Society is a by-product of his interest in explaining mind. Blumer on the other hand is not a philosopher and may have not understood the ramifications of Relativity- few did at the time.

CHAPTER 7

BLUMER AS SOLE INTERPRETER

Thus far we have established the role of positivist bias in generating an invalid critique of interactionsim. We have demonstrated that Mead's perspective resolves the Cartesian duality implicit in that perspective and that his perspective is firmly grounded in the new physics. Sociology has a much stronger basis here for a methodology tailor made for social phenomena. There has recently been much controversy concerning how well Blumer utilized Mead's perspective (Johnson & Shifflett, 1992). This dissertation has established that Mead did have a systems based perspective and it should be clear at this point that Blumer failed to acknowledge and employ fully the systems aspect of Mead's perspective. Two questions arise in relation to these findings. The first regards the extend of Blumer's legitimacy as sole interpreter of Mead. The second question concerns why he avoided using systems theory. A third issue, not directly related to these two but of paramount importance, is the implications of systems theory for Blumer's methodology. How should it be incorporated in that methodology and what advantages might such an incorporation

hold? These are the main issues this chapter will deal with.

Justification for Further Explication of Mead

Inside the interactionist tradition Wood and Wardell (1992), who appear to understand and have read Mead quite well, also argue that Blumer's interpretation of Mead results in a perspective with a basic astructural bias because it is not entirely true to Mead's perspective. They insist that "not even Blumer can claim to know what Mead 'really' meant" (Wood & Wardell, 1992:20) and Johnson and Schifflett (1992:43) echo their sentiments that "No one can ever report finally on what Mead would say." Finally Stryker (1992:184) says "for recent years have made it abundantly clear that there is not and probably cannot be a single authoritative reading of Mead."

There is considerable frustration within SI with Blumer's authoritarianism with regard to Mead and it is most eloquently expressed by Stryker. He despairs the polemics in Blumer's message and the danger of scholasticism and attending dogmas that circumscribe free investigation. Preferred perspectives result in a situation in which,

> We then are used by our perspectives rather than using them, and perspectives themselves are likely to ossify, to become unquestioned Truths and potentially fallible ideas subject to logical and empirical examination and reformulation (Stryker, 1992:189).

Blumer set a tone and style that was combative and disdainful of alternatives and "he must be held importantly responsible

for the scholasticism within symbolic interactionism that I am deploring" (Stryker, 1992:189).

Collins (1992:62) correctly reminds us that Mead "developed a sociological theory of mind, not primarily as a contribution to sociology, but because it enabled him to solve philosophical problems" and "Mead's theories have not been exploited and developed to their full potential" (Collins, 1992:61). There is considerable consensus that not all of Mead has been accounted for and that within what has been lost may be an answer to the astructural bias. It is suggested by many that political reasons played a significant role (Sciulli, 1988).

Blumer (1979) himself acknowledges in his response to Fischer and Strauss (1979) that there were indeed areas of Mead's philosophy which were not utilized by the Chicago department, but that this was because they felt that they were not pertinent to the focus of the department. Blumer (1979:21) explains the "departments interest and concern were with what was happening in certain areas of contemporary society . . .", whereas Mead's agenda was built around broader philosophical concerns. Johnson and Shifflett (1992) echo this point, agreeing with Collins that Mead's concerns were very broad with respect to sociology.

Blumer does not directly refute the suggestion by Fisher and Strauss that there were aspects to Mead's theories that constituted "excess baggage" (Fisher & Strauss, 1979:11), but

he does make it clear that all of Mead's ideas were important to the department with respect to constructing a "perspective" (Blumer, 1979:21). It is clear from this that Blumer did not explicitly employ all of Mead's ideas and it should be obvious that this would have been impractical. However, this does not mean that there is nothing in Mead that was overlooked or that might be dusted off and reconsidered. To argue that the interpretation of Mead stops with Blumer is to argue against scholarship and ignore the fact that Blumer was not a philosopher or trained in that discipline. Others who are trained in philosophy, such as Miller (1973), offer continuing insights into Mead and the implications of his work. It is from this position that I argue for further interpretations of Mead's work and refinement of Blumer's interpretation and implementation.

