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THE DIVINATION OF TRUTH

***From The Quest for Certainty
To A Search for a Method***

by

George Dourdounãs

**A dissertation submitted to the Graduate Faculty in Sociology
in partial fulfillment of the requirements for the degree of
Doctor of Philosophy, The City University of New York**

1997

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This manuscript has been read and accepted for the Graduate Faculty in Sociology in satisfaction of the dissertation requirement for the degree of Doctor of Philosophy.

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THE CITY UNIVERSITY OF NEW YORK

*A probable impossibility is to be preferred
to an improbable possibility - Aristotle*

in vino veritas - anonymous

ABSTRACT***THE DIVINATION OF TRUTH***

*From The Quest for Certainty
To A Search for a Method*

by

George Dourdounās

Adviser: Professor Stanley Aronowitz

The position is taken, throughout the course of the dissertation, that a false distinction is oftentimes made between the natural and the social sciences. This position is supported by an historical inference, if not through the historical recognition, that since the Age of Enlightenment, at which time the scriptural argument concerning the immutability of the heavens was irrevocably challenged, the scientific enterprise has perhaps not so paradoxically reverted from the Aristotelian project of the classification and explanation of Existence, to the Platonic project of unification and the description of Being. It is argued that through the continued acceptance of the Platonic distinction between the primary and secondary qualities of nature, a non-critical scientific community has tacitly accepted the notion that a fundamental ontological distinction exists between the objective world of natural science, and the subjective world of social science. This almost universally accepted misconception, that there is a fundamental ontological difference between nature and society, has led, throughout the past two

and a half centuries, to the desiderata for a purification of the social sciences. But it has also led, within the natural sciences, to an obscuration of the limits of human knowledge.

In order to demonstrate how it was that along with the development of a probabilistic understanding of truth and the concomitant mathematization of probabilism, statistical mechanics and probability theory subsumed the natural as well as the social sciences, the dissertation is divided into four chapters. In the first chapter, the historical background to the sociology of science and knowledge is presented as a prolegomenon to a cultural explanation for the mathematization of knowledge and truth. In the second chapter, the trial of Galileo is discussed within the context of a philosophy of ideas, as a doctrine of probabilism is quantified in the quest for certainty and the elimination of the possibility of human error. The various theoretical and logical formulations of probability theory are examined in detail in the third and fourth chapters, in order to explicate the pluralistic nature of the scientific project and the singular imperative for its democratization.

To Silvia Torres-Saillant
and
George Dourdounas-Vasileo

TABLE OF CONTENTS

CHAPTER ONE: EPISTEMOLOGICAL CONSIDERATIONS	1
The Problem of Order	1
A Probabilistic Solution	7
The Mathematization of Knowledge	13
The Quest for Certainty	14
Toward A Sociology of Scientific Knowledge	18
A Search for a Method	30
Epistemological Considerations	35
An Alternative Conceptualization	38
CHAPTER TWO: METHODOLOGICAL IMPLICATIONS	42
The Crime of Galileo	45
Toward a Philosophy of Ideas	60
A Theological Undertaking	76
CHAPTER THREE: THEORETICAL FORMULATIONS	84
Une Science d'Ignorant	84
The Beginnings of Modern Day Probability Theory	89
The Classical Approach	93
The Analogy of Probability	100
The Problem of Induction	106
The Frequency Definition	118
CHAPTER FOUR: LOGICAL RAMIFICATIONS	133
A Contest of the Faculties	133
The Logical Interpretation of Probability Theory	148
The Propensity Interpretation	160
A Sociological Reinterpretation	169
Implications for Further Research	175
SELECTED BIBLIOGRAPHY	179
Epistemological Considerations	179
Methodological Implications	198
Theoretical Formulations	205
Logical Ramifications	212

LIST OF TABLES

Table 1. Stylometric Ordering of Authenticated Platonic Dialogues in Three General Chronological Periods	66
Table 2. The Divided Line	71

LIST OF ILLUSTRATIONS

Illustration 1. Histogram of the Binomial Distribution, With the Parameters $n=6$ and $p=\frac{1}{2}$	125
Illustration 2. Pascal's Triangle	126

EPIGRAPH

Science, one might be tempted to say at times, is nothing but enlightened and responsible common sense -- common sense broadened by imaginative critical thinking. But it is more. It represents our wish to know, our hope of emancipating ourselves from ignorance and narrow-mindedness, from fear and superstition. And this includes the ignorance of the expert, the narrow-mindedness of the specialist, the fear of being proved wrong, or of being proved 'inexact', or having failed to prove or to justify our case. And it includes the superstitious belief in the authority of science itself (or in the authority of 'inductive procedures' or 'skills').

The nuclear bomb (and possibly also the so-called 'peaceful use of atomic energy' whose consequences may be even worse in the long run) have, I think, shown us the shallowness of the worship of science as an 'instrument' of our 'command over nature' or the 'control of the physical environment': it has shown us that this command, this control, is apt to be self-defeating, and apt to enslave us rather than to make us free - if it does not do away with us altogether. And while knowledge is worth dying for, power is not. (Knowledge is one of the few things that are worth dying for, together with liberty, love, kindness, and helping those who are in need of help.)

All this may be trite. But it has to be said from time to time. The First World War destroyed not only the commonwealth of learning; it very nearly destroyed science and the tradition of rationalism. For it made science technical, instrumental. It led to increased specialization and it estranged science from what ought to be its true users -- the amateur, the lover of wisdom, the ordinary, responsible citizen who has a wish to know. All this was made much worse by the Second World War and the bomb. This is why these things have to be said again. For our Atlantic democracies cannot live without science. Their most fundamental value -- apart from helping to reduce suffering -- is truth. They cannot live if we let the tradition of rationalism decay. But what we can learn from science is that truth is hard to come by; that it is the result of untold defeats, of heartbreaking endeavour, of sleepless nights. This is one of the great messages of science, and I do not think that we can do without it.

But it is just this message which modern specialization and organized research threatens to undermine; and the reemergence of Bacon's naive views of induction -- of the belief that science is the collection and tabulation of instance, and especially of confirming instances -- must be combated by those who believe in human reason.

Karl R. Popper (1983, p. 260)

CHAPTER ONE

EPISTEMOLOGICAL CONSIDERATIONS

The Problem of Order

'Who now reads Parsons? It is difficult for us to realize how great a stir he has made in the world.... He was the author of a once monumental but now archaic work of modernity, which he called *The Structure of Social Action*. Professor Parson's magnum opus may be likened to the death certificate of positivistic-utilitarianism. *Dead by suicide at the hands of person or persons unknown*. But we need not agree with his verdict that scientific theory has evolved beyond the intellectual tradition of Herbert Spencer. Neither must we agree with his formulation of scientific progress based upon systematic theoretical thinking. His work has betrayed him. We have deconstructed his *Structure*.¹ It was there that he wrote how the problem of order, as first posed by Thomas Hobbes in *Leviathan*, (1651), constitutes the most fundamental empirical difficulty of utilitarian thought. (Parsons, 1937, p. 91).

¹ See Talcott Parsons, *The Structure of Social Action*, 1937, p. 3, for the paradigmatic reference to Crane Brinton, *English Political Thought in the Nineteenth Century*, 1933, pp. 226-227, wherein the contemporary irrelevance of the sociology of Herbert Spencer was first so metaphorically proclaimed. See Stillman Drake, *Discoveries and Opinions of Galileo*, 1957, p.1 for a counter reference to Enrico Persico, *Galileo e la fisica* 1942, cf. Leopoldi, *Parini's Discourse on Glory*, ch 11.

Despite the insistence of a positive science that knowledge is cumulative, and despite its insistence that knowledge is progressive, creating order out of disorder continues to justify the historical as well as the scientific project. Order and Progress: the union of these two conditions being asserted by August Comte as the *chief feature of a positive social science*; (Comte, 1830-42, in Lenzer, 1975, p. 197); *order as manifested forth by progress, [and] progress as nothing but the gradual development of order*; order being defined as the *basis of social life*, and progress as its end. (Idem, 1851-1854, in Ibid., p. 391) But if order and progress continues to be the motto for a positive philosophy, chaos and anarchy continues to be its generally accepted assumption concerning the nature of the universe. Somehow, it is posited or implicitly assumed that without the continual intercession of human activity, chaos would reign supreme, but for those inexorable laws of nature that perhaps when in conflict with the hubris of humanity assert themselves as plagues, floods, or as Thomas Hobbes and his deterministic followers would have it, a continual state of war.

Even Karl Marx, with a dialectical method and a revolutionary message, was as influenced by his own time as much as anyone else. He sought after the *natural laws of capitalist society ... to lay bare the economic law of motion of modern society*. (Marx, 1867, pp. 8,10). As such, for Marx, who correctly, I might add, refused to separate humanity from the rest of nature, in spite of the uniquely human capacity for

invention, as well as for so many natural philosophers, who came before and after, those inexorable laws of nature applied to the social arena as well. But suppose there are no laws of nature. Suppose there are only statistical regularities. Such was the sentiment of Max Planck, (1954, p. 78), and a physics community that for almost a century has been able to distinguish between the simple deterministic laws of Newtonian physics, and the probabilistic nature of thermodynamics, physics, and now, even biology. That the discoverer from one perspective, and the inventor from another, of Planck's constant, would take such an empirically pragmatic position requires no explanation. That such a position would necessitate a logical reinterpretation of those historical applications that have been discovered from one perspective, but invented from another, requires no argument. For if there are no laws of nature, but only statistical regularities, then Marx's formulation need only be read backwards for its import to continue to obtain. Rather than searching for those laws of motion that would pertain to the social arena, it would do as well to search within the social arena for those laws of motion.

In his *Cours de Philosophie Positive*, (1830-42), August Comte states that its principle aim is to establish the science of social physics. (Comte, 1830-42, in Martineau, 1853, p. 7; Lenzer, 1975, p. 77). This was to be accomplished through the observation, subsequent experimentation, and comparison of social phenomena, similar to the manner in which

physical, astronomical, chemical, and physiological phenomena were utilized in the establishment of the sciences of mathematics, astronomy, chemistry, and biology. (Idem., 1855, pp. 473-85). For Comte, *le physique social* was to be the dominant science in his system of positive philosophy; it also enjoyed the additional advantage of being able to document what today might be called cultural evolution, or what he described as the three stages of societal development, ie., the theological stage, the metaphysical stage, and the scientific or positivistic stage, which correspond, in the Comtean schema, to the three stages of life, ie., youth, adolescence and maturity. But Comte was unequivocal in his position concerning the use of mathematics in the description of social phenomena, or what he called *the pretension of some geometers to render social investigations positive by subjecting them to a fanciful mathematical theory of chances*. (Ibid., p. 492). He goes on to draw an analogy between geometers and biologists who would *make sociology to be a corollary or appendix to their own science by suppressing the function of historical analysis*. (Ibid.). But for Comte, it is the level of abstraction of mathematics itself which displaces the analysis from its proper object of study, despite what could very well be legitimate positivistic intentions:

It was James Bernoulli who first conceived the notion; and the notion affords evidence of the nascent need to subject social theories to some kind of positivity. None but a high order of mind could have so early felt the need; and if the expedient was vicious, there was no better way discernible by any possibility at that time. The error was much less pardonable when the notion was reproduced by

Condorcet, in a more direct and systematic way; and his expectation from it, as manifested in his celebrated posthumous work, shows the fluctuating state of his mind in regard to the primary conception of social science. But there is no excuse for Laplace's repetition of such a philosophical mistake, at a time when the general human mind had begun to discern the true spirit of political philosophy, prepared as it was for the disclosure by the labors of Montesquieu, and Condorcet himself, and powerfully stimulated besides by a new convulsion of society." (Comte, 1855/1974, pp. 492-93).

Comte is aware of the profound influence of the French revolution on social, political, and moral philosophy. The need to articulate general principles that might facilitate the reestablishment of some kind of order, in a crumbling and chaotic social system, was particularly felt by that growing cadre of intellectuals which was able to influence the great monarchs and rulers of the time. As such, Helvetius dedicates his *Treatise on Man*, (1777), to Catherine II of Russia and Frederick the Great of Prussia. Those principles of order and progress, so integral to Comte's system of positive philosophy, acquire a heightened signification, when viewed from the perspective of the *old regime*. And yet to infer simple causal connections between a crumbling social order and the positive philosophy would be to impose an indefensible interpretation on historical reality. Indeed, these very same principles of the positive philosophy were easily adapted to the new social order, as well. It was, after all, Thomas Jefferson, who translated Destutt de Tracy's *Commentary and Review*, (1811), of Montesquieu's *Spirit of the Laws*, (1748), into English; and posterity is aware of Jefferson's egalitarian position on liberty, equality, and fraternity, as well as his own life,

liberty, and pursuit of happiness. No such simple causal connections need be inferred, although it is possible, to conjure up the spirits of the past, to provide legitimacy for whatever history is being made. As such, it is undeniable that the positive philosophy of August Comte is easily adapted to contradictory if not conflicting social systems. But what does remain a constant, is Comte's opposition to mathematical realism and the concomitant reduction of social, political, and moral philosophy, to the social physics that were to develop in the wake of the French revolution, and continue to follow in the mathematical tradition of James Bernoulli:

From that time a succession of imitators has gone on repeating the fancy, in heavy algebraic language, without adding anything new, abusing the credit which justly belongs to the mathematical spirit; so that, instead of being, as it was a century ago, a token of a premature instinct of scientific investigation, this error is now only an involuntary testimony to the absolute impotence of the political philosophy that would employ it. It is impossible to conceive of a more irrational conception than that which takes for its basis or for its operative method a supposed mathematical theory, in which, signs being taken for ideas, we submit numerical probability to calculation, which amounts to the same thing as offering our own ignorance as the natural measure of the degree of probability of various opinions. While true mathematical theories have made great progress, for a century past, this absurd doctrine has undergone no improvement, except in some matters of abstract calculation which it has given rise to. It still abides in the midst of its circle of original errors, while mankind are learning, more and more, that the strongest proof of the reality of speculation in any science whatever is the fruitfulness of the conceptions belonging to it. (Ibid.).

But the absurd doctrine of submitting numerical probability to calculation and suppressing the function of historical analysis has also made great progress. This progress is manifested throughout all of the sciences, natural as well as

social; and yet Comte's fundamental objections remain unanswered, at least from within the framework of a sociology of science. The suppression of historical analysis and the subjection of social investigation to a mathematical theory of chance has become the dominant paradigm for the social sciences, in the same manner as the suppression of historical analysis and the subjection of natural investigation to the same mathematical theory of chance has become the dominant paradigm for the natural sciences. Over a century ago, Francis Galton, (1889/1973, p. 66), observed how *whenever a large sample of chaotic elements are taken in hand and marshalled in the order of their magnitude, an unsuspected and most beautiful form of regularity proves to have been latent all along.* The structural-functionalist schools of Emile Durkheim (1893), Talcott Parsons (1937), and perhaps even Richard Herrnstein and Charles Murray (1994), continue to flourish within a doctrine of chance that ultimately asserts its own tautological necessity with its insistence that out of chaos must come order.

A Probabilistic Solution

In anticipation of the 1982-1983 academic year, at which time a host of international scholars would be gathered at the University of Bielefeld, West Germany, in order to study what since then has been called the *Probabilistic Revolution*, the Center for Interdisciplinary Research, (Zentrum für interdisziplinäre Forschung, or Zif), sponsored a preparatory conference, during the summer of 1981, from July 2-5. The

papers that were presented at that preparatory conference were published by the Center in 1982, as 22 *REPORT* of the Wissenschaftsforschung Science Studies, under the title, *Probability and Conceptual Change in Scientific Thought*, edited by Michael Heidelberger and Lorenz Krüger. At the end of the following summer, soon after many of the participants in the year long study had first arrived, an opening conference was held, from September 16-20, 1982. The papers that were presented at that opening conference were published by the Center in 1983, as 25 *REPORT* of the Wissenschaftsforschung Science Studies, under the title, *Probability Since 1800: Interdisciplinary Studies of Scientific Development*, edited by Michael Heidelberger, Lorenz Krüger, and Rosemarie Rheinwald. It is interesting to note, and a testimony to the success of the year long project, that it was the final paper of 25 *REPORT*, "Was There a Probabilistic Revolution, 1800-1930?" by Ian Hacking, that also served as an introductory paper for *The Probabilistic Revolution, Volume I: Ideas in History*, (1987), edited by Lorenz Krüger, Lorraine J. Daston, and Michael Heidelberger, and *Volume II: Ideas in Science*, (1987a), edited by Lorenz Krüger, Gerd Gigerenzer, and Mary S. Morgan. One final publication, *The Empire of Chance: How probability changed science and everyday life*, (1989), by Gerd Gigerenzer, Zeno Swijtink, Theodore Porter, Lorraine Daston, John Beatty, and Lorenz Krüger completes the publication history of this extraordinarily successful research project.

As early as the planning stages of the project, the *probabilistic revolution*, had been defined as an historical process, evidenced by the following characteristics:

- (1) the amalgamation of previously distinct fields: from the theory of games of chance, political arithmetic, actuarial mathematics and the theory of errors in astronomy, to theoretical statistics;
- (2) the growth in applications of probabilistic elements in individual sciences;
- (3) the transition from probability as a measure of partial knowledge or ignorance to probability as a feature of reality;
- (4) epistemological changes through probability theory and statistics;
- (5) the intrusion of probabilistic and statistical thinking in the context of ethical, moral and political arguments.

(Heidelberger and Krüger, p. 3).

That probabilistic thinking has intruded into the context of ethical, moral, and political arguments for well over two millenia, is documented throughout my dissertation, not so much to illustrate the importance of probabilistic thinking, but rather to demonstrate how our understanding of probabilism has varied along with our understandings of truth and knowledge. As such, *The Rise of Statistical Thinking*, as Theodore Porter (1986) has described the phenomena, may also be viewed as the contemporary expression of the ages old *Quest for Certainty* (1929). Probability theory must encompass qualitative elements lost in any such reduction to *The Doctrine of Chances*, as first articulated to its fullest by Abraham De Moivre (1718). Unfortunately, none of the participants at the Bielefeld Project ever questioned, at least from my perspec-

tive, the veracity of the reality claims of either uncertainty or indeterminacy. As such, the historical papers, as insightful, informed, and important as they all might be, with the possible exceptions of Ian Hacking's "Nineteenth Century Cracks in the Concept of Determinism," (1982), Thomas Kuhn's "What are Scientific Revolutions," (1987), and perhaps Bernard Cohen's "Revolutions, Scientific and Probabilistic," (1987), issues of epistemology are relativized to the point of equivocation. I am aware, of course, of the caveat that must be associated with any such grand or sweeping generalization, but even the magisterial *History of Statistics*, by George Stigler, (1986), accepts the measurement of uncertainty as a given, rather than as an heuristic. Notwithstanding unasked questions of epistemology, as they were originally conceived, the objectives of the project were threefold:

(a) To summarize and to extend historical research on the genesis, growth and function of probabilistic and statistical ideas in science. This work should focus not only on the histories of the individual disciplines and the methods they employ, but also analyse the effect of these "histories" on their subject matter, i.e. on man [sic] and society.

(b) To utilise the development of these ideas as a case study for the investigation of scientific development in general. There are reasons for thinking that the investigation of probabilistic methods and theory-construction will disclose interdisciplinary patterns of development as well as the interconnections between theory and applications. Moreover, the mutual interaction between natural and social sciences and their connection with the rapid advances induced by the industrial revolution merit careful analysis. This should bring fresh insights into the 'internal' and 'external' factors determining the origin, development and application of scientific ideas.

(c) The scientific analysis of conceptual and theoretical change, and of the corresponding changes on philosophical outlook, should make a contribution to the philosophy of natural and social science as well as to the metatheoretical aspects of rational action. The transition from strictly deterministic thinking in the 19th Century to a predominately probabilistic and pragmatic approach at the present time is poorly understood. Similarly, the ethical implications of the use of probabilistic and statistical methods in the social sciences and the resulting changes in the scope of their practical applications have been little investigated. A diagnosis of the situation resulting from the shift toward probabilism will help improve on our understanding and our directing of current efforts of research. (Ibid., p. 4).

The first two objectives, I believe, were more than admirably met. It was, after all, the participation in a research seminar given by Thomas Kuhn on the development of statistical physics, in 1974, that Lorenz Krüger credits as the beginning of the prehistory of the Bielefeld Project. (Krüger in Krüger, Gigerenzer, and Morgan, p. xvii). The sociology of scientific knowledge, as first developed by Kuhn, (1962), is discussed in detail in the pages that follow. It is no wonder that the satisfaction of the first two objectives, from a Kuhnian perspective, could be accomplished without the difficulties, if not the impossibilities associated with the incommensurabilities of the third and final objective. Nonetheless, with respect to the third objective, no one connected with the Bielefeld Project ever engages in any detailed or extended discussion of induction, other than to deny its theoretical import, as does Michael Heidelberger, (1987, p. 139). No one even acknowledges that today's probabilism is a form of inductive logic, other than to deny its efficacy, as do Zeno Switjink (1987, p.274ff) and Gerd Gigerenzer, (1987a, p.17ff).

No one discusses the problem of induction as formulated by David Hume, or how it expresses itself and is manifest in any theory of probabilism. No one discusses the fact that whether probabilism is applied to regularities in nature, or regularities in society, it is subject to change, by volition as well as chance. *The Empire of Chance* (1987), awaits contestation. No one explicitly questions the notion of the objectivity of chance. No one mentions the problems of logic, let alone the contest between logic and mathematics. Issues of pragmatism already are ignored in the anthology following the completion of the project. The contest between realism and nominalism need not have been conceded so readily by methodological individualism; but no one discusses the analogy of probability, or even the metaphysical relationship between Bernoulli's law of large numbers and the invention of the normal curve. In the pages that follow, I investigate these themes. I address these issues, not with the expectation that such an undertaking will lead to the solution of any of the problems inherent therein, but the supposition that a better understanding will be gained through their further articulation. Ultimately, I hope to have contributed to the explanation of how it is that the omniscience of God has been replaced by the omni-science of statistics. Such an explanation, however, must lie within the process of the mathematization of knowledge itself, as objected to by Comte, and forgotten for too long.

The Mathematization of Knowledge

When Abraham de Moivre dedicated the first edition of his *The Doctrine of Chances* (1718) to Sir Isaac Newton, it was more than the recognition of an intellectual debt. Whether intentionally or not, it was also an affirmation and perhaps an unstated portent that the whole of natural philosophy, or that area of thought which theretofore described the mutable world, was to be subsumed by those abstract mathematical principles Newton so artfully expounded in his *Philosophiæ Naturalis Principia Mathematica* (1686).² It also signaled the beginning, on the Continent as well as on the British Isles, of the acceptance of acausality in place, not only of the Aristotelian doctrine of first or primary cause, but the Cartesian search for effective or efficient causes, as well. Cartesians continued to criticize the Newtonian system for being *occult*, in that the principle of attraction at a distance replaced the teleological explanation with a non-explanatory description that still presupposed some prime mover to keep the system running. And yet, the Newtonian system also demanded the acceptance of acausality in time,

² Although Newton's magnum opus was quite accurately translated into English in 1729, as *Mathematical Principles of Natural Philosophy*, it is usually referred to simply as *Principia*. The Latin is invoked to assure its continued reverence and prestige, while the subject matter of natural philosophy is long forgotten. The project itself, to describe, not to explain, and to establish the mathematical principles of the physical mechanical world in terms of abstract, general, absolute, universal principles or perhaps even occult fictions, has remained unchallenged by most historians of science.

such that cause and effect could only describe a relationship beyond determination, or what later became known as a co-relationship. Philosophers for centuries had described the language of God as being mathematical, but it was only with the publication of Newton's work that the language itself came to circumscribe the scope and the limits, the range and the conditions, of all legitimate human knowledge.¹ In the course of the next two centuries, the means, indeed, would become the ends. As opposed to those tools which could be used to unlock the secrets of the universe, within another century, mathematics would stand in opposition to its inventors. Such an opposition could only occur when the truth itself was considered to be mathematical. But in order for that to occur, the quest for certainty would first demand the reconciliation of the laws of nature with the will of their Creator.

The Quest for Certainty

The movement of bodies seems to be subject to two essentially different kinds of law: the first are necessary consequences of the idea that we have of matter; the second seem to be the effect of the free will of an Intelligent Being who has willed that the World be as it is rather than any other way ... (Condorcet, 1768; in Keith Baker, 1975, p. 104; and in Silvan Schweber, 1982, p. 333)

For Condorcet, a distinction exists between those subjective laws which are necessary consequences of the way humans view the world, and those objective laws which seem to

¹ See Jonathan Powers, *Philosophy and the New Physics*, (1982); and Jürgen Habermas, *Knowledge and Human Interests* (1971), as well as *Legitimacy Crisis*, (1973), for full discussions on problems of epistemology.

be the effect of an higher intelligence, but also may be viewed simply as the way things are, with or without human intercession. He continues:

Perhaps, there is only a distinction between these laws because, according to the present relationship between the phenomena and ourselves, more or less sagacity is necessary to know them ... (Ibid., p. 333).

Condorcet goes on to explain how an intelligence that knew the state of all phenomena, the laws governing matter, and the effect of these laws, would possess a perfect knowledge of the System of the World, which was beyond the human capacity, albeit the goal of philosophical mathematics. (Ibid., pp. 333-4). But it was up to Pierre Simon, Marquis de Laplace to draw the logical conclusion that since the laws of the universe were fixed by the Divine Will, knowledge of that vast intelligence was perhaps only attainable through an understanding of those laws. (Laplace, 1796/1951, p. 4). Both Laplace and Condorcet were working in the Cartesian tradition, which insisted that the efforts of natural philosophy be directed at understanding *efficient or effectual causes*, rather than *first or final causes*, which because of their divine nature could never be understood by humans. Nevertheless, the inductive method itself proscribes the existence of a deity if no practical distinction is made between first and efficient causes.⁴

⁴ David Hume is explicit in his assertion of a lack of distinction between the two types of causes. See Book I, Part III, Section XIV of *A Treatise of Human Understanding*.

"I have no need of that hypothesis!" Laplace is alleged to have replied to Napoleon, when questioned about the place of God in his five-volume magnum opus, *Celestial Mechanics*. Prior to this publication, which spanned the years from 1793 to 1824, a deity was archetypically invoked, by natural philosophers, to mechanistically explain the regularities of the universe. But through Laplace's development of the method of least squares and the binomial distribution, the infinitesimal approximation of which has come to be known as the normal curve, a *deus ex machina* was no longer necessary to keep the cosmological machine running smoothly. A statistical reformation was taking place in that a mathematical conceptualization, the central limit theorem of the law of large numbers, could account for those seemingly changeless aspects of the world which theretofore, or at least since the time of the pre-socratics, had been accounted for through the presence of gods. Thereafter, a god might still be needed to explain the imposition or the creation of order, but no longer would a god be needed for the maintenance of homogeneity.

It was the work of the great sociologist, oceanographer, and astronomer royal, Adolphe Quételet which spread this mathematical method to social physics, from whence it subsumed physics itself, biology, and finally, all of the sciences. It would take almost another century and the development of regression analysis, before creation itself could be statistically described. For that to occur, however, a number of epistemological shifts, in conjunction with the Age of

Enlightenment, first needed to be accomplished. Along with the separation of church and state came the conscious and deliberate bifurcation of religion and science. Theological arguments of causality were supplanted by scientific assertions of co-relation. Although these arguments denied causality, a new form of determinism came into being, through the utilization of statistical mechanics and the institutionalization of specialized scientific disciplines accompanied by the co-related acceleration of the division of social labour. The tautological aspects of the Laplacean method became obscured by its successes in the development of the natural and the social sciences, and the prepostulated false distinction that exists to this day. The fact that all of science is based upon uncertainty,' has been forgotten but for a small number of science scholars. Of course, a growing number of historians of science have recognized that discipline by discipline, statistical mechanics and probability theory have

' A great deal of confusion surrounds the uncertainty principle. It has often been misinterpreted as an allegory of how when the observer attempts to measure either the position or the momentum of an electron, the act of measuring one necessarily effects the magnitude of the other. As such, an uncertainty exists. This interpretation, however, logically leads to an assumption similar to that of Condorcet above. It would seem that increased precision (sagacity) would decrease our uncertainty. "To call this an 'uncertainty principle' as is usually done implies that the position and momentum actually have values but we can't find out what they are because any attempt to measure them must disturb the system. But Bohr and Heisenberg, the proponents of the so-called 'Copenhagen Interpretation' of quantum mechanics, claimed that these properties cannot be said to have an objective existence. It is only the observation that gives them reality. Thus the term 'indeterminacy principle' is more accurate." (Stephen G. Brush, 1988, p. 20).

subsumed the various research programs. How this was possible is the subject matter of this dissertation. Why it was necessary is a position I hope to clarify along the way.

Toward A Sociology of Scientific Knowledge

In the social production of their existence, men inevitably enter into definite relations, which are independent of their will, namely relations of production appropriate to a given stage in the development of their material forces of production. The totality of these relations of production constitutes the economic structure of society, the real foundation, on which arises a legal and political superstructure and to which correspond definite forms of social consciousness. The mode of production of material life conditions the general process of social, political and intellectual life. It is not the consciousness of men that determines their existence, but their social existence that determines their consciousness.
(Marx, 1859/1970).

Karl Marx's formulation of what over a century later would still be referred to as the base/superstructure argument, although well known in certain intellectual circles, could not be characterized as ever having had much impact on the collective consciousness of the mass audience to which Marx addressed his writings. Historical materialism, as a philosophy to which the above quoted passage was to be a definitive statement, came to be represented as a kind of economic determinism which left no space, time, or need for a concerted effort on the part of the working class to throw off those shackles Marx and Engels so often and so convincingly described. In a sense, much of the work of the First International and the reform versus revolution debate could be seen as a practical restatement of the question as to whether or not any ideology emerging from within a closed system of

capitalist exploitation could serve to accomplish anything but the perpetuation of that system. Despite repeated qualifications on the part of its authors, to correct the oft-quoted narrow interpretation of historical materialism as a kind of economic determinism, and to clarify how it was that in different societies in different epochs people satisfied their basic needs before constructing any grand superstructures, Marxism has failed to reconcile the apparent ambiguity of how it was that revolutionary activity was needed to bring about that which was inevitable.

The Sociology of Knowledge, as first articulated by Max Scheler in the anthology, *Versuche zu einer Soziologie des Wissens* (1924), as 'Probleme einer Soziologie des Wissens,' and later expanded and included in his *Die Wissensformen und die Gesellschaft* (1926), was intended to be a counter-argument to the narrow and rigid economic interpretation of Marx's historical materialism.⁶ By resurrecting the Comptean cate-

⁶ The development of Marxism witnessed the economic interpretation of historical materialism as being the most representative of Marx's and Engels' thinking, despite repeated qualification on the part of its authors, long before and after the publication of the *Critique of Political Economy*, in 1859. [Marx to Annenkov, December 28, 1846; Engels to Bloch, September 21-22, 1890; Engels to Mehring, July 14, 1893; in (Marx and Engels, 1955, pp. 30-1; 394-5; 433-435)]. As such, it should be recognized and borne in mind that when Scheler makes the statement that, "Our view is exactly opposite to that of Karl Marx, namely: There is no constant in the effective primacy among real factors;" (Scheler, p. 30); Scheler is responding to Marxism and not necessarily to Marx himself. Also, this recognition should be carried throughout the reading of this entire work, since as an historical study no assumption is made that figurative or even literal meanings remain constant over space and time. Furthermore, even if the evidence indicates that a valid historical interpretation of

gories of religion, metaphysics, and positivism as the forms of ideological spiritual (superstructural) knowledge that could and would assert themselves with respect to an ever-changing reciprocal relationship with the three material instinct-drive real (basic) factors of race (blood), politics (power), and economics (production), Scheler expanded the Marxist foundation of the base/superstructure paradigm. At the same time, by confining his argument to these three trans-historical categories, he was guilty of the same reductive tendency he criticized in Marx, who in fact constantly and consistently argued against all trans-historical theorizing.'

Like Scheler, Karl Mannheim (1929/1936; 1930+/1956) regarded the Sociology of Knowledge as part of the ideological

a particular person's or group's *spiritual theory* (Ibid., p. 35) has been made, and a logical conclusion has been reached, no assumption is made that the holder(s) of a particular *spiritual theory* necessarily consider(s) the theory itself to be constant over space and time. This non-assumption is especially warranted for someone as dialectically informed as Marx or Engels.

' Could not sex be considered a basic real factor, or in Kantian terms a constitutive category? Similarly, could not gender, nationalism or any number of classificatory groupings be considered constitutive of basic real existential factors?

