

THE RHETORIC BEHIND THE RESEARCH
IN AGRICULTURAL NON-CERTAINTY

By

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PREFACE

It would be nice if I could write this thesis in a style similiar to those cherished grade-school essays on "What I did for my Summer Vacation." Just like the kid fresh back from the mountains, I'm dying to raise my hand and tell everyone about my time spent in graduate school studying economics. All the marvelous and provocative ideas that I have come across through the course-work and the thesis research seem just as magnificent as any mountain or fishing hole. But most exciting have been all of the informal conversations with the many people in this department who have made it their business to study what seems to me to be the most interesting of all studies, human action.

But this isn't grade school; I'm told that the child's exuberance must give way to critical and emotionless thinking if a thesis is to be taken seriously at all. But the grandeur and insight contained in the works of writers like Shackle, Knight, McCloskey, Keynes, Popper, and Mill has pushed my power of emotionless thinking to the breaking point. Surely, the seasoned economist will take exception to the many times in this thesis when I have failed to suppress my astonished delight with what these and other writers have said.

What's more, the seasoned economist might also object to direction pursued in this study; there are no quantitative,

positive results for him to sink his teeth into. Most of what is contained here are questions of the most general sort; questions that I believe all scientists must ask themselves at one point or another in their career. Before any "hard" research is initiated, it seems prudent that the scientist, at the beginning of his career, decide for himself just what he expects his theory to deliver for him and how he expects the theory to accomplish that. In my case, the purpose of theory is twofold: First, it must satisfy my personal curiosity about the way things work. Secondly, and more importantly, theory should provide society with an apparatus or tool with which problems can be solved. And the only way in which these dual goals can be accomplished is through incessant, critical discussion.

Thus, this thesis represents a preliminary attempt to come to terms with some questions about economics that have dogged me since my days of undergraduate study. As I hope the reader will see, the area of risk and uncertainty research provides the ideal backdrop for us to probe questions about the epistemic standing of the assumptions in economic theory, and, the value of the method economists have chosen to criticise the theories they create.

Finally, I would like to acknowledge the assistance and support of those without whom this thesis would never have been written. This thesis is dedicated to my undergraduate mentor, Fr. Bertrand LaNoue, O.S.B. who was not only the first to ignite

my love of economics, but also the first to say, "You can do it." I am indebted to Dr. Bryan Schurle for his patient and gracious assistance to me throughout my graduate program. My committee members, Dr. Jeff Williams and Dr. Orlan Buller also deserve my thanks for their suggestions and comments which proved most helpful when thesis was in its earliest stages. Also, I am grateful to Dr. John Riley who guided my entry into K-State and followed my progress along the way.

Finally, I would like to lovingly thank my parents for everything they have so unselfishly done for me. They taught me to work hard, think positive, and to do it all in the name of the Lord.

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CHAPTER I.
INTRODUCTION

1.1 THE POWER OF METHOD

The problem of risk and uncertainty is one that has increasingly occupied economists in recent years. This is not surprising since risk and uncertainty have themselves occupied the minds of men since the first real decision was ever made. What is surprising is that it took economics so long to recognize the impact that an uncertainty of the history-to-come has on the decisions made by people like you and me. So how has economics, the so-called "queen of the social sciences," come to terms with the phenomena of uncertainty? To the average man-on-the-street, the method by which economists deal with uncertainty would seem very strange indeed. If Joe Farmer in Pawtucket, Kansas only knew the way that economists have "modeled" his decision process, he would surely shake his head and wonder "what in the world had ever gotten into them fellas."

What this paper seeks to show is that "what has gotten into them fellas" is a thing called scientific method. And, in the case of agricultural non-certainty research, scientific method, thanks to the economist's loyal and unquestioning allegiance to it, has pushed the research in this critically important area to the brink of irrelevance.

To illustrate the power that method can have on the content

and relevance of intelligent discussion, let us consider for a moment an analogy that exists between scientific method and legal process. In a particular court case, legal precedent requires that the prosecution show beyond a "reasonable doubt" that person A is in fact guilty of the crime as charged. The job of the prosecutor is then to gather up as much evidence and as many witnesses as possible in order to convince the judge and the jury that Person A is indeed guilty of hideous crime X. But there are certain rules that the prosecutor must follow in order to make his case stick. The evidence must be gathered and presented in a manner defined as acceptable by courts in the past. And these standards are subject to change. What may be enough evidence at one time may be insufficient in another. And if the criteria of "reasonable doubt" suddenly became so strict that no crook, however rotten, could be convicted and jailed for the crimes he committed, then the ideal of "justice for all" would wither into a pathetic joke. Court cases would no doubt continue to be heard under such a system, but the relevance and meaning of the decisions rendered would probably leave the public no recourse but to take justice into their own hands.

But this is economics, not law. Economists build theories, not legal cases; they don't try to throw people into jail. But law and economics do share a common purpose: To seek, to ascertain as best we can, and to defend the truth. It is therefore not suprising that like law, science does have its own codified version of what is required of a persuasive argument. Known as the philosophy of science, it seeks, in a manner even more precise than law, to provide criteria for the acceptance or the rejection of scientific arguments.

It is true that a man's life can hang in the balance of a legal decision, but a whole nation's well-being often depends on economic policy choices. Hence, a great deal rides on the standards by which economic arguments are judged. And if these standards of persuasive argument set down by the philosophers and adopted by economists are in reality impossible to adhere to, then, just like the legal system where no man can be found guilty, economics will degenerate into a pitiful charade. Economics would at last become deserving of the epitaph, "the dismal science."

If such is the case, then two results seem most likely to follow: First, the strictures of method will force economists to heed at least the more conspicuous requirements of scientific method. In the methodology section of their papers, the economists will pledge, like a magical incantation of respectability, their allegiance to the truth-revealing power of scientific method. But underneath this loyal exterior will lay the real standards of argument, the economist's authentic criteria for good research. But until the authority of method is overthrown, these authentic standards will go undiscussed and undisclosed to those who seek to make effective economic arguments of their own. Secondly, the public will certainly perceive that something is dreadfully wrong with the discipline they have counted on to advise their policy makers of economic reality. Public confidence in the worth of economic theory may fall to the point where vigilante economics may appear to be the only available alternative. Economists may find themselves to be the silenced voice in a world where economic policy is decided on the grounds of which

group happens to carry the biggest stick.

Clearly then, methodology possesses a tremendous amount of power. It is with no small amount of urgency then, that this thesis seeks to uncover just what damage the force of scientific method has wreaked on the research in agricultural non-certainty.

1.2 POPPER, MODERNISM, AND RHETORIC

A tremendous amount of intellectual effort has gone into the determination of the criteria necessary for persuasive scientific argument, both in science in general and economics in particular. Economists themselves, perhaps because they are haunted by an inferiority complex regarding the "scientific" worth of their discipline, have almost unconditionally accepted the rules of persuasion as laid down by the philosophers of science and a sort of "official" methodology has emerged.

The gist of this "official" methodology, to be referred to hereafter as "modernism" (a term first used by McCloskey) is that certain methodological (or argumentative) "rules" are required of any argument in order for it to be deemed satisfactorily scientific. These rules of argument in economics are the result of the application of a particular, and very persuasive philosophy of science that originated with the German philosopher, Karl Popper, who claimed, early in the twentieth century, that he had solved the Humean problem of induction. As will be shown in fuller detail later, the problem of induction is a problem of the logical justification of beliefs held by people. "Hume was interested in the status of human knowledge or, as he might have said, in the question of whether any of our beliefs --and which

of them-- can be justified by sufficient reason" (Popper, 1972, p. 3).

Men routinely believe in certain regularities, like the belief that the sun will rise tomorrow. Hume was convinced that the belief that the sun will rise tomorrow could in no way be logically justified because, even though the sun has always risen in the past, such justification would require reasoning from the particular to the general. Thus, Popper "solved" the problem of induction by saying that induction does not exist. It is thus impossible to induct (or prove) the truth of any proposition. Therefore, all hypotheses, beliefs, assumptions (whatever we choose to call them) are conjectural and without any basis in truth unless the deductive consequences of those hypotheses can be corroborated with phenomena in the real world.