Many false dilemmas arise in the minds of those who contemplate interactionism or Mead. This is due do the confusing nature of Mead's perspective for individuals with the cartesian cognitive bias because Mead "attempts to resolve the realism versus idealism controversy" (Johnson & Shifflett, 1992:42). Miller (1973) makes it clear that Mead meant to steer a course between determinism and solipsism in his philosophy. Relativity theory had already accomplished this in physics and Mead wished to do the same in philosophy. Blumer remained true to this perspective in developing his methodology, as far as it went. I would also argue that he

could have gone further in fulfilling that perspective.

Blumer's Perspective and Method

What is Blumer's method and how does it compare with positivist methods?

Blumer (1969:9), in line with Mead's overall thrust, is that humans are basically symbolic in their orientation and although,

> in their association human beings engage plentifully in non-symbolic interaction as they respond immediately and unreflectively to each other's bodily movements, expressions, and tones of voice, but their characteristic mode of interaction is on the symbolic level, as they seek to understand the meaning of each other's action (Blumer, 1969:9).

Contrast this with the fact that 90% of communication is non-verbal. Although Blumer disregards Mead's discussions concerning the unconscious and its relation to habitual behavior, he does explicate Mead's position on social objects:

> Out of a process of mutual indications common objects emerge-objects that have the same meaning for a given set of people and are seen in the same manner by them (Blumer, 1969:11).

The emergence of social objects is an objectification of collective dispositions toward percepts and arises form negotiation regarding lines of possible action. This is joint action, which is the fundamental sphere of analysis for Blumer:

> A joint action, while made up of diverse components acts that enter into its formation, is different from any one

> of them and from their mere aggregation. The joint action has a distinctive character in its own right, a character that lies in the articulation of linkage as apart from what may be articulated or linked. Thus, the joint action may be identified as such and may be spoken of and handled without having to break it down into the separate acts that comprise it (Blumer, 1969:17).

Here Blumer is defining an emergent whole, in symbolic form, with its own unique identity. This is in harmony with Meads ideas concerning sociality and emergent wholes. It describes "sums" greater than their aggregate parts derived from the interaction of situated actors. These are system concepts, but Blumer is using a term he coined, "joint action," which appears in retrospect to be an (political) avoidance of systems concepts.

Blumer (1969:17) notes with respect to joint action that "even though it may be a well-established and repetitive form of social action, each instance of it has to be formed anew." This statement reflects Mead's emphasis on the importance of emergence and its introduction of novelty into all events (Cook, 1993). Social objects are highly mutable and reflect the emerging definition of the situation as it proceeds and transforms through interaction.

> The actor selects, checks, suspends, regroups, and transforms the meanings in the light of the situation in which he is placed and the direction of his action. Accordingly, interpretation should not be regarded as a mere automatic application of established meanings but as a formative process in which meanings are used and revised as instruments for the guidance and formation of action (Blumer, 1969:5).

Several sociologists have understandably rejected the notion

that this is an entirely conscious process and have argued it would be overwhelming if it were self-consciously performed (Reynolds, 1993). People however have habitual modes of action and culturally shared expectations with regard to a range of potential definitions with regard to social objects.

> The preponderant portion of social action in a human society, particularly in a settled society, exists in the form of recurrent patterns of joint action. In most situations in which people act toward one another they have in advance a firm understanding of how to act and of how other people will act. They share common and pre-established meanings of what is expected in the action of the participants, and accordingly each participant is able to guide his own behavior by such meanings. Instances of repetitive and pre-established forms of joint action are so frequent and common that it is easy to understand why scholars have viewed them as the essence or natural form of human group life (Blumer, 1969:17-18).

Although the two above statements may appear to be somewhat contradictory, the point Blumer is striving for is that "New situations are constantly arising within the scope of group life that are problematic and for which existing rules are inadequate" (Blumer, 1969:18). He does not want to convey the impression that habitual action is highly stable over time.

> Repetitive and stable joint action is just as much a result of an interpretive process as is a new form of joint action that is being developed for the first time (Blumer, 1969:18).

Blumer often appears to offer us the over intellectualized actor-like Parsons oversocialized man.

> In this situation, he notes, interprets, and assesses things with which he has to deal in order to act." and "Through such self-interaction he constructs his line of

> action, noting what he wants or what is demanded of him, setting up a goal, judging the possibilities of the situation, and prefiguring his line of action (Blumer, 1969:55).