' It should be noted that Marx did consider the history of the exploitation of labour to be the history of *civilization*, but even he recognized his labour theory of value and history to be no more than an historically grounded and specific explanation of the history of human society and its relationship to nature. "Men can be distinguished from animals by consciousness, by religion or anything else you like. They themselves begin to distinguish themselves from animals as soon as they begin to produce their means of subsistence, a step which is conditioned by their physical organisation. By producing their means of subsistence men are indirectly producing their actual material life." (Marx and Engels, 1970, p. 7).

superstructure or Sociology of Culture; and in his writings, he explored how it was that various group affiliations propagated not only particular ideological points-of-view, but a Sociology of Knowledge which would condition and limit the sensible as well as the conceptual apparatus of reason.' However, by continuing to defer the problem of how it was that particular ideologies reflected, enunciated, drove or resulted from given material conditions to the realm of psychology, Mannheim begged the question of consciousness, and accepted Scheler's rudimentary explanation of the role of the intellectual as being that of the perpetuating ideological force, be it for revolution or reform. A dialogue with the Weberian cum Schumpeterian cum Mertonian thesis of charismatic cum entrepreneurial cum puritan leadership, spirituality, and ethos¹⁰ and its contribution to rationalism, capitalism and science, has kept alive what for Scheler or Mannheim could be considered an element of religiosity, spiritualism, or even mysticism, if not through its identification, then at least

⁹ See "The Positive Role of the Sociology of Knowledge," in Mannheim's *Ideology and Utopia*, (1929/1936), pp. 292-306.

¹⁰ Max Weber's *The Protestant Ethic and the Spirit of Capitalism*, (1904-05), Joseph Schumpeter's understanding of the entrepreneurial risk-bearing function, (1942, p. 132; 1954, p. 556); and Merton's thesis of the relationship between Puritanism and the rise of science, (1938/54); all share an explicit opposition to historical materialism and its implicit determinism. That the rise and development of capitalism and science in many instances is beyond the conscious control of its leaders or as Marx observed: "Men make their own history, but they do not make it just as they please," Marx, (1852/1984, p. 15), is beyond the prévision of any of these anti-Marxist theories.

through its non-conceptualization.¹¹ But within the Sociology of Knowledge, by and large, the exact sciences were considered to be exempt from the influence of interest and motivation. (Mannheim, 1929/1936, p. 190).

It was the publication, in 1938, of a revised version of the doctoral dissertation of Robert K. Merton, and the consequent transmutation of the Weberian thesis from the Sociology of Knowledge to the Sociology of Science, that allowed for the recognition of the importance of scientific institutions on the history and development of science. According to Thomas Kuhn, (1968, pp. 115-118), the Merton thesis consists of two separate but related theses. First, it recognizes the importance of scientific institutions on the history and development of science. Through the application of Baconian principles to the study of practical arts, seventeenth century experimenters and scientific practitioners purportedly were able to contribute to the substantive transformation of the scientific enterprise itself. Second, the Merton thesis adapts the anti-historical materialist argument of Weber to the Sociology of Science. For Merton, the supposedly Puritan values of hard work, sacrifice, etc., contributed to the development of science much in the same way as Weber's Protestant Ethic contributed to the development of capitalism.

¹¹ Mannheim elaborated as well on the importance of the noncept: "The absence of certain concepts indicates very often not only the absence of certain points of view, but also the absence of a definite drive to come to grips with certain life-problems." *Ideology and Utopia*, (1929/1936), p. 274).

In his *The Structure of Scientific Revolutions*, (1962), Kuhn extended the Merton thesis to include the importance and influence of the scientific community in its performance of what he called *normal* or day-to-day science. As well as suggesting the possibility of bringing about major paradigmatic shifts through the recognition of anomalies in experimental data, Kuhn also infers that the scientific community itself is able to bring about a *Scientific Revolution*. In his carefully documented, but ultimately Mertonian study of the organizational nature of science, Stephan Fuchs (1992) credits Thomas Kuhn for holding up the inner sanctum of science to sociological examination (Fuchs, 1992, pp. 3, 37). As such, the work of Kuhn may be considered to be the point of departure for the Sociology of Scientific Knowledge from the Sociology of Science. Nevertheless, Fuchs echoes the sentiments of Sal Restivo (1983a), and Stanley Aronowitz (1988, p. 262), when he asserts that Kuhn's "analysis of scientific crises as resulting from perceived anomalies does have strong orthodox overtones, which bring him closer to Merton than many realize." (Op. cit., pp. 36-7).¹² Tant mieux, the Mertonian tradition has been carried on through the work of such historians of science as D.J. Price, (1963), and Joseph Ben-David, (1971), who emphasize the organization and institutionalization of science; it has also been carried on through the work of such

¹² Fuchs also correctly cites Ludwig Fleck's *Genesis and Development of a Scientific Fact*, (1935/79), although the work of Fleck has yet to receive the attention it so well deserves.

empiricists as David Gooding, Trevor Pinch and Simon Schaffer, (1989), who emphasize the experimental nature of science.

With the publication, however, of David Bloor's *Knowledge and Social Imagery* (1976), and concurrent efforts by Bruno Latour and David Woolgar, (1979), Steven Shapin, (1980, 1982), Michael Mulkay, (1981), Karin D. Knorr-Cetina, (1981), Barry Barnes, (1982), Harry Collins, (1982), Andrew Pickering (1984), and others, a pluralistic approach to science has challenged not only the organization and structure of science, but its content, as well. The Edinburgh School or the strong programme¹¹ in the Sociology of Knowledge has sought to demonstrate how science, like all cultural institutions, is socially constructed. Of course, a pluralistic approach necessarily denies any one right way to the attainment of scientific knowledge, and the various efforts emanating from the Edinburgh School should not be thought of as sharing any common grand meta-methodology. Still, the unifying theme of the strong programme begins with the basic assumption that all knowledge, including scientific knowledge, is socially constructed. As such, the identification of evidence, indications, and manifestations of sociological origins has come to be the focus of much of the work in this subfield. Notwithstanding stated intentions to the contrary, however, a positivist's

¹¹ In *Knowledge and Social Imagery*, (1976, p.7), David Bloor identifies causality, impartiality, symmetry, and reflexivity as the four tenets of the strong programme which "represent an amalgam of the more optimistic and scientific strains to be found in [Emile] Durkheim (1938), [Karl] Mannheim (1936) and [Florian] Znaniecki (1965)."

notion of the accumulation of knowledge remains lurking behind the realist's desire for the identification of its origin, be it social, or be it natural. To insist that social factors are responsible for the development of scientific knowledge, is to imply that natural factors are not. The perpetuation of this false distinction contributes to the perpetuation of the positivist approach in that it undermines the foundation from which any alternative epistémé may be established. It merely reverses the arrow of time and directs it from the effect to the cause, rather than doing away with the notion of causality altogether.

The *strong programme*, while ambitious in its claims of the social construction of scientific knowledge, proceeds in what might be considered to be a misguided effort to follow the methods of the *hard, pure, natural sciences*, and consequently begs the question: who or what is it that constructs *society*? Critics such as Richard Rorty, (1979), Larry Laudan, (1981), Hilary Putnam, (1982), and Wesley Salmon, (1989), effectively demand a post-empirical explanation as to why alternative theories are decided upon and accepted before they would be willing to concede the social nature of scientific knowledge. Paul Feyerabend's radical cultural relativism defers the others' acceptance of institutional, organizational, or structural reasons for theoretical currency, and remains steadfast to some Popperian non-explanation of the non-verifiable and ultimately extra-scientific and irrational nature of knowledge. Again, the positivist conception of

reality circumscribes its origin. Nevertheless, the pragmatic acceptance of sensual existence as proof of primary being does not negate the problem of the nondeterminate identification of that which is not yet. As such, it is my opinion that recourse to notions of irrationality remain within the rational confines they wish to escape, as long as a realist notion of Being remains in place, whether we are conscious or not of its existence. The problem of collective consciousness still remains unresolved; and any explanation of an accumulation of knowledge, whether it be empirical, rational, or irrational, remains incomplete. As of yet, no one working within the sociological tradition has been able to convincingly demonstrate how it is that our existential reality conditions and limits our consciousness, which in turn conditions and limits our existential reality. Of course, questions of the extent to which the subject and object may if at all be separated have been entertained from at least as long ago as within the pre-Socratic traditions of Anaximander, Heraclitus and Parmenides,¹⁴ through to that of the Frankfurt School and beyond. Theodor Adorno's *Zur Metakritik der Erkenntnistheorie*,¹⁵ with

¹⁴ Questions of continuity versus change, or the one versus the many, may also be interpreted as forms of the question of the subject versus the object, as long as the object is considered to be beyond the subject's control. In this manner, fate, the prerequisite for all tragedy, may be viewed as those objective conditions that humanity in its hubris attempts to redress. See Martin Heidigger, *Early Greek Thinking*, New York: Harper & Row, 1975.

¹⁵ Not translated as a *Metacritique of Perception Theory*, but as *Against Epistemology*, subtitled and properly translated as *Studies in Husserl and the Phenomenological Antinomies*.

the subtitle *Studien über Husserl und die phänomenologischen Antinomien*, (1956), interrogates the violence necessary to separate the subject from the object, while continuing to accept the conditions for what Edmund Husserl called *The Crisis of European Sciences* (1934),¹⁶ which was brought about through the recognition that "the positive sciences, including pure mathematics and the exact natural sciences ... proved to be changeable in the total style of their systematic theory-building and methodology." (Husserl, 1934/1970, pp. 3-4). More recently, alternative epistemological studies in such areas as religion, race, gender, and ecology have continued in this tradition, as a growing number of historical and socio-political critiques have deliberately distanced themselves from the scientistic paradigm of the social sciences. (Feyerabend, 1975, 1987; West, 1985; Harding, 1986; Bookchin, 1989). But the acceptance of the ideational/real factors themselves, from within a sociology of culture, or even a sociology of knowledge, continues to beg the philosophical questions of what is culture and what is knowledge, by tacitly accepting the postmodern definition of what is science!

It was with Marx and Engels, in their attempt at a post-Comptean positivistic Darwinian unification of the natural and social sciences, that began the arduous and I will argue impossible reconciliation of the physical, natural, objective

¹⁶ Originally published in German under the title *Die Krisis der europäischen Wissenschaften und die transzendente Phänomenologie: Eine Einleitung in die phänomenologische Philosophie*.

sciences, and the social, artificial, subjective sciences. To attempt to unify that which is inseparable while maintaining the dichotomization of the ideational from the real, is to build a straw man to scare away the scientistic bogeyman. To dismiss the problem with a wave of the hand, á la Adorno (1967) or Habermas (1968), in their historically specific but still Hegelian end-of-historistic tales of post Enlightenment dimness, is to grudgingly accept that same bogeyman Durkheim (1901) and the rest of his idealistically imperialistic and empiricist followers from Bloor (1976) to Derrida (1962/1989) have welcomed and embraced. Rather, the natural/socio-historical paradigm itself must be interrogated so that scientific knowledge can be understood as both the ideal as well as real forms of knowledge it represents, describes, and is.

By first attempting to answer the socio-historical question, what is science?, we are in a better position to answer other questions, such as (how) has the role of the intellectual been (able) to propagate constitutive and/or regulative principles and ideologies? By then attempting to answer the socio-historical question, what is knowledge?, we are in a better position to answer other questions, including ones such as what is culture? But the scholars working within the research areas of a sociology of culture, a sociology of knowledge, a sociology of science, and a sociology of scientific knowledge, have been content to accept certain types of knowledge as being immutable. As such, there has been agreement, within these lines of inquiry, that the truth is either something to

be uncovered, as described by Martin Heidigger, (1922/1992), or discovered, as described by Latour and Woolgar, (1979).

Following the empirical scientific rules of observation, measurement, experimentation, and experience, has come to be considered the only pathway to legitimate scientific knowledge; but I will argue that this is not necessarily so. All expressible knowledge is symbolic, and science no less than any other societal cultural artifact symbolizes the dominant world view of a particular time and place. Through the inclusion of other forms of knowledge or methods we may avoid the conclusion that an allinclusive and eternal science is anything but one cultural expression of a particular paradigm of thought, and no longer continue to constrain ourselves intellectually and/or by inversion, materially.¹⁷

¹⁷ Even mathematics and geometry are subject to change. To the extent that mathematics and geometry are eternal, they belong to the world of *Being*. To the extent they are not, they are regulative, not constitutive and belong to the world of *Becoming*. To wit: $2 + 2 = 4$ in Base₁₀, but 11 in Base₂, and 10 in Base₃. Furthermore, even $1 + 1 = 2$ assumes no reproduction or any process of generation or decay. Also, post-Euclidian geometry has demonstrated that time and space, ergo, geometry is not as perfect as it was previously believed. Finally, the Logic of the Syllogism may not be that of the mathematical equation. See Herbert Dingle, *Science at the Crossroads*, (1972), and Sandra Harding, *The Science Question in Feminism*, (1986). Also investigate the logical progression from Wolff to Kant to Hegel to Mill. See Anthony Giddens in the *Introduction to Max Weber's, The Protestant Ethic and the Spirit of Capitalism*, (1976, p., 2), for his statement on how, "In Britain, J.S. Mill's *System of Logic* (1843) unified the natural and social sciences in a framework that fitted comfortably within the existing traditions in that country." On scarcity, and how "competitive allocation cannot operate without institutionalization of a set of norms defining the limits of legitimate action," see Parsons, (1951), pp. 116-18.

A Search for a Method

My position is that science consists of mutable exorable regulative principles; a non-exhaustive list of such would include the positivistic concepts of order, progress, continuity, perspicuity, homogeneity, gradualism, and hierarchy. In order to demonstrate the relevance of the socio-historical and cultural character of these concepts, I will discuss how the *probabilistic revolution* apparently has been able to subsume all of the sciences, natural as well as social. Through a careful examination of the history of the conceptual development of probability and statistics, the methodological basis for any substantive distinction between the natural and social sciences will be challenged. Through the critical examination of the mathematics of probabilistic and statistical thinking, the epistemological basis for the crisis in European science will be further exposed. But such an historical/genealogical analysis should also consider the possible presence of a potential political problem, perhaps as it is posited below by Sondra Farganis:

Science, as now practiced, contributes to and even constitutes a political ideology of domination. For (neo) positivism, in treating physical and social entities as objects to be described, measured, and classified, trains us to think of controlling and, in a sense, dominating the world around us and the people in that world. (Farganis, 1989, p. 214).

At first blush, the relationship between this contemporary restatement of the problem and the traditional statement, as quoted above from Condorcet, may not be obvious. Perhaps, for those working within a Cartesian tradition, or at least in

such a manner as did Condorcet conceive of the problem, the power relationship between God and *man*¹¹ is a given. Within feminist theory, the gendered relationship between *man* and *wo(man)* is derived. According to Allison M. Jaggar and Susan R. Bordo, (1989), "the claim that the Cartesian framework, in addition to other biases, is not gender neutral distinguishes contemporary feminism from these other approaches [of Marxist historicism, psychoanalytic theory, literary theory, and a sociology of knowledge] ... The body, notoriously and ubiquitously associated with the female, regularly has been cast, from Plato to Descartes to modern positivism as the chief enemy of objectivity." (Idem, p. 4). On the other hand, for Hilary Rose, (1996), "Apparent oxymorons bind the feminist discourse: feminist sciences, feminist empiricism, feminist objectivity, feminist rationality; these mark out a normative discourse and delineate it from the deliberately non-normative discourse of mainstream SSK."¹² (Idem, pp. 91-2). If this is so, feminist research may have had as slight an impact on

¹¹ The use of the word *man*, rather than *people*, or some other gender neutral signifier is deliberate. It should be assumed that when the word *man* is used, it is meant to exclude *women*.

¹² "Key figures in the production of these oxymorons would include Patricia Hill Collins, [1991], Donna Haraway, [1989], Sandra Harding, [1986, 1991], Nancy Hartsock, [1983], Helen Longino, [1990], Elizabeth Potter, [1993], Hilary Rose, [1994] and Vandana Shiva, [1989]." To this list might also be added, Nancy Cartwright, (1983), Janet Biehl, (1991), Roslyn W. Bologh (1990), Ruth Blier, (1984), Nancy Chodorow, (1978), Elizabeth Fee, (1986), Evelyn Fox Keller, (1985), Jane Flax, (1993), Carol Gilligan, (1982), Ruth Hubbard, (1990), Dorothy Nelkin, (1987), Carolyn Merchant (1980), Ann Oakley, (1984), Susan Moller Okin, (1979), Dorothy Smith, (1987), and Sharon Traweek, (1988).

the advocates for a sociology of scientific knowledge, (SSK), as it has had on the apologists for a value-free science. Nevertheless, the political ideology of domination, which according to Farganis, and Jaggar and Bordo, for that matter, is inherent in the (neo)positivist quest for objectivity, may also be interpreted as existing within the scientific discourse on nature as rhetorically questioned by Cornel West:

If science is ... a value laden enterprise, is there an ideological character intrinsic to the very methods of the natural sciences owing to an agreed-upon conception of and disposition toward nature which may promote the domination not only of our environment but also those people subsumed under the rubric nature such as women, non-Europeans and even earthy workers? (West, 1985, p. 268).

The fundamental question, however, in both the traditional and contemporary restatements of the problem remains unanswered. How is it that such inequitable relationships have been able to be maintained? Paulo Freire, (1968), writing in what may be considered to be a pluralistic counter-Hegelian tradition, concedes that subjugation and oppression is a mutual process. Without specifically blaming the victim, Freire reminds us how in order for cultural colonization to occur, the colonized must in some way be made to feel inferior to the colonizer. The fact of the power relationship itself guarantees the super-altern position of the colonizer, but the question remains as to how it is that this hierarchical relationship is analogous to the relationship between the Christian God and *man*. Otherwise put, is the power relationship that exists between the Christian God and *man* reproduced in the power relationship that exists between the oppressor

and the oppressed? If so, is this reproduction in any way accomplished through the methodology of science? Is an ideology patent within the methodology of science that facilitates oppression and subjugation? For F.W. Nietzsche, (1901), it is the imposition of an apparent state of Being, ie., a world where the deliberate transformation of the objective conditions is impossible from the point of view of the powerless individual that allows for the concession Freire laments. It might also be a prerequisite for the identification and establishment of conscious, reflective, recollective knowledge:

The principle of identity has behind it the apparent fact of things that are the same. A world in a state of becoming could not, in a strict sense, be *comprehended* or *known*; only to the extent that the *comprehending* and *knowing* intellect encounters a coarse, already-created world, fabricated out of mere appearances but firm to the extent that this kind of appearance has preserved life -- only to this extent is there anything like *knowledge*; i.e., a measuring of earlier and later errors by one another. ... [But] our needs have made our senses so precise that the *same apparent world* always reappears and has thus acquired the semblance of reality. ... To impose upon becoming the character of being -- that is the supreme will to power. (Nietzsche, pp. 281-2, 330).

It is my contention that the mathematization of science has contributed to the perpetuation of a neo-Platonist world view wherein ideal changeless forms have allowed for the imposition of an apparently changeless objective world. This has undermined all practical hope of intentionality, at least from the perspective of the individual. This hopelessness is reified through the invention of tradition, as articulated by Eric Hobsbawm and Terence Ranger, (1983), and perhaps through the establishment of pseudo-identity, by which I mean the type of

identification that could occur only in a world of Becoming that pretends to be a world of Being. But (how) has the mathe-
matization of science equivocated and facilitated this task?

In his lectures on physics, at the University of Hamburg, Carl Friedrich von Weizsäcker, a student of Werner Heisenberg, would draw on the blackboard a circle or a triangle and ask the class if they thought the circle was really a circle or if the sum of the angles of the triangle really added up to 180°:

The answer, of course, is 'No.' Neither a triangle drawn freehand nor one drawn with a ruler is accurate; indeed none is accurate that can be perceived by the senses or produced by whatever means. Therefore, mathematics does not deal with objects of senses. It deals with what we are capable of thinking, what the spirit or mind -- in Greek: the *nous* -- can grasp. How, then, can our thinking furnish the laws for governing what we perceive with the senses? That is the problem. ... Kepler offers a well thought out answer. He says: God created the world in accordance with His creative thoughts. These thoughts are mathematical. God created man in His image. Man is an image of God in those attributes that can at all be conceived of as predicates of God. The highest of these predicates is spirituality, spiritual being. As a spiritual being, man is an image of God and can as such rethink God's creative thoughts and recognize them in the material creation. This recognition, this divine service, is natural science. (von Weizsäcker, 1924, p. 90).

This is Platonism in Christian language., (Ibid.), and although there is nothing new in any of von Weizsäcker's arguments, they carry a common-sense authority with anyone whose common sense accepts a distinction between the worlds of Being and Becoming. Of course, von Weizsäcker's philosophy is not so far removed from that of his mentor: "Heisenberg consistently rejected what he called 'materialism' -- the belief in 'an objectively real world whose smallest parts

exist objectively in the same sense as stones or trees exist, independently of whether or not we observe them.' ([Werner Heisenberg], *Physics and Philosophy*, 1954, p. 76). But in his later years he returned to the Platonic worldview that had impressed him before 1920; the ultimate reality is neither our sensations nor substantial atoms, but mathematical forms." (Stephen G. Brush, 1988, pp. 401-2). Such conflicting world views, continue to contribute to the confusion that surrounds the imposition of any universal claim to Truth, or even its very existence, for that matter.

Epistemological Considerations

In order to examine these claims, within the context of the discourse on the sociology of science, access to the historical record is the single most important requirement. Unfortunately, the existence of an historical record presupposes some continuity of thought which may not reflect the epistemological changes associated with revolutions in scientific thought, great or small. Moreover, the meanings of words change over time, and from place to place, as do the languages in which they are written and spoken.²⁰ To account for these differences, as well as those pertaining to mathematics, as described above, it should be obvious that various translations and editions from the popular and scientific literatures of the eras in question need to be compared and

²⁰For an in-depth discussion of the relationship between problems of realism and problems of translation, see Steve Fuller, *Social Epistemology*, (1991).

contrasted with each other. Old dictionaries also need to be consulted, in order to avoid having the texts rewrite themselves in the languages of today. With the language of natural philosophy becoming more and more mathematical, it is difficult at times to interpret meaning, other than in mathematical terms. To accomplish such an interpretation, it should never be forgotten that mathematical symbols do not necessarily represent some other level of reality. In fact, many of the symbols and concepts of classical and contemporary science are often totally fictitious, with no imagined correspondence to the real world. The ancients understood this, and much of the controversy between the Church of Rome and the scientific community has centered around this very distinction.²¹ This distinction, however, has become increasingly blurred, as a faith in science and technology has replaced that which was once reserved for theology.

The relevance of the study lies in its explicit challenge to all those epistemological claims, subjective, objective, or combinatorial, which continue to perpetuate the division between the natural and social sciences. Every little makes a mickle. The move toward a relativistic multicultural science is as untenable as the continuation of any universal

²¹ Even in the celebrated trial of Galileo, the position of Pope Urban VI was that the Copernican system could be taught, as long as it was considered to be an hypothesis or a model, until further scientific proof was available. The debate has many times been less over an interpretation of the scriptures and more over an interpretation of reality. After all, the Church had already reconciled itself with the fact that the earth was not flat. (Stillman Drake, *en passim*).

monolithic scientific discourse; within the sociological community, the voluntarism of methodological individualism is as untenable as any of the popular forms of determinism, including the indeterminate determinism of the postmodern appropriation of chaos theory. The implication, for a scientific curriculum, is not just that the object of science is socially constructed, but that society itself is conditioned and limited by its own history. To ignore the dead hand of the past is to allow it to negotiate the course of the present. To ignore the dead hand of the past is to allow it to write the course of the future. To ignore the dead hand of the past is to pretend that we as humans have no control over our destiny, while we curse our luck and deny the existence of a God who would never play dice. The relevance of the study lies in its recognition that we are certainly not living at the end of history. Rather, through the recognition that society and nature are not only inseparable, but conceptual limitations, we are forced into the recognition that we are only at the beginning.

With the revitalization of eugenics, it may be argued that we once again have a need for that hypothesis, dismissed by Laplace, over a century and a half ago. We have a need to ask what or who will regulate the new world order. If not God, then how will the eugenicist, the molecular biologist, the genetic engineer, reform that which was supposedly changeless? For as the technician substitutes what is normal for that which deviates from the mean, a number of practical questions

do arise: Who is it that is deciding what is considered to be *normal* versus what is considered to be *pathological*? (See Georges Canguilhem, 1966/1989). What possibilities are aborted through the methodological application of the preconditions of order, progress, continuity, perspicuity, homogeneity, gradualism, and hierarchy? What monstrosities are created through the elimination of error? What if the outlier is a necessary aspect of the essential structure? What if the movement toward a lower state of energy has ensured the structural integrity through the movement toward functional diversity? Who is to claim what constitutes substitutionality²² or even substitutability? What are the philosophical implications of the imposition of order where there is chaos? These questions, unanswerable from within the present scientific paradigm, need to be reformulated with the understanding that all knowledge is somehow imbued with a large element of faith.

An Alternative Conceptualization

The position is taken, throughout the course of this dissertation, that a false distinction is oftentimes made between the natural and the social sciences. This position is supported by an historical inference, if not through the historical recognition, that since the Age of Enlightenment, at which time the scriptural argument concerning the immutability of the heavens was irrevocably challenged, the

²² The idea of substitution is discussed by Henri Poincaré, as a necessary condition for the *Calcul des Probabilités*, (Paris: Gauthier-Villars, 1912).

scientific enterprise has perhaps not so paradoxically reverted from the Aristotelian project of the classification and explanation of Existence, to the Platonic project of unification and the description of Being. It is argued that through the continued acceptance of the Platonic distinction between the primary and secondary qualities of nature, a non-critical scientific community has tacitly accepted the notion that a fundamental ontological distinction exists between the objective world of natural science, and the subjective world of social science. This almost universally accepted misconception, that there is a fundamental ontological difference between nature and society, has led, throughout the past two and a half centuries, to the desiderata for a purification of the social sciences. But it has also led, within the natural sciences, to an obscuration of the limits of human knowledge.

In order to demonstrate how it was that along with the development of a probabilistic understanding of truth and the concomitant mathematization of probabilism, statistical mechanics and probability theory subsumed the natural as well as the social sciences, the dissertation is divided into four chapters. In this first chapter, the historical background to the sociology of science and knowledge is presented as a prolegomenon to a cultural explanation for the mathematization of knowledge and truth. In the second chapter, the trial of Galileo is discussed within the context of a philosophy of ideas, as a doctrine of probabilism is quantified in the quest for certainty and the elimination of the possibility of human

error. The various theoretical and logical formulations of probability theory are examined in detail in the third and fourth chapters, in order to explicate the pluralistic nature of the scientific project and the singular imperative for its democratization.

The mathematization of knowledge is the result of more than the simple application of some scientific method. It is the result of an ideology which has no provision for variation other than to encompass it within a larger distribution or to view it as a type of error, at most to be eliminated or at least to be ignored. It is the result of an ideology of homogeneity and uniformity, obtained through strict obedience to the rules of method. Of course, these rules are necessary, if not sufficient, to utilize those statistical techniques which are based upon the calculus of probability. Perhaps they only appear to be sufficient when "probability as an ontological, rather than an epistemological, element enters the physical sciences through the biological and the social sciences." (Schweber, 1982, pp. 348-9). Is probability an ontological element of the universe or an epistemological element of science? God only knows; but the determinism associated with the central limit theorem of the law of large numbers has been transubstantiated into the pragmatism associated with the ability to predict; and this ability to predict, I fear, is no more than a mathematical expression of what Merton described as a self-fulfilling prophesy, or what Nietzsche described as the will to power.

An ancient riddle poses the question: Who is better off? The king each night who dreams he is a beggar, or the beggar each night who dreams he is king? Of course, the riddle presupposes the inability of either the beggar or the king to accept or transcend those existential realities that are beyond all conscious control. As human beings, it is our responsibility not only to recognize our existential realities, but to ask ourselves how our dreams determine our material conditions as much as our material conditions determine our dreams. Through the acknowledgement that even science is a cultural artifact, perhaps someday its techniques will be utilized such that there will be no more beggars. Perhaps at that time neither will there be more kings. Maybe then the riddle itself will cease to be.

CHAPTER TWO

METHODOLOGICAL IMPLICATIONS

Whether they have been carefully documented to develop and further a general understanding of conflicting world views, or they have been loosely assembled to present a particular literary theme, the events surrounding the trial of Galileo invariably have been utilized to dramatize what is considered to be a turning point in the history of science. His articulation of the Copernican world view, that the earth was not the center of the universe, was in violation of an injunction issued by the Church of Rome, on February 25, 1616, that this cosmological view was in contradiction of Holy Scripture. This warning had been issued after the publication of his *Letter to the Grand Duchess Christina*, (1615), which included his thoughts on the relationship between science and religion, as well as a non-detailed explication of the Copernican world view. In conjunction with the publication of his *Dialogue On the Great World Systems*, (1632), and earlier writings on starry messages, (1610), floating bodies (1612), sunspots, (1613), comets, and a rudimentary exposition of a modern scientific approach, (1623), Galileo was found, by the Vatican, as being guilty of heresy, on June 22, 1633. Before he was placed under house arrest, for the final nine years of his seventy-eight years of life, he was forced to recant, and by some accounts he died in disgrace; but the truth or the

certainities of science, as the story goes, eventually triumph over the mythology and the vagaries of religion.

That the life and work of Galileo has had a profound impact on the history and development of the culture of European science is indisputable. What is disputable, however, is the non-critical ideological notion that through the application of some basic scientific principles, the truth will be established in the end. Furthermore, it requires very little intellectual reflection for it to become increasingly obvious, despite unremitting protestations to the contrary,¹ that the notion of truth in itself is ideological. In the pages that follow, the paradox of how an ideologically loaded modern scientific quest for truth and certainty has denied its own idealistic foundations, through the assertion of an empirical scientific method that precludes the recognition of primary causes or even pre-existing *a priori* general principles, will be examined in detail. The idea that the criteria that were used to judge Galileo were in keeping with a casuistic methodology that did in fact recognize the Holy

¹ "Truths do exist. They are mathematical in form and statistical in essence. Scientific research that is performed in the belief that such truths can be found has never been disappointed. In fact, such truths lead to the only verifiable statements that can be made; and they are verifiable by anybody." (Gernot M.R. Winkler, "Necessity, Chance, and Freedom," in George M. von Furstenberg, *Acting Under Uncertainty: Multidisciplinary Conceptions*. Boston: Kluwer Academic Publishers, 1990, pp. 154-56). How it is that this notion of truth has come to gain such widespread acceptance, while failing to recognize its own historical/ideological prevarication, is consistent with the denial of those meta-physical principles upon which any such notion ultimately must be based.

Scriptures as the word of God, but allowed for those words to be interpreted on a case by case basis,² will also be developed in order to establish an alternative interpretation of the Copernican revolution and the life and work of Galileo. It is hoped that through a better understanding of these criteria, as well as their ideological underpinnings, the casuistic tradition, which extends back at least as far as the Stoics,³ will not continue to be viewed as merely a methodology that is antagonistic to modern science and the rest of the Enlightenment project. Neither is it hoped that casuistry will belatedly be viewed as a precursor or some necessary condition for the development of what some scholars consider to be a probabilistic revolution. Rather, it is simply hoped that the fictitious dichotomy of subject and object, as it has been retold through the story of the

² For a sympathetic account of the utility of casuistry, as well as the attendant problems of a probabilistic methodology within the Church of England, see Kenneth E. Kirk's *Conscience and its Problems: An Introduction to Casuistry*, (1933). Also see *The Abuse of Casuistry*, (1988), by Albert R. Jonsen and Stephen Toulmin. For scathing attacks concerning this very same method of analysis and its application, see Pascal's *Provincial Letters*, (1656/1967), and Paul Bert's editorial comments in *The Doctrine of the Jesuits*, (1879/ 1880), his translation of Jean-Pierre Gury's *Compendium Theologiae Moralis*, (1850), and *Casus Conscientiae In Praecipuas Quaestiones Theologiae Moralis*, (1865).

³ Indeed, the case has been made that the "Christian faith came into being within the bosom of Rabbinic Judaism which already had a flourishing casuistry." (Jonsen & Toulmin, 1988, p. 91). Of course no Decalogue existed before the time of Moses, but it would be difficult to support the suggestion that monotheism rather than written language, or even a rudimentary system of semiotics, was a sufficient condition for the development of a casuistic approach.

supposed incompatibility of religion and science, will further evidence itself through a multidisciplinary retelling of the story. By so doing, the groundwork also will have been laid for a better understanding of those abstract mathematical concepts, the theoretical development and practical application of which served as the basis for what already has been referred to as a probabilistic revolution.