In the Popperian system, the belief that the sun will rise tomorrow is only justified if that belief is put to the test every day. As time passes, that belief becomes corroborated through repeated "experiments." It is this necessity of corroboration with the real world that requires methodological rules to specify proper modes of corroboration. Among these rules, the most conspicuous one is that any scientific theory (or argument) which is proposed must be objectively falsifiable. And the only theories which are objectively falsifiable are those which make predictions. It is not enough just to come up with an argumentative theory which makes predictions; the predictions themselves must be tested against the phenomena of the objective world.

In this scheme, theories are like vessels which we seek to fill with what Popper has called verisimilitude (closeness to the truth). It matters little whether or not the vessel is ornate or

simple, what is important is how much verisimilitude the theory is logically capable of holding and how much our experiments reveal that it actually does hold.

Conceivably, therefore, the methodological rules of modernism would be the acid test which would determine the value of economic argument. This paper too, seeks to make a persuasive economic argument. Hence, the reader might naturally expect this testimony to be accompanied by reams of computer printout paper and to be loaded with tables upon tables of regression results which would document that the obligatory testing had in fact been done. However, this writer would object vehemently to the rejection of his case on the grounds that it does not conform to the scientific "rules of law" if other economic theories were routinely accepted and rejected by criteria different from the "official" methodology. If "modernism" could be shown to be just an aggregation of words that most theorists dutifully invoke in the introduction of their papers and which are then quickly forgotten, no economist should feel any obligation to follow such an empty and illusory methodology. Rather, the writer would be free to attempt to convince in the most persuasive manner he could possibly devise.

Assume for the moment that such is the case; that the canons of scientific methodology are empty and illusory. Assume that however plausible they might appear to be, there are no laws of scientific methodology that have not at some point in the history of science been violated with impunity. Assume further that some of the world's greatest scientists succeeded only because they deliberately broke all the rules. If such is the case, by what

then should an economic argument be judged? Mathematical complexity? Statistical dexterity? Verbal Obtusity? Certainly not. While it is readily conceded that sometimes these are the necessary components of effective argument, they are not its defining features. But if economic conversation clearly and simply moves from point to point, carefully considering all of the relevant supporting evidence, and deducing conclusions that are justified by the evidence, then any reasonable person ought to be persuaded by the results.

The art of persuading reasonable people is what is known as rhetoric. Rhetoric as it is used here should not be confused with the more common and derogatory use of the term which often implies a lot of talk without action or i.e., a lot of hot air. On the contrary, rhetoric as it is used here comes from the ancient tradition of Aristotle, Cicero, and Quintilian --each of whom happened to be very persuasive people in their own right. In Modern Dogma and the Rhetoric of Assent, Wayne Booth defines rhetoric in a number of ways: Rhetoric is "the art of probing what men believe they ought to believe, rather than proving what is true according to abstract methods"; it is "the art of discovering good reasons, finding what really warrants assent, because any reasonable person ought to be persuaded"; it is careful weighing of more-or-less good reasons to arrive at more-or-less probable or plausible conclusions --none too secure but better than would be arrived at by chance or unthinking impulse" (pp. xiii. xiv. 59, quoted from McCloskey, p. 482).

One might then suppose that it ought to be a simple matter to subsume the methodology of science as a species under the genus rhetoric. Perhaps this can be done, but only with great caution.

The truth or falsity of the assumption above must first be argued: It must be determined whether or not economists themselves actually use and are persuaded by arguments which exclusively adhere to the laws of the "official" methodology, modernism. Also it should be recognized that the methodology of science is directed towards a certain type of knowledge: objective knowledge. But rhetoric is directed towards persuasion. Therefore, the methodology of science can be a self-sufficient species of rhetoric only if the audience is persuaded by only those arguments that are based on objective (or scientific) knowledge.

If it can be shown that the assumptions above are valid; that economists, and all scientists for that matter, commonly are persuaded by arguments not based on objectively falsifiable theories, then the need for an "official" methodology disappears. If such is the case, then the first question to be asked is "Why?" Why should economists (remember, they are scientists) be persuaded by anything less than hard theories with deductive consequences that repeatedly stand up to attempts to falsify them? Could it be that the economists recognize that this objective knowledge, however attractive it may appear to be, is in fact impossible to obtain? If this is so, then what does it take to make a persuasive economic argument? Put in another way, what are the real criteria whereby economic arguments and the evidence which support them are accepted or rejected?

If the study of a particular research program reveals that the "real" rhetoric of the discipline is indeed modernist, then there is no problem and the value of this speculation

disintegrates. But if it can be shown that the "real" rhetoric turns out to be different from the "official" standards of persuasive scientific argument, then some hard questions need to be asked. If such is the case, it would be most important that economists recognize and be prepared to criticize the standards of persuasive argument that they have set for themselves.

1.3 A CASE STUDY: NON-CERTAINTY RESEARCH IN AGRICULTURE

Like so many other studies, this thesis is a response to economic arguments made in the past. In his 1983 article entitled "The Rhetoric of Economics," Donald McCloskey issues what amounts to a challenge to his colleagues. He urges economists to critically reappraise the standards of effective argument they have allowed the philosophers to set for them. This challenge is based on three conclusions McCloskey has reached about the state of "methodology" in economics. This thesis seeks to corroborate McCloskey's conclusions by considering a particular research program in economics, agricultural non-certainty research. McCloskey's three conclusions that we will attempt to verify are as follows:

1) Economists don't practice what they preach in terms of their methodology. Over the years, the "official" methodology of economics has changed. But from Mill to Marshall to Friedman and Samuelson, McCloskey argues that the most influential economists rarely live up to methodological norms they extol (see also Blaug, 1980). "And it is a good thing, too," McCloskey writes, "If they did they would stand silent on human capital, the law of demand, random walks down Wall Street, the elasticity of demand

for gasoline, and most other matters about which they commonly speak...Economic science would stop progressing if the methodology were in fact used" (p. 482).

2) The reason why economists do not adhere to their "official" rhetoric in practice is that any method is in fact impossible to follow. Reasoning from theories of knowledge and notions of the ideal science, the philosophers have set rigid limits defining which arguments are acceptable and which are not. But by the very nature of its subject matter, economics can not simultaneously fit those methodological norms and remain persuasive. To support this point, McCloskey argues that modernism a) is now considered obsolete in philosophy, and b) is not followed in the other "hard" sciences.

3) Since modernism has served as a cover for the rhetoric of economics, the real rhetoric has gone unexamined. Just what are considered acceptable arguments in economics is a question that goes undiscussed. And whenever such a crucial feature of a discipline goes unexamined, in this case the criteria for the acceptance and the rejection of arguments, there is great danger that the discipline might be lead astray and that growth will be stunted.

But the breadth of economic thought makes it impossible to examine carefully rhetoric of the entire discipline. The most persuasive argument in support of McCloskey's assertions would be to see how they stand up against a particular research program in economics.

"Oh no," the reader might exclaim, "Not another paper on method." If so, the present writer shares your fatigue. It is true that the library shelves are full of books on method. It is

also true that, as J. N. Keynes has written, "that it is one thing to establish the right method for building up a science, and quite another to succeed in building it up" (p. 4). Keynes was right about more than he realized. If McCloskey is right, then building a method and building a science are two different things because they are two mutually incompatible goals. Lest we forget, economics is supposed to be more than just an academic game pursued for no other reason than its intellectual attraction. Much of it is, and perhaps this is due to a passive allegiance to a particular method, any method, which artificially constrains discourse between reasonable people about real world problems. It is more than just an interesting fact of intellectual history that the writers who seemed most concerned about directing their economics towards the betterment of mankind and improvement in the wealth of nations were the very writers that seem to most violate the canons of method which, often, they were they very ones to set down (see McCloskey, p. 489). Therefore, if this study of the rhetoric of non-certainty research can lead to a more coherent discussion of the real phenomena of uncertainty as it confronts decision-maker, then the trek through the dark, dusty hallways of epistemology will have been worth it.

There are three reasons why non-certainty research was chosen as program with which we would attempt to corroborate McCloskey's conclusions. First, especially in ag-economics, risk research, as measured by the number of journal articles which now incorporate risk, is growing.

Secondly, the modernist non-certainty research program (NCRP) is a program that is clearly conceived in a modernist vein.