This seems to somewhat contradict Mead's idea of the spontaneous aspect of the "I" with regard to his conception of self. In his conceptualization of joint action there is very little room for other forces to enter into the decisionmaking process which is portrayed as highly voluntary in each individual. This removes the possibility of deterministic forces sneaking in through unconscious factors. It also contradicts the very concept of situatedness in a system of actors, as Mead would define them. If they are a complex system, and they exist in a complex system, it is unlikely that they could be aware of all factors in play at once. Things are habitualized and routinized in order to handle the large amounts of information that are being processed. For Mead this is the role of the nervous system in higher organisms as he discusses it in The Philosophy of the Present. Mead discusses the unconscious as follows:

> Sophisticated attention picks out and isolates things, but beyond the spot of light we attend to, we work unconsciously. Such unconscious conduct may be highly intelligent, but it is not in a perceptual world. In perception there is a content beyond what sets free the response or the consummation of the act. Thus one can take a meal without perception and still enjoy it (Mead, 1982:141).

The situatedness of the individual in the group, according to Mead, makes one vulnerable on the unconscious level to the attitudes of others.

> Addressing oneself requires having a delayed reaction; it deals with the earlier organization of the act, before it reaches expression or completion. As the individual takes this attitude or group of attitudes there is the emergence of the subconscious. The attitudes of others are the beginnings of their acts, a relationship which in turn marks the behavior of the individual in question (Mead, 1982:149).

Based on Mead's definition of society as a system, this follows inevitably. It should be obvious that Mead has a conceptualization of the subconscious and that it is tied to his ideas regarding percepts, systems, and attitudes. Excluding systems allows one to exclude the emergent adjusting mechanism of the subconscious.

Blumer (1969:58) may argue against the idea of overt coercive social forces in a deterministic sense but acknowledges that "At any one point the participants are confronted by the organized activities of other people into which they have to fit their own acts." This perspective on self-determination agrees with Mead in the following: "The self may be involved in a social process, but the process determines one's behavior only insofar as one takes the attitude of others" (Mead, 1982:166). But Mead goes on to qualify this position:

> The social process shows the means by which the individual comes back to himself and becomes an object to himself. The child answering to his own stimuli acts as others act toward him. The fact that the social group makes uniform demands upon the individual gives his self-consciousness. This is how individuals are controlled through mores. Where the group is closely organized, this power becomes overwhelming and there cannot even be a difference of opinion. But when there is such complete adjustment and integration there is

> stagnation, and the only escape for the individual is to leave the community or group. The power of this is shown in the remorse experienced by criminals of those who have crossed social line (Mead, 1982:147).

Thus the definition of the situation is jointly constructed and to this extent it must by definition be coercive with regard to expectations.

Blumer does not deny the existence of social structure, just the reification of it:

> The differences do not mean, incidentally, that Mead's view rejects the existence of structure in human society. Such a position would be ridiculous (Blumer, 1969:75).

Blumer wishes to deny structure as having any life of its own apart from the actors. Yet this denies the emergence of any level beyond actors in evolution (contrary to Mead-especially if the act is primary) and he has already said that the joint act is an emergent whole. The whole, as we have seen in Mead's definition, determines to some degree the organization of the parts. Blumer's (1969:75) main argument is against rigid determinism arising from reification:

> But their importance does not lie in an alleged determination of action not in an alleged existence as parts of a self-operating societal system (Blumer, 1969:75).

What about reactive systems, what about Mead's objects pushing back, and what about social objects having objective reality (and force)?

Blumer says, "Social interaction is obviously an interaction between people and not between roles," but this

seems to overlook the capacity to totally identify with a role and associated scripts. But he does admit "It is only in highly ritualistic relations that the direction and content of conduct can be explained by roles (Blumer, 1969:75)." But isn't this common? Goffman has made a career out of the ritual of the interaction order.

Blumer's real point is that structure does not cause things but people do-but Mead went further than this. Causality is not a valid issue. Actor and context cannot be truly divided. In this there is no real actor-only acts. This is in fact closer to Goffman. Volition for Mead is not absolute- but relative. The self is a relative emergent. Blumer fights for ghosts in the Meadian world. Mead's theory has a system aspect and a concept of the subconscious that goes beyond Blumer's conceptualization. It is closer to Gergen's (1991) "contextualized self."