The Crime of Galileo

The story of the emergence of modern science must begin with a tale of the trial of Galileo. No other tale has been referred to as often and utilized as much to illustrate the contradictions between and within so many academic disciplines. Science, philosophy, history, astronomy, physics, mechanics, dynamics, theology, mathematics, geometry, art history, literary criticism, psychology, and even mythology have all gained academic currency through an historical association with this definitive allegory of apodeictic knowledge and unconditioned faith. But in the opening passage of *Dialogue On the Great World Systems*, (1632), it is with a question of cosmology, that Salviatus, who is recognized as the voice of Galileo, discloses his intentions:

It was our resolution of yesterday that we should discourse as distinctly and concretely as we could on the natural reasons, and their efficacy, that have been hitherto alleged on one side by the maintainers of the Aristotelian and Ptolemaic positions and on the other by the followers of the Copernican system. Copernicus, placing the Earth among the movable bodies of Heaven,

makes of it a globe like a planet; therefore, it would be good if we began our disputation with the examination of what and how great the strength of the Peripatetics' argument is when they demonstrate that this hypothesis is impossible." (Galileo, 1629/1953, p. 12).

It is almost ironic that this introduction to a theory of astronomy has become the epilogue to a theological explanation for the emergence of modern science from its classical background. It is ironic in that the science of Aristotle is empirical science, with metaphysical presuppositions no grander than those of today,⁴ and it was the Aristotelians who Galileo apparently targeted as the adversary. Perhaps it is even more ironic that Jesuit missionaries working under the supervision of the *Sacred Congregation of the Propaganda in Rome*, were responsible for the establishment of the Copernican system and the doctrine of the motion of the earth, throughout China and Japan, from the end of the seventeenth century onward. (Koestler, p. 503). That the Church of Rome had already reconciled its authoritative interpretation of the Holy Bible to incorporate apparently contradictory statements

⁴"The simple theoretical acceptance of the quantitative, experimental, relativistic way of thinking and investigating obliterated the basic principles of their fundamental conceptions and conclusions. Hence this new science had no authority, support, or confirmation, other than the logical necessity of its mathematical demonstrations and the concrete evidence of its experimental tests. There was no possibility of connecting this method of natural investigation with the deductive, distinctive, dialectical, and analogical procedure which made the Aristotelian system of knowledge an almost gapless texture of orderly cognitions." (Olschki, p. 357). See Chapter One, footnote 2, concerning the assignation of occult qualities to the Newtonian system. These qualities have been supplemented but not supplanted by the development of quantum mechanics.

concerning the shape of the earth, bespeaks the prognostication that it would also reconcile its interpretation to incorporate apparently contradictory passages concerning its motion.

In accord with the conceptualization of perhaps the first truly modern historian of science, Pierre Duhem, who had already described the scientific project as one in which it was obligatory *To Save the Phenomena*, (1908/1969), Karl R. Popper, in *Conjectures and Refutations*, (1962), discusses what he considers to be the instrumentalist position of the Church of Rome. (Popper, 1962, p. 99n). Popper, perhaps also following the lead of Arthur Koestler (1959, pp. 170-175, and 477), a bit ingenuously suggests that the controversy surrounding the conflict between diurnal motion and the Holy Scriptures could have been avoided if Galileo would have conceded to a disclaimer such as that which was made by Andreas Osiander, in his preface to Copernicus' *De Revolutionibus*:

There is no need for these hypotheses to be true, or even to be at all like the truth; rather, one thing is sufficient for them -- that they should yield calculations which agree with the observations. (Osiander in Popper, 1962, p. 98).

*Pereant qui ante nos nostra dixerunt.*⁹ Pietro Redondi,

⁹ May they perish who have expressed our bright ideas before. "He [Galileo] had received an unofficial authorization to go ahead, and he had striven to get his book officially accepted and approved. He had been given permission and could talk about Copernicanism provided he did so in a manner that was 'hypothetical and without reference to Scripture.' In the *Dialogue*, 'Scripture' had certainly been avoided; as for the first warning, however, it was more difficult to evaluate the work's degree of sincerity. Nevertheless, in order to avoid misapprehensions, there had actually been inserted, as in

in *Galileo Heretic*, (1987), makes the novel claim that the conflict over motion, at least at the time of the publication of *The Assayer*, (1623), was more subtle than the well cited but relatively less important one of planetary motion. The heresy of Galileo, according to Redondi, was grounded instead in his implicit acceptance of the doctrine of the motion of atoms, (Ibid., p. 146), in contradistinction to the Christian doctrine of transubstantiation, the cornerstone of the most important of Christian sacraments, the Holy Eucharist.⁶ Maurice A. Finocchiarro, (1986), on the other hand, takes the

Copernicus's *De revolutionibus*, an ad hoc introduction and a final summation with the intent of preserving appearances from any reasonable suspicion. But we know there were unresolved suspicions, and perhaps Father [Vincenzo] Scheiner [the author of *Rosa ursina*, (completed in 1626 but not published until 1631), which contained a 'bitter claim to priority in the discovery of sunspots,' and also 'made public the results of the scientific debate, held in 1612 at the Collegio Romano, on the cosmological consequences of that discovery with respect to the Aristotelian teaching which stipulated the inalterable nature of the celestial substance.'] could have found pretexts for slander in Galileo's work. (Redondi, p. 234).

⁶ "St. Thomas had the intellectual audacity to affirm that which his predecessors ... had only timidly suggested: The Eucharistic phenomena are sensible phenomena separated from substance, accidents without a subject. Hence, the quantity (extension) of the Consecrated Host is not sustained either by the material of the bread or by the surrounding air. It persists, miraculously, without substance. So, too, with all the other accidents that adhere to extension: the notorious "color, odor, taste." These persist and act "as if" they depend on the substance, but in reality they persist and act without substance." (Redondi, p. 213). ... "for Galileo, it was essential to defend the homogeneity of the heavens and earth, both of which were formed by a common nature, a nature entirely and everywhere knowable through an identical rational observation of phenomena, or "accidents." Also, in the Dialogue came the need, first of all, to challenge Aristotelian philosophy, attacking the separation between substance and accidents." (Ibid., p. 235).

non-secular position that "the anti-clerical myth that Galileo was tried and condemned by the Catholic Church for having seen or proved that the earth moved ... is also the myth that tries to exploit the Galileo affair to advocate an irreconcilable conflict between science and religion or at least Catholicism," (Idem, 1986, p. 246), although he later conjectures that "the available documents make it overwhelmingly obvious that the paramount issue was the earth's motion." (Idem, 1989, p. 43). Still, there is no substantive contradiction of the epiphenomenal refutation that the issue was not simply one of science versus religion, nor even that it was exacerbated by the personalities involved in the drama.'

The Copernican system itself was never the issue. Rather, the issue was whether or not Galileo or any of his contemporaries had provided sufficient evidence for the motion of the earth to be acknowledged as a probable opinion. This aspect of the trial of Galileo had been made quite explicit some seventeen years before the publication of his

' Of course, for Popper, the scientific project proceeds through a series of conjectures and refutations, and it is through a critical 'rationalist tradition,' that what he considers to be the limitations of essentialism as well as those of instrumentalism, may be avoided. (Ibid. pp. 101-119). Alternatively, "The only method used in positive science is the experimental method, no matter whether its tool is the most refined microscope or the newly perfected radiotelescope. What does not fall within the realm of sensible experience is not an empirical datum and cannot constitute the object of empirical investigation. ... Hence when a scientist transcends the boundaries of his field of investigation and makes statements that go beyond the realm of sensible experience, he steps into an area that has traditionally [and I might add quite understandably - GD.] been reserved for philosophy and theology." (Bonansea, p. 238).

Dialogue Concerning the Two Chief World Systems, (1632), in a letter dated April 4, 1615, from Lord Cardinal Bellarmine, Consultor of the Holy Office and Master of Controversial Questions at the Roman College, to Cardinal Piero Dini. The letter was concerned with statements made by Galileo in what later was to become known as the *Letter to the Grand Duchess Christina*, (1615). The complete text of Bellarmine's letter, as translated by Giorgio de Santillana, may be found in *The Crime of Galileo*, (1955/1967). The relevant passages, for the present discussion, may also be found in Arthur Koestler's, *The Sleepwalkers*, (1959), and read as follows:

As you ask for my opinion ... First, I say it seems to me that your Reverence and Signor Galileo act prudently when you content yourselves with speaking hypothetically and not absolutely, as I always understood Copernicus spoke. ... Second, I say that as you know, the Council of Trent forbids the interpretation of the Scriptures in a way contrary to the common agreement of the holy Fathers. ... Third, I say that, if there is real proof [Koestler's italics] that the Sun is in the center of the Universe, that the earth is in the third sphere, and that the Sun does not go around the Earth but the Earth round the Sun, then we should proceed with great circumspection in explaining passages of the Scripture which appear to teach the contrary, and we should rather have to say that we did not understand them than declare an opinion to be false which is proved to be true. [My italics] But I do not think there is any such proof since none has been shown to me.... and in case of doubt one may not abandon the Holy Scriptures as expounded by the holy Fathers.... (Koestler, pp. 454-5).

The position of Cardinal Bellarmine, as established through a probable opinion, was in perfect harmony with the casuistic approach of the Society of Jesus, at the beginning of the seventeenth century. This aspect of the story, the historical conditions under which the verdict of the court was

given, has often been lost or overlooked in many of the romanticized versions and retellings of the story. What did Bellarmine mean by *an opinion which is proved to be true*? What constituted pre-scientific provability? How has our understanding of what constitutes the truth and certainty and our methodology in its obtenance been determined by our acceptance of a mathematized notion? How would we understand the verdict of the court if we did not *believe* the earth was not the center of the universe? How would we understand the verdict of the court if we did not *believe* the earth was in motion? For Bellarmine there was a truth, but it could never be known. For Bellarmine, at best an opinion could be substantiated; therein, it would demonstrate its provability, and establish its inherent probability.

Perhaps this premodern attitude is not all that different from the postmodern attitude that there is no one truth; therefore, we can never know it. Indeed, the renewed popularity of ethnography and case study, in many ways fills a gap that had been left by the previously widespread application of the inductive scientific method, which has relied so heavily on mathematical models. Perhaps this reaction to the widespread application of the scientific method has also perpetuated a system of semiotics that relies for its proof on the sign or the symbol that is generated by the inscription device or instrument, rather than the thing that is signified. (Latour and Woolgar, 1979, p. 51). Such a literary approach to science, along with its attendant

concession of all that is scientistic, posits a relative notion of truth that still can be demonstrated only through an appeal to the senses, extended as they may be. Again, perhaps at best we are able to substantiate our opinions.

On the other hand, perhaps this attitude, that we can never know the truth, whether or not it is conceded that a truth in fact exists, is incommensurable with the modern attitude that the truth is mathematical in nature, and it can only be obtained through the use of reason, and certainly not through sensual experience. The implication of this Platonic position is that perhaps we should not trust our senses; after all, they just might be wrong. It is true that no one in the world can feel the motion of the earth. Its motion is beyond our senses; but it is not beyond our reason. Strictly speaking, a not unexpected corollary of the advent of the Age of Reason is that rationalism, and certainly not experiential empiricism, won out in the end, as the methodology of choice in the search for truth. We are all of us Platonists who believe in the motion of the earth.

The trial of Galileo was about suppression. It was about suppression of the lie. Maybe the earth was at the center of the universe. But through a strangely perverse institutionalization of the Aristotelian doctrine that oftentimes life imitates art, science fiction had become impossible to distinguish from scientific fact. Thereafter, the consensus of a developing bourgeois scientific community would determine what constituted fact and what constituted fancy. The rest of

us poor mortals would need be content to be assured that the earth did in fact move, whether we believed it or not. Only uncivilized persons or religious fanatics could believe anything to the contrary. Neither the Holy Bible nor its interpreters need any longer be turned to for their opinions concerning the truth. All this, without a shred of tangible evidence, other than some limited and contradictory telescopic observations;¹ all this, without a justification for how it was that they, the Galileans, arrived at their conclusions, other than what would later be formalized as the right to an opinion.'

In an early passage from his own *The Copernican Revolution*, (1957), Thomas Kuhn ascribes a philosophical meaning to the adoption of the heliocentric world view: "Men who believed that their terrestrial home was only a planet circulating blindly about one of an infinity of stars evaluated their place in the cosmic scheme quite differently than had their predecessors who saw the earth as the unique and focal center of God's creation. The Copernican Revolution was therefore also part of a transition in the Western man's sense of

¹ "Sticking to his theory in the face of recalcitrant evidence, he [Galileo] used it to produce new facts which were inconsistent with the reigning dogma." John Krige, *What's so Great about Facts?* in John Irvine, et al. eds. (1979), p. 58.

² The tension between the emerging discourse on the bourgeois rights of the individual and the hierarchical authority of the Roman Catholic Church, is implicit in any discussion of the trial of Galileo. That the Church of Rome would impede the development of science, however, is a major assumption that is not beyond reproach.

values." (Kuhn. 1957, p. 2). But who are the *Western men* to whom Kuhn refers, other than those with ease of access to the writings of ancient and contemporary scholars of European science? The assertion, that a transition in the sense of values of *Western man* has occurred in part due to the adoption of the Copernican world view, is as unfounded as it is chauvinistic. The logic behind it is reminiscent, in an inverted sort of way, of the logic behind the accusation of religious demagogues, no matter the faith, that atheists possess a different set of values than their God-fearing counterparts. It is in keeping, however, with Kuhn's notion, as he later developed it in *The Structure of Scientific Revolutions*, (1962), of the importance of paradigm shifts in the development of the scientific enterprise.

Paul K. Feyerabend, in the chapter "Galileo and the Tyranny of Truth," in *Farewell to Reason*, (1987), describes "the conflict between Galileo and the Church as a conflict between traditions ... (or paradigms, or research programmes, or themes, to use terms from more narrow fields than that before us) ... traditions concerning the role of experts in society." (Feyerabend, 1987, p. 248). The one tradition, according to Feyerabend, "regards an expert as the final authority on the use and interpretations of expert views and expert procedures, the other subjects the pronouncements of experts to higher courts." (Ibid.). Such a simplistic exposition of conflicting paradigms in no way abrogates the importance of Feyerabend's work with respect to the scientific

revolution; (1975); nor does it epitomize that which may be viewed as his important contribution to a theory of knowledge. What Feyerabend is able to do, throughout the body of his work, is to postulate that there is an anarchic, non-determined, random, irrational element to our thinking and subsequent behavior. How that element becomes institutionalized into "traditions or paradigms or research programmes," has become an important research agenda for the history of scientific thought.

In many ways and in many times, the trial of Galileo has been characterized over and over again as a conflict between science and faith, (Koestler, p. 503), or "the right of a scientist to teach and defend his scientific views;" (Drake, 1957, p. 145); to wit, the trial of Galileo invariably has been characterized as a problem of conscience. (Brecht, *en passim*). Indeed, in order to be absolved of his "sins", it was first necessary, that "with a sincere heart and unfeigned faith," Galileo had to "abjure, curse, and detest ... the aforesaid errors and every other error and heresy contrary to the Catholic and Apostolic Roman Church ..." (de Santillana, 1955, p. 310). For the non-fundamentalist/catholic/ orthodox twentieth century bourgeois scholar, indoctrinated to hold the belief that academic freedom is a basic individual right, the sentence imposed upon Galileo was harsh. The banning of the book itself, by today's standards, was an egregious abuse of power. That he was also placed under house arrest, for the rest of his life, by today's standards, seems unconscionable.

The fact that the most productive stage of his academic career still lay ahead is often left unmentioned.¹⁰

Galileo, who was allowed to live out his life in relative peace and tranquility, has come to be considered the martyr of science. On the other hand, Giordano Bruno of Nola, who was burned at the stake by Catholics in Rome, on February 16, 1600, and Michael Servetus, who was burned at the stake by Calvinists in Geneva, in 1553, have come to be relegated to relative obscurity. The explanation that was advanced by Arthur Koestler, (1959), is that Bruno and Servetus were persecuted because of their religious opinions, and not their scientific writings. (Koestler, p. 451). The interpretation of Montucla, at least as it was advanced by William Whewell, (1875), is that it was Bruno's satirization of religion and the papacy in *Spacio della Bestia Trionfante*, and not his Copernican views, as expressed by "Il Nolano" in *La Cena de le Cenere*, (c. 1583), nor even the publication of *De innumerabilibus, immenso, et infigurabili, seu de Universo et Mundis*, (1591), "in which he maintains that each star is a sun, about which revolve planets like our earth," (Whewell, pp. 272,73), that was the basis of the heresy for which he was immolated. Nevertheless, it is my contention that the burden of proof, for what I consider to be an anachronistic argument, remains

¹⁰ The publication of *Dialogue Concerning Two New Sciences*, in 1638, marks the crowning achievement of Galileo's academic career. It was here that he developed the sciences of mechanics and dynamics that would have such a strong influence on the work of Isaac M. Newton, and the science of physics that was to follow.

to be made; and without the corroboration of the verdict itself, the conclusion that Bruno's martyrdom was exclusively the result of his religious views, is less than satisfactory. The antinomic relationship between religion and science had yet to be postulated. The subject/object split had yet to occur. At that time, there was no clear cut distinction between religion and science, despite the present-day anachronistic conceptualization to the contrary. The explanation of how it was that Galileo came to be thought of as the martyr of science must lie elsewhere.

As it was documented in the letter from Bellarmine to Dini, in 1615, to speak hypothetically and not absolutely about the Copernican system was "to speak with excellent good sense and to run no risk at all." (Koestler, p. 454). But to present the Copernican system as a fact, rather than as a model that could be used to save the "celestial appearances" better than the Ptolemaic system, was to claim veracity where probability had yet to be demonstrated. The Copernican system, as it was advanced by the work of Tycho Brahe and Johannes Kepler remains to this day an interesting alternative to the Ptolemaic system in its explanation of the rotation of the earth and its motion around the sun. The fact is, however, that the Ptolemaic system, as incorrect as it was in its assumption of the immobility of the earth with respect to the sun, was reasonably accurate in its understanding and predictions of celestial motion. The Copernican system, on the other hand, despite its correct assumption of the mobility of

the earth with respect to the sun, still relied on the Ptolemaic notion of epicycles, and it failed to appreciably improve upon the contemporary understanding or predictions of celestial motion. Perhaps a close reading of the actual verdict of the trial of Galileo, as it was pronounced in the sentence of June 22, 1633, will ameliorate this explication of the basis for an alternative interpretation of the crime of Galileo:

And whereas a book appeared here recently, printed last year at Florence, the title of which shows that you were the author, this title being: "Dialogue of Galileo Galilei on the Great World Systems"; and whereas the Holy Congregation was afterward informed that through the publication of the said book the false opinions of the motion of the Earth and the stability of the Sun was daily gaining ground, the said book was taken into careful consideration, and in it there was discovered a patent violation of the aforesaid injunction that had been imposed upon you, [See Santillana for the complete text of the Injunction of 1615], for in this book you have defended the said opinion previously condemned and to your face declared to be so, although in the said book you strive by various devices to produce the impression that you leave it undecided, and in express terms as probable: which, however, is a most grievous error, as an opinion can in no wise be probable which has been declared and defined to be contrary to divine Scripture.

... although it is there stated [within the injunction of 1615] that said opinion [the Copernican system] is contrary to Holy Scripture, you have nevertheless dared to discuss and defend it and to argue its probability ...

We say, pronounce, sentence, and declare that you, the said Galileo, by reason of the matters adduced in trial, and by you confessed as above, have rendered yourself in the judgement of this Holy Office vehemently suspected of heresy, namely, of having believed and held the doctrine - which is false and contrary to the sacred and divine Scriptures - the Sun is the center of the world and does not move from east to west and that the Earth moves, and is not the center of the world; and that an opinion may be held and defended as probable after it has been declared and defined to be contrary to the Holy Scripture; and that consequently you have incurred all the censures and

penalties imposed and promulgated in the sacred canons and other constitutions, general and particular, against such delinquents. From which we are content that you be absolved, provided that, first, with a sincere heart and unfeigned faith, you abjure curse and detest before us the aforesaid errors and heresies and every other error and heresy contrary to the Catholic and Apostolic Roman Church in the form prescribed by us for you. [*Italics mine*]. (de Santillana, pp. 306-10; Koestler, pp. 606-610).

The process of the declaration and definition of that which was contrary to Holy Scripture was familiar indeed to Galileo. But in order to understand this familiarity, perhaps we must first ask ourselves a few questions of faith. Precisely what was meant by a *probable opinion*? What constituted pre-scientific *probability*? By what authority was the Church able to adjudicate two conflicting *opinions*? Precisely what was meant by an *opinion*? These questions can only be answered by first considering the philosophical foundations upon which they were based. That the writings of Saint Augustine were used unsuccessfully by Galileo in his own defense, (Drake, 1957, pp. 175-216), does not elide the importance of Augustine's non-literal approach to his interpretation of Holy Scripture. (Robertson, p. xiii). That Augustine denies that "the lessons of Jesus Christ ... were learned from the writings of Plato," and insists that "the Platonists took from our literature whatever they said is good and truthful ..." (Augustine in Robertson, p. 65), recalls the importance of Platonic thought in the history and development of Christian doctrine.

This is not meant to suggest that even a comprehensive understanding of the many controversies surrounding the numerous interpretations of Platonic thought would serve as sufficient preparation for a cursory understanding of the many controversies surrounding the numerous interpretations of Christian thought. On the contrary, it is solely in regard to the development of an understanding of what was meant by a *probable opinion* at the time of Galileo, that a brief discussion of Plato's philosophy of knowledge is at least cursorily undertaken. To that end, the influence of Aristotle is also briefly discussed, followed, in turn, by a discussion of the casuistic method and its relationship to the scientific method. It is hoped that through this brief review, the foundation will have been laid for an understanding of how it was that the Copernican system had yet to be accepted as a probable opinion in the long enduring quest for certainty and truth.

Toward a Philosophy of Ideas

Referring to Alexander Koyré's *Etudes Galiléennes* (1939), Stillman Drake, (1978), describes it as a radical break from previous studies, in that Koyré emphasizes the abstract Platonic elements of Galileo's work, as opposed to the classificatory and authoritative Aristotelian influences, and as contrasted with the inductive experimental¹¹ approach more

¹¹"Experiment - in contradistinction to mere experience - is a question we put to Nature. In order to receive an answer we must formulate it in some definite language. The Galilean revolution can be boiled down to the discovery of that lan-

frequently associated with the Scientific Revolution. (Drake, 1978, p. xvii). A few years after the initial publication of *Etudes Galiléennes*, referring back to this very same work which he himself had already described as ill-fated, Koyré, (1943, p. 334), continued to emphasize, as a philosopher of science, that which distinguished the world of *Being* from the world of *Becoming*, as the difference between the primary and secondary qualities of things. (Koyré, 1948/65, pp. 3-24).

Of course, the discourse on what distinguishes primary qualities from secondary qualities has been perpetuated throughout the centuries as the discourse on the distinction between what constitutes objectivity versus what constitutes subjectivity. This is so at least with respect to our objective knowledge of position, number, shape, and motion, as contrasted with our subjective understanding of color, taste, smell, touch, etc. This self-same dichotomic conceptualization of Being and Becoming has also asserted itself over and again through dualistic mind/body, lord/bondsman, male/female, yin/yang, base/superstructure, and countless other arguments, to list but a few,¹² but it is important to recognize the

guage, to the discovery of the fact that mathematics is the grammar of science. It is this discovery of the rational structure of Nature which gave the *apriori* foundations to the modern experimental science and made its constitution possible." (Koyré, 1943, p. 347n).

¹² How this dualism asserted itself in the development of probability theory will be examined in the following chapter. For the present, it is sufficient to refer to I.J. Good, for his recognition that in one way or another, the myriad theories of probability may be classified as being either subjectivist or objectivist. (Good, 1965, p. 8).

general argument as existing between that which never changes, and that which is in constant flux.¹¹

In a sense, the subject/object split is predicated upon the formulation and subsequent denial of a dialectical relationship between being and becoming. This can only be accomplished so long as the incommensurable notion of changelessness is maintained. The eternal changeless subject is posited as being able to transform the transitory changeable object, without itself being transformed in the process. The logical basis for the subject/object split is established, along with the reactionary imperative that for the subject to deny its sovereignty necessarily results in its own constitution as or deference to the object. In order for the subject to maintain its identity and sovereignty, it must continue to transform and rule over the object. This general principle is exemplified through the representation of subject qua being as soul and object qua becoming as body in the following passage from the *Phaedo*:

So long as we keep to the body and our soul is contaminated with this imperfection, there is no chance of our ever attaining satisfactorily to our object, which we assert to be truth. In the first place, the body provides us with innumerable distractions in the pursuit of our necessary sustenance, and any diseases which attack us hinder our quest for reality. Besides, the body fills us with loves and desires and fears and all sorts of fancies

¹¹ From this perspective, when the subject is defined as the agent of change, and the object is considered to be the patient or that which is acted upon, the confusion that arises when subjectivity is associated with the secondary qualities or all that is sensible, and objectivity is associated with the primary qualities or all that is intelligible, is avoided. (See *Theaetetus*, en passim).

and a great deal of nonsense, with the result that we literally never get an opportunity to think at all about anything. Wars and revolutions and battles are due simply and solely to the body and its desires. All wars are undertaken for the acquisition of wealth, and the reason why we have to acquire wealth is the body, because we are slaves in its service. That is why, on all these accounts, we have so little time for philosophy. Worst of all, if we do obtain any leisure from the body's claims and turn to some line of inquiry, the body intrudes once more into our investigation, interrupting, disturbing, distracting, and preventing us from getting a glimpse of the truth. We are in fact convinced that if we are ever to have pure knowledge of anything, we must get rid of the body and contemplate things by themselves with the soul itself. It seems, to judge from the argument, that the wisdom which we desire and upon which we profess to have set our hearts will be attainable only when we are dead, and not in our lifetime. If no pure knowledge is possible in the company of the body, then either it is totally impossible to acquire knowledge, or it is only possible after death, because it is only then that the soul will be separate and independent of the body. It seems that so long as we are alive, we shall continue closest to knowledge if we avoid as much as we can all contact and association with the body, except when they are absolutely necessary, and instead of allowing ourselves to become infected with its nature, purify ourselves from it until God himself gives us deliverance. In this way, by keeping ourselves uncontaminated by the follies of the body, we shall probably reach the company of others like ourselves and gain direct knowledge of all that is pure and uncontaminated—that is, presumably, of truth. For one who is not pure himself to attain to the realm of purity would no doubt be a breach of universal justice." (Phaedo 66b, in Hamilton p. 49).

For Plato, it is the body, with its numerous interruptions, disturbances, and distractions, which prevents us from getting a glimpse of the truth. The path to this truth, is predicated upon the denunciation of the body and all that is sensible, in favor of the mind and only that which is intelligible. Pure knowledge is considered to be obtainable only in death, when the body and the soul have been separated. In *Against Epistemology*, Theodor Adorno implies that in order to separate the subject from the object, an act of violence must

be committed. (Adorno, 1956/83, pp. 12-18). I have interrogated the assertion of such an implication and reasserted elsewhere how such a violent separation of that which is otherwise inseparable often precludes and circumscribes any argument for or against its reformation;¹⁴ whenever dialectical contradictions are understood as antinomies, antinomies they remain.

An antinomic relationship was defined as existing between the world of Being and the world of Becoming. This relationship was contradicted by the insistence that the new cosmology no longer contrasted the immutability of the heavens with the mutability of the earth. Thereafter, the world was to be understood as consisting of but one universe of which the earth was as much a part as the heavens. This universe, however, was constantly changing. Nevertheless, the science of Galileo and those who were to follow would still somehow be concerned with those aspects of the universe that supposedly do not change. But rather than continuing to enter into the fray of philosophical discussions concerning the efficacy - indeed, the possibility - of a subject/object split,¹⁵ I will defer from any further discussion of Koyré's extrapolation of the primary and secondary qualities as being explicative of Being and Becoming. Instead, I will concentrate on the

¹⁴ See Dourounãs, *Hic Rhodus! Hic Salta!* (1995).

¹⁵ For a concise but comprehensive account of "The Subject-Object Problem in the Philosophy of the Renaissance," see Ernst Cassirer, *The Individual and the Cosmos in Renaissance Philosophy*, (1926/62), pp. 123-91.

theoretical relationships between Being and Becoming and Knowledge and Opinion, as they were developed throughout the Platonic dialogues," and adopted pro forma by the Catholic Church. It would otherwise be very difficult to appreciate the distinction between Knowledge and Opinion, as it was understood at the time of Galileo, or the various guises under which it has been able to continue to assert itself, to this very day.

Of course, the literature abounds with epistemological studies of Platonic thought. Especially in Great Britain, a long standing tradition of classical intellectual activity and scholarly work has resulted in numerous interpretations of Plato's so-called theory of knowledge, as well as classificatory schemes concerning the logical order of the dialogues themselves. In *Plato's Theory of Ideas*, (1951), David Ross compares the schemes of classical scholars Armin, Lutoslawski, Raeder, Ritter, and Wilamowitz, (Ross, 1951, p. 2), before offering a probable chronological order of those dialogues concerned with questions of epistemology and the development of a theory of ideas. That such a chronological ordering historically has received a great deal of negative criticism

" "As being is to becoming, so is truth to belief. If then ... amidst the many opinions about the gods and the generation of the universe, we are not able to give notions which are altogether and in every respect exact and consistent with one another, do not be surprised. Enough if we adduce probabilities as likely as any others, for we must remember that I who am the speaker and you who are the judges are only mortal men, and we ought to accept the tale which is probable and inquire no further." (*Timaeus*, 29cd in Hamilton, p. 1162).

on scholarly as well as methodological grounds,¹⁷ does not belie its usefulness, especially when it is utilized in conjunction with the type of stylometric ordering made famous by Lewis Campbell. (Stewart, 1909, p. 14; and Ross, 1951, p. 3). Employing just such a procedure, I. M. Crombie, (1962, p. 11; and 1964, p. 5), stylometrically arranges the thirty-five published works of Plato and his thirteen letters. He then reduces and chronologically rearranges the works into the following groupings, based upon the recognition of what he describes as the two big breaks in Plato's life: ¹⁸

Table 1. A Stylometric Ordering of the Authenticated Platonic Dialogues into Three General Chronological Periods

EARLY	MIDDLE	LATE
Apology	Parmenides	Laws
Charmides	Phaedo	Philebus
Cratylus	Phaedrus	Sophist
Crito	Republic	Statesman
Euthydemus	Symposium	(also called
Euthyphro	(also called	Politicus)
Gorgias	Banquet)	Timaeus and
Hippias Minor	Theaetetus	Critias (un-
Laches		finished se-
Lysis		quel to
Menexenus		Timaeus)
Meno		
Protagorus		

¹⁷In *Plato's Doctrine of Ideas*, J.A. Stewart adopts the order presented by Lutoslawski in *The Origin and Growth of Plato's Logic*, (1897), as a "matter of convenience," while conceding that "any light which 'chronology' may throw on the Doctrine [of Ideas] will not be lost." (Stewart, 1909, p. 14).

¹⁸ "Since there are two big breaks in Plato's life after the death of Socrates (the founding of the Academy, and the Syracusan imbroglio) it is reasonable to guess that most of the early dialogues were written in the nineties, [c. 490 BC], most of the late ones in the fifties, [c. 450 BC], and the middle ones in the period in between." (Crombie, 1962, p. 11).

Source: I. M. Crombie, 1962, p. 11.

Whatever formal disputes may exist with respect to the chronological ordering and even the authenticity of certain Platonic dialogues, there is veritable universal agreement among the classical scholars, that *Theatetus*, *The Sophist*, and *The Statesman*, form a trilogy within the seven dialogues, *Theatetus*, *Euthyphro*, *Sophist*, *Statesman*, *Apology*, *Crito*, and *Phaedo*, that center around the death of Socrates. (Klein, 1977 p. 3; Benardete, 1986, p. ix). A problem arises, however, since there is certainly no such veritable agreement concerning Plato's theory of knowledge. Because of the diversity of the points of view that are to be found within and across the different dialogues, it is possible to move from one dialogue to another, and cull support for what might be antithetical interpretations of the corpus magnum. As such, any cursory attempt to summarize the various dialogues and their contributions to epistemological theory is doomed from the outset to provide no more than a one-sided account of a particular interpretation. Moreover, as I.M. Crombie so cautiously advises, "we should not suppose that Plato had from the beginning and retained to the end a fully worked-out theory of knowledge expressible in such formulas as *"that knowledge is of the forms, and that there can be no knowledge of physical things."* (Crombie, 1964, p. 88). Be that as it may, it remains an important task to come to some basic if not universal understanding of what is meant by Plato's theory of knowledge.