Uncertainty is said to exist whenever an individual is uncertain about the possible consequences of a given action. But the essence of the modernist NCRP is that the uncertain decision-maker is assumed to face a situation of risk, where he has the power to calculate the probabilities of possible outcomes of his decision. Though the actual research form varies greatly, from MOTAD to E-V analysis to stochastic dominance criteria, the core is essentially the same. We define modernist non-certainty research as that set of theories which assumes that individual's preferences follow the von Neumann-Morgenstern axioms of Ordering, Transitivity, and Independence. As a logical consequence of this, individuals are assumed to form subjective probability distributions about the possible consequences of specific acts. The individual will choose that act which maximizes his expected utility. Hence, if the theorist can estimate risk preferences and subjective probability distributions, then it is possible that behavior under uncertainty might be predictable. The NCRP is clearly a program geared towards the modernist method.

This apparent loyalty to the modernist dogmas really isn't all that startling since risk and uncertainty research began in earnest at just about the same time that the modernist methodology was being accepted by the economists. Friedman's landmark essay "The Methodology of Positive Economics" and Samuelson's influential Foundations of Economic Analysis appeared just a few years after the Von Neumann and Morgenstern book Theory of Games and Economic Behaviour. The former marked the beginning of the acceptance of the modernist methodology and the latter showed that predictions of behavior under risk were possible. Thus, the current research in risk and uncertainty is in many ways the

"baby" of the "official" methodology.

An examination of the offspring of the current methodology provides an opportunity to view the "official" rhetoric in a rather pure and unadulterated form. Unfortunately, the consequences of that purity have been that the non-certainty research program, by modernist standards, appears to be degenerating rapidly. Thus, the third reason why the NCRP was chosen as the illustrating case for McCloskey's argument is that uncertainty seems to be a topic that clashes vividly and violently with the nature of the modernist methodology, and it does so on essentially two levels.

First, the metaphysical theory that the modernist methodology is built on, has a peculiar relationship to uncertainty. Few have stepped forward to challenge the applicability of Popper's epistemological theory of knowledge in economics, and the research in risk and uncertainty creates a unique opportunity to do so. That metaphysical theory, which Karl Popper advertised as the solution to Hume's problem of induction, argues that knowledge is never certain, and that it is always of a tentative sort. He writes that "Thus the idea of truth is absolutist, but no claim can be made for absolute certainty [my emphasis]: We are seekers after truth but we are not its possessors" (1972, p 46).

And surely, Popper is correct. The uncertainty of scientific knowledge is just as real as the uncertainty that grips the mind of the decision-maker as he contemplates possible action schemes. Popper seeks to show that even though induction does not exist, reason still has a part to play in the growth of knowledge. If men's beliefs can be transformed from the subjective to the

objective and if the resulting conjectural theories can be put to the falsifying test, then men can rationally choose those theories that make the best predictions. Even Donald McCloskey, whose paper attacks modernism on all levels, concedes the appeal of Popper's argument on a purely epistemological level (p. 486). But in the case of economic behavior under risk and uncertainty, using Popper's method of uncertain knowledge to study the uncertain knowledge of economic actors seems a bit like using a microscope to study a microscope.

As Popper has remarked, metaphysical theories are non-demonstrable, but they can be argued. And a persuasive case against modernism and the tenets it implies can be made by comparing uncertainty in the real world with uncertainty in the search for scientific knowledge. When they conjecture about possible research designs, scientists trade risk of error and truth-potential in the same way that the economic actor must trade risk and expected income. In both cases, imagination and reason work together: Imagination is the originator of both scientific hypotheses and the possible alternatives which a decision-maker under uncertainty considers. Reason is used by the scientist to formulate persuasive arguments, and, people employ reason when making decisions. However, since imagination is an inherently subjective phenomena, it is all but ignored by the modernist rhetoric. If modernism requires that predictions of human behavior under uncertainty are possible, and indeed, necessary, then modernism should also contend that predictions of scientific behavior are also possible and, indeed, necessary. But of course, this fertile area of modernist research has not yet been exploited. Who would dare to predict the future actions

of a Pascal or an Einstein? Nobody. So why is the prediction of economic behavior which exists under essentially the same circumstances also attempted? What this paper argues is that there is no good reason why.

The second level where the recognition of uncertainty clashes noticeably with modernism is on the research level. We seek to demonstrate that the non-certainty research program's attempt to at least outwardly adhere to the tenets of modernism has greatly stunted the growth of knowledge in the area.

In a modernist comparison between standard, neoclassical certainty theory and non-certainty theory, certainty theory clearly comes out on top. Certainty theories 1) are easier to falsify, 2) generate predictions that are reproducible, 3) explain long observed empirical regularities, like the law of demand which non-certainty theory does not imply, and 4) focus on markets rather than individuals so that the theories are more vulnerable to inter-subjective testing. This is an odd, almost perverse, result. By the rules of modernism, economists would be better off sticking with certainty theory and ignoring risk. When the economist accepts the tenets of modernism, it seems that he forgoes any possibility of coming to terms with the phenomena of uncertainty.

The NCRP is therefore a program which makes McCloskey's case quite strong. On both the epistemological and the research level, modernism is the inappropriate criteria to judge the strength of arguments about human action. Yet the research in risk continues, and is even growing. What can justify that

growth? How have researchers in non-certainty managed to salvage any respectability in a discipline where modernism appears to have set the standards of persuasive argument? The answer is that they have given only a token nod to modernism, and that the "real" rhetoric of the program is something quite different from Popper's prescription that theories should always put to the test.

In fact, the non-certainty research program seems to be becoming less and less vocal with their allegiance to modernism. In fact, because the falsification of theories in this field is becoming increasingly difficult, many participants have now openly opted for a normative approach over the positive. This switch effectively dissolves any pretense that modernism is the "real" rhetoric of the research program. It turns out that the program is essentially held together by assumptions assumed to be true on apriori grounds, which makes uncertainty theory no more advanced than Mill's economic man of the 1860's.

Once the normative viewpoint of the program is recognized, the search for the "real" rhetoric behind non-certainty research can begin. It suddenly becomes possible to criticize the assumptions on grounds that are not even conceivable when the theories are assumed to be merely conjectural until corroborated by falsifying tests. Since the approach is normative, the intuitive palatability of the assumptions of the research program can be attacked on a variety of levels.

The first question to be asked is whether or not the NCRP explains that phenomena that caused economists to invent non-certainty theory in the first place. Does uncertainty theory

explain the method by which people make decisions under conditions of uncertainty? More importantly, is it possible to predict people's behavior under uncertainty?

If choice under uncertain circumstances is anything like we subjectively perceive it, then the business of prediction reduces choice to mere calculation, or i.e., determinism. Economists themselves are plainly aware, as is everybody else, that what it takes to be successful in the capitalist world (and life, for that matter) is good judgement; prudence. Yet modernists do not hesitate to completely abstract this crucial feature out of the analysis; one decision-maker is assumed to be just as prudent as the next. There is no model, no gambling game, and no probability distribution that can take the place of prudence. No argument can be persuasive that assumes the importance of prudence away.

When economists recognize the existence and nature of uncertainty, they are bound to confront the fact that, as G. L. S. Shackle has written, "Economics is about thoughts. It is therefore a branch or an application of epistemics, the theory of thoughts. Economics is concerned about thoughts about things, both directly, when business men consider the intended uses of their resources, and indirectly, when they consider and conjecture each others thoughts about what to do with the resources entrusted to them" (1972, preface).

But the non-certainty research refuses to recognize this self-evident proposition. Why else would Professor Shackle, an economist who has written a half a dozen books on uncertainty be

all but ignored by the mainstream profession? Shackle is persuasive for the very reason that he calls it like it is. Uncertainty is a fact that comes with time. The act of choosing among possible means to achieve given ends (read: economic behavior) is not aided by probability distributions. Choice is more human than that. It is folly to search for something that can't exist.

Blinded by the canons of method, the modernist troopers have struggled on under literally impossible odds, trying to predict behavior that is inherently unpredictable. We are told that Knight's distinction between risk (where we know the odds) and uncertainty (where we don't) doesn't mean much anymore. The golden rule of modernism is that all hypotheses are mere conjectures without any basis in truth until predictions come true which corroborate those conjectures. And if it takes the transubstantiation of a decision from a situation of uncertainty to a situation of risk to come up with predictions of behavior, then so be it. This approach might be reasonable in the case of a theory about the orbit of the planets, but in the case of decisions under uncertainty, it is plainly unacceptable and definitely not persuasive. If the economist really is capable of predicting what an economic actor will do under the conditions of uncertainty that commonly face people in the real world, then that economist ought to be rich. As McCloskey has written, "At the margin (because that is where economics works) and on average (because some people are lucky) the industry of making economic predictions, which includes universities, earns only normal returns" (p. 488). Yet, when their predictions fail, as they

inevitably must, the modernist militia always say, "We know our empirical methods are imperfect. But you have to crawl before you can walk." But of course, one doesn't want to crawl off the edge of the table either.