Joint Action

Positivists and qualitative researchers as well as theorists have been talking past each other for some time now. Their brilliant and complex arguments against each other have been based on a fundamental set of assumptions which are never addressed and consequently the edifice of their arguments is a vain expenditure of intellectual prowess. The unresolved assumptions which will forever divide these two camps need to be explicated if they are ever

to be addressed and resolve.

A central and ancient on-going debate which best exemplifies this confusion revolves around Blumer's Interactionism and positivist methodologists. Consequently we will explore Blumer's methodology regarding joint action and the reasoning behind it. According to Maines and Morrione (1990), joint action is Blumer's manner of dealing with social phenomena beyond the micro-level.

Joint Action vs Systems

What is Blumer's definition of system and why does he regard it as part of an erroneous perspective. What is joint action and how well does it explicate Mead's perspective? How does it compare to the concept of system.

These appear to be central questions to answer because one of the most important places to integrate the concept of systems is in the area of joint action. This is where it has the most promise in advancing Interactionist theory as it will allow for greater considerations of power, coercion, and unanticipated consequences.

Maines (1992) as well as Maines and Morrione (1990) claim that joint action is Blumer's method for dealing with social structure or macro sociology. They emphasize that Blumer strives for an otologically correct perspective, but they fail to explicate the meaning of that statement. This is a fundamental point of confusion for all involved. Maines

and Morrione (1990) emphasize Blumer's interpretation of society as a framework, but this is not Mead's perspective (It is important to note their recognizing the similarity in some aspect of Blumer's idea to chaos theory. The reason these perspective dovetail so neatly is because Mead based his perspective on Relativity!). Having considered their comment on this perspective, we will inspect Blumer's own comments on the subject. We will further consider how effective it is and why the addition of the concept of systems would improve this position.

To fully understand Blumer's discussion on joint action one must have a good understanding of Mead. Blumer transformed the essence of Meads perspective into method, but failed to import the important concept of system. This would have made his efforts at macro sociology more accessible to the understanding of positivists in conjunction with a detailed explanation of Mead's use of the concept and how it extends into social research. It may be that Blumer felt any discussion of system would be interpreted in a structural-functionalist vein and SI would be confused or co-opted. The definition of system that Blumer employs is not Mead's, but Parson's.

Blumer opens his discussion of joint action by reminding us that: "Mead saw joint action, or the social act, as the distinguishing characteristic of society" (1969:70). It is consequently a fundamental unit of analysis for Blumer. It

can be applied to the negotiated action between two people or two corporations. For this reason the unit of analysis within his analytic process does not vary with macro or micro structures (Blumer, 1969). The same analytic concepts are employed in the same manner for all levels of social analysis. Examples of joint action he offers range from family dinner to a war.

Blumer says with respect to the family dinner that, "Each participant necessarily occupies a different position, acts from that position, and engages in a separate and distinctive act" (1969:70). It is the fitting together of acts under the shared definition of family dinner that makes family dinner the unit of analysis (joint action). Participants are related in a network defined by their shared definition of what is happening. This coincides with the idea that "a set of elements" defining "a network of relations" constitutes a system.

For Blumer the coming together of acts "or alignment" is not mechanical, actors actively interpret and define what is taking place. This is consistent with Mead's argument that the manipulatory phase of human action mediates stimulus and response.

The identity of the act is a key to the interpretation and orientation for the actor to decide on his behavior. Blumer never acknowledges the fact that much of this process is not conscious, it is habitual. The habitual aspect then

becomes constraining and coercive. As we have noted, for Mead actors are not always aware their interpretive process. This is also reflected in the fact that the "I" is innovative, creative, and emergent. Definitions, such as labels, define the limits of lines of action to be considered.

> Usually, the course of a joint action is outlined in advance by the fact that the participants make a common identification of it; this makes for regularity, stability, and repetitiveness of joint action (Blumer, 1969:72).

According to Mead, this is a process that defines the subconscious arena of activity as mentioned above.

Having defined joint action, Blumer explains its importance with respect to the analytic process.