In his review of George Grote's *Plato and other Companions of Sokrates*, (1865), or what Lewis Campbell describes as a refutation of Grote's theory of knowledge, (Campbell, 1883, p. viii), John Stuart Mill, (1866), by referring to different dialogues, discusses what Grote considers to be discrepancies in Plato's point of view concerning the Protagorean doctrine that *man is the measure of all things.*¹⁹ Mill then proceeds to discuss what he considers to be discrepancies in Plato's point of view concerning the relationship between True Opinion and Knowledge. It is the interpretation of these discrepancies that undoubtedly constitutes the refutation to which Campbell refers.²⁰ Mill, unlike Grote, takes the position that the error of the Protagorean doctrine, as it has been variously interpreted, is not one of language, but of thought. Also, Mill is of the opinion that the distinction between Truth and Opinion "gave

¹⁹"'Man is the measure of all things; of things which are, that they are, and of things which are not, that they are not. As things appear to me, so they are to me: as they appear to you, such they really are to you.' In other words, the doctrine of the Subjective nature of truth: which is a scandal to philosophers, as seeming to make all opinions equally true, and truth 'that each man troweth.'" (Mill, 1866, p. 348).

²⁰"That everything is true to me, which appears so to me, he [Grote] understands to mean, that my reception of it as truth depends, and ought to depend, on the impression which the evidence makes upon my own mind. Mr. Grote, therefore, defends the Protagorean doctrine against the Sokrates of the *Theatetus*; but his defence, though useful and instructive, does not satisfy us, and is the only important point in the whole work on which we find ourselves differing from Mr. Grote. For the truth of an opinion, even to myself, is a different thing from my reception of it as true, since it implies reference to an external standard." (Mill, 1866, p. 350).

Plato great trouble, and the whole subject of the truth and falsity of opinions was full of intricacy and logical embarrassment to him and his contemporaries."²¹ (Mill, 1866, p. 351). Nevertheless, Mill, in agreement with Grote, is able to direct the reader to those passages wherein Plato's theory of knowledge is most fully developed:

The most elaborate, but the obscurest exposition we find of this subject, [knowledge], is in the sixth and seventh books of the *Republic*. We cannot give it at length, but its leading point is, that knowledge is of Forms or Ideas, while Opinion relates to the world of sense, composed of mere images of those Forms. But the knowledge of Forms is only to be acquired by Dialectics. (Grote, vol. iii. pp. 84-93, and pp. 101,2; in Mill, 1866, p. 354).

Mill's direction should in no way be construed as the type of facile distillation to which Crombie above so prudently objected. Indeed, the passages found in Book VI of the *Republic* that describe the *Analogy of the Sun and the Good*, (505-509), and the *Divided Line*, (509-514), as well as the passage in Book VII, also known as the *Allegory of the Cave*, (514-520), have all received a great deal of scholarly attention.²² Again, it would be foolhardy to attempt to

²¹Whether or not, the distinction between Truth and Opinion was, in fact, a 'logical embarrassment to Plato and his contemporaries,' it has continued to be a conundrum to classical scholars to this very day. For a more thorough understanding of the problems concerning this distinction, see, in addition to those authors cited above: Lewis, 1850/1963; Cornford, 1935; Gulley, 1962; Sinaikko, 1965; Matthews, 1972; Lafrance, 1981; Moline, 1981.

²²The following may be mentioned: H. Sidgwick in *J. of Philol.* ii (1869), 96-103; H. Jackson, *ibid.* x. (1882), 132-50; J. Cook Wilson in *Class. Rev.* xviii (1904), 257-60; J.L. Stocks in *Class. Qu.* xv (1921), 73-88; A.S. Ferguson, *ibid.* 131-52, xvi (1922), 15-28, xxviii (1934), 190-210; H.J. Paton in *R. Soc. Prov.* xxii (1922), 69-104; F. M. Cornford in *Mind*,

briefly summarize the countless debates surrounding these passages, or to attempt to resolve arguments that have remained unresolved for over two thousand years. But simply in order to explicate the concepts associated with the various interpretations of Platonic thought, it is appropriate to refer directly to the source:

Suppose you take a line [AB], cut into two unequal parts [at C] to represent in proportion the worlds of things seen and things thought, and then cut each part in the same proportion [at B and D]. Your two parts [AB and BC] in the world of things seen will differ in degree of clearness and dimness, and one part [AB] will contain images; by images I mean first of all shadows, then reflections in water and in surfaces which are of close texture, smooth and shiny, and everything of that kind.

... Take the second part of this [BC] for the things which the images resemble, the animals about us and all trees and plants and all kinds of manufactured articles. ... Would you be willing to admit ... that in respect of truth and untruth there is the same distinction between the opinable and the knowable as there is between the image and the model? (*Republic*, 509d in Rouse, p. 309).

Plato begins with a discussion of the world of things seen, the generative world, the world of becoming, the mutable world of change, the world of opinion. He continues with a discussion of the world of things thought, the world of being, the immutable world of changelessness, the world of knowledge:

Now then consider how the section for 'things thought' should be divided. ... In the first part [CD] the soul in its search is compelled to use as images the things imitated - the realities of the former part [BC] - and from things taken for granted passes not to a new beginning, a first principle, but to an end, a conclusion; in the

xli (1932), 37-52, 173-90; N.R. Murphy in *Class. Qu.* xxvi (1932), 93-102, xxviii (1934), 211-13; R. Robinson, *Plato's Earlier Dialectic*, 151-213; H.W.B. Joseph, *Knowledge and the Good in Plato's Republic*, 13-60," (Ross, 1951, n. 39). Also: H.W.B. Joseph, 1948; J.E. Raven in *Class. Qu.* xlvii (1953), 22-32; I. M. Crombie, 1962, and 1963; H. L. Sinaiko, 1965.

second part [DE] it passes from an assumption to a first principle free from assumption, without the help of images which the other part [CD] uses, and makes its path of enquiry amongst ideals themselves by means of them alone. ... Now then accept these four affections of the soul for my divisions of the line: Exercise of Reason for the highest, Understanding for the second; put Belief for the third and Conjecture for the last. Then arrange the divisions in proportion, believing they partake of clearness just as affections which they represent partake of truth. (Ibid.).

There exists a world of things of which we have access through our senses. There also exists a world of ideas of which we have access through our minds. But a dialectical relationship exists within and between these two worlds. How this relationship is defined may be summarized as in the table below:

TABLE 2. -- The Divided Line

BECOMING (The Sun) The Light and Power of the Sun		BEING (The Good) The Offspring or Influence of the Good	
The World of Sight and of things seen		The World of Mind and of things thought	
A=	=B=	=C=	=D=
Images such as Shadows, reflec- tions	Objects such as Ani- mals, Trees and Manufac- tured things	Thought- images, Ideas such as Ideal Squares and Cubes	Ideas or Ideals such as Perfect Beauty Justice Goodness
The changing world of the senses		Mathematical Thought	Dialectical Thought
Con- jecture	Belief	Under- standing	Exercise of Reason
-OPINION-		-KNOWLEDGE-	
A		E	

Note: since $CE/AC = DE/CD = BC/AB$,
it follows that $BC = CD$.

Source: Rouse, W.H.D., 1956, p. 309.

A great deal can be made of the fact that Belief, which is represented by the line segment [BC], is equal to Understand-

ing, which is represented by the line segment [CD].²³ As to whether or not Plato was aware of this equality, in my opinion, is idle speculation. Similarly, without being able to refer to the original texts, it is impossible to state authoritatively whether or not the apparent lack of precision in Plato's utilization of terms was deliberate. This apparent lack of precision becomes obvious in the following passage which immediately succeeds the *Allegory of the Cave*:

We are content, then ... as before to call the first part science, and the second understanding, and the third belief, and the fourth conjecture: these last two together we may call opinion, and the first two exercise of reason. Opinion is concerned with becoming, and exercise of reason with being; and what being is to becoming, that exercise of reason is to opinion, and what exercise of reason is to opinion, that science is to belief, and understanding to conjecture. (Ibid., p. 333).²⁴

²³"It follows from this, though Plato does not mention it, that the second and third [line segments] are the same length." (Joseph, 1948, p. 25). Also see Rouse above.

²⁴At this point, W.H.D. Rouse makes a note that apparent ambiguities exist with respect to what Plato describes as the affections of the soul. "These four correspond with 'the affections of the soul' on p. 311 except that there Socrates put Exercise of Reason (instead of science) 'for the highest.' ... Here Exercise of Reason takes the place occupied by Knowledge on the diagram." (Rouse, p. 333n). In the passage preceding the *Allegory of the Cave*, Socrates advises Glaucon to "accept these four affections of the soul for my divisions of the line: Exercise of Reason for the highest, Understanding for the second; put Belief for the third and Conjecture for the last." (In Rouse, p. 311). The two passages agree with respect to Conjecture, Belief, and Understanding as being the last three affections of the soul, with Conjecture and Belief constituting Opinion. With respect to the first affection of the soul, according to Rouse, in the earlier passage, Socrates calls it the Exercise of Reason; in the later passage, he calls it Science. Also, what he calls Knowledge in the earlier passage, that which is comprised of the Exercise of Reason and Understanding, in the later passage he calls the Exercise of Reason, that which is comprised of Science and Understanding.

The Platonic schema itself is consistent. For the purposes of the discussion at hand, it matters little what Plato really meant, although a number of assertions and conclusions can be made with confidence and assurance. Science, the Exercise of Reason necessarily outweighs Conjecture, in the Platonic conceptualization of the Divided Line of Knowledge. Also, the separation of the world of Being from the world of Becoming is absolute, according, at least, to most scholars of Platonic thought.²³ Still, there does almost seem to be a somewhat vaguely reticent concession that whatever the equality that may be posited to exist between Understanding and Belief, it is analogous to the equality that may be

²³"The *Timaeus* also shows that he [Plato] still assumes an absolute distinction between knowledge (which is of Forms) and belief (*doxa*, which is of sensibles) (27d-29d, 51d-52a). The same absolute distinction is also found in the *Philebus* (58e-59c). It is a distinction which gives rise to serious difficulties in Plato's theory of knowledge;" (Gulley, 1962, p. 139); for Gulley, perhaps the greatest difficulty with this absolute distinction is that "Plato has failed adequately to distinguish the procedures of the philosopher and the physical scientist in a way which will justify his ascription of knowledge and certainty to the first, 'belief' and probability to the second." (Gulley, p. 146). Rather than the distinction between a priori and empirical knowledge, as described in the *Philebus* and the *Timaeus*, Gulley feels a "more satisfactory distinction would have been possible if Plato had developed further the distinction implied in the *Theatetus* between a priori and a posteriori knowledge. To claim as does Burnet in *Greek Philosophy*, "that an image is not the same thing as its original, but it is just as real," (Burnet, p. 349, in Cornford, p. 322), and therefore the absolute distinction between belief and knowledge does not obtain, would amount to "a revolution in Platonism" ... "The *Theatetus* admitted that perception was infallible; it was not knowledge because it lacked the other mark of knowledge; its objects are not real. If we now say that the objects are just as real as Forms, perception has every claim to be knowledge. This cannot be reconciled with the *Timaeus*." (Cornford, 1935, p. 322).

posited to exist between thought images and ideas, and objects such as animals, trees, and manufactured things. This, in turn, therefore or somehow implies that the boundary between the generative world and the world of essence," the world of Becoming and the world of Being, is itself as intentional as are the boundaries that are deliberately drawn to describe those shapes, so fleeting for philosophers, but fixed and familiar to their followers:

Students of geometry and arithmetic ... use visible figures and give lectures about them, while they are not thinking of these they can see but the ideas which these are like; a square in itself is what they speak of, and a diameter in itself, not the one they are drawing. It is always so; the very things which they model or draw, which have shadows of their own and images in water, they use now as images; but what they seek is to see those ideals which can be seen only by the mind. (Ibid., p.310)

An important metaphysical ramification of such an odyssey for those ideals which can be seen only by the mind, is that when Belief turns to Understanding, perhaps as Socrates opines (p. 57, above), no first principle is ever achieved, but only an end, a conclusion. In intellectual practice, therefore, Understanding, which is represented by mathematical thought, may be often confused with Science, or the Exercise of Reason, which is represented by dialectical thought.²⁷

²⁷See the translation of *The Republic*, by Paul Shorey, found in *Plato: The Collected Dialogues*. Edith Hamilton and Huntington Cairns, Eds. Princeton: Princeton University Press, 1989. Chapter VII, Verse 510a, in reference to the generative and the essential.

²⁸Any attempt to subdivide the so-called affections of the soul, as is illustrated by the Jesuit gradation, necessarily establishes criteria from which to critique the activity of the soul itself, or that which they called reason and its

But perhaps the most important implication of this blurring of the boundary between Being and Becoming, at least with respect to the discussion at hand, is the expectation that conjecture can become belief and in turn become understanding; opinion can become knowledge, and understanding can result in the exercise of reason or, otherwise put, become science. This, of course, may also be characterized as the movement, or at least the *hope* for movement, from Ignorance to the Truth.

Experience confronts us with becoming; that is *sensible* reality. But the *intelligible* reality, that which ought to be, is more real still and that reality does not change. Beneath the qualitative becoming, beneath the evolutionary becoming, beneath the extensive becoming, the mind must seek that which defies change, the definable quality, the form or essence, the end. Such was the fundamental principle of the philosophy which developed throughout the classic age, the philosophy of Forms, or, to use a term more akin to the Greek, the philosophy of ideas. (Bergson, 1911, p. 341).

The mind must seek that which defies change, the definable quality, the form or essence, the end. Perhaps this fundamental principle of the classical period has continued to hold sway up to this very moment. Perhaps the Pythagorean shapes and the Platonic forms that Kepler was unable to reconcile with the cosmology of Copernicus, were in a certain sense merely superseded by ellipses and centres of gravity, which in turn have been superseded by standards of deviation

attendant conscience. These criteria, however, subject to human intercourse and opinion, are arguably as veiled and presumably more esoteric than the mathematical constructs that were substituted for them in response to the inefficacy of denying cardinal requests. See *A Theological Undertaking*, pp. 76-83 below.

and bell curves. Perhaps the philosophy of forms, the philosophy of ideas, has been able to reconstitute its universal claim to truth. How such a claim does in fact continue to be made will be examined in the pages that follow. The validity of the claim, however, might need to be challenged or ultimately confirmed on a case by case basis. Such has been the development, to use a term more akin to the Enlightenment project, of the philosophy of science.

A Theological Undertaking

It would be absurd to imagine, that because at the time of Galileo the printed word was available to a relatively small constituency, there was ever any more of a general consensus on what constitutes truth and certainty than there is today. But the hierarchical structure of the Church of Rome has always insured that certain currents of thought, be they Platonic, Aristotelian, or esoteric for that matter, have contributed more or less to a mainstream of thought, as literally pontificated by the Apostolic See. That the foundations of Enlightenment thought may be discovered in various competing medieval philosophies is a thesis put forward by John Henry Randall, Jr. in *The Career of Philosophy*, (1962, pp. 13-43), in a conscious extension of the groundbreaking work of Pierre Duhem.²⁸ As to whether or not a continuity of

²⁸The works of Moody and Clagett, (1952), Edward Rosen, (1961), as well as those of Alexander Koyré cited above, have effectively challenged the thesis first put forward by Pierre Duhem in his *Etudes sur Leonard de Vinci*, (1906-1913), and *Systeme du Monde*, (1913-1959), that the science of mechanics,

thought did in fact exist from the time of the Middle Ages to that of the Enlightenment, or even to that of today, is an interesting academic problem which need not be broached in the present discussion.

What does need to be introduced, however, is the identification of a problematic that is implicit in any historical approach to the philosophy of knowledge. In his investigation of the prescientific or medieval Aristotelian-Thomistic notions of probability and opinion, Edmund F. Byrne, in *Probability and Opinion: A Study in the Medieval Presuppositions of Post-Medieval Theories of Probability* (1968), states the problem(atic) as follows: "how (to) correlate the fixity of thought with the flux of things?" (Byrne, p. xxi). For Byrne, "the difference between naive realism and critical idealism, between order in things and order in thought ... is due in no small measure to a gradual liberation of mathematics from the flux and instability of matter, which in turn has made possible a mathematical domination over that very flux and instability." (Ibid. p. xxii). To this might also be added a number of subsidiary problems, the solutions to which might be as illusory as their formulations. To wit, (how) has "the liberation of mathematics from the flux and instability of matter," if

as developed by Galileo, could be traced back to the organized schools of fourteenth century Paris and Oxford. But again these arguments are based on the assumption that the organized schools of fourteenth century Paris and Oxford were concerned with questions of theology rather than questions of natural science. See Randall, (1962), p. 272.

indeed such a liberation has occurred, impacted upon such questions as: What is meant by knowledge? What is meant by truth? What is meant by (un)certainty?

That Byrne has chosen to study the *medieval presuppositions of post-medieval theories of probability*, is less a testimonial to the continuity of human thought than it is to the fixity of scientific method. How it was that Thomas Aquinas "modifies Aristotle's rigorous conception of science in a variety of ways ... not so much (due) to reflection upon the nature of science as to what Thomas considers to be exigencies of the Christian faith ... (involving) above all, the need to defend the superiority of divine over human knowledge, and hence revelation over reason," (Ibid, p. 76, n3), is representative of a general line of inquiry most contemporary theorists of science and probabilism have been content to ignore. In his own review of the probabilistic literature, Byrne discusses the work of Pius Servien-Coculesco (1941-1952), and other continental authors such as Henri Poincaré, (1912, 1929), and J.L. Gendre, (1947), who have challenged the epistemic basis of classical probability theory, as it variously has been devalued by those historians and logicians of science already working in a mathematized tradition. A complete survey of these literatures will be presented in the chapters to follow. For the present, it is sufficient to note that a problematic exists, and alternative approaches have yielded alternative formulations as well as conclusions.

There can be no universal definitions of science and knowledge, or opinion and probability, as they may or may not be conflated, without the subsumption of numerous historical trends of thought and the assumption of a particular epistemological point of view. Furthermore, this particular epistemological point of view may in fact delineate a conceptualization of the Truth which precludes without presupposition any or all alternative postulations of human knowledge. Be they realist or idealist conceptualizations, without exception, human knowledge historically has been considered to be inferior to the knowledge of God." That the language of God has continued to be construed as being mathematical in nature has only served to equivocate rather

"Referring to the work of William G. Pollard, (1958), "that at least one scientist sees a reconciliation between science and religion precisely in terms of the scientific notion of chance and the theological notion of providence," (Byrne, p. 7), allows for the speculation that it is as much a belief in law(s) as it ever was a belief in god(s) that continues to regulate if not constitute the so-called quest for certainty. Moreover, as Byrne so convincingly draws the parallel between Thomistic theology and modern day positivism: "mutatis mutandis, this (Thomistic) theological vision of human knowledge is not unlike the vision of many modern scientists who have expressed themselves on the subject. For, whether one talks about God or about a beatific vision or about an ultimate comprehension of the universe, the epistemic goal remains the same, and opinionative knowledge of the probable is man's most familiar means of approaching it. The scientist as such, of course, does not speak about God, nor does the theologian as such speak about degrees of confirmation (levels of confidence) or relative frequency. But each is in some way aware of a postulated culmination of human reasoning which, however, he may care to describe it, gives finality to his intellectual endeavors. Indeed, it is only in the light of this postulated perfection of knowledge that he can speak at all meaningfully about the imperfection of what he already knows." (Byrne, pp. 282-3).

than excogitate the problematic. Whether or not the Jesuits or the Jansenists understood this is a speculative question which also need not be entertained in the present discussion. More in keeping with the scientific imperative of perspicuity, a description of the various casuistic approaches to the establishment of truth and the postulation of uncertainty would complete the foundation from which the contemporary approaches to probability theory may be categorized and criticized in the effort to expose that which has been implicitly defined as the limit(s) of human knowledge.

It would simplify matters to a great extent, in the development and establishment of a moral code of conduct or even a scientific method, if law were understood as that which is immutable. Moral philosophy and science could then be content with the theoretical articulation of any given law of nature or science, in conjunction with the practical consideration of whether or not a particular action or procedure is bound by or free of the law. Edmund F. Byrne identifies this conceptualization as the basis and impetus for the development of various theological systems, "in ascending order according to the amount of emphasis given to liberty over law: tutiorism, probabiliorism, equiprobabilism, probabilism, and laxism." (Byrne, p. 6). That which Byrne never does identify, however, is the ascending order according to which law itself approaches certainty or immutability. At least according to M. Paul Bert's translation of the works of Father Gury, S.J., Professor of Moral Theology in the College Romain, *Compendium*

Theologia Moral̃s and *Casus Conscientĩæ*, as *Compendium of Moral Theology and Cases of Conscience*, in his own *The Doctrine of the Jesuits*, (1879/1880), Bert describes the ascending order according to which law itself approaches certainty or immutability as: favorable or privileged law; unwritten and traditional law; penal and annulling law; ecclesiastical and civil law; and natural and divine law. (Bert, 1880, pp. 84-7). Through the presentation of these various theoretical categorizations of law, as well as their attendant practical codes of conduct or courses of action, a better understanding may be obtained of the theoretical and practical conditions against which arose the newly formed mathematical conceptualizations of knowledge and opinion, science and probability. Such a presentation constitutes *The Doctrine of the Jesuits*, which asserts that "Law is the external rule for human acts, as conscience, the practical voice of reason, is the internal rule for the same acts. (Ibid., p. 78).

In Kenneth E. Kirk's *Conscience and its Problem: An Introduction to Casuistry*, (1933), law is categorized as being either divine law or human law. In turn, these categories are subdivided into natural and revealed law, and civil and ecclesiastical law. (Kirk, 1933, p. 71). As it has already been alluded to above, the method of casuistry developed along with the ritual of confession, as a means for the absolution of sin, which was and still is defined as the willful transgression of that which is considered to be the law.

(Hardon, 1985, p. 408). As an aid to the confessor, the system of casuistry developed along with the understanding that the identification of theory and practice is interminably subject to interpretation. The practical recognition arose that perfection, the truth, or even the laws of nature, were ideals or general principles that could not always easily be applied to particular situations. This resulted in a meta-theory that attempted to interrogate the provability, and later the probability that the application of a general principle to a particular situation was reasonable.

Of course, this system, as it became institutionalized by the Dominican and Jesuit Orders of the Roman Catholic Church, became increasingly scrutinized from within and without, by the more rigorous supporters of a more literal and less interpretable understanding of divine or natural law. The stated impetus for this scrutiny, more often than not, was the charge of laxity in the application of the general principles and laws to particular situations, especially when the potential transgressors were privileged members of nobility or aristocracy. But no such charge of laxity could be leveled at the findings of the court at the trial of Galileo. The adherence to a rigorous application of the casuistic method is evidenced in the explanation as it was provided by Lord Cardinal Bellarmine. The question as to whether it was the abuse of casuistry or the casuistic method itself, which culminated in the antagonistic position of Blaise Pascal and inspired the *Fifth* and *Sixth* of his *Letters Written to a*

Provincial Gentleman [Antoine Arnauld, principle author of the *Port-Royal Logic*, (1662)], by *One of His Friends on the Subject of the Present Debates in the Sorbonne*, (1656), remains unanswered to this very day. (Jonsen and Toulmin, 1988, pp. 15, 233). The historical paradox, that it was Pascal's own quest for certainty that contributed in no small measure to what at least nominalistically has been called a probabilistic revolution, will be recounted in the chapter to follow. It will also be demonstrated that along with the demise of casuistry, at least in its application to questions of scientific veracity, came the acceptance of a new approach to probabilism: a probabilism determined and adjudicated by the roll of the dice, a probabilism circumscribed by the abstract world of mathematical reason and evidenced by belief in ideas beyond our senses, a probabilism unchecked by experimental or even experiential authority.

CHAPTER THREE

THEORETICAL FORMULATIONS

Une Science d'Ignorant

C'est en nous appuyant sur l'équipossibilité, telle qu'elle vient d'être définie*, que nous allons pouvoir mesurer le caractère aléatoire d'un événement. Au point de vue scientifique, on s'en tient exclusivement à la définition suivante: PROBABILITÉ, n.f., grandeur numérique choisie pour apprécier quantitativement le caractère aléatoire d'un événement. Le Dictionnaire de l'Académie ajouterait sans doute: ALÉATOIRE, adj., se dit d'un événement auquel on peut attribuer une certaine probabilité. *Pierre-Simon Laplace, *Essai philosophique sur les probabilités* (1814), rééd. (Chiron, 1920), quoted in Boll, (1941/62), pp. 13.

Marcel Boll, in *Les Certitudes du Hasard*, (1941), recognizes the vicious circle that presents itself when probability is defined in terms of its aleatory character, and aleatory is defined in terms of its attribute of possessing a certain probability.¹ He also recognizes that while probability does not possess any one single synonym, aleatory suffers

¹ "On a reconnu là un cercle vicieux ou tout au moins un pléonasme. Toutefois, cette insistance n'est pas inutile pour nous, car il s'agit de bien voir que ces deux mots se correspondent exactement. Ajoutons que, si "probabilité" ne possède aucun synonyme, "aleatoire" souffrir de l'excès contraire. Aux médiocres équivalences, que nous citons tout à l'heure, nous pouvons joindre: fortuit, éventuel, hasardeux, casuel, imprévu, inattendu, contingent. Tous ces termes aussi mal délimités les uns que les autres et auxquels nous renoncrons en principe. Les penseurs vigoureux sont d'ailleurs unanimes: Malheur au vague! mieux le faux; la vérité sort plutôt de l'erreur que de la confusion.* Les causes de notre ignorance sont multiples, mais la principale est dans l'imperfection du langage. Le vague des mots produit le vague des idées. Si nous prenions plus de soin de définir les termes au moyen desquels nous raisonnons, nos idées seraient plus nettes et plus sûres.** * Ernest Revan, *Souvenirs d'enfance et de jeunesse*, (1883). ** Anatole France, *Les sept femmes de la Barbe-Bleue*, (Calmann-Lévy, 1909), quoted in Marcel Boll, *Les Certitudes du Hasard* (Paris: Presses Universitaires de France, 1941/62), pp. 13-14.

from an excess to the contrary. It would be futile as well as inappropriate to attempt a literal translation of the list of words that Boll describes as mediocre equivalents of what is meant by aleatory. Roughly speaking, fortuitous, random, lucky, accidental, unforeseen, unexpected, and contingent, may all be used in describing that which is aleatory. As a matter of practice, these words are used interchangeably if not indiscriminately, as a consequence of and resulting in a lack of precision in the definition and articulation of the concept itself. "The causes of our ignorance are multiple, but the principle cause lies in the imperfection of the language. Vague words produce vague ideas." (Loc. cit.). As a general principle, Boll goes on to claim, the apparent vicious circle is in reality, a paradox of language, which holds true for all mathematical notions of experimental origin.³

That Boll has presented the reader with a number of fascinating and original intimations in the first few pages of his treatise, and then builds upon these intimations, makes it

³ "La définition de la probabilité n'est pas fondée sur un 'cercle vicieux'*, comme on l'a soutenu avec une pointe de paradoxe**. De même que toutes les notions mathématiques, la probabilité est d'origine expérimentale. * Voici où l'on avait cru découvrir une pétition de principe: chacune des 32 cartes a la même probabilité d'être choisie soit 1/32; on ne peut donc introduire la probabilité qu'en se servant ... de la probabilité. Objection d'ordre purement linguistique, qui se retrouve dans toutes les sciences et que Ferdinand Gonset (1936) a nommé le 'paradoxe du langage' (comment est-il possible de conférer à certains mots -- et en se servant que de ces mots eux-mêmes - un sens qu'ils n'ont encore jamais eu ?). ** Henri Poincaré, *Calcul des probabilités*, Naud, 1896." (Ibid., p. 15).

incumbent upon the reader to return to the original assumptions. First, Boll makes the claim that a vicious circle occurs because of the imprecise use of words. He then claims that this vicious circle is a paradox of language when it is being utilized to describe mathematical notions of experimental origin. Otherwise put, the mathematical reality precedes the theoretical conceptualization, which is then inadequately utilized to describe the mathematical reality. But to carry the argument one step further, these mathematical notions ultimately may also be considered to be geometrical, in a platonic sense, encompassing concepts or notions that are more abstract than those which are allowed by any concrete language.¹ Any attempt to articulate these concepts would

¹"At the beginning of the early modern period, it has been suggested in a provocative recent work, (*Les mots et les choses*, par Michel Foucault), European thought underwent a profound transmutation, the effect of which was to place the relationship between method and language at the heart of the epistemological concern. For the philosophers of the Renaissance, Foucault has argued, words and things were necessarily intertwined. Language was a thing in the sense that it existed in the world to be understood and interpreted by men, rather than being fashioned as their intellectual creation; things had a language in the sense that they exhibited external characteristics (or signatures) which constituted the mark of an essential quality or signified their relationship to other things. As a result, the basic epistemological procedure could be defined as the divination or interpretation that made possible the ordering of phenomena in an analogical hierarchy. [Hence the title of the English translation: *The Order of Things*]. For European thought as it took form in the Enlightenment, however, this intimate relationship between words and things was broken. In the structure and organization of thought, signatures gave way to signs, interpretation of the language of things to the representation of things in language. As a result of this new binary relationship between words, as signs, and the things that they represent, the essential epistemological procedure became one of structuring

necessarily result in confusion. As reasonable as this argument may appear, if indeed this is the logical conclusion of Boll's argument, it is fraught with inconsistencies. Perhaps the most blatant but compelling inconsistency arises from the assumption that experimental mathematical concepts are not amenable to theoretical symbolic articulation, coupled with the insistence that it is in the vagueness of the words that the ambiguity of the ideas may be found. But rather than attempt to refute or support the possible inconsistencies of this so-called paradox, the argument is ameliorated by turning to the historical moment at which this supposedly vicious probabilistic-aleatory circle of confusion first arose.

Probability may be tautologically defined in terms of the "quantitative attribute of the aleatory character of an event." But that the Latin word for dice, *alea*, also refers to that which is dependent upon contingent events, calls for a definition of contingency, rather than a premature assertion of tautology. Of course, contingency, like all the other mediocre equivalents in Boll's aleatory list, carries its own list of rough equivalents. But the etymological touching upon,

the world of phenomena by ordering a system of signs from simple to complex. Accordingly, the model of science became that of classification: the decomposition of sense data of the phenomenal world into the simplest elements, the denotation of these elements by the creation of signs, and the ordering and reordering of signs into systematic logical collections. It was this fundamental procedure that the philosophes came to understand as the fundamental scientific method vindicated in Newton's *Principia*." Keith M. Baker, *Condorcet: From Natural Philosophy to Social Mathematics* (Chicago: University of Chicago Press, 1975/1982), p. 110.

the tangential geometrical contiguous continuous understanding of that which is contingent, informs the definition of something happening by chance or some unknown cause.

The linguistic paradox associated with the specific definition of an experimental mathematical notion might be less important than the general definition of an event which is dependent upon or conditioned by something that is uncertain or unknown. The individual outcome of the throw of a set of fair dice is unknown because it is uncertain, and Boll does provide the reader with the following scale of degrees of aleation: "impossible, invraisemblable, douteux, plausible, très plausible, presque certain, certain." (Ibid., p. 9). Strongly but not strangely reminiscent of the Jesuitistic gradation, that there exists a continuous gradation from uncertainty to certainty deplatonizes the discussion to the point where the distinction between becoming and being is one of degree rather than kind. By definition, this is a sisyphian quantitative approach. But it was not until after the introduction of a normal curve that any such sisyphian quantitative conceptualization of truth, be it linear or not, could ever be reconceptualized in any way into an either/or proposition; it is the notion of a peak that allows for the division between either one side or the other, when no other qualitative typological or topological distinction is made. How it is that the uncertainty associated with such an arbitrary aleatory scale, linearly directed or not, as well

as the individual roll of the dice, are so amenable to precise quantification and easily divisible into groups, as a result, eg. of the central limit theorem of the law of large numbers, is perhaps one of the great mysteries of probability theory. It is hoped that through the course of the remainder of this chapter, on the various interpretations of probability theory, this particular mystery, which is based on but a few common assumptions, will be unraveled. In addition, a number of other common assumptions will be investigated in the hope of uncovering the bases of many other mysteries, perhaps even those as of yet still unknown.