Given the impossibility of their situation, it is not surprising that the modernists have sunk to methods which aren't very persuasive. The problem of what Leamer has called "ad hocery" is rampant in non-certainty research:

"Theoretical econometrics nearly always proceeds as if there were a single, 'known' model that correctly describes the probability distribution [which the decision-maker faces]. What are unknown are only the values of some parameters in the known model...The sooner [we recognize] that nearly all applied work is shot through with applications of uncertain, subjective knowledge...the better" (Sims, in a review of Leamer's book Ad hoc Inference, p. 566, 567).

Even beyond the problems of ad hocery, there is reason to wonder whether the single, known model postulated by the applied econometricians actually does, or even can, exist. The literature just assumes that people form subjective probability distributions when making decisions. Rarely, is the cogency of a subjective probability distribution questioned. Do individuals attach probabilities to possible states of nature in such a manner that all the probabilities add to unity? A number of very vexing questions immediately suggest themselves if such is the case. How does the decision-maker put a probability value on those outcomes which he has yet to imagine? What probability value corresponds with the most likely outcome? Why should the probability of one possible outcome be affected by the probabili-

ty of another possible outcome?

What the analysis forces us to conclude is that the NCRP has boxed itself into a very tight corner. On one hand, the NCRP decidedly fails the modernist test of persuasiveness. And on the other, when we accept the NCRP's normative approach, the assumptions of the theory simply do not stand up to apriori scrutiny. In the end, one wonders just what contribution the research program is capable of making to man's stock of understanding about economic behavior. It appears that it is the attempted adherence to the modernist strictures which has pushed the NCRP to the brink of irrelevance.

Finally, the title of this paper mentioned that the rhetoric under consideration here is the rhetoric of agricultural non-certainty. Since most of the analysis in this paper can be easily generalized to decision theory in a variety of situations, the specific inclusion of agriculture in the title warrants an explanation. First, the tremendous variability that farmers face, from the weather and the insects to prices and costs, has created an urgent need for an economics of uncertainty in agricultural economics. Just as farmers deal with risk continually, it seems that ag-economists are, and justifiably so, very interested in the economic impacts of stochastic phenomena.

The second reason is perhaps more important than the first. Agricultural economics is an applied science; the general procedure is to adapt and apply approaches developed elsewhere in economic theory. Consequently, in their haste to provide useful analysis for the farmer and the policy maker, there is a danger that the methods of research will be applied uncritically. Thus,

there appears to be a need to address some of the questions about non-certainty theory that are too often passed over. It is hoped that, in particular, the agricultural economist will benefit from this analysis.

1.4 IN THE CHAPTERS WHICH FOLLOW...

In short, this paper seeks to show that the research in agricultural non-certainty is headed for non-relevance fast and that modernism is the reason why. In what follows, meat will be added to the bare-bones arguments which have been given here. The organizational scheme to be used in the following chapters is not dissimilar to that applied in many quantitative research reports. Modernism is, after all, a model (a model of models, perhaps). Hence, the first chapter roughly coincides with the aim of the traditional "Review of the Literature" chapter. In the same way that a "Review of the Literature" chapter would attempt to justify the use of a particular empirical approach, Chapter II endeavors to demonstrate that modernism is indeed the "official" methodology of economics (which includes, of course, non-certainty research). By showing that modernism is the "official" rhetoric of economics, the way is set for a comparison of the research in agricultural non-certainty with the modernist standards of science.

The third chapter contains the development of the model. Herein, the methodology of modernism is built from Hume's problem of induction on up. Modernism is compared and contrasted with the methodology of verificationism that it supplanted as the

"official" model of economic research some thirty years ago.

In Chapter IV, the specific application of the modernist model to the theory of decision under uncertainty is considered. The chapter opens with some preliminary remarks on the components of a scientific research program. In order to get to the "hard core" of the modernist non-certainty research program, the two fundamental sources of uncertainty theory are discussed: First, part of the reason for uncertainty theory in the first place is that economists would like to explicitly come to terms with the subjective feelings of uncertainty that we all have about the future. This prompts us to ask the question, "What is choice?" Second, because there are a number of phenomena in the real world that can not be explained unless we assume that individuals were adapting to conditions of uncertainty, there is a desire to create such a theory. Finally, the chapter closes with a presentation of the defining features of the modernist non-certainty research program.

Chapter V is in a way a presentation of results. Given that modernism is the accepted methodology of economics, and given that modernist economists have developed a theory of decision-making under uncertainty, how well does this non-certainty theory stand up to the modernist rules of persuasive scientific argument? We conclude that, by its very nature, research in non-certainty violates some of the fundamental tenets of the modernist rhetoric. At this point, the search for the real rhetoric behind the non-certainty research begins. The chapter closes with a critique of that rhetoric.

Chapter VI is, as one might expect, a conclusion that sums the results thus far and makes suggestions for further research. Most importantly, the chapter points to non-certainty research as an example of what loyal acceptance to the modernist method can do to the content of a research program. What the chapter advocates is an abandonment of a rhetoric that requires prediction at all times. Also, there is a plea for economists to become more aware of the criteria of persuasiveness that, in no small way, define the essence of a discipline. Finally, the paper closes with a suggestion for a rhetoric of economics aimed at persuasion rather than prediction.

In all, this paper seeks to wind together three different strands of thought; rhetoric, choice, and uncertainty. Along the way, however, it will be necessary to unwind modernism.

CHAPTER II.

MODERNISM: ECONOMICS' "OFFICIAL" METHODOLOGY

2.1 INTRODUCTION

Historically, studies in scientific methodology have primarily sought to specify the criterions of good scientific argument. In addition to this, methodological studies have been used to explain the historic development of persuasive scientific argument. Hence, methodology is a two-edged sword: It provides "how to" assistance to the scientist and it offers rules by which competing scientific theories can be appraised. Whether or not this sword is made of tin or steel is not the question at hand. The aim of methodology is simply to establish and enforce "the rules of the game." The "object" of the game is to persuade your colleagues that in fact the theory you defend is indeed the champion. Scientific progress, viewed in this light, chronicles the defeat of older scientific arguments by newer ones still unbloodied by the sword of methodology.

The ancients termed the art of good argument "rhetoric." Their lofty conception of rhetoric, with Aristotle and Cicero as exemplaries, requires divorcing the essence of the word in the classical sense from the derogatory implications that the word "rhetoric" carries today. In a recent article by Donald McCloskey, "The Rhetoric of Economics," rhetoric in the original

sense is said to be "a fine and honorable word" and should be treated as such. McCloskey calls rhetoric the art of "disciplined conversation." And inasmuch as scientific methodology is used to determine the demarcation between good and bad scientific practice, such methodology is a particular type of rhetoric meant to apply in the particular sphere of scientific conversation.

As in law, a body of methodological precedent has historically evolved out of the stirred-up dust of theoretical controversy. But since the early fifties, despite multitudinous theoretical dust storms that of late appear to be growing in both fury and frequency, the methodological norms of economic research have become progressively more codified, and are treated now almost as if they were irrevocably set in stone on the day of creation. In order to show that the last sentence is no hyperbole, this chapter will begin with a brief examination of the pervasivity of the codified rhetoric which McCloskey has termed "modernism." The first commandment of modernism is that all scientific theory, including that of economics, must be formulated in such a way that the theory is capable of being falsified by empirical data. The second commandment is that only those theories which have resisted efforts of falsification can be accepted.

Since it is the purpose of this paper to examine the rhetoric of the research in agricultural non-certainty, the next step in the chapter is to explicitly set forth the justifications for, and the criterions of, scientific method. Why do economists believe that we need method at all? By answering this question, the

justification for the rules by which the uncertainty research will be appraised (in Chapter 3) will have been set forth unambiguously. Pursuant to this, it is necessary to consider briefly the methodological precedents that modernism has replaced. This chapter makes no effort to challenge modernism's claim as the rhetoric actually employed by economists. Nor does the chapter challenge the justification for any methodology whatever. These challenges will come later when we examine the research in agricultural non-certainty from the sterile light of pure modernism. The purpose here is only to distill the essence of modernism from the vast crock of philosophic thought that has gone into the making and defense of the "official" rhetoric of economics.