1. "It acknowledges that society resides in action which is an on going process and "not in a posited structure of relations" (1969:71). Blumer's statements that we are dealing with frameworks, networks, and situatedness indicates we are dealing with situated relations, defined by their position in the network. These assertions could not be more systems in quality.

2. "Without action, any structure of relations between people is meaningless" (1969:71). Action should be the focus of interest, not structure as positions. This agrees with Mead's assertion that sociality generates action which defines actors in any system (Mead uses the concept of system). Actors are defined by their active relating.

Blumer also argues next,

> such action has to be seen and treated, not by tracing the separate lines of action of the participants-whether the participants be single individuals, collectivities, or organizations-but in terms of the joint action into which the separate lines of action fit and merge (Blumer, 1969:71).

This is consistent with Meads assertion that relations of actors constitute a whole and those relations must be regarded with respect to that whole.

3. Each joint action has a career and history. Uniqueness of local context and individual systems is acknowledged as well as complexity of each variable under consideration.

4. The career or history of a joint act is orderly, fixed, and repetitious ie a pattern. Participants frequently define situations together in habitual patterns. Mead was especially sensitive to the habitual nature of action.

5. At this point Blumer details why joint acts are subject to considerable uncertainty and very difficult to predict. These reasons give support to and reflect Maines' and Morroine's (1990) statement that this is found in Chaos theory. In systems terms, we could simply say that intersystem connections as well as local system connections are so dense in number that these joint actions are rich in fuzzy entropy or rich in potential meanings. Emergence dominates these joint actions because:

a. They have to be initiated by someone (although this does not acknowledge the power of habit and ritual)

b. They can be interrupted, abandoned, or transformed

c. No common definition may emerge

d. Actor definitions may vary widely, confusing interaction

e. New elements may call for innovation

f. Alternative information enters the situation

In his discussion of method Blumer (1969:17) reflects Mead's concern with wholeness of the act, "The joint action has a distinctive characteristic in its own right, a character that lies in the articulation or linkage as apart from what may be articulated or linked." This is a veiled way of saying the sum is greater than the parts, which is a system concept, and it "may be spoken of and handled without having to break it down into the separate acts that comprise it" (Blumer, 1969:17). This is a clumsy description of Mead's perspective which avoids politically incorrect terms.

Structurally Determined Action

Blumer (1969) observes that psychology and sociology see action as determined by outside forces and individuals as passive. This is his main complaint. He fails to indicate that this is a natural consequence of positivist perspective which biases our cultural ontology. He instead complains that behavior cannot be accounted for by ego demands, attitudes, role requirements, values, status expectations, or structural stresses alone. Within a few paragraphs however, he acknowledges the importance of these ideas.

Blumer (1969:73) emphasizes the need to account for the manipulatory phase of human behavior more fully and efforts "to study and explain social action should respect and accommodate these features." It is necessary to understand actors definitions of situations and taking the role of the other as the most effective tool for this, as calculations would be too complex. Blumer further complains that outside observers may distort the actors perspective to the point of obscuring the actors perspective.

Blumer wishes to reverse the emphasis that determinative positivist bias interjects into social analysis. Social action is not divided between conformity and deviance. This is too simplistic. In fact, as mentioned in the previous chapter, Blumer acknowledges the constraining aspect of society:

At this point we uncover Blumer definition of systems: "But their importance does not lie in an alleged determination of action not in an alleged existence as parts of a self-operating societal system" (Blumer, 1969:75). Blumer is defining a mechanistic determinative closed system. This usually posits the elements as independent of the system. This is like Parson's and Pareto's definition of system. Society emerges from joint action and "not the requirements of a system" and "it sees society not as a system, whether in the form of a static, moving, or whatever kind of equilibrium, . . ."(Blumer, 1969:75). This

equilibrium concept is also unique to Parsons and his misapprehension of systems theory. Clearly Blumer is reacting to a concept of systems that is Parsonian and not Median. From the earlier discussion on systems this should be clear.

> Society, in Blumer's perspective, is seen
>
> as a vast number of occurring joint actions, many closely linked, many not at all, many prefigured and repetitious, others being carved out in new directions, and all being pursued to serve the purposes of the participants (well not always. . .(Blumer, 1969:75).