The Beginnings of Modern Day Probability Theory

A genuinely dialectical relationship exists between the development of probability theory and the practical problems to which it has been applied. Would that it were true, it is certainly no accident of history that Blaise Pascal, the very same person who wrote and launched such a scathing attack against the case by case casuistic method in *The Provincial Letters*, and also devised such an unique argument as to why all people should believe in the existence of God, later to become known as *Pascal's Wager*,⁴ would also propose a mathematical solution to a problem associated with when it is wise

⁴ In *Le Dieu Cache*, translated into English by Philip Thody as *The Hidden God* (London: Routledge & Kegan Paul, 1964), Lucien Goldman develops the thesis that Pascal was not only the first modern scientist, but the first modern dialectician, as well.

not to gamble.' The manner in which probabilistic thinking has come to be the paradigm of legitimate rational human thought has been through its subsumption of the scientific project and what constitutes an epistemological position from which all knowledge may be judged: "'I am not satisfied with probability,' I said, 'I want certainty!'" Pascal wrote in *The Provincial Letters*. (Pascal, 1656/1967, p. 81). The irony of this statement is that in his quest for certainty, Pascal was in fact a major contributor to the mathematization of probabilism, which in turn has contributed to the establishment of scientific principles which ultimately testify to the uncertain nature of the universe.

"The story of the chevalier de Méré, the mathematically-minded gamester who brought the problem of points to the attention of Pascal in 1654, is a familiar one in the history of probability theory. In the subsequent correspondence between Pascal and Fermat, the calculus of probabilities was born, and with it the germs of the modern mathematical theory of decision-making.* In essence, Pascal was asked to estimate what would be a fair division of the stake between two players unable to finish a game of chance, taking in to account the expectations and risks that would be involved if the game were played to completion. In his formulation of the problem, the mathematician drew upon a body of reasoning developed by jurists concerned to establish the grounds for equity in such 'aleatory contracts' as gaming, maritime insurance, and life annuities, and to explore the notions of risk and expectation in contracts particularly subject to contingencies that might prevent their fulfillment (for example, shares in maritime commerce).** Given this background, it is perhaps not surprising that the calculus of probabilities, once elaborated in terms of games of chance, was quickly seized upon as potentially applicable to those probabilities in the social world for which the jurists had prepared the field. Nor is it surprising that it was Leibniz -- the mathematical genius concerned from his youth with the problem of formalizing the rationale of judicial evidence -- who first conceived the possibility of generalizing its applications in a logic of probable knowledge."*** Baker, (1975), pp. 155-56. Also see pp. 429-30, for full citations concerning the footnotes above.

Pascal's correspondence with Fermat, concerning the problem first posed by the Marquis de la Méré, (1654), as well Galileo's own *Considerazione sopra il Giuoco dei Dadi*, (c. 1600), concerning the possible outcomes for the tossing of three dice, and Huygens' *De Rationciniis in Ludo Alea* (Computations in Games of Chance), (1657), are generally cited as the beginnings of modern day probability theory. (Ore, 1953, p. 160). In point of fact, it was Hieronymus Cardanus, otherwise known as Jerome Cardan, *The Gambling Scholar*, (Ibid., en passim), who wrote the first known treatise based upon mathematical speculations concerning probabilism, *Liber de Ludo Alea*, (1564/1663). (Ibid., p. 122). "Whether one wants to call Cardano the inventor of the theory of probability, contrary to all present histories of science, or to name him its foremost pioneer is a matter of evaluation, but he is one of these two things." (Ibid., p. 50). In its own time, Cardan's analysis never entered the mainstream of scientific discourse, and for that reason alone, it is reasonable to infer, it has remained unheralded. It has had no traceable influence on the historical development of probability theory. But within its pages, Cardan does make what for the purposes of the present discussion might be one of the most important albeit neglected formulations of the genuinely two-fold nature of probability theory:

Card games differ from dice games when these require skill, because play with dice depends more on judgement of future events; mostly, to be sure, on the success of one's opponent but also on one's own success, while play with cards requires only judgement of one's present holding and

of one's opponents. To conjecture about the present is more the part of a prudent man skilled in human wisdom; but to conjecture about the future, although it is another kind of guessing, not as to what will be, but what we may rightly count on, is nevertheless the part rather of a divine man, or of an insane one, for the melancholy are given to prophesy. (Cardan, (1663), ch. 24; in Ore, (1953), p. 223).

The divine nature of conjecturing about the future, as compared to the mundane nature of conjecturing about the present, may be considered to be a theological formulation of the difference between *a priori* and *a posteriori* probabilistic applications, or inverse probability, as it was later called by Augustus De Morgan.⁴ Alternatively, without wishing to belabor the point, this distinction, between that which is divine and that which is mundane, or even that which is sacred and that which is profane, as Emile Durkheim was later wont to make, may again be interpreted as but another formulation, in a secular world, of the distinction between Being and Becoming. While Cardan's contribution to probability theory has been tenuous at best, he was aware of a temporal distinction which to this very day has continued to divide the scientific community in its understanding of such general principles as the second law of thermodynamics, as well as in its interpretation of entire subfields, such as molecular biology and quantum mechanics. Perhaps even more importantly, at least from a taxonomic or a more practical perspective, his work is representative of a classical approach to probability

⁴See the "Editors Preface," to *Facsimiles of Two Papers by Bayes*, Prepared under the direction of W. Edwards Deming, (New York & London: Hafner Publishing Company, 1963, p. iii).

theory which has yet to be reconciled with its rival (post) modern interpretations.

The Classical Approach

In his *Principles of the Theory of Probability*, (1939), Ernest Nagle distinguishes between what he considers to be three approaches to probability: The *classical* approach associated with Laplace; the *logical* interpretation of his own school of thought, associated with the economist John Maynard Keynes, and the *frequentist* approach hinted at by Aristotle, later "proposed by Bolzano and Cournot during the last century and further developed by Ellis, Venn, and Peirce, and ... finally made the basis for a subtle mathematical treatment of the subject by von Mises ..." (Nagle, pp. 18-19).

The inclusion of elements of rationalism as well as subjectivism in the notion of belief or expectation is what distinguishes the classical approach from the logical and the frequentist approaches, according to Nagle. (Ibid., p.18). In that sense, this classification also includes, along with the work of Laplace, that of Cardan, as well as Pascal, Liebnitz, Bernoulli, Bayes, and certainly Condorcet, for their attempts to attach a probability calculus to social issues, whether they be games of chance, trials by jury, or life assurance annuities:

Un grand homme*, dont je regretterai toujours les leçons, les exemples, & sur-tout l'amitié, étoit persuadé que les vérités des Sciences morales & politiques, sont susceptibles de la même certitude que celles qui forment le système des Sciences physiques, & même que les branches de

ces Sciences qui, comme l'Astronomie, paroissent approcher de la certitude mathématique.' * M. Turgot. (Condorcet, p. iii). [See translation below].

The distinction between the natural sciences, the prototype of which has always been astronomy, and the social, moral, or political sciences, is able to manifest itself to its fullest when the laws or axioms associated with the particular science are able to be reduced to basic mathematical principles. Paradoxically, the difficulties associated with reducing the social sciences to a set of basic mathematical principles has often been regarded as proof of the unscientific character of the disciplines so designated. An alternative interpretation would be to regard or consider the facility with which certain of the natural sciences are able to be reduced to basic mathematical principles merely as proof of a narrowly reductive definition of science itself.'

'"A great man whose teaching and example, and above all whose friendship I shall always mourn, was convinced that the truths of the moral and political sciences are susceptible of the same certainty as those forming the system of the physical sciences, even those branches of these sciences which, like astronomy, seem to approach mathematical certainty." (Baker, 1975, p. 197).

'"Let us make an exact analysis of things,' proclaimed Voltaire in the *Traité de métaphysique*.* It was the methodological cry of the century. The abbé Yvon provided a good working definition of the method to be followed in the appropriate article of the *Encyclopédie*. The 'analysis' he presented is essentially Locke's 'historical plain method' cast into the Cartesian mold of mathematics. It consists in going back to the origins of our ideas, generating them from their simplest components in sense experience and constantly creating different combinations of ideas in an attempt to reveal their relations from all points of view. Understood in this sense, Yvon argued, analysis is the secret of all scientific discovery; it proceeds always by the simplest steps; it is the enemy of all vague principles and everything contrary to

That a number of basic mathematical principles do in fact form the bases for many of the common sense assumptions associated with both the natural as well as the social sciences, whether or not any such distinction is deemed to be valid, but have remained hidden and disguised, is one of the underlying themes of this dissertation on the mathematization of knowledge. For the present, the above stated provisional distinction between the natural sciences and the social sciences will continue to be respected, simply in order to expedite the discussion of what Nagle has defined as the classical approach to probability theory.

Indeed, Laplace is considered to have written the quintessential statement of classical probability, in the introduction to his *Théorie Analytique des Probabilités*, 3d

exactness and precision. It seeks truth not by arbitrary definition but by explaining the generation of each idea; not by reference to general propositions but by 'a kind of calculus,' decomposing and recomposing ideas from the simplest components. It is the only method that can bring to the other sciences the certainty and precision of mathematics. For all the knowledge that man can attain, Yvon emphasized, derives ultimately from the simple ideas that come from sensation and reflexion. To arrive at more complex ideas we have only to bring these simple ideas together into different 'collections' as in mathematics, to follow the same order in proceeding from the most simple to the most complex, and to employ the same caution in the choice of language. In thus reasoning on the basis of simple ideas, Yvon maintained, man will never lose himself in vague abstractions, nor will he lose sight of the true limits of his knowledge. In destroying the myth of innate ideas, Locke had demonstrated that since man forms ideas on the basis of sensations he can make and remake them. As in mathematics, what it is in man's power to make, it is also in his power to make clearly and precisely."** *Voltaire, *Traité de métaphysique*, Louis Moland, ed., *Oeuvres complètes de Voltaire*, 52 vols. (Paris, 1877-85), 22:203 in Baker, (1975), pp. 110. ** *Encyclopédie*, 1:401 (s.v. "Analyse") in Baker, (1975), p. 111.

ed., (1820), also published separately, the preceding year, as *Essai Philosophique sur les Probabilités*. This introduction was "the développement d'une Leçon sur les Probabilités ... donnai en 1795, aux Écoles Normales," but it has retained its countenance as an expression of classical probability theory:

La théorie des hasards consiste à réduire tous les événements du même genre à un certain nombre de cas également possibles, cest-à-dire tels que nous soyons également indécis sur leur existence, et à déterminer le nombre de cas favorables à l'événement dont on cherche la probabilité. Le rapport de ce nombre é celui de tous les cas possibles est la mesure de cette probabilité, qui n'est ainsi qu'une fraction dont le numérateur est le nombre des cas favorables, et dont le dénominateur est le nombre de tous les cas possibles. (Laplace, 1820, pp. viii-ix).'

Although this classical definition of probability has been the subject of methodological as well as intellectual criticism for over two hundred years, it encapsulates a line of inquiry that persists to this day. As to whether or not the objections that have been raised concerning the assumptions of equipossibility and more importantly the assignation of a particular value, ie. the ratio of favorable to all possible cases have been satisfied through subsequent

' "The theory of chance consists in reducing all the events of the same kind to a certain number of cases equally possible, that is to say, to such as we may be equally undecided about in regard to their existence, and in determining the number of cases favorable to the event whose probability is sought. The ratio of this number to that of all the cases possible is the measure of this probability; which is thus simply a fraction whose numerator is the number of favorable cases and whose denominator is the number of all the cases possible." Laplace, *A Philosophical Essay on Probabilities*, Translated from the Sixth French Edition by F.W. Truscott and F.L. Emory, (New York: Dover Publications, 1951), pp. 6-7.

theorization, the reader should be able to make a sound judgement after these arguments have been presented.

But perhaps what is of far greater interest is the argument for determinism, implicit in the work of all the philosophes who were working in the classical tradition, but manifest in the work of Condorcet and Laplace, which, in the words of Silvan S. Schweber (1979/1982), allowed for the creation of an *impersonal, uncommunicative super-intelligence [which] replaced the ubiquitous, omnipresent personal Newtonian God in which they no longer believed*. (Schweber (1979); in Shimony, (1982), p. 333):

An Intelligence that knew the state of all phenomena at any given moment, the laws governing matter and the effect of these laws after any given period of time, would have perfect knowledge of the *System of the World*. Such knowledge is beyond our power, but it is the goal towards which all the efforts of philosophical mathematicians must be directed, and toward which they will draw closer and closer, without ever being able to attain it.

An intelligence that for one given instant could comprehend all the forces by which nature is animated and the respective situation of the beings who compose it, were she sufficiently vast to submit these data to analysis, would embrace in the same formula the movements of the greatest bodies of the universe and those of the lightest atom,; nothing would be uncertain for her and the future, as the past, would be present to her eyes. (Condorcet, (1768), and Laplace, (1819); in Schweber, (1979); found in Shimony, (1982), pp. 333f).

It is interesting to note that although Schweber insists that Condorcet and Laplace have replaced the *ubiquitous, omnipresent, personal Newtonian God* with an *impersonal, uncommunicative super-intelligence*, he correctly translates the Laplacian intelligence as being feminine, unlike Truscott and Emory (1902/1951), who translated the words of Laplace in

exactly the same manner, except for the intelligence which they presented as being neuter.¹⁰ As complicated as it is compelling, Schweber's thesis traces the links between the super-intelligent beings of Condorcet and Laplace, Descartes, Herschel, Darwin, Lyell and Maxwell, with the Platonic Demiurge found in *The Timaeus*.¹¹

¹⁰ "We ought then to regard the present state of the universe as the effect of its anterior state and as the cause of the one which is to follow. Given for one instant an intelligence which could comprehend all the forces by which nature is animated and the respective situation of the beings who compose it -- an intelligence sufficiently vast to submit these data to analysis -- it would embrace in the same formula the movements of the greatest bodies of the universe and those of the lightest atom; for it, nothing would be uncertain and the future, as the past, would be present to its eyes." (Tr. by Frederick Wilson Truscott and Frederick Lincoln Emory, *A Philosophical Essay on Probabilities*, New York: Wiley and Sons, 1902. Republished by Dover Books, 1951).

The translation above misses the fact that in the words of Laplace, the intelligence is gendered female: "Nous devons donc envisager l'état présent de l'univers comme l'effet de son état antérieur et comme la cause celui qui va suivre. Une intelligence qui, pour un instant donné, connaîtrait toutes les forces dont la nature est animée et la situation respective des êtres qui la composent, si d'ailleurs elle était assez vaste pour soumettre ces données à l'Analyse, embrasserait dans la même formule les mouvements des plus grands corps de l'univers et ceux du plus léger atome: rien ne serait incertain pour elle, et l'avenir, comme le passé, serait pré à ses yeux. (Laplace, Paris: Gauthier-Villars, 1886, Tome Septième, pp. vi-vii).

It is also interesting to note that the English equivalent of the French word *donné*, loses its actively objective connotation of being given, when it is stripped of case and simply translated as data. Gender and case are both lost in the English translation.

¹¹ That the selection process of the super-intelligent being becomes randomized in the Darwinian conceptualization is critical for an understanding of how the deterministic Laplacian schema which had no need for the hypothesis of a God could be replaced by the Darwinian one which had no need for the hypothesis of determinism. More of this later.

But for the purposes of the discussion at hand, what is important to understand is that as a result of the work of Condorcet and Laplace, at least according to my reading of Silvan Schweber, the knowledge of the demiurge, the demon, the god could now be approximated by that of the super-intelligent being, ie. the scientist. (Loc. cit., p. 353). It is the method of inquiry with which the scientist is able to acquire the knowledge, that has redefined the classical approach:

I present here without the aid of analysis the principles and general results of this theory, [*The Analytical Theory of Probabilities*], applying them to the most important questions of life, which are indeed for the most part only problems of probability. Strictly speaking it may be said that nearly all our knowledge is problematical; and in the small number of things which we are able to know with certainty, even in the mathematical sciences themselves, the principal means for ascertaining truth -- induction, and analogy -- are based on probabilities; so that the entire system of human knowledge is connected with the theory set forth in this essay.
(Laplace, 1819/ 1951, pp. 1f.).

That analogy and induction could be subsumed by a mathematical theory of probability, represents a major epistemological shift in the philosophy of knowledge that could only be accomplished through a radical departure from the understanding that analogy and induction were forms of probable reasoning, rather than the other way around. Before proceeding to discuss alternative interpretations of probability theory, therefore, it is appropriate to discuss alternative interpretations of analogy and induction, and their relationship to probability, at least as it was understood before the development of a calculus of probability.

The Analogy of Probability

At first glance, the statement made by Laplace concerning the relationship between analogy and induction, and probability, appears to corroborate the *Epigraph* of William Fitzgerald, A.M. (1849), to *The Analogy of Religion*, (1736/1849), by Bishop Joseph Butler, LL.D., where probable reasoning is described as reasoning by analogy, which, it is also posited, merely differs from induction with respect to its degree of accuracy:

[The term analogy has been used in different senses by logical writers. In the passage which Butler has taken for his motto,¹² Quintilian is speaking of grammatical analogy ... Analogy was, indeed, amongst the ancients, an established part of grammar. By the schoolmen analogy was generally referred to terms rather than propositions. Analogous words were made to hold a kind of middle place between equivocal and unequivocal; and were divided into *analogia proportionis* and *analogia attributionis*. ...]

Analogy is properly the resemblance of relations ... But analogy, in the common use of modern metaphysical writers, is used to express such arguments from resemblance as fall short of full proof; and as such is contradistinguished from strict induction. Thus Locke distinguishes it from direct experience, and treats it as an useful but lower kind of probable reasoning, b. iv. c. 16. So Hartley: "Where the instances from which the induction is made are alike, as far as we know, to that under consideration, at least in all things that affect the present inquiry, it affords the highest probability, and may be termed induction in the proper sense of the word. ... But if the instance under consideration be in some respects like the foregoing ones, in others, not, this kind of proof is generally termed one taken from analogy." -- *On Man*, c. iii. s. 2, prop. 87, p. 215, Lond. 1834. ... An exacter description is given by Mr. Mill (*Logic*, II. pp. 97-8): "Analogical reasoning may be reduced to this formula: two things resemble each other in one or more respects; a proposition is true of the one:

¹²"Ejus (Analogiæ) hæc vis est, ut id quod dubium est, ad aliquid simile, de quo non quæritur, referat ut incerta certis probet." *Quinct. Inst. Orat.*, l.i.c.vi. (Butler, motto).

therefore it is true of the other. But we have here nothing to discriminate analogy from induction, since this type will serve for all reasoning from experience. In the most rigid induction, equally with the faintest analogy, we conclude because A resembles B in one or more properties, that it does so in a certain other property. The difference is, that, in the case of a real induction, it has been previously shown, by a due comparison of instances, that there is an invariable conjunction between the former property or properties and that the latter property: but in what is called analogical reasoning no such conjunction has been made out;" -- nor the reverse, as he presently adds. Butler, however, appears manifestly to use the term analogical reasoning, or reasoning by analogy, in a still more extensive sense, as embracing all reasoning from resemblance, i.e. all probable reasoning whatever. The analogies with which this argument deals are indeed, in general of that imperfect sort which do not amount to strict inductions. But, according to his view, they differ from inductions not so much in kind as in degree. They differ in the degree of accuracy with which we have ascertained the circumstance, in which a known and an unknown event to resemble each other to be material] (Butler, pp. 1-2).

As instructive as this editorial epigraph may be in its description of the nuanced distinctions between probable reasoning as reasoning by analogy, and reasoning by experience as a higher kind of probable reasoning, also known as induction, it fails to address the problem of induction, as it was contemporaneously being formulated by Hume, and perhaps even more important for the purposes of the discussion at hand, the distinction between what Butler meant by probability¹¹ and

¹¹ Very early in his treatise on religion, Butler proceeds to abjure from any further inquiry into probability theory: "It is not my design to inquire further into the nature, the foundation, and measure of probability; or whence it proceeds that likeness should beget that presumption, opinion and full conviction, which the human mind is formed to receive from it, and which it does necessarily produce in every one; or to guard against the errors, to which reasoning from analogy is liable. This belongs to the subject of Logic; and is a part of that subject which has not yet been thoroughly considered." (Butler, pp. 6-7; and in Keynes, 1921, p. 79n).

how the notion of probability itself was in the process of being reformulated:

Probable evidence, in its very nature, affords but an imperfect kind of information; and is to be considered as relative only to beings of limited capacities. For nothing which is the possible object of knowledge, whether past, present, or future, can be probable to an infinite Intelligence; since it cannot but be discerned absolutely as it is in itself, certainly true, or certainly false. But to Us, probability is the very guide to life. (Ibid., p. 5).

It is to the credit of the scholarship of I.J. Good, that the common sense statement of how probability is the very guide to life is properly attributed to Cicero; (Good, 1950, p. v); but the probabilities to which Butler and Cicero refer are very different from the probability of Laplace. It is no small wonder that John Maynard Keynes, who has written such a comprehensive logical interpretation of probability theory, in *A Treatise on Probability*, (1921), would refer to the historical record for authoritative support. After locating the modern origins of a logical interpretation of probability theory in the work of Butler, (see footnote 13 above), he appeals to the authority of Aristotle:

The natural man is disposed to the opinion that probability is essentially connected with the inductions of experience and, if he is a little more sophisticated, with the Laws of Causation and of the Uniformity of Nature. As Aristotle says, "the probable is that which usually happens." Events do not always occur in accordance with the expectations of experience; but the laws of experience afford us a good ground for supposing that they usually will. The occasional disappointment of these expectations prevents our predictions from being more than probable; But the ground of their probability must be sought in this experience, and in this experience only. (Keynes, 1921, p. 79f).

Keynes begins the chapter he has called an *Historical Retrospect* with a statement of what might today be called the

dissolution of the problem of induction. As it presently will be discussed in detail, the fact that "events do not always occur in accordance with the expectations of experience," gives rise to the question as to what it is in experience that allows for the expectation of a recurrence of that which has already passed. But for Keynes, the logician, the logic of probability is based upon experience. He continues:

This is, in substance, the argument of the authors of the Port Royal Logic (1662), who were the first to deal with the logic of probability in the modern manner: 'In order for me to judge of the truth of an event, and to be determined to believe it or not believe it, it is not necessary to consider it abstractly, and in itself, as we should consider a proposition in geometry; but it is necessary to pay attention to all the circumstances which accompany it, internal as well as external. I call internal circumstances those which belong to the persons by whose testimony we are led to believe it. This being done, if all the circumstances are such that it rarely or never happens that the like circumstances are the concomitants of falsehood, our mind is led, naturally to believe that it is true.' Eng. Trans, p. 353. (Loc.Cit.).

Keynes continues with an abbreviated quotation from a passage in *An Essay Concerning Human Understanding*, (1690), where John Locke "follows the Port Royal logicians very closely," (Ibid.), in his description of the manner in which probable reasoning proceeds:

Probability is likeliness to be true. ... The grounds of it are, in short, these two following. First, the conformity¹⁴ of anything with our own knowledge, observation, and

¹⁴ "'conformity.' -- Analogy or likeness to what each man has already had experience of -- the custom of his previous experience -- is taken as the basis of what appears probable to him. And as in these respects men differ indefinitely, what appears probable to one may seem the reverse to another, whose analogies of personal experience, and the reports he has received from others, have formed a different criterion of likelihood in his mind." (Locke, n3, in Keynes, p. 365).

and experience. Secondly, the testimony of others, vouching their observation and experience"; *An Essay concerning Human Understanding*, book iv. "Of Knowledge and Opinion." (Loc. Cit.).

Keynes was correct in linking the tradition of the Port Royal Logic, with that of John Locke and Bishop Butler, but perhaps the links are even stronger than Keynes imagines:

... essentially the same opinion is maintained by Bishop Butler: 'When we determine a thing to be probably true, suppose that an event has or will come to pass, it is from the mind's remarking in it a likeness to some other event, which we have observed has come to pass. And this observation forms, in numberless instances, a presumption, opinion, or full conviction that such event has or will come to pass.' Introduction to the Analogy. (Ibid., 80).

This decontextualization by Keynes ignores the subsequent passages, wherein Butler, like Locke, the authors of the *Port Royal Logic*, and the Jesuits before them, for that matter, assumes the existence of a moral faculty that is capable of distinguishing between belief and knowledge;¹⁵ and it is here that the difference between a logical approach to probability, and the so-called classical interpretation of probability theory begins to become apparent. Keynes, the empiricist, proceeds to appeal to the authority of David Hume, the

¹⁵ "This section suggests the consideration of testimony in its wide meaning, including historical criticism and credibility; also the weight due to the evidence of authority in disputed questions of science, and of philosophical and religious thought." (Ibid., n3). Locke continues: "In the testimony of others, is to be considered: 1. The number. 2. The integrity. 3. The skill of the witness. 4. The design of the author, where it is a testimony out of a book cited. 5. The consistency of the parts, and circumstances of the relation. 6. Contrary testimonies." (Ibid., p. 366).

¹⁶ "... in all the parts of knowledge, there is intuition; each immediate idea, each step has its visible and certain connexion: in belief, not so." (Locke, 1690/1959, p. 365).

philosopher, in what Keynes perceives to be a bifurcation of the points of view of the philosophical or logical tradition from that of the mathematical. But Keynes continues to ignore the problem of induction, and even goes on to claim that probability based upon experience was never repudiated, even by James (Jacob) Bernoulli, who Keynes considered to be "the real founder of the classical school of mathematical probability." (Ibid., 81).

Nevertheless, the problems he identifies with the classical approach to probability theory deserve a full and explicit expostulation and may be found in sections three through eight of Keynes' historical chapter. The argument may be summarized as two-fold: First, it was in the development of his own central limit theorem and the law of large numbers, that Bernoulli implicitly accepted the principle of non-sufficient reason, or what Keynes described as the principle of indifference. To cut to the quick, the argument was an explanation or a metaphor, if you will, for the assumption of equi-possibility. Second, the application, by Laplace, of the law of succession, is perhaps an illustration of the problem of induction; but it is interpreted, by Keynes, as evidence of the weakness of the classical theory.

That Keynes ignored the problem of induction in his own logical interpretation of the theory of probability in no way diminishes the importance of his criticism of the so-called mathematicians, as long as the problem continues to be ignored in the classical interpretation, as well. These were Keynes'

seemingly unimpeachable objections to what he called the classical school of mathematical probability; but as compelling as they are, to a great extent, they remain unexceptionable, only as long as his assumptions concerning experience and the Humean problem of induction are collectively ignored. For that reason, therefore, before proceeding to an explication of any of the other various contemporary interpretations of probability theory, the consideration, first, of a more in-depth account of the problem of induction, as it was originally formulated by David Hume, would seem to be appropriate. After all, it was Hume's skepticism of the power of reason, and his flat out denial that the future need ever resemble the past, that has posed a challenge to posterity that has yet to be satisfied, and a conundrum for probability theory that has yet to be resolved.

The Problem of Induction

Throughout the corpus of his work, Karl R. Popper imagines that he has solved the problem of induction: "Of course, I may be mistaken; but I think that I have solved a major philosophical problem; the problem of induction. (I must have reached the solution in 1927 or thereabouts)," he immodestly claims in the opening passage of *Objective Knowledge: An Evolutionary Approach*, (1972). But he does realize that his critics remain unconvinced by his solution, since his solution amounts to the assertion, or recognition if you will,

that not even "an appeal to probability," can satisfactorily resolve the "invalidity of any 'principle of induction.'"

The idea of a principle of induction is that a statement -- to be regarded as a metaphysical principle, or as a valid *a priori*, or as probable, or perhaps as a mere conjecture -- which, if true, would give good reasons for our reliance upon regularities. If by 'reliance' is meant merely pragmatic reliance, in the sense of Pr2, ['Which theory should we prefer for practical action, from a rational point of view?' Ibid. p. 21], upon the rationality of our theoretical preferences, then clearly no principle of induction is needed; we do not need to rely on regularities -- that is, on the truth of theories -- to justify this preference. If on the other hand, 'reliance' in the sense of Pr1 ['Upon which theory should we rely for practical action, from a rational point of view?' Ibid. p. 21], is intended, then any such principle of induction would simply be false. Indeed in the following sense it would even be paradoxical. It would entitle us to rely on science; whereas today's science tells us that only under very special and improbable conditions can situations arise in which regularities, or instances of regularities can be observed. In fact, science tells us such conditions occur hardly anywhere in the universe, and if they occur somewhere (on earth, say) they are liable to occur for periods which will be short from a cosmological point of view. (Ibid. pp. 28-29).

Of course, this might all be shrugged off as the relativistic theorizing of an anti-positivist who has capitulated to Hume's skepticism, while failing to recognize the astounding advances and achievements of modern science. And indeed, Popper is perhaps one of modern science's most ardent critics. [See Epigraph]. But Popper remains steadfast in his defense of empiricism, while he continues to question what very early in his career he described as *The Logic of Scientific Discovery*,¹⁷ (1934/1959). "I never said that we

¹⁷ Although *The Logic of Research* would be a more literal translation of the title of Popper's *Logik der Forschung*, with so much contemporary scientific research being confined to those repetitive processes associated with what Thomas Kuhn

cannot learn from experience. Nor did I ever say that we cannot successfully use the simple inductive rule -- if the objective conditions are appropriate. But I do assert that we cannot find out by induction whether the objective conditions are appropriate for using the simple inductive rule." (Popper, 1983, p. 322).

Nevertheless, Popper believes he has done away with the problem of induction. In the first case, which deals with what he calls *theoretical preference*, or "Which theory should we prefer for practical action, from a rational point of view?" (Loc. cit. p. 21), Popper claims that "no principle of induction is needed, [since] we do not need to rely on regularities -- that is, on the truth of theories -- to justify this [or any theoretical] preference." In the second case, which deals with what Popper calls *practical action*, or "Upon which theory should we rely for practical action, from a rational point of view?" (Loc. cit. p. 21), he claims that "any such principle of induction would simply be false." His assertion, that "today's science tells us that only under very special and improbable conditions can situations arise in which regularities, or instances of regularities can be observed," in my opinion, unfortunately begs the question. It is precisely the identification of regularities, whether they are considered to be laws of nature, constant conjunctions, or

has called *normal science*, the English word *research* fails to convey the searching or exploratory connotation of its German equivalent. *Scientific Discovery* is a much closer approximation of what Popper had in mind.

simple resemblances, that traditionally has been understood to be the primary task of science. That these regularities might today be considered to be statistical and verifiable through the mathematical techniques that have been developed over the past three centuries is one of the basic theses of this dissertation. But this historical fact does not obviate the problem of induction as it was stated by Hume and acknowledged if not, at least to my satisfaction, resolved by Popper:

Hume argued that there can be no valid demonstrative (or logical) argument which would allow us to show '*that those instances, of which we have had no experience, resemble those, of which we have had experience*'. Consequently, '*even after the observation of the frequent or constant conjunction of objects, we have no reason to draw any inference concerning any object beyond those of which we have had experience*'. For '*should it be said that we experience*' -- experience teaching us that objects constantly conjoined with certain other objects continue to be so conjoined -- then, Hume says, '*I would renew my question, why from this experience we form any conclusion beyond those past instances, of which we have had experience*'. In other words, an attempt to justify the practice of inductive logic is untenable, and the attempt to justify it by an appeal to experience must lead to an infinite regress." The italics are all Hume's. Cf. Hume's *Treatise of Human Nature*, 1739-40, book i, part iii, sections vi and xii. (Selby Bigge's edition, pp. 89, 139, and 91.) Hume was very clear that an appeal to probability would not alter the situation. Thus he writes in his *Abstract*: '*Tis evident that Adam, with all his science, would never have been able to demonstrate, that the course of nature must continue uniformly the same ... Nay, I will go farther, and assert, that he could not so much prove by any probable arguments, that the future must be conformable to the past. All probable arguments are built on the supposition, that there is this conformity betwixt the future and the past, and therefore can never prove it.*' Cf. *An Abstract of a book lately published entitled a Treatise of Human Nature*, 1740, ed. by J. M. Keynes and P. Sraffa, 1938, p. 15. (The italics are Hume's) (Popper, 1983, p. 330).

Popper's solution to the problem of induction, is to deny its resolution. Like Hume, he agrees that with respect to the

identification of the regularities, the inductive principle is false. Unlike Hume, he goes on to develop a theory wherein rational arguments may be devised to prefer one theory or explanation over another. This aspect of Popper's work need not be delved into to any greater extent in the present discourse. That or whether or not he failed or succeeded in his solution of the Humean problem of induction is subsidiary to the contradictory nature of the relationship between an objectivistic understanding of *Being* and a subjectivistic understanding of *Becoming*. Again, this, the philosophical aspect of Popper's work need not detain us here. But a fuller understanding of Hume's skeptical empiricism and the Humean distinction between conjecture and belief is prerequisite to a critical understanding of neo-classical probability theory in general, or certainly Popper's propensity theory in particular; it is also a primer to the logic behind the various alternative contemporary theories of probability.