2.2 MODERNISM: IT'S THE ONLY GAME IN TOWN

Research in economics typically begins with what is termed a "statement of methodology." By this the researcher seeks to set forth and defend the method he has chosen to tackle the problem at hand. The methodology chapter is important, we are usually told, because it is "imperative that the economist should seek to define as accurately as possible the nature and limits of his sphere of inquiry" (p. 3). John Neville Keynes penned that statement in his book on methodology, and surely he and the methodology-chapter writers are correct. The tools of the trade must be understood if economics wishes to add anything meaningful to man's stock of understanding.

But what this section seeks to show, is that for all intents and purposes, modernism is treated as if it was the only tool in the box. In other words, the mainstream opinion is that it is only the formulation of theories with empirically refutable consequences (i.e., predictions of human behavior) that can build the science of economics. This widespread acceptance is demonstrated here by looking at 1) the introductory textbooks, 2) the books on methodology, and, 3) in the case of agricultural non-certainty research, texts and survey articles which summarize the state of the art in that area. In fact, the orthodox economist has little choice except modernism; the business of making economic predictions has become the sine qua non of nearly all (publishable) economic research.

The young economist first encounters modernism in the first chapter of nearly every introductory textbook in the discipline. Here he is usually introduced to four facts of economic theory. First, he is told that economics seeks answers to economic problems. An economic problem, as Friedman has written, "exists whenever scarce resources are used to satisfy alternative ends" (1953, p. 6). Next, the student will usually read a woeful description of the procedural difficulties associated with a science where laboratory experimentation is impossible.

Third, and this is the clincher, our fledgling economist is told, in words such as the following used by Mansfield in his popular introductory text, that "The basic procedure [of economics] is the formation of models. A model is composed of a number of assumptions from which conclusions --or predictions-- are deduced ...the most important test of a model is how well it

predicts these phenomena" (p. 13-14). Of course it is true, the reader is quickly assured, "that the real test of a theory is its ability to illuminate reality" (Samuelson, p. 10).

However, there is but one path to such insight or "illumination," and that is the predictive power of economic theories.

Consequently, the reader is urged to look upon economists, to use Friedman's metaphor (1953, p 8), as file clerks of human behaviour. Economic theories should "serve as a filing system for organizing empirical matter and facilitating our understanding of it" (p. 7). In this scheme, economic theory would take on the cosmos of human behaviour as if it was a great stack of paper of all different sizes and colors with an infinite variety of languages and pictures written upon it. Such theory would, in a manner similar to zoology or botany, classify the variety of human behaviour by separating the "stack of paper" into categories possessing distinct qualities. Such an approach would provide insight; we would realize that the great stack of paper is indeed not as chaotic as it might appear to be as it sits on our worldly desk. The usefulness of the filing classifications depends on the the number of cross-references required to make the system consistent and complete; the filing system is mere tautology unless the classificatory divisions are capable of being tested via predictive experimentation.

And though there are no guaranteed paths to insight, the reader is assured that economics is a science like all the other "hard" sciences. Both seek the same goal. Both gain their methodology by the application of the philosophy of science

applied to a particular sphere of inquiry. All scientists are file clerks of one sort or another; the natural human response to chaos has always been to attempt to classify the diveristy of experience. A cogent classificatory scheme (or, stereotypes that really fit) will yield insight. Indeed, throughout its history, all of science has endeavored to render explanation out of chaos: "It is the desire for explanations that are at once systematic and controlled by factual evidence that generates science; and it is the organization and classification of knowledge on the basis of explanatory principles that is the distinctive goal of the sciences" (Nagel, p. 4).

Fourth, and finally, the student is warned against putting too much stock into the validity of the assumptions. "Economists build," to quote Nicholson's introductory text, "rather simplified models (usually mathematical) which are intended to more or less represent reality" (p. 35). But the important thing, to use another one of Friedman's illustrations, is not whether the expert billard player actually does calculate geometrically all of the angles of the shots facing him, but rather, whether he acts as if he calculated those angles. The intuitive palatability of the assumptions is only an indirect test of a theory, which is subservient to the test of predictive accuracy.

One would expect that the methodological norms set forth in the introductory texts are reflections of the accepted books on methodology. And such is usually the case. The libraries are loaded with books on economic methodology that argue for modernism. The first that comes to mind is Friedman's 1953 essay "The Methodology of Positive Economics" which marks in many

respects the watershed for the acceptance of the tenets of modernism in economics. Yet to mention Friedman first is to bypass one of the original proponents of modernism, Terrence Hutchison. His 1938 book the Significance and Basic Postulates of Economic Theory represents one of the first attacks on apriori reasoning and it argues for predictive tests of theoretical constructs. Hutchison's latest book, The Politics and Philosophy of Economics (1981) shows that his views have changed little over the years. Also recently, Mark Blaug's 1980 book, The Methodology of Economics, or How Economists Explain, could easily be retitled to Why Economists must Predict. Since this section is concerned only with the pervasivity of modernism, these writers will be examined in greater detail later on. But the point is that when it comes to discussions of methodology, most of the conversation today revolves around either some of the finer points of modernism or the criticism of theorists who fail to formulate their theories in such a way as would make them capable of testing. If such were not the case, problems of methodology wouldn't be so hastily mentioned in the first chapter of economic textbooks for undergraduates. As Blaug writes "For the most part, the battle for [modernism] has been won in modern economics (would that we could say as much about some of the other social sciences). The problem is now to persuade economists to take [modernism] seriously" (1980, p. 260).

Nevertheless, different areas of research adhere to the tenets of modernism in different degrees. So, before going through the long and involved discussion of exactly what

modernism is, it seems prudent to first check and make sure that modernism is indeed the "official" rhetoric of the research in agricultural non-certainty. Perhaps the most expedient method of doing this is to briefly review some of the methodological conventions as presented in texts and survey articles in the field.

Kenneth Arrow, a Nobel Laureate who has written widely on the theory of decision under uncertain prospects, lends credence to the belief that the current risk research is conceived in a modernist vein. In the 1959 article "Functions of a Theory of Behavior Under Uncertainty," Arrow sets forth the essential problem confronting economists willing to face up to the existence of uncertainty. He argues that economists deal with uncertainty for two reasons. First, there is the subjective feeling of unknowledge about the future that we all perceive and secondly, there is the objective existence of certain phenomena in human affairs that would never occur in the world of perfect certainty. Arrow points out "that these two viewpoints interplay, of course, as indeed the subjective and objective viewpoints always do in the social sciences. We interpret the actions of others by sympathetic understanding generated by an imagined perception of our own actions in similiar situations." Then Arrow sets the methodological tone for the research in non-certainty by adding in parentheses that "To be sure, any such interpretations are only hypotheses which must be verified by their ability to predict human behavior" (p. 12).

And Arrow is not alone. The further one probes into the research in non-certainty, the more convincing becomes the

argument that the risk and uncertainty research has accepted the modernist precepts. For example, consider the 1971 book by two agriculture economists, Albert Halter and Gerald Dean's Decisions Under Uncertainty (with Research Applications). Their book is intended for ag-econ research, which makes it particularly appropriate to the question at hand. The authors write that "The purpose of modern decision theory is to provide a systematic approach to decision making under conditions of imperfect knowledge" (p. 1). So far so good, they are seeking something akin to Friedman's filing system. But the crucial question is whether or not they are going to demand that the theory be formulated in falsifiable manner. The authors are elusive on this issue, they are quick to point out that their's is a book meant for application of the concepts of decision theory rather than the testing of the theory per se. However, in the concluding chapter of their book, the authors argue that it is indeed impossible, however difficult the task of prediction may be, to evade the edicts of modernism. They write that "the social scientist must be a more careful observer than his counterpart in the physical sciences, and that he must be more critical of his data. [But] this does not mean that hypotheses can not be tested in the social sciences; it just means the analyst must try harder to refute his hypotheses" (p. 239). We can safely assume that these words were intended to apply to the narrow case of non-certainty research.