Mead's definition of system is more profound and refined than the idea of joint action, and there is no need other than political (as often is the case) to create a new term or vocabulary.

I have demonstrated that Blumer left out many important ideas of Mead's, but in fairness to Blumer it is necessary to acknowledge his awareness of this himself.

> In closing I wish to say that my presentation has necessarily skipped much in Mead's scheme that is of great significance. Further, I have not sought to demonstrate the validity of his analysis (Blumer, 1969:77).

The Value of Adding Systems

Mead utilized the concept of system in his original formulation of how sociality emerges as an act. Mead uses the concept of two systems to explain phenomena at many different levels and clearly considered it central to his overall perspective (Miller, 1973). Central to the concept

of systems is the reflexivity involved between elements or actors. Both sociality and constraint are present as ontological factors generating identity. Elements shape their action and hence their identity using the system as a whole to guide them. This is also the essence of joint action. Joint action however communicates a sense of aggregation where forces act in only one direction, and this is something Mead clearly was against. Elements of a system shape each other through the system as a whole. The whole/part relationship as we have seen is fundamental to Mead's perspective. It is the absence of conceptual emphasis on this point that alienates many sociologists.

Although Blumer translated Mead with considerable accuracy, we should not consider this the final word, unless we are interested in dogma or religion. The evidence is quite clear for those willing to read Mead in the original. Blumer himself acknowledges there is more in Mead than he has used. System is a concept of great heuristic value that has been omitted from the interactionist tradition. Cooley's abundant use of the term also supports this notion, and Mead in his eulogy to Cooley explained that his main dispute with him was concerning his solipsistic stance (Cooley, 1956). He said nothing about a problem with Cooley's systems perspective.

The addition of this concept of system is important to reintroduce because it allows for greater clarity and

efficiency in analysis:

1. It would clarify actor/environment distinctions.
2. It would articulates more finely the connection points in networks/frameworks.
3. It would provide for a more extensive and consistent frame work to link analytic studies (cumulativity).
4. It would account for coerciveness more clearly and in accurate terms.
5. It would renders SI better able to incorporate power differentials.
6. It would clarify the observers position in the framework
7. It would make the perspective more accessible to positivists (Increases interactivity of perspective).
8. It would makes it easier to integrate more fully additional aspects of Meads perspective.
9. Its acknowledgement opens the door to further refinements in light of Mead's theories (not to mention Cooley, Dewey, and James).

It is from this position that I argue for further interpretations of Mead's work and refinement of Blumer's interpretation and implementation. As Johnson and Schifflett (1992:43) note: "No one can ever report finally on what Mead would say." The arguments of McPhail and Rextroat attempt to disqualify Blumer entirely, which would seem to be counterproductive, if not folly. Their argument in combination with others does indicate how frustrated

positivists are with Blumer's perspective. Mead's ideas are very compelling, yet Blumer's methodology and historical association seems to deny access in theory experimental approaches.

To argue that Mead had an explicit positivist research methodology is an interesting strategy, but fails on an epistemological level besides being insupportable in terms of a review of his writings as both Blumer and Johnson and Shifflett note. Johnson and Shifflett (1992:40) are well supported in their statement that "Mead studied science not as a research methodologist, nor even as a philosopher of science in the Vienna tradition." Mead's interest in science was in relation to his philosophical agenda. Both Cook (1993) and Miller (1973) indicate that Mead was interested in the ontological construction of reality and he wanted to use the latest findings in physics as guideposts and referents in his analysis. To this end he took courses in Relativity and studied Whitehead.

Mead was not a methodologist nor a sociologist. Blumer was not a philosopher. There are bound to be some errors in the translation, but it is doubtful that they would be so profound as to disqualify Blumer in view of his tutelage. It is my thesis that Blumer's perspective is fundamentally correct, but limited in extent due to political battles within the discipline as well as Blumer's lack of training in philosophy and physics.

Why Did Blumer Omit Systems?