Perhaps for those who believe in the transitory nature of the universe, there can be no world of *Being*. But for those who believe in the existence, the phenomena, the surface, rather than the essence, the noumena, the substance, the distinction between conjecture and belief may be as kindred as is the distinction between belief and knowledge, for those who believe in the eternal nature of the universe. No one to my knowledge describes as well, one such interpretation of the distinction between conjecture and belief, as does David Hume in *An Abstract of A Treatise of Human Nature*, (1740). Much

like the Platonic distinction between Becoming and Being, the Humean distinction between conjecture and belief is one of kind; it is not one of degree.

As Hume (1740) describes his own work (1739), in the third person, "He calls a *perception* whatever can be present to the mind, whether we employ our senses, or are actuated with passion, or exercise our thought and reflection. He divides our perceptions into two kinds, viz. *impressions* and *ideas*. When we feel a passion or emotion of any kind, or have the images of external objects conveyed by our senses; the perception of the mind is what he calls an *impression*, which is a word that he employs in a new sense. When we reflect on a passion or an object which is not present, this perception is an *idea*. *Impressions*, therefore, are our lively and strong perceptions; *ideas* are fainter and weaker. This distinction is evident; as evident as that betwixt feeling and thinking." (Hume, 1740, pp. 8-9). This kindred distinction between feeling and thinking, the stronger unmediated *impression* and the weaker mediated derivative *idea*, is carried over, albeit inversely, to the qualitative distinction between conjecture or what Hume calls "reveries of the imagination" or "loose conceptions" and belief. A belief is an *idea*, and in that sense, it is weaker than an *impression*. But because of experience, the mind "feels something different in the conception of it from a mere reverie of the imagination. The presence of this visible object [of experience], and the constant conjunction of that particular effect, render the *idea*

different to the *feeling* from those loose ideas, which come into the mind without any introduction." (Ibid., pp. 18-19).

But in order to "ease the reader's memory," as Hume implicitly admits the complexity of his presentation, perhaps it would be helpful to summarize the *chain of propositions* Hume develops in the course of his argument:

No matter of fact can be proved but from its cause or its effect. Nothing can be known to be the cause of another but by experience. We can give no reason for extending to the future our experience in the past; but are entirely determined by custom, when we conceive an effect to follow from its usual cause. But we also believe an effect to follow, as well as conceive it. This belief joins no new idea to the conception. It only varies the manner of conceiving, and makes a difference to the feeling or sentiment. Belief, therefore, in all matters of fact arises only from custom, and is an idea conceived in a peculiar manner. (Ibid., p. 19).

Later in the passage, Hume attempts to explain "the manner or feeling, which renders belief different from a loose conception. He seems sensible, that 'tis impossible by words to describe this feeling, which every one must be conscious of in his own breast. He calls it sometimes a stronger conception, sometimes more lively, a more vivid, a firmer, or a more intense conception. And indeed, whatever name we may give to this feeling, which constitutes belief, our author thinks it is evident, that it has a more forcible effect on the mind than fiction and mere conception. This he proves by its influence on the passions and on the imagination: which are only moved by truth or what is taken for such. (Ibid., pp. 19-20).

Perhaps Hume's review of his own work is a bit overstated as to whether or not he did in fact prove any or all of the above propositions, assumptions, or assertions. But one thing is clear. It is here that a close reading of Hume allows for the analogy to be made between a subjectivistic conceptualization of a qualitative distinction between conjecture and belief, and an objectivistic conceptualization of a quali-

tative distinction between belief and knowledge. It is here that a close reading of Hume allows for a bridge to be made between objectivist and subjectivist conceptualizations of truth. Without a doubt, the Platonic distinction between Knowledge and Belief is predicated upon a distinction between Truth and Falsehood, whether that distinction is one of kind or degree. Without a doubt, the distinction between Truth and Falsehood is also based upon the *presupposition* that some Truth does in fact exist.

In *Higher Superstition: The Academic Left and Its Quarrels With Science*, (1994), Paul R. Gross and Norman Levitt attempt to illustrate the folly if not danger of anyone who has not had sufficient training in science or engineering to attack what the liner notes describe as "the principles and practices that underlie 300 years of scientific achievement." Unlike Popper, who believes that the true users of science ought to be "the amateur, the lover of wisdom, the ordinary, responsible citizen who has a wish to know," (See *Epigraph*), Gross and Levitt believe that it is only through years of highly technical and instrumental training that the student is able to develop "the fundamental frame of mind, the attitudes, the intellectual rhythms needed if one is to acquire useful knowledge." (Ibid., p. 245). Gross and Levitt quite passionately articulate the concerns of the authoritarian mind, as intolerant of alternative interpretations of *objective* phenomena, as it remains deliberately ignorant of *The Crisis of European Sciences*, (1954/1970), of which Husserl

wrote over sixty years ago. Their concern over what may or may not be the implied threat of the need for the renewed democratization of science, at least as expostulated by such contemporary critics as Stanley Aronowitz, insures that they remain committed to a rigid set of arbitrary standards to which by definition only those who have been indoctrinated and initiated into the priesthood of science and mathematics could possibly adhere.

To illustrate what Gross and Levitt consider to be the well-intentioned but misguided approach of Aronowitz, they launch a lengthy ad hominem attack against his rather conventional account in *Science As Power*, (1988), of what is generally referred to as the Copenhagen interpretation of quantum mechanics, (Cf. Popper, 1982, p. 4f. and en passim; and Brush, 1988, p. 20), and alternative interpretations of the uncertainty principle. For Gross and Levitt, "the uncertainty principle," as they insist on referring to what is also known as the indeterminacy principle," "is undoubtedly one of the cornerstones of quantum mechanics and one of the most philosophically provocative developments in the history of science. Under Aronowitz's description, however, it seems rather to refer to a kind of epistemological and spiritual

¹¹See Ch. 1, footnote 5 above. Also see Popper, on quantum theory, in *The Logic of Scientific Discovery*, 1957, pp. 215-250; in *Objective Knowledge*, 1972, p. 303; on the relationship between uncertainty and a subjective lack of knowledge, the Copenhagen interpretation, in *Quantum Theory and the Schism in Physics*, 1982, p.4f and passim; and on subjective idealism, in *Realism and the Aim of Science*, 1983, p. 126.

malaise, plaguing the minds and souls of contemporary physicists." (Op. cit., p. 51). How it is that such a philosophically provocative development could become a cornerstone of quantum mechanics and fail to plague the minds and souls of contemporary physicists remains unexplained. But as Gross and Levitt boldly assert, the *uncertainty principle* "has been verified as fully and irrefutably as is possible for an empirical proposition. In other words, when viewed as a law of physics, the uncertainty principle is a very certain item indeed. *It is an objective truth about the world.* (If that were not so, there would never have been so much fuss about it!)" (Ibid., p. 52). That Gross and Levitt include such a parenthetical statement of pragmatic realism would be commendable if they could only take the next step into realizing that it is precisely the result of such varying epistemological positions that allows for the various and sundry interpretations of this theoretical principle which they consider to be an *objective truth about the world*.

Gross and Levitt make the presupposition that some Truth exists which they, like many others, call the uncertainty principle. Here is an excellent example of the Humean distinction between conjecture and belief. For Aronowitz, or Heisenberg for that matter, the uncertainty or indeterminacy principle is just that -- a principle, a theory. For Gross and Levitt, it has become a belief, which in Hume's words "has a more forcible effect on the mind than fiction and mere conception." This effect is demonstrated for Hume, by "its

influence on the passions and the imagination: which are only moved by truth or what is taken for such." (Hume, op. cit., p. 20). But Gross and Levitt, other than a tongue-in-cheek parenthetical, fail to define that which it is that constitutes truth from their pragmatic realist position of power. Aronowitz does (1988, en passim); and Popper is explicit throughout the corpus of his work, as to what he considers to be the *demarcation* between science, metaphysics, and pseudo-science. It is no small wonder that in such a full-scale polemic against any and all criticism of an elitist approach to science, Gross and Levitt would fail to acknowledge the vast contribution Popper has made to the history, philosophy and practice of science," let alone the problems inherent in his own brand of objective realism.

I believe that Popper was correct in his emendation of the Humean quote above, "that there can be no *demonstrative* arguments to prove, that those *instances*, of which we have had no experience, resemble those, of which we have had experience" to include logical arguments. But whether the Truth resides neither in demonstration nor in logic, I do also believe that Hume was correct that "we could never foretel the effect, merely from the consideration of the cause, without

¹⁹ Aside from a reference in the index to a non-existent entry, there is one solitary reference to Popper in a citation from "New Age Technicatures," p. 535, by Andrew Ross, so jealously but flatteringly referred to as "a glamorous cult figure in the [fashionably postmodern-leftist] movement." (Gross and Levitt, p. 89). That postmodernism is considered to be a fashion is perfectly consistent with an epistemology wherein modernity itself has yet to acquire scientific status.

experience. And even after we have experience of these effects, 'tis custom alone, not reason, which determines us to make it the standard of our future judgements." (Abstract, pp. 21-22). That all our judgements are based on custom, and that which passes for reason is based on convention, is a position, that at this moment in history, would seem to defy the understanding of those scientific pundits who maintain that science alone is the pathway if not the information superhighway to the truth. Whether from demonstration or from logic, or even from a classical understanding of probabilism, the problem of induction remains unresolved:

The only connexion or relations of objects, which can lead us beyond the immediate impressions of our memory and senses, is that of cause and effect; and that because 'tis the only one, on which we can found a just inference from one object to another. The idea of cause and effect is deriv'd from experience, which informs us, that such particular objects, in all past instances, have been constantly conjoin'd with each other: And as an object similar to one of these suppos'd to be immediately present in its impression, we thence presume on the existence of one similar to its usual attendant. According to this account of things, which is, I think, in every point questionable, probability is founded on the presumption of a resemblance betwixt those objects, of which we have had experience, and those, of which we have had none; and therefore 'tis impossible this presumption can arise from probability. The same principle cannot be both the cause and effect of another; and this is, perhaps, the only proposition concerning that relation, which is either intuitively or demonstratively certain. (Hume, 1739, p. 90).

That a doctrine of probabilism now passes for scientific reasoning and what constitutes the truth should in no way be construed as having resolved the Humean problem of induction; but neither should a declaration of its invalidity.

The Frequency Definition

The classical interpretation of probability theory, as discussed at length above, defines the probability of an event, p , as the ratio of the number of possible occurrences of the event x , to the total number of possible outcomes, n . In the language of mathematics, this may be expressed by the equation: $p = (x/n)$. A popular illustration of this concept is the example of the six-sided die; the probability of the occurrence of a given value or event, on a single throw, is defined as being equal to $(1/6)$. Similarly, the probability of obtaining either a head or a tail, after a single flip of a fair coin, is equal to $(1/2)$. In deference to the problem of induction, whether or not a logical solution could be obtained as such, the frequency interpretation redefines mathematical probability theory such that the theoretical mean, μ , of an *a priori* probability distribution function is defined as the limiting case or value towards which the sum of the products of the *a posteriori* occurrences of given values or events, x_i , and their individual *a priori* probabilities, p_i , tend, over the long run. In the language of mathematics, this may be expressed by the equation: $\mu = \sum (x_i)(p_i)$. More will be written on this particular formulation of the frequency definition in the pages that immediately follow; but first, a brief view of the genesis of this formulation and its relationship to the rest of the discourse is in order.

In *A Treatise on Probability*, (1921), John Maynard Keynes describes the frequency theory as follows:

The theory in question, which I shall call the Frequency Theory of Probability, first appears as the basis of a proposed logical scheme in a brief essay by Leslie Ellis *On the Foundations of the Theory of Probabilities*, [1842], and is somewhat further developed in his *Remarks on the Fundamental Principles of the Theory of Probabilities*, [1854]. "If the probability of a given event be correctly determined," he says, "the event will on the long run of trials tend to recur with frequency proportional to their probability. This is generally proved mathematically. It seems to me to be true a priori.... I have been unable to sever the judgement that one event is more likely to happen than another from the belief that in the long run it will occur more frequently." Ellis explicitly introduces the conception that probability is essentially concerned with a group or series. (Keynes, p. 93).

Although Keynes is correct in his identification of an historical connection within Ellis' recognition of the relationship between the classical interpretation of probability theory and a frequency definition, he is quick to identify *The Logic of Chance*, (1866/1888), by John Venn, as the most "elaborate and systematic treatment" of the theory, and the only "comprehensive attempt to meet the theory's special difficulties" and "the criticisms directed against it." (Ibid.). By beginning his own criticism of the frequency theory with the work of John Venn, however, Keynes fails to concede those historical connections which may have preceded this account. Keynes is explicit in his description of James Bernoulli as "the real founder of [the classical school of] mathematical probability;" (Op. cit., pp. 41, 81); but it is precisely within his denial of the efficacy of Bernoulli's Theorem for the rapprochement of the problems of induction with the classical theory, that for Keynes, "the link between

the empirical and the *a priori* methods" is destroyed. (Ibid., p. 86). This assertion is supported by a quotation from Venn, in which he rejects the applications of Bernoulli's theorem, and goes on to describe it as "one of the last remaining relics of realism." (Ibid.). Be that as it may, it should be cognized that Keynes is concerned with a logical interpretation of probability, in which it is posited that "not all probabilities can be measured." (Ibid., p. 34 and *en passim*). On this, more will be written in the chapter to follow; but an examination of Bernoulli's theorem is prerequisite to any critical understanding of the frequency theory as it was first expostulated by Venn (1866), and later developed and refined, most notably by Reichenbach (1934/49) and von Mises (1928/57).

It is interesting to note, that it was not until the *Third Edition* (1888), of *The Logic of Chance*, (1866), that an entire chapter is dedicated to a discussion of averages; and yet, it is apparent, in retrospect, at least, that the link between the classical mathematical theory of probability as the ratio of (x/n) , and a frequency theory, is the existence of the concept of an average.²⁰ In turn, the existence of the concept of an average itself is predicated upon the conditions of sameness and repetition; or in the words of Keynes, a *Principle of Indifference*, (*op. cit.*, pp. 41-70), which, for

²⁰In present day statistical analysis, a distinction is made between the theoretical mean of a frequency distribution, as designated by the Greek letter, μ , and the average value of actually observed occurrences or events, as signified by the Latin derivative x . See Miller and Freund, pp. 53, 117.

the mathematical theory, reduces the possible outcomes to equipossible cases, and a "Law of Succession, by which the influence of any experience, however limited, could be numerically measured." (Ibid., p. 82). The recognition, however, that these conditions and their logical consequences are no more than human inventions, has not detracted from the appeal of that last remaining relic of realism. Perhaps Bernoulli's own words will help to demonstrate the power of his argument:

... what you cannot deduce *a priori*, you can at least deduce *a posteriori* -- i.e., you will be able to make a deduction from many observed outcomes of similar events. For it must be presumed that every single thing is able to happen and not to happen in as many cases as it was previously observed to have happened and not to happen in like circumstances. ... Further, it cannot escape anyone that for judging in this way about any event at all, it is not enough to use one or two trials, but rather a great number of trials is required. ... Moreover, although this fact [that the more observations ... that are taken, the less danger there will be of straying from the mark] is known naturally to all, a proof in which this is evinced from first principles is by no means obvious, and therefore it is incumbent upon me to give such a proof here, since I think I would be doing a poor job if I stood still in proving this one thing of which no one is unaware. (Bernoulli, 1713/1966, pp. 37-39).

These words, from the fourth chapter of the *Pars Quarta* of James Bernoulli's *Ars Conjectandi*, (1713), introduce his rudimentary mathematical proof of what later became known as the law of large numbers. In concert with this rudimentary mathematical proof, the heuristic of the Subtitle of *Pars Quarta*, "*Showing the use and application of [the art of conjecturing] in civil, moral, and economic affairs,*" is manifest throughout the entire corpus of his work, and recapitulated in the closing passage:

... if observations of all events were to be continued throughout all eternity, (and hence the ultimate probability would tend toward perfect certainty), everything in the world would be perceived to happen in fixed ratios and according to a constant law of alternation, so that even in the most accidental and fortuitous occurrences we would be bound to recognize, as it were, a certain necessity and, so to speak, a certain fate. I do not know whether Plato wished to aim at this in his doctrine of the universal return of things, according to which he predicted that all things will return to their original state after countless ages have past. (Ibid, p. 66).

So ends Bing Sung's translation of the *Pars Quarta*, with Bernoulli's admission of the analogy between his law of large numbers and what he calls the Platonic "doctrine of the universal return of things." That such an interpretation has continued to hold sway now for almost three hundred years, is unconvincingly disputed by Georg Henrik von Wright:

... it is an old idea that this theorem [of Bernoulli] amounted to a proof that -- although irregularities and chance-events may upset our calculations when applied only to a narrow sector of the world -- in the course of nature as a whole regularity, law and order prevail. It is therefore intelligible that the proof of the theorem was regarded as an intellectual achievement of the greatest philosophical significance. In the nineteenth century mathematicians and philosophers still spoke with the deepest awe about the wonderful philosophical implications of this theorem. A deeper insight into the logical nature of probability, however, has led in our days to a common abandonment of the philosophical aspirations originally connected with the theorem of Bernoulli and the Laws of Great Numbers in general. (von Wright, 1941/1957, p. 145).

Perhaps it is wishful thinking on the part of this follower of Keynes, and logical interpreter of the theory of probability, to ignore the continuation of the mystification of the law of large numbers, not only in its applications to the natural and social sciences, but as a justification for what has in so many ways been identified as a metaphysical

property of a mathematical function. Indeed, it was Leibnitz, who, in 1703, took issue with such an interpretation, as stated in the following letter to Bernoulli, dated 3 December, a decade before the publication of *Ars Conjectandi*:

The estimation of probabilities is extremely useful, although in several political and legal situations there is not as much need for fine calculation as there is for the accurate recapitulation of all the circumstances. I remember learning for the first time not from your brother [Daniel] but from somewhere else that these matters had been dealt with by you. When we estimate empirically, by means of experiments, the probabilities of successes, you ask whether a perfect estimation can be finally obtained in this manner. You write that you have found this to be so. There appears to me to be a difficulty in this conclusion: that happenings which depend upon an infinite number of cases cannot be determined by a finite number of experiments; indeed, nature has her own habits, born from the return of causes, but only 'in general.' And so, who will say whether a subsequent experiment will not stray somewhat from the rule of all the preceding experiments, because of the very mutability of things? New diseases continually inundate the human race, but if you had performed as many experiments as you please on the nature of deaths, you have not on that account set up the boundaries of the world so that it cannot change in the future. . . . " (Leibnitz, 1703, in Idem, 1855, translated by Sung, 1966, pp. 72-3).

It is perhaps worth remarking, that even though this correspondence preceded the publication of Bernoulli's *Ars Conjectandi*, (1713), the importance of the objections of Leibnitz, to the applications of Bernoulli's theorem, has continued to remain largely ignored to this very day. The obviation of the logical problem of induction, as it is elided by the mathematical formulation of the law of large numbers, is only valid if the laws of logic are subsumed by the laws of mathematics. That Venn was able to embrace a frequency theory of probability, without the concomitant assumption of mathematical realism

is testimony to his own understanding of *The Logic of Chance* (1866), and is evidenced in the following quotation, also referred to by Keynes, (loc. cit., p. 93), above:

The reader who is familiar with Probability is of course acquainted with the celebrated theorem of James (Jacques) Bernoulli. This theorem ... is generally expressed somewhat as follows: -- in the long run all events will tend to occur with the relative frequency proportional to their objective probabilities. With the mathematical proof of this theorem we need not trouble ourselves, as it lies outside the province of this work; but indeed if there is any value in the foregoing criticism, the basis on which the mathematics rest is faulty, owing to there being really nothing which we can with propriety call an objective probability.

If one might judge by the interpretation and uses to which this theorem is sometimes exposed, we should regard it as one of the last remaining relics of Realism, which after being banished elsewhere still manages to linger in the remote province of Probability. It would be an illustration of the inveterate tendency to objectify our conceptions, even in cases where the conceptions had no right to exist at all. (John Venn. *The Logic of Chance*. First edition 1866; second edition 1876; third edition 1888; reprinted as the fourth edition, New York: Chelsea Publishing Company, 1962, pp. 91-2).

But just what does it mean to state that "in the long run all events will tend to occur with the relative frequency proportional to their objective probabilities."? Perhaps a basic understanding of a frequency distribution will provide a sufficient, albeit not necessary, point of reference from which to illustrate the mathematical expression of the law of large numbers. To that end, the following description of the binomial distribution has been undertaken. But in order to avoid needless speculation and possible objections, warranted or otherwise, concerning my own understanding of the binomial distribution, the reader should consult any standard reference work, such as the *Dictionary/Outline of Basic Statistics*, by John E. Freund and Frank J. Williams, (1966), or perhaps an

even more specialized statistics text, such as *Probability and Statistics for Engineers*, by Irwin Miller and John E. Freund, (1965), which includes the following formulation and description of the binomial distribution, where $n!$ = the product of $n, n-1, \dots, 1$: " $b(x; n, p) = n! / x!(n-x)! p^x (1-p)^{n-x}$ for $x=0, 1, \dots, n$ defines a family of probability distributions, with each member of this family being characterized by given values of the parameters n and p . The expression $n! / x!(n-x)!$ is often referred to as a *binomial coefficient* since it is the coefficient of a^x in the binomial expansion $(a + b)^n$; it is often represented by the symbol $\binom{n}{x}$ ". (Miller and Freund, p. 40). Having arbitrarily chosen $p=1/2$, and an $n=6$, and by substituting values of x from 0 to 6, a graphical representation of the binomial distribution may be depicted as in the histogram below:

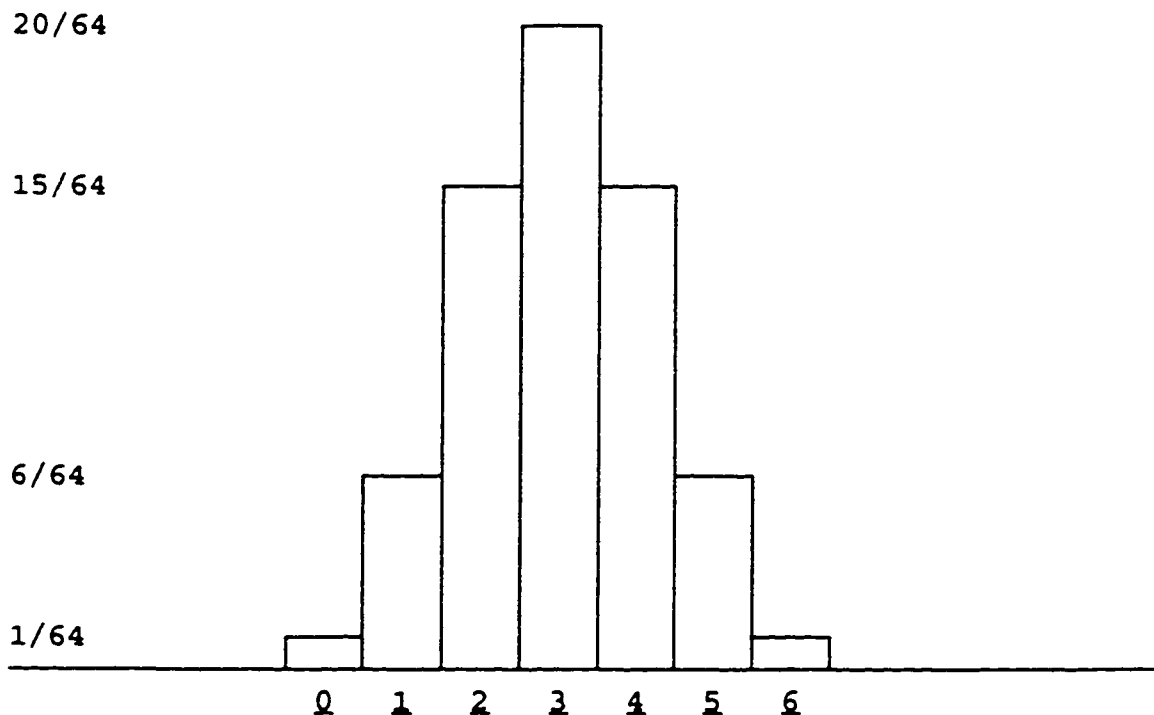


Fig. 1 Histogram of the Binomial Distribution,
With the Parameters $n=6$ and $p=1/2$

Utilizing the example of the tossing of a fair coin, there is one (1) chance in sixty-four (64) of not throwing a head in six (6) tosses of the coin. There are six (6) chances in sixty-four (64) of throwing one head. There are fifteen (15) chances in sixty-four (64) of throwing two heads. There are twenty (20) chances in sixty-four (64) of throwing three heads, etc. Alternatively, the binomial coefficients themselves may be calculated through the construction of what has come to be known as *Pascal's Triangle*, wherein "Each row begins and ends with a 1, each other number is the sum of the two nearest numbers in the row immediately above, and the binomial coefficient is given by the $(x+1)$ st number in the $(n+1)$ st row." (Adapted from Freund and Williams, p.10). For the case at hand, by reading across the sixth row of the triangle, the binomial coefficients 1, 6, 15, 20, 15, 6, 1 represent the various possible combinations associated with a binomial distribution, $n = 6$.

						1													
						1		1											
						1		2		1									
						1		3		3		1							
						1		4		6		4		1					
						1		5		10		10		5		1			
						1		6		15		20		15		6		1	
						1		7		21		35		35		21		7	
						1		8		28		56		70		56		28	
						1		9		36		84		126		126		84	
						1		10		45		120		210		256		210	
						1		11		55		165		330		466		466	
						1		12		66		220		495		796		932	
						1		12		66		220		495		796		932	
						1		12		66		220		495		796		932	

Fig. 2

Pascal's Triangle

Although the figure above has a long and venerable history, it has come to be known as Pascal's triangle, I think, more as a result of good publicity, than the recognition of some unique creative genius. Florence N. David, in *Games, Gods, and Gambling*, (1962), does in fact refer to the work of Apollonius, (1527), Stifel, (1545), Tartaglia (1556), Buckley (1567), and even Galileo (1613-23), as evidence that "the arithmetical triangle commonly attributed to Pascal is of much earlier provenance." (David, p. 81). But it is L.E. Maistrov, (1967/1974), who provides a much fuller account, and dates the use of the arithmetical triangle, as well as other methods for computing combinations and permutations, by Hindu mathematicians, to as long ago as 200 B.C. Maistrov goes on to cite the work of Bhāskara II (ca., 1150), Nārāyana and Al-Kashi (fourteenth century A.D.), Chu Shih-Chien (1303), Levi ben Gerson (1321), Stifel (1544), Herigone (1634), Tacquet (1656), as well as Leibnitz (1666), among others. (Maistrov, p. 34-6). But Maistrov is clear in his assertion that probability theory should be considered to be a modern science, since it was not until the middle of the seventeenth century that any rigorous mathematical solutions to the problems of probability were able to be formulated. (Ibid., p. 38). To this assertion, should be appended the almost tautologous statement that it was not until the development of the calculus of probability that any rigorous solutions to the problems of probability could be mathematically, ergo, scientifically formulated.

De Moivre, eg., was explicit in his recognition of an intellectual debt to Huygens, (1651), and Monmort (1708), as well as the Bernoullis, Jean, Jacob, and Nicholas, for their numerous formulations and solutions to problems of probability. (Demoivre, 1756, pp. i, ii, ix, 243, and Walker, 1934, in idem, 1967, pp. 356-58). But it was through his application of the newly emerging mathematical calculus of Sir Isaac Newton (1687) and G.W.F. Leibnitz, (c. 1700), that Demoivre was able to simplify and generalize the work of his predecessors, in his own *Doctrine of Chances* (Op. cit., p. viii, ix). From as early as 1695, in the first of at least fifteen memoirs to be published in the *Philosophical Transactions* (of the Royal Society), (Ibid., p. 357), including *De Mensuri Sortis* (1711), through the publication of his major works, *The Doctrine of Chances* (in English, 1717, 1738, and 1756), and *Miscellanea Analytica*, (1730), De Moivre recognized the far-reaching importance of the idea of fluxions and the differential calculus. His *Approximatio ad Summam Terminorum Binomii $(a+b)^n$ in Seriem Expansi* (1733), predated, respectively, the Laplacian and Gaussian formulations by half and three-quarters of a century, and is considered, by modern historians, to be the earliest expression of the normal approximation of the binomial distribution. (Pearson, 1924, 1925; Archibald, 1926; D.E. Smith, 1929; and Helen Walker, 1929, 1934/67).

Of course, the calculus of probability did not develop unaffected by the application of the calculus to other areas as well. It is no accident of history that Isaac M. Todhunter,

the author of *A History of the Mathematical Theory of Probability* (1865), had already seen published his *A History of the Progress of the Calculus of Variations during the Nineteenth Century*, in 1861. With the exception of Euler's *Methodus inveniendi Lineas curvas proprietate maximi minimive gaudentes*, (1744), *A Treatise on Isoperimetrical Problems and the Calculus of Variations*, (1810), later to be republished as *A History of the Calculus of Variations in the Eighteenth Century*, (1810/ 1965), by Robert Woodhouse, was the only other full treatment, and indeed, the only one in English, of the maxima and minima of curves, otherwise known as the calculus of variations. (Woodhouse, p. i). This is not the place, however, to discuss the relative worth or impact of the history of the calculus of variations on the history of the calculus of probability; although its relationship, mythical as well as historical, with the development of geodesy, ie., the measuring of the earth, would make for a fascinating sociological study, in its own right. Similar claims could also be made for the development of the calculus of errors and astronomy, or any other mathematical technique and the object of knowledge to which it is applied, as being demonstrative of the dialectical relationship between science and technology. Arguments for and against such an approach to the sociology of scientific knowledge, typical of the Edinburgh School, were introduced earlier in the discussion. (See Chapter I, above).

The importance of the development of the calculus of variations, for the discussion at hand, is as an illustration of

the more general doctrine that the development of the mathematical calculus has been accompanied by the obfuscation of those geometrical and metaphysical principles upon which the calculus of mathematics is based. As long ago as 1810, Robert Woodhouse drew attention to the fact that the Bernoullis, Euler, and Lagrange, all introduced diagrams and geometrical figures, for purposes of illustration, in their earlier works, but with the improvement of the calculus, the use of the diagrams and their properties eventually was discontinued. That an inverse relationship between the development of the calculus and the use of geometrical figures and diagrams was common to every other method of calculation, (Ibid., p. vii, viii and en passim), has been documented not only by Woodhouse, but by contemporary historians of mathematics and science, as well. Carl B. Boyer, in *The History of the Calculus and Its Conceptual Development*, (1949), states it thusly:

[Leibnitz] felt that the calculus, as a *modus operandi*, brought its demonstrations with it. He did not wish to make of the infinitely small a mystery, as had Pascal; nor did he turn to geometrical intuition for clarification. In appealing only to intelligence, he stressed rather the algorithmic nature of the method, as he himself spoke of it. In this sense, he may justly be considered one of the founders of formalism, as opposed to intuitionism, in mathematics. He had confidence that if he formulated clearly the appropriate rules of operation and that if these were properly applied, something reasonable and correct would result, however doubtful might be the meaning of the symbols involved. (Boyer, p. 209).

Within that very spirit for the arithmetization of mathematical analysis, J.W. Dauben, in the *Dictionary of the History of Science*, (1981), warns us, on the one hand, of the

inadequacy of intuition in mathematics, while he extols, on the other, the virtues of *rigor in formal analysis*:

In 1812 C.F. Gauss (1777-1855) studied the convergence of trigonometric series. But the greatest contribution to the advance of rigour in mathematical demonstrations was A. L. Cauchy's (1789-1857) *Cours d'Analyse* (1821). ... In 1850 [Riemann (1826-66)] generalized Cauchy's concept of the integral to cover even wider classes of discontinuous functions, thereby considering functions with infinitely many oscillations and discontinuities. Riemann also introduced the first example of a continuous function that was nowhere differentiable. ... Riemann's discovery of the existence of continuous functions without derivatives demonstrated further the inadequacy of intuition in mathematics and the need for rigour in analysis. K. Weierstrass (1815-97) also developed such examples in his work, introducing the concept of limit, not by appeals to geometry or to motion, but through the delta-epsilon approach that reduced limits to formulations involving only real numbers. Weierstrass' goal, to arithmetize analysis, eventually established the autonomy of the subject as a separate branch of mathematics. (Dauben, p. 15).