A still more recent example, John D. Hey's Uncertainty in Microeconomics (1979), adds weight to the notion that uncertainty

theorists have embraced the methodology of modernism. In his introduction, Hey writes "As is now common-place in economics, we will follow the axiomatic approach; that is, we start with a set of axioms, which appear attractive in the light of our intuitive notions of 'rational behavior'. On the foundations of these axioms we construct our theory, a theory that will enable us to characterize the behavior of any individual who obeys the axioms, and more importantly, a theory that will enable us to predict how that individual will act in new situations" (p. 26).

A final example is the 1979 Proceedings Issue of the AJAE. In his opening address, President Richard King exhorts his colleagues to continue working on "establishing explicit, refutable, hypotheses" (p. 840). Later in the issue, as part of a general session on risk management and risk preferences, four different articles, all intended to summarize and comment on the state of the art in agricultural non-certainty research, argue implicitly for the necessity of modeling in order to predict. The word "implicitly" is used above because none of the four discussants (Young, Mapp et al., Bessler, Miller, and Sonka) ever overtly state that prediction is the purpose of the research they are discussing, but the fact is obvious from the methods they advocate. The authors present a number of conventions from which to choose: 1) Gaming approaches in the Von Neumann and Morganstern vein are used to estimate producer risk preferences. 2) Given some assumed risk preferences of producers, programming models are developed to estimate optimal farm plans under conditions of risk 3) One of the most advanced techniques currently in use applies stochastic dominance criteria to

empirical data in order to isolate risk efficient farm plans. 4)
Finally, questionnaires are often sent out to representative farmers in order to get a "feel" for how much variability the producers are experiencing.

For those with a background in statistics and econometrics, these conventions are a heartening invitation to research. All are quantitative in one form or another. All seek predictive results of some kind. All can be swept together under the methodological carpet of modernism.

Finally, perhaps the best way to illustrate the place that modernism occupies in the research in agricultural non-certainty is to focus for a moment on what the mainstream has excluded, rather than what they have included, as acceptable method. One particular author, G.L.S. Shackle has spent his entire professional life writing books and journal articles about the existence and impact of uncertainty on economic actors. At first, it is most surprising that he is never quoted in the non-certainty research in ag-econ. The present writer has only managed to find one citation in the Ag journals for Professor Shackle (See Boussard, 1967). This is most surprising considering the fact that this man has written books with titles like Uncertainty in Economics, Expectation in Economics, Decision, Order, and Time in Human Affairs, Imagination, Formalism and Choice, among others. He is like his modernist colleagues in his sympathetic understanding of the impact that uncertainty has on human behavior. Just as much as they, Shackle sees it as critically important that economic theory incorporate uncertain

prospects into it's theoretical framework. Why then, this ostracism? The only possible reason is that Shackle has steadfastly refused to accept one crucial element that dominates the agricultural non-certainty research, prediction and "falsificationism," i.e., modernism. In his Expectation in Economics, Shackle writes the words that will forever separate him and his work from the on-going research in agricultural non-certainty. Shackle's contempt for the idea of the possibility and desirability of predicting human action is nothing less than heresy to modernists. He writes

"Complete prediction would require the predictor to know in complete detail at the moment of making his prediction, first, all 'future' advances of knowledge and inventions, and, secondly, all 'future' decisions. To know in advance what an invention will consist of is evidently to make that invention in advance" (p. 103-104).

"Predictability of the world's future history implies predictability of decisions, and this is either a contradiction in terms or an abolition of the concept of decision except in a perfectly empty sense...Predicted man is less than human, predicting man is more than human" (p. 104).

Of course it is possible that the ag-economists have simply overlooked Shackle and his work. It is really impossible to tell from their silence whether this is due to his anti-modernist methodology or for some other reason. However, Blaug in his book on methodology does briefly mention Shackle just long enough to "repudiate such anti-[modernist] conclusions" (1980, p. 185). It is with a fair degree of confidence then that we can assume that the silence on Shackle is just another indication of the acceptance of modernism in the non-certainty research camp.

Shackle himself will be dealt with in greater detail later on. The point of this section was only to justify the use of modernism to evaluate the progress of the research in agricultural uncertainty. By showing that modernism is indeed the "official rhetoric" of economics in general and non-certainty work in particular, the groundwork has been laid for a more complete treatment of modernism and what this means for the content of economic conversation.

2.3 THE ORIGIN OF ECONOMIC METHOD

This section examines the origin of statements on methodology. As such, we might consider this part of the paper to be somewhat of a digression from the primary theme. However, a consideration of the origin of methodological thought need not mire itself in the long history of methodological questions. Instead, the question addressed here is short and simple: Why have economists deemed an understanding of methodology to be an essential prerequisite to economic theorizing?

Again and again, we find that books on methodology (and, as we saw in the previous section, introductory textbooks in economics) seem to begin with apologetic arguments about the worth of methodological discussion. And this is understandable; scientists are much more comfortable working as discoverers of scientific knowledge than just talking about scientific "discovery." But the detractors of methodology have their point, as Paul Feyerabend put it, methodology is "one of those bastard subjects... which have not a single discovery to their

credit" (p. 302). Scientific methodology is, after all, a branch of philosophy and even the modernist founder, Karl Popper, felt he needed to apologize for it: "Apart perhaps from some Marxists, most professional philosophers seem to have lost touch with reality...Under these circumstances there is a need to apologize for being a philosopher..." (1972, p. 33).

Perhaps the first function of science is problem solving. And towards that end, methodology might be viewed as a handy aid for scientists to apply in their mission as problem solvers. Popper, with his usual eloquence, writes

"Our main concern in philosophy and in science should be the search for truth. Justification is not an aim; and brilliance and cleverness as such are boring. We should seek to see or discover the most urgent problems, and we should try to solve them by proposing true theories...or at any rate by proposing theories which come a little nearer to the truth than those of our predecessors" (1972, p. 44).

Consequently, if a clear idea of methodology could make us more productive in the recognition of problems, coming to terms with their nature, and in the conjecture of true (or at least, better) theories regarding those problems, then the time spent with methodology might be well worth the effort involved. F.S.C. Northrup, on the first page of his book on methodology, remarks on the cost of failing to respect the laws of scientific discovery.

"Again and again, investigators have plunged into a subject matter, sending out questionnaires, gathering a tremendous amount of data, even performing experiments, only to come out in the end wondering what it all proves, and realizing that after years of industry and effort that the real difficulty has slipped through their fingers. Others noting the success

of a given scientific method in one field, have carried this method hastily and uncritically into their own, only to end later on in similiar disillusionment. All such experiences are a sign that the initiation of an inquiry has been passed over too hastily, without any appreciation of its importance or its difficulty" (p. 1).

Surely, the above seems to be a sufficiently emphatic and persuasive exhortation on behalf of methodology. But there is more. In the case of economics, there are other forces at work which, owing to the peculiarities involved in discussions of human action, have prompted frequent methodological discussions from economists.

The first and most powerful of these forces is the relationship between economics and the natural sciences; even today, economists still argue about whether or not the study of human behavior should be treated as qualitatively distinct from the study of physical objects and their properties. As we know, the natural sciences gain in knowledge by the controlled experimental testing of theories. Also, economists are well aware of the difficulties associated with experimentation in the social sciences. The consensus among economists today, which seems to have emerged from Friedman's Essays in Positive Economics is that "the inability to conduct so-called 'controlled experiments' does not... reflect a basic difference between the social and physical sciences" (1953, p. 10).

Therefore, it is not suprising that economists are somewhat defensive of their adoption of the methods of the natural sciences. The fact is, as George Shackle writes, that modern economics itself has evolved from the methods of the natural sciences.

"Economic theory for two-hundred years modelled itself increasingly on the science of the inanimate creation; upon celestial mechanics for its large-scale conception and upon the isolable, purifiable experiment for the small-scale. The end-product was the neo-classical conception of the general equilibrium, the economic system fully adjusted to an underlying body of complete relevant knowledge. Such a method and its models have given us sharp and brilliant tools of illuminations, lightning flashes in which the scene is stilled to immobility by the brevity of the glimpse" (1972, p. 4).