Sciulli (1988) notes that Parson's is Blumer's negative straw man and acknowledges that he had a very poor understanding of Parson's perspective. Structural functionalism was closely identified with systems theory and we established above that it was Parson's version of systems theory that Blumer felt he opposed. Rose has noted many similarities between Parson's and Blumer according to Reynolds (1993). It is also true that Turner(1974) claimed that they had very similar perspectives. Blumer was clearly concerned that his perspective would be confused with Parson's which we have identified as positivist in terms of basic assumptions. Sciulli (1988:65) argues "Blumer's sociology challenges paradigms based on a distinction between subjective and objective that assign primacy to the objective dimension;... "

I believe it would not be unreasonable to suggest that Blumer felt that the use of the concept of system would overidentify him with the prevailing perspective which umbrellaed the positivist methodology. Blumer was offering an alternative method of derivation and verification of theory. "His conception challenges sociological positivists who would limit that which is empirical to that which is quantifiable" (Sciulli, 1988:61). Generating his own term would give him greater control over the development of methodology. Durkheim had made this strategy quite clear in

his "Rules" (Durkheim, 1950).

The omission of the systems element from Mead's thought may have left the hole in Blumer's perspective and method which has been justifiably critiqued by other theorists. In his analysis Reynolds (1993) cogently argues that it is the astructural bias in the interactionist perspective has lead to an inadequate approach to macro-structure resulting in the apolitical and ahistorical aspects in SI. This dissertation argues that the astructural bias is a consequence of SI's grounding in the new physics in combination with incomplete explication of Mead's philosophy in sociology. A systems component (which was omitted) added to SI could go a long way toward addressing this problem.

In this chapter we have considered the Blumer's perspective in relation to systems theory and method. We attempted to clarify where Blumer's concept of joint action distorted Mead's perspective and why Blumer may have chosen his particular position. We have briefly stated what the addition of systems might do for Interactionist theory. In the next chapter I will attempt to roughly sketch the contours of a new slightly adjusted version of interactionism and what its implications might be.

CHAPTER 8

CONCLUSION

Blumer was keenly aware that perspective and method go hand and hand. Sciulli (1988:61) reports that he argued that "positivism in sociology actually misapprehends and distorts empirical reality." I believe we have found strong support for that position up to this point. The question remains as to how we would expand Interactionism through systems without falling into the positivist position. What follows is a rough outline of what might be done and how it may be rationalized. The result would be a more comprehensive perspective with greater descriptive and explanatory power.

Wittgenstein had established the idea of "general language" and argued that no formal meta-language could be logically established. He was therefore skeptical of Carnap's work. Carnap, however, came to the conclusion that the formal language he was proposing was a sub-language of general language. This type of specialized language often develops around crafts of various types. If we accept Lundberg's analysis that general language is objective, as in agreement with Mead's argument that perspectives are objective, then positivism is merely attempting to develop a

sub-language which is no more objective than general language. Carnap's efforts with this language may have failed because the relation between sub-languages and general language may be too complex to map.

I think it would be foolish to disqualify formal languages and mathematics as valuable to sociology, they are a special tool of great value in carefully defined contexts. However, to attempt to entirely capture social phenomena through their deployment is not only philosophically unsound, but consistently proved to be a disastrous failure throughout the entire century. I suggest that certain categories of variables can be legitimately analyzed utilizing this approach. Even then they may require qualitative reinforcement. Quantitative analysis is a sub-domain of qualitative analysis which utilizes a richer and more interactive set of research concepts with regard to sociological data.

I believe Blumer has established a very sound program in general with regard to method. His general thrust is to employ generic ideas in local contexts to uncover patterns of interaction. By continually comparing concepts that emerge from a dialectic between generic concepts and observation, concepts which capture the local meanings emerge which can be related to the general language system. Through this procedure a full empirical description of local patterns of social phenomena is produced. Explanations can then be

abstracted once the description is imported back into the general language context.

The weakness in Blumer's method I argued in the last chapter was in the generic concept of joint action. It is insufficient to produce a rich account of action within local contexts. It tends to isolate local contexts too much from other contexts. No context survives in a vacuum. By considering contexts and actors as systems we can trace coercive elements that enter contexts both consciously and unconsciously. I believe it was these unconscious coercive aspects of everyday life that Goffman so thoroughly reported on. Altercasting as a generic concept also takes on a new dimension from this point of view.

Damasio (1994) has produce strong evidence from a neurophysiological perspective that decisionmaking is dependent on somatic markers. These markers provide an emotional valence to ideas and images in the mind. Decision-making then is dependent on emotion and emotions should be taken into account if we are to have a full empirical report. There is strong evidence that much of emotional phenomena works to a large extent on a subconscious level, mainly because it is non-rational due to its association with older brain structures.