The general doctrine that the development of the mathematical calculus has been accompanied by the obfuscation of those geometrical and metaphysical principles upon which the calculus of mathematics was based, is apparently superseded by the development of an abstract idealized arithmetic calculus not limited to or by any concrete human conceptualization. But the fact is that the metaphysical concept of a limit, μ , is what has redefined the classical theory of probability: "According to the frequency interpretation... the probability of a sequence is defined by the limit of the relative frequency." (Reichenbach, p. 338). The necessity of the connection between the law of large numbers, and the arithmetic concept of an average, x , is lost with the separation of the mathematical from the logical; but the science of

probability is an ignorant science, and with no alternative conceptualization, the realism of the law of averages will continue to adhere to any axiomatic method that ignores the problem of induction. Perhaps an incommensurable epistemology necessarily exists between those who cling to the equation of mathematics and logic, and those who would subsume one faculty by the other. That such an equation would be able to survive beyond the close of the nineteenth century is the legacy that will be explored in the following chapter. That the consequences of such a deconceptualization would constitute the historical foundation from which an indeterministic science would emanate, remains the legacy of the contest between logic and mathematics, to this day.

CHAPTER FOUR

LOGICAL RAMIFICATIONS

A Contest of the Faculties

Sunt qui mathematicum vigorem extra ipsas scientias, quas vulgo mathematicas appellamus, locum habere non putant. Sed illi ignorant, idem esse mathematice scribere quod in forma, ut logici vocant. ratiocinari. Leibnitz, *De vera methodo Philosophiæ et Theologiæ* (about 1690).

Cave ne tibi imponant mathematici logici, qui splendor suas figuras et algebraicos meandros universale inventionis veri medium crepant. Rüdiger, *De sensu veri et falsi*, Lib. ii. Cap. iv. & xi. (1722).

In the *Introduction* to his own *Symbolic Logic* (1894), John Venn addresses the question as to whether mathematics should be considered to be a branch of logic or logic should be considered to be a branch of mathematics. Although Venn concedes the point that symbolic logic and mathematics possess a few common laws of combination, he regards the principles of the logical calculus as being entirely independent of those of the mathematical calculus. Symbolic logic and mathematics are, in his view, branches of one language of symbols. (Venn, 1894, pp. xiii, xvi). The citations above, from Leibnitz and Rüdiger, which also serve as mottoes for Venn, demonstrate to him, that he is not alone in this interpretation. Although other authors, including George Boole, might have adopted the title of mathematical, rather than symbolic logic, if by mathematics is meant a language of pure symbols, the distinction is purely definitional and the question is moot. To support this contention, Venn quotes R. Harley (1866), and directly from Boole, "It is simply a fact that the ultimate

laws of Logic, - those alone upon which it is possible to construct a science of Logic, -- are mathematical in their form and expression, though not belonging to the mathematics of quantity." (*Lecture delivered at Cork, 1851, in Venn, 1894, p. xviii*). That numerous counterexamples may be readily found throughout the corpus of Boole's work, (See ex., 1857, pp. 6, 11, 37, and *en passim*), would seem, however, to refute Venn's interpretation, at least with respect to Boole's position concerning the relationship between logic and mathematics. Nevertheless, very few logicians, mathematicians, or historians of science, for that matter, are as clear and precise as John Venn, in the statement of their positions concerning the confusions that may arise as a consequence of the conflation of that which is mathematical with that which is logical. I suspect, moreover, notwithstanding the problems of clarity and precision, for contradictory reasons, very few logicians or mathematicians could ever have whole-heartedly accepted such an interpretation as that of Venn.

In *The Substitution of Similars, The True Principle of Reasoning, Derived from a Modification of Aristotle's Dictum*, (1869), W. Stanley Jevons, for example, discusses why he considers the quantification of the predicate to be of such monumental importance in the history of logic. Contrasting the classically styled Aristotelian proposition, *All metals are elements*, with the newly formulated, *All metals are some elements*, Jevons reasons that: "[a]lthough the change seems trifling, the consequences are momentous. The proposition no

longer asserts the inclusion of one class in the other, but the identity of group with group. The proposition becomes an equation of subject and predicate, and the significance of this change will be fully apparent only to those who see that logical science thus acquires a point of contact with mathematical science." (Jevons, 1869/1971, p. 85). Jevons continues to argue, "Dr. Boole's remarkable investigations prove that, when once we view the proposition as an equation, all the deductions of the ancient doctrine of logic, and many more, may be arrived at by the process of algebra. Logic is found to resemble a calculus in which there are only two numbers, 0 and 1, [the two numbers which remain unchanged when multiplied by themselves], and the analogy of the calculus of quality proves to be perfect." (Ibid.). If, in fact, Jevons believes that it was through Boole's investigations concerning the quantification of the predicate that it was proved that "all the deductions of the ancient doctrine of logic, and many more, may be arrived at by the process of algebra," such an interpretation would seem to contradict another statement made by Jevons in *The Principles of Science*, (1874), concerning Boole's position on the relationship between logic and mathematics: "Logic resembles algebra as the mould resembles that which is cast in it. Boole mistook the cast for the mould. Considering that logic imposes its own laws upon every branch of mathematical science, it is no wonder that we constantly meet with the traces of logical laws in mathematical processes." (Jevons, 1874/1958).

Jevons is careful to assign priority credit to George Bentham, (1827), for the introduction of the quantification of the predicate, and to Archbishop William Thomson, (1842), William Hamilton, (1860), and Augustus De Morgan (1847, 1872), for the rediscovery and development of the "new idea." (Jevons, (1869), p. 83; (1895), p. 187). Quoting from De Morgan's *Budget of Paradoxes*, (1872), Jevons goes on to credit Thomas Hobbes for "a remote glimpse of some of the points which are placed in the light of day by Mr. Boole." (De Morgan in Jevons, 1869, pp. 83-4). But the discussions are brief, and the veracity of the historical connections remains unsubstantiated. The relationship between logic and mathematics remains tentative and subject to interpretation. Ambiguities abound, and these ambiguities are then further compounded by apparent gaps in the historical record. These gaps have lent credence to origination myths, and, a bit paradoxically as well, to myths of historical connectedness, such as might be attributed to Venn, as well as Jevons, for their unarguably creative interpretations of the position of Boole. Before proceeding with the discussion of the subtle distinctions that exist within the various schools of thought that collectively constitute what is considered to be the logical interpretation of probability theory, it is important to examine these myths, and the various manners in which they have come to manifest themselves in those subtle distinctions referred to above.

Notwithstanding his own posthumous *Logic* of 1800, Immanuel Kant, as we are perhaps a bit ingenuously reminded by

Ernest Nagel and James R. Newman, (1958), writes in 1787, that the history of formal logic, since the time of Aristotle, "has not been able to advance a single step, and is to all appearances a closed and completed body of doctrine." (Kant, (1787), in Nagel and Newman, p. 40). Over a century and a half later, Bertrand Russell, co-author with Alfred North Whitehead of the monumental *Principia Mathematica* (1913/1927), perhaps even a bit more ingenuously, writes that "Plato and Aristotle were the most influential of all philosophers, ancient, medieval, or modern." (Russell, 1945, p. 104). The ingenuousness to which I refer, lies not so much in the overstatement of the importance of Plato and Aristotle in the history of the development of western philosophy and logic, as in the understatement of the importance of the scores if not hundreds of historical figures who contributed to the development of each. To qualify their position, Nagel and Newman submit that "A renaissance of logical studies in modern times began with the publication in 1847 of George Boole's *The Mathematical Analysis of Logic*." (Nagel and Newman, loc. cit.) According to Nagel and Newman, "The primary concern of Boole and his immediate successors was to develop an algebra of logic which would provide a precise notation for handling more general and varied types of deduction than were covered by traditional logical principles." (Ibid.).

Among those immediate successors to whom Nagel and Newman refer, must belong Gottlob Frege, who in 1879 witnessed the publication of his own *Begriffsschrift*, which according to

Jean van Heijenoort, (1967), the editor of a collection of papers, *From Frege to Gödel: A Source Book in Mathematical Logic, 1879-1931*, "presented to the world, in full-fledged form, the propositional calculus and quantification theory. Although Frege's work was slow in winning recognition, [at least according to Heijenoort], the next decades saw striking advances in logic. Two new fields, set theory and foundations of mathematics emerged on the borders of logic, mathematics, and philosophy." (Heijenoort, p. vi). Despite the authorship of his *The Logic of Chance* (1866), from such ahistorical positions as these, John Venn must also be included as an immediate successor to Boole; and although the theory of classes, the development of which so often is associated with the name of Venn, (1894), was viewed as an improvement upon Boole's work, according to Emil Leon Post, it also "had this incurable defect as a logic of propositions [in] that it used informally in its proofs the very propositions whose formal statements it tried to prove." (Post, (1921) in Heijenoort, p. 265). As to whether or not this incurable defect of the theory of classes was spirited away by the publication of the general theory of logic of Whitehead and Russell (1913/1927), is as unimportant to the present discussion as is the question as to whether or not the publication in 1931 of Gödel's proof, "*On Formally Undecidable Propositions of Principia Mathematica and Related Systems*," served to undermine the edifice of the *Principia Mathematica*, in that the limitations of the axiomatic method were exposed, or merely that "the resources of the human

intellect have not been, and cannot be, fully formalized," as Nagel and Newman maintain. (Op. cit., p. 100). For the purposes of the present discussion, the importance of symbolic, formal, or mathematical logic, as it has been variously called, is its relationship to the development of probability theory and the calculus of probabilities; although a few points of history are still in want of editorial clarification.

John Venn stands in contradistinction to Nagel and Newman, who consider the publication in 1847 of George Boole's *The Mathematical Analysis of Logic* as the beginning of a renaissance of logical studies in modern times, as well as Jean van Heijenoort, who agrees that the middle of the nineteenth century witnessed a renaissance in the science of logic, "which, many [read Kant] felt, had reached its completion and lacked any future." (Heijenoort, p. vi). According to Heijenoort, that rebirth, which was heralded by the work of Leibnitz, and perhaps initiated by Boole, De Morgan and Jevons, entered a new phase as a great epoch in the history of logic, with the publication in 1879, of the *Begriffsschrift* of Frege. (Ibid). For John Venn, (1894), on the other hand, it was perhaps in good measure due to the work and influence of Kant, (which has included the explicit denial of any significant advances in formal logic, since the time of Aristotle), that there existed an apparent gap of eighty or ninety years, in the history of symbolic logic, from the time of Kant, until the time of Boole and his contemporaries. (Venn: xxx, xxxv).

Perhaps to a certain extent, in order to rectify this situation, in the *Introduction* to his *Symbolic Logic* (1894), Venn presents the reader with a concise history of that rebirth of formal, aristotelian, or scholastic logic in its modern form. By familiarizing the reader with the work, not only of Leibnitz (1690), and Rüdiger (1722), but of Godfrey Ploucquet (1763), J. G. Holland, (1764), and J. H. Lambert (1787), all of whom were writing at the end of the eighteenth century, as well as the more contemporary work of William Hamilton (1841), Augustus De Morgan (1847), H. L. Mansel (1856), W. Spalding (1857), Leslie Ellis (1858), R. Harley (1866), and W. Stanley Jevons (1874), among others,¹ Venn

¹ Venn is critical of Jevons, for having "roundly asserted (*Principles of Science*, Ed. I. 1874, p. 39 [2nd edition, 1877, p. 33]) that 'Boole is the only logician in modern times who has drawn attention to the logical law' that $A=AA$." (Venn, 1894, pp. xxix). A similar criticism is then leveled at "Frege [who] (*Begriffsschrift*, 1877) has no reference to any symbolic predecessor except a vague mention of Leibnitz." (Ibid. p. xxx). In opposition and reaction to such blatant disregard, "a disregard which had already astonished [James] Bernoulli, the editor of some of Lambert's posthumous works and of his letters," (Loc. cit., p. xxxv), and to whom De Morgan credits for having "import[ed] the algebraic mode of thought to logic," (De Morgan, 1858, reprinted in 1966, p. 141n), the following contributions also merit consideration: George Bentham, *Outlines of a New System of Logic* (1827); M.W. Drobitsch, *Neue Darstellung der Logik ... Nebst einen logisch-mathematischen Anhang*, Leipzig, (1836); T. Solly, *Syllabus of Logic* (1839); J. S. Mill, *A System of Logic, Ratiocinative and Inductive*, New York: Harper & Brothers, (1843/1858); Wm. Hamilton, *A Prospectus of the intended New Analytic and Requirements for a prize Essay* (1846) and *Lectures on Logic* (1860); Rev. Wm. Thomson, *Outline of the Necessary Laws of Thought*, 2nd. edition (1849), first published anonymously, London and Oxford: Pickering and Graham (1842); T. Spencer Baynes, *Essay on the New Analytic of Logical Forms*, the prize Essay of 1846, (1850, July); Dean Aldrich, *Compendium of Logic*, edited by H. L. Mansel, (1856); and W. Spalding, *Introduction to Logical Science* (1857). Also, "The second

compels the reader to be less inclined to accept the idea that a single publication could mark the beginning, as compared perhaps to a turning point or a deflection, in the history of symbolic logic.

The legacy of the scholastic skepticism of John Venn, however, should not be confined to the area of symbolic logic; rather, it should be extended more generally throughout the entire scientific enterprise, if for that matter, the history of such an enterprise is ever to be understood as more than just a simple narrative of mediocrity, punctuated by tales of occasional flashes of brilliance or unexpected moments of accidental greatness. Such was the sentiment of Benjamin Farrington, (1949), as quoted by Edwin Schrödinger in his article, "Are There Quantum Leaps?" (1952) below:

'Perhaps the most decisive defeat of the scientific spirit in antiquity had been the loss of the sense of history. History is the most fundamental science, for there is no human knowledge which cannot lose its scientific character when men forget the conditions under which it originated, the questions which it answered, and the functions it was created to serve. A great part of the mysticism and superstition of educated men consists of knowledge which has broken loose from its historical moorings.'² The disregard for historical connectedness, nay the pride of embarking on new ways of thought, of production and of action, the keen endeavour of shaking off, as it were, the indebtedness to our predecessors, are no doubt a general trend of our time. (Ibid., p. 110).

edition of Mr. Blakey's 'Essay on Logic' recently published, contains a catalogue of upwards of a thousand works on logic, briefly titled." (De Morgan, 1847, p. 333n).

² Benjamin Farrington, *Greek Science*, vol. 2, London: Pelican Books, (1949), p. 173, in Edwin Schrödinger, "Are There Quantum Jumps?" *The British Journal for the Philosophy of Science* Volume III, No. 10, (August, 1952), p. 110.

But perhaps the loss of the sense of history to which Benjamin Farrington refers, as well as the disregard for historical precedence, which astonished J. Bernoulli (see p. 127 n1 above), and was addressed by Venn in the *Introduction* to his *Symbolic Logic*, indicate that the general trend of our time to which Schrödinger refers, is an example of the *disregard for historical connectedness*, against which he so eloquently rails. Be that as it may, the task of the historian of science does not end with the identification of historical connectedness. In his essay on 'Gaston Bachelard's Historical Epistemology,' Dominique Lecourt recalls the opening words of Bachelard's *A Study in the Evolution of a Problem in Physics: the Propagation of Heat in Solids*:

'It is easy to believe that scientific problems follow each other historically in an ascending order of complexity, without always making the effort to move in thought so as to confront the problem as it appeared in the primitive observation.' [For Lecourt, informed as he is by his reading of Bachelard], The history of the Sciences can make its judgements only when instructed by epistemology. But where does epistemology itself obtain its instruction? From living science, as we have seen, in the thick of its researches. ... But historical epistemology has already taught us that science progresses by jerks, sudden mutations, re-organizations of its principles: in short by dialectical slices. It is for this reason that the History of Sciences must itself be dialectical: it will fasten especially on those 'critical' moments in which the bases of science are being re-organized. Lecourt, *Marxism and Epistemology*, London and Atlantic Highlands: NLB and Humanities Press, (1975), pp. 84, 85.

But at those moments, as I understand Lecourt, the historian of science must identify the *rupture* from the pre-existing ideology, as well as the *recasting (refonte)* or re-organization, "after the entry into scientificity when a de-

terminate science reorganizes its bases." (Ibid.). Occasionally, however, no such identification of a rupture or recasting is required, especially when the conscious stimulus for such a reorganization is identified by the authors themselves.

Such has been the effect of those correspondences between authors which have oftentimes shaped, formed, coloured and shaded our perceptions of the history and philosophy of science. After a long and acrimonious public debate between William Hamilton and Augustus De Morgan concerning Hamilton's priority claim for the quantification of the middle term of the syllogism, be it subject or predicate, (De Morgan, 1847, p. 299), the assertion of an historical connection between the work of De Morgan and that of Boole is unexceptionable.¹ Moreover, the relationship between De Morgan's explanation of demonstrative and testimonial or evidential truth and Boole's primary and secondary propositions become manifest in the work of the logical interpreters of probability theory that follow.

The opening passage of the Chapter on Probability in De Morgan's *Formal Logic*, reads as follows:

The most difficult inquiry which one can propose to himself is to find out what anything is: in all probability we do not know what we are talking about when we ask such

¹ See ex., the letter from Boole to De Morgan, November 21, 1861, in which he good-naturedly refuses to perpetuate the priority debate, but expresses a sympathy with the interpretation of De Morgan. An excerpt from the letter is reprinted in the Introduction to *On the Syllogism and Other Logical Writings* by Augustus De Morgan. Edited, with an Introduction by Peter Heath, New Haven: Yale University Press, 1966, p. xxii. An account of the correspondence between De Morgan and Hamilton themselves is reprinted in the Appendix to De Morgan's *Formal Logic*, 1847/1926.

a question. The philosophers of the middle ages were much concerned with the *is*, or *essence*, of things: they argued to their own minds, with great justice, that if they could only find out what a thing is, they should find out all about it: they tried, and failed. Their successors, taking warning by their example, have inverted the proposition; and have satisfied themselves that the only way of finding what a thing is, lies in finding what we can about it; that modes of relation and connexion are all we can know of the essence of any thing; in short, that the proverb "tell me who you are with, and I will tell you what you are," applies as much to the nature of things as to the characters of men. (De Morgan, 1847, p. 170).

De Morgan goes on to discuss what he considers to be those *necessary truths*,⁴ which we arrive at through reflection, such as $2 + 2 = 4$, and that absolute certainty, "of which we are well assured nothing could persuade us to the contrary, [such as] the existence of our own minds, thoughts, and perceptions, the last two when actually present." (Ibid). He contrasts the knowledge of our own existence and those *necessary truths* of which we are certain, with "lower grades

⁴ Earlier in the treatise, De Morgan recalls the words of William Whewell: "... how can we conceive truth, otherwise than as something known? How can we conceive things as existing, without conceiving them as objects of perception? Ideas and Things are constantly opposed, yet necessarily coexistent. How they are thus opposite and yet identical, is the ultimate problem of all philosophy. The successive phases of philosophy have consisted in separating and again uniting these two opposite elements; in dwelling sometimes upon the one and sometimes upon the other, as the principal or original or only element; and then in discovering that such an account of the state of the case was insufficient. Knowledge requires ideas. Reality requires things. Ideas and things coexist. Truth is, and is known. But the complete explanation of these things appears beyond our reach." De Morgan, (1847), p. 33n. Of course, long since then, Thomas S. Kuhn, (1962), perhaps in agreement with R. Leslie Ellis, (1854), p. 171 below, has continued to successfully argue how, "Philosophers of science have repeatedly demonstrated that more than one theoretical construction can always be placed upon a given collection of data." (Ibid., p. 12).

of knowledge, which we usually call *degrees of belief*, but they are really *degrees of knowledge*." (Loc. cit.). To the higher grade of knowledge, De Morgan ascribes the category of certainty; to the latter, probability. That this approach forms the basis for what later becomes known as the subjective theory of probability, will be examined forthwith. For the present, it serves to move the discussion to the question as to how it is that those supposedly lower grades of knowledge are able to approximate the higher:

There are two sources of conviction, *argument* and *testimony*, reason why the thing should be, statement that the thing is. When the argument is necessarily good, we call it *demonstration*: when the statement can be absolutely relied on, we call it *authority*. Both words are used in lower than their absolute senses; thus, very cogent arguments are often called demonstration, and very good evidence, authority. . . .

It is generally said that we throw away authority, and judge by argument alone; that our reason is to be convinced, and not biassed by the opinions of others; that no conclusions are worth anything, except those which a man forms for himself. (Ibid., p. 191, 194).

Although De Morgan goes on to caution the reader to allow no more weight to either argument or authority than each is due, earlier he asserts "When argument is demonstration, it establishes its conclusion against all testimony. The idea of an infallible witness bearing evidence against a demonstrated conclusion, is a contradiction." (Ibid., p. 192). In his own way, De Morgan reestablishes the primacy of the intellect to that of the senses: a necessarily good argument (demonstration) is timeless and of the higher order of knowledge; an absolutely reliable statement or testimony (authority), on the other hand, is accidental, contingent, temporal, evidential

and of the lower order of knowledge. This distinction is maintained by Boole in the development of what he calls Primary and Secondary Propositions of Logic:

The relations of *things* we express by primary propositions. But we are able to make Propositions themselves also the subject of thought, and to express our judgements concerning them. The expression of any such judgement constitutes a secondary proposition. There exists no proposition whatever of which a competent degree of knowledge would not enable us to make one or the other of these two assertions, viz., either the proposition is true, or that it is false; and each of these assertions is a secondary proposition. (Boole, 1854, p. 160).

Although Venn is able to recognize the correlation between these two approaches, as well as their relationship to the development of the calculus of probabilities, (Venn, 1894, p. 28), he expresses confusion as to what he considers to be the "rather far-fetched and unnecessary doctrine that propositions of this secondary kind were peculiarly interpretable in terms of time ..." (Idem, p. 435). Perhaps a closer reading of the passage in question will serve to alleviate the confusion:

There are indeed propositions, the truth of which is not ... limited to particular periods or conjunctures; propositions which are true throughout all time, and have received the appellation of "eternal truth." The distinction must be familiar to every reader of Plato and Aristotle, by the latter of whom, especially, it is employed to denote the contrast between the abstract verities of science, such as the propositions of geometry which are always true, and those contingent or phenomenal relations of things which are sometimes true and sometimes false. But the forms of language in which both kinds of propositions are expressed manifest a common dependence upon the idea of time; in the one case as limited to some finite duration, in the other as stretched out to eternity. ... (Op. cit., p. 164).

Perhaps Venn's confusion is predicated upon his insistence that Boole's conceptualization of the limits of symbol-

ism is the same as his own. It is my contention that it is within the notion such as that which is expressed by the subtle distinction between De Morgan's assignation of argument and necessary truths as being demonstrative, and Boole's assignation of eternal truths as being of the order of secondary propositions, that the logical interpretation of probability theory begins a series of irreversible bifurcations. As Boole continues to argue:

... I shall avail myself of the notion of time in order to determine the laws of expression of secondary propositions, as well as the laws of combination of the symbols by which they are expressed. But when those laws or forms are once determined, this notion of time (essential, as I believe it to be, to the above end) may practically be dispensed with. We may then pass from forms of common language to the closely analogous forms of the symbolical instrument of thought here developed, and use its process, and interpret its results, without any conscious recognition of the idea of time whatever. (Ibid.).

By availing himself of the notion of time, Boole is able to impose the certainty of demonstrative argument over the uncertainty of secondary propositions, as soon as the conditional nature of those secondary propositions has been ignored or forgotten. Despite Venn's protestations to the contrary, in his ill-fated effort to force agreement between his own schema and that of Boole, the primacy of mathematics in Boole's mathematical logic is ultimately undeniable:

The laws of thought, in all its processes of conception and of reasoning, in all those operations of which language is the expression or the instrument, are of the same kind as are the laws of the acknowledged processes of Mathematics. (Ibid., p. 422).

De Morgan was explicit in his assertion that an objective theory of probability was impossible, because of our incapaci-

ty for perfect knowledge, due to the constantly changing conditions of that knowledge. (De Morgan, 1847, p. 172). Boole, on the other hand, redefined the mathematical to include that which is logical. As such, the mathematical calculus of probability, could now masquerade as an *eternal truth*. In one way or another, the various logical interpretations of probability theory adhere, in some manner, or at least claim historical connectedness, to either one or the other of these two subtly distinct interpretations of the relationship between logic and mathematics.

The Logical Interpretation of Probability

Perhaps as much in keeping with the distinction between primary propositions concerned with things and secondary propositions concerned with relations between those things, as represented in the discussion above concerning Boole and De Morgan; ie., the distinction between that which is timeless and that which is not; or even somehow in keeping with the distinction between that which is mathematical and that which is logical, as oversimplified to signify the distinction between that which is objective and that which is subjective; but without either attempting to abandon or to resolve the classical dichotomy as epitomized by Cardanus between that which is divine and that which is human, Rudolph Carnap contributes to the reification of the conceptualization of a division between the sacred and the profane, through the expostulation of two kinds of probability:

It seems to me that the number of explicanda in all the various theories of probability is neither one nor about a dozen, but in all essential respects -- leaving aside slight variations -- very few and chiefly two. In the following discussions we shall use subscripts in order to distinguish these two principle meanings of the term 'probability' from which most of the various theories of probability start; we are, of course, distinguishing between two explicanda and not between the various explicata offered by these theories, whose number is much greater. The two concepts are (i) probability, = degree of confirmation; (ii) probability, = relative frequency in the long run. (Carnap, 1950, pp. 24-25).

Carnap certainly is in good company. While the classical branch of the tripartite classical, frequency, and logical, classificatory schema of probability theory, as it was first introduced by Ernest Nagle, (1939, see p. 80 above), had long ago withered away, the frequency approach became associated with the objective realism associated with mathematics, and the logical approach continued to examine its own subjectivity. (Good, 1965, p. 7; Kyburg, 1990, p. 79). The company is good, but the dichotomization remains unsatisfactory. The quest for certainty was preempted by a search for a method; but the newer methods were devoid of any moral heuristic, the existence of which is characteristic of the classical interpretation, that had served so many, so well, for so long. Nevertheless, in the tradition of W.B. Johnson, (1921), John Maynard Keynes, (1921), A.D. Ritchie, (1926), C.D. Broad, (1926), Harold Jeffreys, (1931, 1961), A.J. Ayer, (1936), Ernest Nagle, (1939), B.O. Koopman, (1940), Georg Henrik von Wright, (1941), R.B. Braithwaite, (1946), William Kneale, (1949), Rudolf Carnap, (1950), Henry E. Kyburg, Jr., (1961), John Patrick Day, (1961), Wesley C. Salmon, (1966), and

Richard Jeffrey, (1980), the importance of rational belief as a criterion for some degree of confirmation has continued to fashion the research agenda of what is properly concerned with inductive logic. Still, the playing field remains uneven, as the rules of the game continue to be (re)formulated and the boundary lines remain blurred. The logical interpretation of probability theory as explicated by Keynes remains the standard by which the work of the rest must be measured.

In his comments at the first session of the Ninth Symposium of the Colston Research Society held in the University of Bristol from April 1st through April 4th, 1957, and later published in *Observation and Interpretation*, edited by S. Körner, (1957), as "The conception of probability as a logical relation," A. J. Ayer begins by identifying the Keynesian formula, " $a/h = p$, where a is the proposition which is said to be probable, h is the evidence on which it is probable, and p is the degree of probability that h confers on a , a quantity which may or may not be numerically measurable." (Ayer, 1957, p. 12). But for Ayer, in this definition of a logical interpretation of probability theory, "the existence of a probability relation between statements is made to depend, not on any contingent matter of fact, but solely on the meaning of the statements." (Ibid., p. 13). It is in this sense, that any such logical interpretation of probability theory is more accurately termed a logical relation theory, and is subject, at least according to Ayer, to a very simple objection:

... to say that we have sufficient warrant for accepting a given statement must mean, for Keynes, that it follows from, or is made probable by, another statement, or set of statements, which we have sufficient warrant for accepting: and then one appears to be threatened with an infinite regress. Keynes meets this difficulty, however, by assuming that there are certain statements which we can know directly to be true: and it is on statements of this sort that all rational judgements of probability must depend. This assumption may be questioned; but even if it be admitted, our original objection still holds. Once we have established some trustworthy data by these means, there can be no reason, on Keynes's system, why we should trouble to carry our investigations any further. The addition of more evidence may, indeed, yield a higher or lower probability for the statement in which we are interested. But unless we have made some logical mistake, this probability cannot be said to be more, or less, correct than the one that was yielded by the evidence with which we started. Neither can any sense be made to the claim that it is a better estimate of what is likely to happen. (Ibid., pp. 13-14).

Ayer then goes on to recognize how it was Carnap who tried to meet this objection by introducing what he called "the principle of total evidence," wherein "we have to take as evidence ... the total evidence available to the person in question at the time in question, that is to say, his total knowledge of the results of his observations." (Carnap, 1947, in Ibid., p. 14). This corresponds with the common sense notion that the greater the amount of information, the closer the observer will be to the attainment of truth. Ayer identifies this as a common sense notion, but it is also perhaps a qualitative restatement of the law of large numbers. Unfortunately, there is no logical reason to expect that the proposition that is concerned with all the available evidence is in any way superior to any one of the propositions that are

concerned with any one of statements of evidence, since all the statements of evidence are necessary truths.

The concrete example that Ayer utilizes to illustrate his primary objection to the Keynesian formulation is one of trying to estimate the chances of winning for a given horse, appropriately named *Eclipse*, in the running of a horse race. The hypothetical "disciple of Keynes ... is determined to be rational and so to bring his degree of belief in the horse's victory into exact accordance with the objective probabilities." (Op. cit., p. 13). The evidence he then assembles takes the form of such statements as: " h_1 that *Eclipse* will be ridden by a champion jockey; h_2 that the going will be hard; h_3 that *Eclipse* is suited by the distance; h_4 that it went lame after its last race ... and so forth. ... taking a to be the proposition that *Eclipse* will win, he decides that the probability of a on $h_1 = p_1$, $a/h_1 = p_2$, $a/h_1 = p_3$, $a/h_1 h_2 = p_4$, $a/h_{1-4} = p_5, \dots$; and finally that a/h_{1-n} , where h_{1-n} represents the totality of the relevant evidence at his command, $= p_n$. How is he to place his bet?" (Ibid.). After all, knowing h_4 , that *Eclipse* went lame after the last race, should be enough information for even the most ardent disciple of Keynes to refrain from betting on *Eclipse*, without the accumulation of any further evidence.

With respect to this first objection, Professor Ayer is roundly criticized by E.H. Hutten, for having "forgotten his Hume, because [at least, according to Hutten] if you make too much of the idea of total evidence, you require really another

version of justification of induction." (Hutten, in Körner, 1957, p. 18). For Hutten, the fact that a statement of probabilities changes with the evidence is nothing to be concerned about, and it is only in an absolute sense, when the idea of total evidence is involved in the attempt to justify induction as a whole, that the problem of induction, as described by Hume, reemerges. (Ibid.). In other words, Hutten is criticizing Ayer for having forgotten his Hume, when it is precisely the Humean problem that Ayer is unwilling to ignore. Nevertheless, what has come to be known as the logical relation theory continues to hold sway as the dominant logical interpretation of probability theory. As such, it has been more than adequately described in a relatively recent contribution to the field by F.C. Benenson, (1984), in *Probability, Objectivity, and Evidence*:

... probability judgements are always relative to evidence, and in fact merely indicate the degree of certainty a particular body of evidence gives to the hypothesis we are concerned with. Considerable attention has been given in recent years to developing this conception of probability under the name of the Logical Relation Theory of Probability, which, in Carnap's work, has become the basis of contemporary inductive logic. Where Keynes had been concerned to develop a system of comparative probability based on relations of equality and inequality of evidential support, Carnap worked to develop the quantitative theory of probability based on a quantitative inductive logic. (Benenson, pp. 13-14).

Of course, the development of a quantitative inductive logic has enjoyed unprecedented acceptance and success, not only through the work of Keynes and Carnap and their followers, but perhaps a bit paradoxically through the work of those who might otherwise also be classified as frequency theorists.

R. L. Ellis, (1842, 1854), John Venn, (1866), F. Y. Edgeworth, (1884), Karl R. Pearson, (1892), W. S. Gosset, (1908), Richard von Mises, (1928), A.N. Kolmogorov, (1933), Hans Reichenbach, (1934), Jerzy Neyman, (1950), B. V. Gnendko, (1954), and perhaps most notably, R. A. Fisher, (1925, 1935, 1956), have all contributed to the success of such an interpretation of probability theory, whether or not they explicitly consider the philosophical limitations of inductivism or subjectivism, for that matter. Still, the frequency theory remains committed to a mathematical conceptualization, and its influence on the logical relations theory should not be exaggerated, in spite of the unarguably obvious analogy between the law of large numbers and the principle of total evidence.