But this union between the study of man and the study of nature has not been a completely harmonious one. There are radical differences in the subject matter in the two instances. Hence, the origin of economic methodology is first of all an effort to encourage economists to continue to imitate the methods of the natural sciences. Friedman writes that "no experiment can be completely controlled [this is in reference to the problems with experimentation that frequently occur in the natural sciences.] Evidence cast up by experience is abundant and frequently as conclusive as that from contrived experiments; the inability to conduct experiments is not a fundamental obstacle to testing hypothesis." Friedman exhorts his colleagues to continue the struggle against the problems associated with data from the real world; it is "difficult to interpret. It is frequently complex and always indirect and incomplete. Its collection is often arduous, and its interpretation requires subtle analysis and involved chains of reasoning, which seldom carry real conviction" (1953, p. 10). What this means is that it is precisely the difficulties associated with interpreting the phenomena associated with human behavior from the light of the

natural sciences which has begotten methodological discussions in economics.

However, there is another reason why the binding strictures of the natural sciences have been applied to economics and that is because of the existence of what we might call "truck-driver" economics. Of course, by this, no offense is meant to over-the-road personnel; we simply seek to point out that misconceptions abound in economics because, as Friedman put it "The subject matter of economics is regarded by almost everyone as vitally important to himself and within the range of his own experience and competence; it is the source of continuous and extensive controversy and the occasion for frequent legislation" (1953, p. 3).

In addition, as Neville Keynes said, "A not unnatural consequence is that people think themselves competent to reason about economic problems, however complex, without any such preparatory scientific training that would universally be considered in other departments of enquiry" (p. 7). Thus, given the complexity and the material importance of economic events, we have a propensity for what we have called truck-driver economics.

Perhaps the avoidance of truck-driver economics has been an even more powerful impetus to the growth of economic methodology than Popper's rather plutonic sounding desire for truth, which was mentioned above. In a society founded on democratic capitalism, the crucial policy issues cry out for careful, objective treatment. The fact that we have discussions of methodology now is testament to the fact the problems of society, and the difficulties associated with those problems, require

careful methodological treatment. Because of this, we expect economists to present society with a more balanced analysis of social problems relating to scarcity. What we have then is a mandate for a "positive economics" which as J.N. Keynes defined it, is a search after "what is," not what ought to be. Keynes' Cambridge colleague, Alfred Marshall, describes what this "positive" economist ought to do.

"The economist should study mental states rather through their manifestations than in themselves; and if he finds they afford evenly balanced incentives to action, he treats the *prima facie* as for his purposes equal. He follows indeed in a more patient and thoughtful way, and with greater precautions, what everybody is always doing everyday in ordinary life. He does not attempt to weigh the real value of the higher affectations of our nature against those of our lower: he does not balance the love for virtue against the desire for agreeable food. He estimates the incentive to action by their effect just in the same way as people do in common life. He follows the course of ordinary conversation, differing from it only in making clear the limits of his knowledge as he goes" (p. 16).

Marshall's conception of the economist is the ideal. But recall from Friedman that this task is made immensely difficult by the nature of the subject matter. It is the economist's search for guidance in this endeavor that has lead him to the door of the philosophers of science. For the last thirty years, economists have stood at the feet of the great philosophers of science, most notably Karl Popper and Imre Lakatos, and have been tutored in the ways and means of scientific conversation. In the last page of his book on methodology, Blaug reveals just what it is that the economists are looking for.

"What methodology can do is to provide criteria for the acceptance and rejection of research programs, setting

standards that will help us to discriminate between wheat and chaff. The ultimate question we can and indeed must pose about any research program is the one made familiar by Popper: what events, if they materialized, would lead us to reject that program. A program that cannot meet that question has fallen short of the highest standard that scientific knowledge can attain" (1980, p. 264).

Therefore, we can conclude that the origin of scientific methodology is really two-fold: on one hand it is the desire of the social scientist to push his discipline to the rigour (and prestige) of the physical sciences. On the other, methodology is a tool that is used to combat the peculiar tendencies of people to distort the truth in matters of wealth and scarcity. As we saw in the previous section, economists are admiring students of the thought of the philosophers of science to such a degree that today there is a fear that to abandon Popper's methodology is tantamount to abandoning economics to the demagogues and merchants who would like to use the weight of the discipline to their own advantage. McCloskey writes,

"If we abandon the notion that econometrics is by itself a method of science in economics, if we admit that our arguments require comparative standards, if we agree that personal knowledge of various sorts plays a part in economic knowledge, if we look at economic argument with a literary eye, will we not be abandoning science to its enemies: Will not scientific questions come to be decided by politics or whim: Is the routine of Scientific Method not a wall against irrational and authoritarian threats to inquiry? Are not the barbarians at the gates?" (p. 509).

It is the fear that people just can't be disinterested with regard to economic questions that seems to cause this great allegiance to method. This paper is an investigation into the rhetoric of non-certainty research. Yet the first commandment of

rhetorical discussion is that the discussants be reasonable people. Could it be that the reason why we hear so little talk of the "rhetoric of economics" be that we simply don't trust each other? If this is the situation, then there is little hope for intelligent conversation and there is the very real possibility for what one philosopher of science, Imre Lakatos, termed elitism. "When once the conception of objective truth is abandoned, it is clear that the question of 'what we shall believe' is one to be settled by the appeal of force and the arbitration of big battalions" (p. 119).

Clearly then, the stakes associated with methodology are high. If the origin of methodology is indeed fear (which makes the "sword of methodology" metaphor used at the beginning of this chapter seem particularly apt now), and if that fear is justified, then we ought to be mercilessly aware of the degree of predictive power contained in scientific theories. If methodology is all there is to keep the barbarians out, then we should embrace that sword with a vengeance. But this is getting ahead of the story. Modernism has not yet been given more than just the briefest mention. In the next chapter, this will be remedied by detailing the roots and implications of modernism first from the realm of pure science and then from the specific area of economics.

CHAPTER III.
PHILOSOPHICAL FOUNDATIONS OF MODERNISM

3.1 INTRODUCTION.

Thus far, it has been said repeatedly that the core of the doctrine of falsification (modernism) is that scientific theories must, if they are at all to approach the truth, make predictions that can be objectively tested against the phenomena that they seek to explain. The overriding question of this chapter then is "Why?" Why is it necessary that scientists build theories that make predictions? Secondly, despite the fact that modernism has been shown to be the "official" rhetoric of economics, do alternative methodologies exist that offer equal opportunity for truth-seeking scientists? Or, just what has modernism got that makes it such a preeminent force in methodological thought?

And if there is to be one man whose name and thoughts will dominate this chapter, that man is Karl Popper. His 1934 book The Logic of Scientific Discovery marks a major watershed in the philosophy of science. The mere labelling of his doctrine of falsification with the general term "modernism" is testament enough to the force and impact of this man's ideas on the philosophers and practitioners of scientific conversation.

Since 1934, as one might expect, a legion of disciples have arisen from Popper's shadow and the result has been that his

doctrine of falsification has become a bit blurred around the edges. The result is that among adherents to the principles of falsification (modernism), there is no generic brand that is universally accepted. This presents a problem as it is not within the scope of this paper to detail the subtle philosophical differences between theorists. Since the purpose of this chapter is to present and discuss the methodology of modernism in reasonably exhaustive fashion, it is, in the interest of clarity, necessary that the following discussion limit itself to the broad, widely held convictions of the adherents. This implies some problems with semantics since most philosophers seem to have a penchant for coining their own words. Nonetheless, in the following we will use the terms modernism, positivism, demarcationism, falsificationism, and logical positivism interchangeably.

Also, it is necessary to recognize at the outset that modernism, owing to the philosophy of Lakatos, is usually construed as more than just a "how-to" cookbook intended to aid the scientist in his search for insight. Modernism is also used as a measuring rod to evaluate the truth content (verisimilitude) of theories proposed in the past. Since this paper neither presents nor tests any novel conjectures, our interest in modernism stems from its potential use as a measuring rod to evaluate the verisimilitude of the research in agricultural non-certainty. However, the presentation of modernism in this chapter focuses primarily on the methodology's "how-to" function rather than its alternative use as an indicator of scientific progress.

Also, despite its widespread acceptance today, modernism is

not a methodology without a rival. From John Stuart Mill to the Austrian economists of today, an alternative methodology called verificationism has repeatedly challenged the precepts of modernism. Hopefully, our effort to come to know the essence of "the official rhetoric of economics" can be assisted by a comparison with the methodology that modernism appears to have replaced.