Attitudes, emotions, and habitual behavior may all have profound effects on the individual through group dispositions and gestures as they unfold in the interaction context on

non-conscious level. Not only may attitudes and beliefs be coercive as Mead discusses, but also emotions. As a consequence we would also require an account of the researchers ongoing emotions and what part they played in his analysis.

Mead's actual position on this is very similar to Weber's Verstehen sociology. As Parkin (1982) notes, a totally conscious actor taking into account all action would not be capable of false-consciousness in the Marxist sense. Coercion in a symbolic universe frequently comes on a non-conscious level. Weber remarks: "In the great majority of cases actual action goes on in a state of articulate half-consciousness or actual unconsciousness of its subjective meaning" (Weber in Parkin, 1982:27). It is important to also note the similarities between Weber's method of ideal types and Blumer's generic sensitizing concepts. Weber, unlike Blumer, was willing to explicitly concede the place of mathematics as a sub-domain of Verstehen analysis.

In an analysis of local contexts we should attempt to be clear on what percentage of an emergent social object is derived from outside the interaction system. What other systems are exporting information and bias into the context we are observing? Are actors consciously aware of these other influences and in what form does that awareness come. To what degree would their options change if their awareness of these influences changed? Are they acting in "bad faith"

or "false consciousness?" Systems theory offers sensitizing guides with regard to the situatedness of local concepts and how they might be connected to other contexts.

In this research scenario generalization is always at the expense of local accuracy. Mead himself made a great point of this (Miller, 1973). As a consequence descriptions imported into general language for analysis could be abstracted to derive patterns for later comparisons with other investigations. Cumulativity could be utilized to determine if general patterns emerge from findings in similar categories of contexts. This is much along the same lines suggested by Lincoln and Guba with their concept of transferability. This is not to be confused with positivist ideas regarding general laws and axioms. The limitations of these general patterns could perhaps be qualified with an emotional valence.

There is a corresponding mathematics to support Blumer's method in Fuzzy logic systems. Fuzzy systems theory (Multivalent systems theory) is associated with chaos theory (Kosko, 1993). Fuzzy sets have percentages of qualities present and not present and define variables with higher numbers of features than binary system analysis affords. A set "house" may include cardboard box or mansion and everything in between. The greater the density of the set, the greater its fuzzy entropy. Fuzzy sets may be employed with simple generic rules in local contexts to grow local

rules for prediction and control of local phenomena. This approach is employed as a technology at present to control physical systems too complex for traditional logic systems. It is based on a sound mathematics derived from Russell's paradoxes (Kosko, 1993).

The point I am making is not that Blumer's perspective should be mathematicized, only that there is support for the logic of his approach in an existing mathematical system. This approach, however, requires computer hardware to operate and is limited in scope. I would suggest that the human bio-computer is more capable of analyzing local rules. The other problem is that fuzzy rules grown locally won't apply elsewhere. It is an interesting paradox that increased local control means decreased general knowledge. The explanation of multiple contexts is latent in the generic rules themselves. With regard to social phenomena, general laws of the kind developed with physical phenomena may not be practical or ethically desirable.

This is a brief outline attempt to only roughly describe the contours of the alteration I am proposing. The exact details would constitute another effort of the same magnitude as the present. I have tried to provide an in depth rational for pursuing sociology from a perspective grounded in the sociological tradition, anchored by physics, and carefully reasoned from a philosophical perspective. Through the use of an interactionist emergent systems perspective, elements

of non-conscious interaction, solidarity, and coercion could be included in interactionist analysis in a manner which would reflect a dimension of social experience it has not yet addressed. Room would be made to employ mathematics in a limited domain, instead of dogmatic decrees that math has no place in sociology. Most important of all, is the idea of doing sociology primarily from the perspective of meaning, as conceived of in dramaturgy ie "emerges out of a behavioral consensus between human beings" (Brissett & Edgley, 1990:2), with positivistic approaches operating as a sub-domain and not the reverse as is the situation today. Mead has given us a clear explanation of meaning. We could build a better sociology on that foundation.

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