That which E.H. Hutten describes as another objection to the Keynesian formulation, but may also be simply described, at least from a personalistic point of view, as a statement of fact, is that some element of subjectivity must necessarily enter judgements of probability, when the totality of evidence available to one person is necessarily different than the totality of evidence available to another. (Ayer, 1957, p. 15, and Hutten, *Op. Cit.*, p. 19). Indeed, the case can and has been made, by Henry E. Kyburg, Jr., (1961), for one, in his own extension of the logical interpretation of probability theory, that the personalistic theory of probability, as it was first developed by Frank Plumpton Ramsey in *The Foundations of Mathematics [1925] and Other Essays*, (1931), is little more than a vehicle for the justification of the axioms

of confirmation utilized in the development of logical theory. (Kyburg, 1961, p. 30). In a similar vein, although certainly with a different emphasis, J. R. Lucas, in *The Concept of Probability* (1970), has described the subjective approach of Ramsey, (1931), de Finetti, (1937), Good, (1950), and Savage, (1954), as overlapping with the Logical Relation Theory. (Lucas, 1970, p. 215). Ramsey, himself, in the foreword to *Truth and Probability*, (1926), an essay to a great extent indebted to the writings of C. S. Peirce, (Ramsey, 1926, in 1931, p. 194n), writes that "the general difference of opinion between statisticians who for the most part adopt the frequency theory of probability and logicians who mostly reject it renders it likely that the two schools are really discussing different things, and that the word 'probability' is used by logicians in one sense and by statisticians in another." (Ibid., p. 157). But in the essay itself, Ramsey is warranted to distinguish his own work from that of other logical theorists:

Logic as the science of argument and inference is traditionally and rightly divided into deductive and inductive; but the difference and relation between these two divisions of the subject can be conceived in extremely different ways. According to Mr. Keynes valid deductive and inductive arguments are fundamentally alike; both are justified by logical relations between premiss and conclusion which differ only in degree. This position ... I cannot accept. I do not see what these inconclusive logical relations can be or how they can justify partial beliefs. In the case of conclusive logical arguments I can accept the account of their validity which has been given by many authorities, and can be found substantially the same in Kant, De Morgan, Peirce, and Wittgenstein. All these authors agree that the conclusion of a formally valid argument is contained in its premisses; that to deny the conclusion while accepting the premisses would be

self-contradictory; that a formal deduction does not increase our knowledge, but only brings out clearly what we already know in another form; and that we are bound to accept its validity on pain of being inconsistent with ourselves. The logical relation which justifies the inference is that the sense or import of the conclusion is contained in that of the premisses.

But in the case of an inductive argument this does not happen in the least; it is impossible to represent it as resembling a deductive argument and merely weaker in degree; it is absurd to say the sense of the conclusion is partially contained in that of the premisses. We could accept the premisses and utterly reject the conclusion without any sort of inconsistency or contradiction. (Ibid., pp. 185-86).

But the contribution of Ramsey, I think, is not so much a function of his understanding of the asymbiotically uneasy relationship between deductive and inductive logic. He is far from alone in this understanding. Rather, the most important contribution of Ramsey, I believe, is in his continuation of the tradition of De Morgan, in his emphasis on belief, as compared to reasonableness, in the definition of probability as rational or reasonable degree of belief. Temporality alone should force even the least discriminating reader into making a distinction between belief and expectation. Nowhere have I read of any such or similar inspiration to allow even the most discriminating reader into contemplating a distinction between expectation and *l'espoir*. But no less important than the nuanced differences in meaning between the Romance languages and the English language, or even the nuanced differences within the English language itself, I think, is the mathematization of the concept. Otherwise stated, I believe the significance of subtle differences in meaning becomes moot at that level of abstraction wherein the philosophers of science

under erasure knowledge are in disagreement concerning the quantifiability of qualitative concepts. It is here that the premise-conclusion distinction between deduction and induction asserts itself in the inability of the mathematical concept to yield a non-mathematical solution. The conceptual apparatus simply does not exist. It is impossible to distinguish between any one given premise, or evidential statement, and another, in Keynesian terms; in an infinite universe of evidential statements, it is impossible to reach any conclusion with certainty. Consequently, nothing certain can be conjectured about the outcome of the single case. Such is the argument of Hutten, at least in his recognition of this limitation of the logical interpretation of probability theory; but he also correctly recognizes the analogous limitation of the frequency theory. (Ibid., p. 19). Still, at any given particular level of abstraction, subtly nuanced differences of meaning can and often do lead to contradictory conclusions, especially when competing epistemological frameworks such as the objective and the subjective are involved. For that reason, it would be inappropriate not to acknowledge my own approbation of the following passage from De Morgan:

We shall ... take it for granted that external objects actually exist, independently of the mind which perceives them. And this brings us to an important distinction, which we must carry with us throughout the whole of this work. Besides the actual external object, there is also the mind which perceives it, and what (for want of better words or rather for want of knowing whether they be good words or not) we must call the *image of that object in the mind*, or the *idea* which it communicates. The term *subject* is applied by metaphysicians to the perceiving mind: and thus it is said that a thing may be considered *subjective-*

ly (with reference to what it is in the mind) or *objectively* (with reference to what it is independently of any particular mind). But logicians use the word *subject* in another sense. In a proposition such as "bread is wholesome," the thing spoken of, "bread", is called the subject of the proposition: and in fact the word *subject* is in common language so frequently confounded with *object*, that it is almost hopeless to speak clearly to beginners about themselves as *subjects*. I shall therefore adopt the words *ideal* and *objective*, *idea* and *object*, as being, under explanations, as good as any other: and better than *subject* and *object* for a work on logic.

The word *idea*, as here used, does not enter into that vague sense in which it is generally used, as if it were an opinion that might be right or wrong. It is that which the object gives to the mind, or the state of mind produced by the object. Thus the idea of a horse is *the horse in the mind*: and we know no other horse. We admit that there is an external object, a horse, which may give a *horse in the mind* to twenty different persons: but no one of these twenty knows the object; each one only knows his *idea*. (De Morgan, 1847, p. 29).

No one knows the object; each one only knows his idea. That we can never know the object, that we can only ever know our idea(s) concerning the object, sheds an entirely new light on the epistemological debate. This, I think, must be the legacy of Augustus De Morgan. Similar to the manner in which he shifted the emphasis from *reasonable* to *belief* in the definition of probability as *reasonable degree of belief*, he also shifted the emphasis away from the object, in the ambiguous, and, as I have argued earlier in the dissertation, false distinction between the subjective and the objective. No one knows the object. Each one only knows his or her idea of the object. This is no more true nor less false for any of those objects that are not to be found in nature. One such object is the concept of probability. Evidence abounds. But no one can know what it is; it is no more than an idea in the

mind. For Bellarmine, it was compatibility with the inviolability of Holy Scripture. For Laplace, it was the ratio of favorable case to all the cases possible. For Reichenbach and Von Mises, it was the limit of the relative frequency. For De Morgan as well as Keynes, it was the degree of reasonable belief. But all of this is not meant to contribute to or to degenerate into some sort of relativistic equivocation of the conceptualization itself. (See Chapter 1, pp. 17-21). Rather, it is meant to point out that whatever objective reality may exist, all our knowledge of that objective reality, in the end, is subjective. Unfortunately, such ideal conceptualizations as the binomial distribution, the central limit theorem, the law of large numbers, and perhaps most notoriously, the normal curve, have come to be considered objective realities by those who would confound the logical with the metaphysical. Moreover, the immediate relationship between whatever objective reality may exist and any subjective knowledge of that reality has been reconceptualized as proof of the existence of that which may be imaginary. As such, one last theory of probability is in need of explication.

The Propensity Interpretation

Of course, the problem of induction, as it was formulated by Hume, has been pragmatically characterized as irrelevant to an understanding of probability as a science of prediction based upon available evidence.⁵ Notwithstanding such a blatant and widespread disregard for the ideological import of the Humean problem, however, the propensity theory, as first articulated by C.S. Peirce, (1878, 1910⁶), and later espoused, of course with certain qualifications, by Karl R. Popper, (1957, 1959), Ian Hacking, (1965, 1975), D. H. Mellor, (1971), and Ronald N. Giere, (1973), does lead us to a position from which at least some rapprochement with the problem of induction seems tenable. The latter two theorists, in particular, insist upon the veracity of a realist single-case interpretation of probability.⁷ Would that the propensity theory of

⁵ See the full discussion, referred to above, between E.H. Hutten, A.J. Ayer, W.C. Kneale, R.B. Braithwaite, P.K. Feyerabend, M. Polanyi, B.D. Bohm, U. Öpik, W.B. Gallie, L. Rosenfeld, J.P. Vigier, M. Fierz, H.A. Thurston, G. Süssmann, M. Scriven, A.J.B. Cruickshank, and P.T. Landsberg, following the articles "On unknown probabilities," by R.B. Braithwaite, and "The conception of probability as a logical relation," by A.J. Ayer, in S. Körner, ed., *Observation and Interpretation: A Symposium of Philosophers and Physicists*. (London: Butterworth Scientific Publications, 1957), pp.18-30.

⁶ It should be noted, at the outset, that in 1910, C. S. Peirce reperused, as he stated, his 1878 writings on induction and probability. His later writings, more than those of any other author to whom I have had occasion to refer, with the possible exception of Sir Karl Popper, remain in constant dialogue with his earlier ones, thereby affording the reader more of an opportunity to historicize the conceptual issues.

⁷For a pointed critique of the propensity theory as it is articulated by Giere, the reader is referred to Peter Milne, (1986); also Tom Settle, (1973), for a good solid overview.

probability allow for the frequency theory to account for the single case, a revolution in science would certainly be at hand. Unfortunately, the propensity theory falls short when it deviates from the Humean conceptualization of the problem. Notwithstanding the introduction of what in the past has been called an occult or metaphysical force as an explanation for the apparent veracity of the probabilistic model, it remains, in many ways, faithful to the classical conceptualization.

In keeping with the Laplacian definition, Peirce at first defines probability as "the quotient of the *number* of occurrences of the event divided by the *number* of occurrences of the occasion." (Peirce, 1878, in *idem*, 1910, in Justus Buchler, ed., 1940/1955, pp. 164-65). But Peirce, after what he calls a full generation of reflection, comes to embrace a position which today is also characteristic of the frequency definition: "For it is plain that, if probability be the ratio of the occurrences of the specific event to the occurrences of the generic occasion, it is the ratio that there would be in the long run, and has nothing to do with any supposed cessation of the occasions." (*Ibid.*). But by this time, Peirce is also clear about a number of theoretical problems inherent in this interpretation: "This long run can be nothing but an endlessly long run; and even if it be correct to speak of an infinite 'number,' yet ∞/∞ (infinity divided by infinity) has certainly, in itself, no definite value." (*Ibid.*). Of course, this difficulty may be and, I think, more than adequately has been met, through the develop-

ment of the concept of a virtual frequency or limit, toward which the actual ratio of the number of occurrences of those specific events and the number of occurrences of the generic occasion, necessarily approaches, as the number of trials approaches infinity.⁶ From his own perspective, Peirce was very much aware of a number of practical problems that may arise in the application of this frequentist emendation to the classical approach to probability, namely the accuracy with which the occurrences of the specific events and generic occasions are considered, the assumption of independence for each of the events, and perhaps most importantly, in recognition of the Humean problem, that there could be no reason to assume the probability will not vary over time. (Ibid). Still, Peirce goes on to espouse what R.B. Braithwaite (1957, p. 78), first identified as a precursor to the propensity theory of Karl R. Popper, in his articulation of that which would-be, given the major assumption that the future will continue to resemble the past:

... the die has a certain "would-be"; and to say that a die has a "would-be" is to say that it has a property, quite analogous to any *habit* that a man might have. Only the "would-be" of the die is presumably as much simpler and more definite than the man's habit as the die's homogeneous composition and cubical shape is simpler than the nature of the man's nervous system and soul; and just as it would be necessary, in order to define a man's habit, to describe how it would lead him to behave and upon what occasion -- albeit this statement would by no means imply that the habit consists in that action -- so to define the die's "would-be," it is necessary to say how

⁶See, eg., Popper, (1957, pp. 34-7), and Giere, (1973, p. 477), for alternative positions within the propensity theory, concerning virtuality.

it would lead the die to behave on an occasion that would bring out the full consequence of the "would-be; and this statement will not itself imply that the "would-be' of the die consists in such behavior.

Now in order that the full effect of the die's "would-be" may find expression, it is necessary that the die should undergo an endless series of throws from the dice box, the result of no throw having the slightest influence upon the result of any other throw, or, as we express it, the throws must be *independent* each of every other. (Peirce, 1910, in Justus Buchler, ed., 1940/1955, p. 169).

But the future need never resemble the past, and without wishing to continue to belabour the point, the objections raised by Leibnitz against Bernoulli, so many years ago, continue to obtain. Unlike his successors, however, Peirce is neither willing to concede the Humean problem, a concession, I believe, that is implicit in any frequency interpretation, even one which posits a finite limit; nor is he willing to deny its ideological import, as it is so egregiously denied by the single-case propensity theory, and perhaps implicitly denied by the others, as well. But this is certainly not the place to critique the nuanced differences between the various propensity interpretations of probability theory. For that, the reader is referred to the comprehensive article by Tom Settle, (1973), already mentioned above. Moreover, I am also of the opinion, that no detailed critique of any contemporary propensity interpretation need be made, as long as its specificity is neither antithetical to nor illustrative of the discussion at hand.

In revealing his own classical bent, Charles S. Peirce does contribute conceptually to the history of probability theory in a manner that has long since been forgotten or

deemed as being irrelevant. Peirce raises the hypothetical situation wherein one is forced to choose a card from either a stack containing twenty-five red cards and one black one, or a stack containing twenty-five black cards and one red one. With eternal felicity awaiting the procurement of a red card, and damnation awaiting the procurement of a black one, Peirce convincingly argues that a choice from the heavily proportioned red stack is the obvious and reasonable one. But he continues to argue that if the one black one is chosen from the otherwise all red stack, despite the odds, the reasoning is demonstrated to be worthless. (Ibid., 1878, p. 160). How then is the basis of the decision to be justified? Peirce was unequivocal in his expression of admiration and respect for that type of inductive reasoning by which "[man] observes the finite and he seems to know the possible infinite." (Idem, 1866 in 1976, p. 227). On the other hand, he also was decidedly unambiguous in his position that "matters of fact never can be demonstrably proved." (Idem, 1878, op. cit., p. 167). How then to justify inductive reasoning? For Peirce:

... mathematical reasoning is explicative reasoning, that is to say, that, unless it be unsound and quite worthless, it concludes no more than is asserted in its premisses ... [On the other hand], ampliative induction ... is the very best kind of reasoning in the world, -- better than the demonstrative reasoning of mathematics, because it assures us of what we had not previously known even in dismembered fragments; and although this assurance is not absolute and may be erroneous, yet perseverance in the same kind of reasoning will gradually correct every error, and will enable us to approach indefinitely near to certainty. (Peirce, October 24-28, 1910; in Carolyn Bisele, ed., 1976, p. 214 ff.).

Perhaps the moral of Peirce's demonstration of the limits of human reason, in the rational but unfortunate decision to choose a card from the predominately red deck, and to pick the only black one, is that it is better to have lost for good reason, than to have won for a wrong one. Better to be hanged as a sheep than as a lamb. But in an alternative hypothetical situation, posed in a contemporary fable by Damon Runyon, a modern-day *Polonius* offers the following advice to a modern-day *Laertes*:

... no matter how far you travel, or how smart you get, always remember this: Someday, somewhere ... a guy is going to come to you and show you a nice brand-new deck of cards on which the seal is never broken, and this guy is going to offer to bet you that the jack of spades will jump out of this deck and squirt cider in your ear. But ... do not bet him, for as sure as you do you are going to get an ear full of cider. (Runyon, 1933/1992, pp. 14-15).

The reason for the inclusion of this anecdote as a counterexample to Peirce's cynical view of mathematical reason is predicated upon the understanding that in both Peirce's instance and in this, that which appeared to be certain was not. In the first example, this is so because reality only approached certainty. With a possible improbability, there was a chance in twenty-six of eternal damnation. In the second example, certainty only approached reality. It was, in the words of Aristotle, a probable impossibility, and this was to be preferred to a possible improbability. (See *Poetics*, xxv, 1461^b15). In either case, however, enough discord should have been present, at least to have delayed a decision. In that time, a conjecture could have been made, and in that time, an

induction could also have been made. For Peirce, this was what comprised what he called ampliative reason, and all things considered, it was what he also believed brings us closer to God. He continues:

On the other hand, it is a far higher kind of reasoning to that instinctive operation by which on considering a surprising experience we are enabled to make a good guess at its cause or reason. Galileo called this power *il lume naturale*. I liken it to instincts of the lower animals, such for example as that whereby a wasp is led to lay her eggs where the young will best find food, although she has never seen such a thing as an egg; even if she remembers that former state of her existence when she used to eat the kind of food that will, at a later time, grow in the place where she is going to lay her first eggs. Induction is a higher kind of reasoning than that for the very reason that it understands itself, and consequently able to regulate itself and to attain a higher degree of certainty without end; while in most human beings the instinctive power of divining truth is almost atrophied by their habit of self-control, which they come to regard as essential to rational thought and to reasonable conduct. And in this they are mainly right, though inspiration is requisite if one is to be a veritable discoverer. In order that one may start an induction, two things are requisite: 1st, that one should be in some small degree inspired with a power of divination; and 2nd, that one should be profoundly sceptical of any such power. (Loc. Cit.).

The point to be made is that the future is yet to be determined. If, in order to justify its definition, God's will or existence is offered up as an explanation of the accidental nature of the universe, human activity serves just as well. The lesson I learned, so long ago from the Damon Runyon anecdote was antithetical to the lesson taught by Peirce. But in fairness to his level-headed pragmatism, he too would be looking at the experimental conditions, when any specific event exceeded too far and more importantly too long, its expected relationship to the generic occasion. Still, Peirce

the theist, turns to God, or at least divination, to resolve the problem of induction, while Runyon turns to his own *deus ex humana*. Peirce, in his writings on probability, is ever mindful of the Humean problem of induction. His resolution, however, in the end, reads more like an abdication, never to adequately respond to that which Hume so cogently argued in *An Abstract of a Treatise of Human Nature*:

'tis commonly suppos'd, that there is a necessary connexion betwixt the cause and effect, and that the cause possesses something, which we call a power, or force, or energy. The question is, what idea is annex'd to these terms? If all our ideas or thoughts be derived from our impressions, this power must either discover itself to our senses, or to our internal feeling. But so little does any power discover itself to the senses in the operation of matter, that the Cartesians have made no scruple to assert, that matter is utterly deprived of energy, and that all its operations are perform'd merely by the energy of the supreme Being. But the question still recurs, *What idea have we of energy or power even in the Supreme Being?* All our idea of a Deity (according to those who deny innate ideas) is nothing but a composition of those ideas, which we acquire from reflecting on the operations of our own minds. Now our own minds afford us no more notion of energy than matter does. When we consider our will or volition *a priori*, abstracting from experience, we should never be able to infer any effect from it. And when we take the assistance of experience, it only shows us objects contiguous, successive, and constantly conjoined. Upon the whole, then, either we have no idea at all of force and energy, and these words are altogether insignificant, or they can mean nothing but that determination of the thought, acquir'd by habit, to pass from the cause to its usual effect. But who-ever would thoroughly understand this must consult the author himself. 'Tis sufficient that I can make the learned world apprehend, that there is some difficulty in the case, and that who-ever solves the difficulty must say some thing very new and extraordinary; as new as the difficulty itself. (Hume, 1740 in Keynes and Sraffa, 1938, pp. 22-3).

That Peirce at least apprehended there is some difficulty in the case cannot be disputed. Neither can it be disputed that the propensity theory does, in fact, say something new

and extraordinary. The propensity theory, however, as an example of a realist approach, neglects the role of the subject when the natural or virtual tendency of randomly generated outcomes are in fact able to be interfered with through actual human intervention. The dualistic trap is set, as much through virtuality, ie., the denial of the role of the subject, as it is through actuality, ie., the antithetical overdetermination of the universe through human intervention.

The conceptualization of a universe that is free to evolve as a result of random variation, whether it be so described as providence or chance, may absolve the conceptualizer of any responsibility for those outcomes that are in fact or deed controllable.' The propensity interpretation of probability theory entails the belief that there is some metaphysical property by which not only do we reason, but towards which also we must move. I have deliberately avoided any detailed discussion of the various propensity interpretations other than that of Peirce, for the simple reason that the objections to Peirce's reasoning which ultimately arrives at a notion of divinity, become obscured in the responses to the more contemporary literature which assume an atheistic universe. Devoid of gods, perhaps, but it is a universe that remains invested in divination, nevertheless. For what, in the end, is any prediction of the future, but its divination?

' The ideological ramifications of chaos theory, as they are evidenced if not reified through practical applications, will be investigated in a forthcoming work.

As such, whether the so-called propensity is inherent in the object, as presumed by Peirce, or in the experimental conditions or set-up, as presumed by Popper and Hacking, or conceivable in the single case, as insisted upon by Mellor and Giere, in the end, the conceptualization of a propensitory property is defined as an *objective* reality beyond human understanding if not control. The conceptual framework from which a critique of objectivity is possible, ie. the sociology of science, remains obscured by its own contingency and is undermined from within. Ahistorically conceived, human invention, and creation, itself, are abnegated to the status of objective reality, as well. How then to avoid the action of inaction? How then to avoid the kind of equivocated relativism alluded to (see p. 159) above?

A Sociological Reinterpretation

In *The Principles of Science*, (1873), W. Stanley Jevons shifts the argument from the subject/object split, to the oftentimes contradictory relationship between the individual and the collective; but in order to develop this line of inquiry, Jevons first needed to investigate and enunciate the limitations of sequential thinking:

A little reflection will show that knowledge in the highest perfection would consist in the *simultaneous* possession of a multitude of facts. To comprehend a science perfectly we should have every fact present with every other fact. We must write a book and we must read it successively word by word, but how infinitely higher would be our powers of thought if we could grasp the whole in one collective act of consciousness! Compared with the brutes we do possess some slight approximation to such power, and it is conceivable that in the indefinite future

mind may acquire an increase in capacity, and be less restricted to the piecemeal examination of a subject. But I wish here to make plain that there is no logical foundation for the successive character of thought and reasoning unavoidable under our present mental conditions. *We are logically weak and imperfect in respect of the fact that we are obliged to think of one thing after another.* We must describe metal as 'hard and opaque,' or 'opaque and hard,' but in the metal itself there is no such difference of order; the properties are simultaneous and coextensive in existence. (Jevons, 1873, p. 34).

That the properties of metal, which are simultaneous and coextensive in existence, are comprehended sequentially, displays, for Jevons, a logical weakness and imperfection in human thought. This, of course, ignores the Kantian dictum that neither abstract nor concrete reasoning is necessarily better or inherently superior or universally higher, as one form of reasoning compared to another.¹⁰ (Kant, 1800/1988, pp. 105-06). For Jevons, it is the more abstract or general conceptualization that overcomes the limitations of sequential thinking. Similar to the ampliative reasoning of Peirce, it

¹⁰ The expressions of the *abstract* and the *concrete* ... refer not so much to the concepts in themselves - for every concept is an abstract concept - as rather to their use only. And this use again can have varying degrees, according as one treats a concept now in one way, that is, either omits or adds a greater or smaller number of determinations. Through abstract use a concept gets nearer to the highest genus; through concrete use, however, nearer to the individual.

Which use of concepts, the abstract or the concrete, is to be given preference over the other? On this, nothing can be decided. Neither use is to be deemed less valuable than the other. By very abstract concepts we cognize *little* in many things, by very concrete concepts *much* in few things; what we therefore gain on the one hand, we lose on the other. A concept that has a large sphere is very useful in so far as one can apply it to many things; but on account of this there is the less contained in it. In the concept substance, for example, I do not think as much as in the concept chalk. (Kant, 1800/1988, pp. 105-6).

represents some higher order of thought. But for Jevons, the explanation lies not in the dichotomy between the subject and the object, nor in Peircian terms, between the ampliative and the explicative. Believing in the inferiority of sequential thinking, for Jevons, it immediately follows that the collective necessarily supercedes the individual:

We have not always in the English language sufficient means of distinguishing conveniently between the general and the collective use of terms. In Latin this distinctive use was exactly expressed by *omnes*, meaning all distributively, and *cuncti* meaning all taken together, a contracted form of *conjuncti* (joined together). In English all men may mean any man or all men together. Even the more exact word every is sometimes misused, as in the old proverb, 'Every little makes a mickle,' where it is obvious that every little portion cannot by itself make much, but only when joined to other little portions. (Jevons, 1895, p. 20).

It is here, I believe, that a major assumption, implicit but never stated, in the propensity interpretation, or any theory of realism, for that matter, manifests itself explicitly in the belief that the abstraction, the generalization, the mickle, if you will, is the result of the summation of the concrete, specific events that precede the formation of the whole. The collective is defined as a sequence of uniform events; (von Mises, 1928/1981, p. 12); and here, the problem of the conflation, or perhaps more properly, the equation of the subject and the object, or the quantification of the predicate, manifests itself not only in the equivocal relativization, but in the denial of the temporal element, as well. The collective, the generalized conceptualization, the absolute idealization exists prior to the relative, individu-

alistic, concrete, specific sequence of events. How is this possible? Quite simply stated, the actual individual sequence of events is immaterial to the virtual ideal universalization, which is defined *a priori*, in this case, as the *a posteriori* result of that sequence of events, whatever the actual sequence! The idealized collective remains abstracted from its individual components; disingenuously, it is imagined to be independent of its own creator, the thinking subject, as well. Referring to the contest between the realists and the nominalists, R. Leslie Ellis wrote:

The former in asserting the reality of universals did not maintain that what we think of when we use a general term is an actually existing thing. Like every one else they admitted, that in one sense nothing can exist but the individual, nevertheless they held that universals are not mere figments of the mind, but that they have a reality of their own which is the foundation of the truth of general propositions.¹¹ (Ellis, 1854, p. 50).

Ultimately, neither the socio-logical argument of Jevons, nor the ongoing contest between realism and nominalism is able to keep the major premiss of an equivocated argument away from the need for a subject/object split, at least, to any considerable degree. On the other hand, in the same passage quoted above, Ellis does clarify how the epistemological unification of the subject/object would certainly determine how so-called facts and observations were subsequently interpreted. The procedure then would consequently determine how knowledge is formulated and construed:

¹¹ In a very real sense, Emile Durkeim's theory of social facts and their relationship to society *sui generis*, is an articulation of the type of realism Ellis is describing.

If it be said that the grouping phenomena together is merely a mental act wholly disconnected from outward reality and altogether arbitrary, it may be replied that no mental act can be so. Why and how facts and ideas correspond is no doubt one of the great questions of philosophy; but the answer to it is surely to be developed from the consideration, that man in relation to the universe is not *spectator ab extra*, but is some sort a part of that which he contemplates, and that the *rebus avolsa ratio*, which is in truth the fundamental postulate of nominalism, is therefore inconcessible. (Loc. Cit., p. 50 ff).

"That man in relation to the universe is not *spectator ab extra*," is certainly a startling formulation of what later becomes, in physics, a linchpin for relativity theory; although, to this day, it remains well outside the epistémé of objective realism. Despite his own metaphysical insight, however, R. Leslie Ellis, much like his philosophical successor, Albert Einstein, remains committed to a realist position; but it is a realism that continues to distinguish between the fact, which is associated with an *objective* reality that is not readily accessible to human understanding, and the idea, which is associated with some *subjective* imperfect human conceptualization:

In every science the fact and the idea correspond because the former is the realization of the latter, but as this realization is of necessity partial and incomplete -- or rather because in the same fact are simultaneously realized a variety of separate ideas, separate, that is, as we conceive them -- this correspondence is but imperfect and approximate. (Ibid., p. 51).

Of course, the exposition of the epistemological basis for the crisis in European science, must consider how contradictory epistemological positions may result in contradictory if not oppositional contextual interpretations. But the problem remains that along with the unresolved philosophical

debates concerning the essential nature of the subject/object, an ambiguity remains concerning the subject/object of the study itself. Is the subject/object considered to be in a state of flux, constantly changing, developing, degenerating, or is it considered to be a changeless form, fixed and eternal? Perhaps even most importantly, does the subjective idea itself reconstitute the objective fact? Reminiscent of the ultimate problem of all philosophy, attributable to William Whewell, (see p. 144n4 above), Alfred North Whitehead helps to clarify how it is that:

The distinction between realism and idealism does not coincide with that between objectivism and subjectivism. Both realists and idealists can start from an objective standpoint. They may both agree that the world disclosed in sense-perception is a common world, transcending the individual recipient. But the objective idealist, when he comes to analyse what the reality of this world involves, finds that the cognitive mentality is in some way inextricably concerned in every detail. This position the realist denies. Accordingly these two classes do not part company till they have arrived at the ultimate problem of metaphysics. (Whitehead, 1926, p. 112).

It is unclear as to what Whitehead means by the ultimate problem of metaphysics. If it is the relationship between the idea and the thing, in keeping with the assertion of Whewell, perhaps a complete explanation is beyond our reach, especially if the thing itself is an idea. If it is the reconciliation of the separation of the object from the subject, perhaps as Adorno implies, the recognition of the role of the subject in altering the outcome does not elide the reification of the accomplishment, nor, for that matter, even the bifurcation. If it is in the definition of objective reality, some agree-

ment concerning the level of abstraction of that reality, be it individualistic or collective, is required, since the variations in the rates of change of the constitutions of the individual and the collective remain indeterminate. Regardless of the conceptualization of the relationship between the subject and the object, the problem of being and becoming implicitly reasserts itself, as a result of the perceived distinction between the growth and development, or the decay and diminution, of the individual event or substance versus the collective form or ideal. Whether it be under the guise of a propensity theory, a logical interpretation, a frequency theory, or even the classical interpretation, the problems of induction inherent in any theory of probability remain unresolved, along with their attendant metaphysics.

Implications for Further Research

The legacy of the probabilistic revolution remains uncertain. Devoid of that moral faculty which was such an essential component of both the classical and logical interpretations, albeit oftentimes denied by the former as well as the latter, the frequency interpretation of probability theory provides an empirical argument for the inviolability of contingent reality. Within the natural sciences, the laws of thermodynamics, physics, chemistry, and even biology, have all been rewritten as necessary facts of life, such that irreversibility is able to be accounted for through the postulation of a closed system. Similarly, the problem of order within the social

sciences is also negated through the imposition of a structural-functionalist account of the nature of social being. Perhaps it is here that a return to those questions concerning the methodology of science is in order. I have attempted to demonstrate, in this chapter on the logical ramifications of probability theory, as well as throughout the entire dissertation, for that matter, wherein the responsibility for the formulation of the laws of nature ultimately lies. Perhaps the competing epistemologies have served to equivocate the task; but the problem of the assignation of responsibility for the formulation of the laws of nature, within the context of a sociology of science, need not continue to be placed in the hands of God or gods or any other conceptualization of an objective reality, be it fact or fancy, and certainly not in any of the methodologies that have been developed to facilitate the task. How utopian the scientific project in a truly democratic society. The problem lies not in the mathematics; neither does it lie in the logic; the problem lies, rather, in how and by whom the mathematics and the logic are applied.

The recognition of statistical regularities within the social system, *a priori*, presents no more of a demonstrative proof for the inviolability of a contingent reality than does the recognition of statistical regularities within the planetary system. The problem arises, in each instance, *a posteriori*, when those statistical regularities are viewed as laws of nature; and, as laws of nature, those regularities are deemed to be ineluctable, immutable, inevitable, and

ultimately irresistible. The problem arises when, as an expression of the will to power, the character of being is imposed upon becoming. Whether it be articulated through some propensity to commit a crime, fall to the earth, or approach an average, the problem arises when explanation is characterized as objective reality; and that objective reality, be it real or be it imaginary, is allowed to determine our subjective reality, ie., our lives, be they ever so humble; and our death, be it the only veritability. Therein lies the problem. Would that resolution lie within articulation.

But the divination of truth, by definition, is open-ended. Unlike the simple deterministic relationships characteristic of Newtonian physics, the process of induction, to be sure, is subject to elements of chance as well as volition. The outcome of the single-case, the fate of the individual, remains indeterminate, no matter the logic or evidence proffered to the contrary. To pretend otherwise is to play the most dangerous of games; for if God were not to play dice, in an uncertain universe, how dare we? Alternatively, and perhaps even more urgently, if the creation itself were of an accidental nature, and if there is no God or gods to correct our follies, what hubris on the part of humanity to imagine the exclusion of those elements of chance which heretofore have underdetermined our existence? What unimagined consequences await such ill-conceived violations of divine and/or natural law? A probable science need not be such an ignorant science; that is, if every little a mickle does make. We are, after all, the

creators of the world in which we live; and the scientific community, as a member of the greater body politic, must quickly own up to this fact, lest the imperfect workings of authoritarianism grind less slowly than those of that democracy which has yet to be, and the pluripotentiality of the scientific project is subsumed by the omnipotentiality of the totalitarian state.

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