With these disclaimers in mind, this chapter will proceed to present and discuss in reasonably exhaustive fashion the philosophy of science known as modernism. The natural starting point for any discussion of scientific methodology is the Humean Problem of Induction (so labelled by Popper) which has historically served as the starting point for the philosophy of science. This is because different methodological philosophies usually part company with Hume's problem of induction. Next, it is also necessary to consider the metaphysical theory of objective knowledge which Popper suggests complements (rather than indubitably supports) his idea of falsification. This is included in the discussion because there are important implications of this "objective knowledge" for the study of behavior under uncertainty. Finally, the chapter ends with an examination of the ways in which economics has adopted to ideas of the philosophers of science.

3.2 TWO SOLUTIONS TO THE HUMEAN PROBLEM OF INDUCTION

If there is a common scheme that runs through the rival interpretations of scientific methodology, it is the general structure and components of logical argument. In fact, it used to be said that all truly scientific explanations have a common logical structure. From Carl Hempel and Peter Oppenheim, we divide that structure into the following three components. The first is the universal law. By this we mean some such proposition as "in all cases where A occurs, event B will occur." Accompanying the universal law is a statement of relevant boundary or initial conditions which constitute the explanans or, as it is sometimes termed, the premises. From the universal law and the explanans is deduced an explanandum. Consider as a quick example the universal law: "The sun rises every day." Given the explanans "today is a new day," then our explanandum, or prediction, would be that the sun would rise today.

Next, the scientific argument requires what Northrup has called an "epistemic correlation." The logical structure used above exists only in the mind. To make these formulations meaningful, they must be linked with some phenomena in the real world, e.g. "We saw the sun rise today." As Northrup describes it (p. 119, 121),

"...these relations are termed 'epistemic;' to distinguish them from other correlations in scientific or philosophical knowledge. the adjective 'epistemic' derives from the noun 'epistemology,' which refers to the science of knowledge. Thus an epistemic correlation joins a thing known in one way to what is in some sense the same thing known in a different way."

"The task of the deductive scientist ...is to begin with the

postulated entities and relations of his deductively formulated theory and to find directly inspected data with which certain of his postulated entities can be epistemically correlated, so that the existence of the latter entities can be put to an experimental test."

Experimentation is designed to shed light on the truth or falsity of universal statements. True universal statements are what the insight-seeking scientist is looking for. Popper's point is that the truth of universal statement A is not logically demonstrated by ascertaining through experimentation the truth of deductive consequence B. However, if the existence of B is experimentally denied, then it can be logically demonstrated that A could not be the case.

It sounds grand, but there are real problems that lie just below the skin of this lovely construction. People routinely believe that the sun will indeed rise tomorrow. "The sun has always risen in the past," they say, "So I have no doubt that it will rise tomorrow." The question is, are they justified in that belief? Philosophers have long realized the fallacy of reasoning from the particular to the general, i.e. Hume's Problem of Induction. In this case, the universal law infers from a finite number of sunrises to a greater number of days.

There have been numerous attempts to get around this problem and explain somehow just how people come to believe what they believe. These attempts at the solution to the problem of induction can most relevantly be looked at by considering the case of human behavior. The economist seeks to gain knowledge about human behavior by formulating theories. We recognize that no knowledge will be gained unless at some point in the chain of

reasoning, truth is inserted. In other words, where do we put the epistemic correlation? There are two possibilities. Either the relation with the real world is made with the universal statement (apriorism) or with the explanandum. Induction is the latter process whereby the epistemic correlation is placed at the end of the chain of reasoning. From this we infer inductively about the truth or falsity of the universal statement. For example, the question is which of the following is the proper way to insert truth into a theory: "All men are rational," which inserts truth in the universal statement. Or, "The price of wheat has fallen," which inserts truth at the end of the chain of reasoning.

In the following two sections, we consider the thought of two different philosophers who gave two conflicting answers to the problem of Induction. The first is John Stuart Mill and the second is Karl Popper. The former wrote on behalf of the method of verification, and the latter advocated falsification. The major point of contention between the two skeins of thought is over the proper place for the epistemic correlation to be inserted in scientific argument.

3.3 MILL'S METHODOLOGY OF VERIFICATION

Mill believed that the economist should attempt to insert epistemic correlations in both the universal law and in the explanandum. But he believed the inductive method to be far less powerful in truth-inserting power than the apriori approach. Given his preference, Mill would have never chosen to abandon the

inductive method as the proper approach to Political Economy. Mill was convinced that he himself had solved the Humean problem of induction by the pronouncement of the "ultimate major premise," a universal law of conservation. Since, he said, "there are such things in nature as parallel cases; that what happen once, will, under a sufficient degree of similiarity of circumstance, happen again" (1884, p. 223), Mill felt confident in reasoning from the particular to the general.

Also Mill was convinced that human behavior had all the necessary ingredients for scientific study. "It is the common notion," he wrote, "That the thoughts, feelings, and actions of sentient beings are not a subject of science, in the same strict sense in which this is true of objects of outward nature. This notion seems to involve a confusion of ideas..." (1884, p. 586). What Mill argues is that any facts are fit to be the subject of scientific inquiry provided that they follow one another according to constant laws. Even though the study of human behavior "is the most difficult subject of study on which the human mind can be engaged" (1884, p. 579), and even though the fundamental laws of human action have not yet been discovered, there is no reason to abandon the search.

Moreover, and this seems to be the lynchpin of Mill's argument, in order for these laws to exist, the same laws of causality which govern physical behavior must also determine human behavior. To the modern reader, this sounds like determinism, which it is. What Mill calls the doctrine of Philosophic Necessity is simply this:

"That given the motives which are present to an individual's mind, and given likewise the character and the disposition of the individual, the manner in which he will act might be unerringly inferred, that if we knew the person thoroughly, and knew all the inducements which are acting upon him, we could foretell his conduct with as much certainty as we can predict any physical event" (1884, p. 581-582).

However, for several reasons, Mill opted for a deductive approach over the inductive. First, "though he was a foremost advocate for employing the logical methods of the natural sciences in social inquiry, he was convinced that experimentation towards the establishment of general laws was not feasible in the social sciences" (Nagel, p. 454). In his Logic, Mill's two main methods of experimental inquiry, the Method of Agreement and the Method of Difference, required controlled experiments where one and only one factor could be varied in two instances of a particular phenomena. Of course, as Nagel has observed, this strictly controlled experimentation is rarely possible, even in the natural sciences (p. 456).

Secondly, Mill recognized that economics is not a self-sufficient science. By this we mean that an economic explanation is not possible in terms as exact as the natural sciences. This is because "we can not foresee the whole of the circumstances in which those individuals will be placed. But further, in any given combination of (present) circumstances, no assertion, which is both precise and universally true can be made with respect to the way that human beings think, feel, or act" (1884, p. 588). Hence, the difference between the natural sciences and "moral" sciences is one of degree not kind. Mill likens the study of human behavior to the study of the tides. No one doubts that

tides can be predicted with reasonable accuracy, but there are a multitude of minor causes (wind, temp. etc.) which conflict with the major causes of the sun and moon.

In his Political Economy, Mill thought that he had isolated the major causes of human behavior, but he was cognizant of the minor forces and so he framed his theory in the form of "tendency laws" whereby the general direction of change might be predicted. And the source of these tendency laws, for the reason cited above, could not be the inductive method of experimentation. Neither could it be abstract, "geometrical method" where only one cause could be identified at one time" (1884, p. 615). Mill was convinced, again, that all human behavior was governed by psychological and ethological laws and that these laws work through individuals and not groups.

What Mill settled on was what he called the concrete deductive method whereby the tendencies would be deduced from a priori laws of human nature. Positive predictions are therefore impossible in the case of human behavior owing to the immense complexity of life. A scientist would have to know absolutely everything about the a person's habits, personality, and expectations in order to be able to make predictions. Clearly, in the case of economics where so many different personalities are involved, such prediction is impossible. However, Mill didn't doubt his or anyone else's ability to know apriori the laws of human nature. What he did doubt was anyone's ability to predict because such prediction is grounded on some suppositions the sense of circumstances that exist. Hence the predictions are hypothetical and yet derived apriori. Therefore, no theory was

capable of falsification, all theories would be verified if only we could know the true circumstances under which the individual operated.

Let us look closely at the notion of apriorism for a moment. From the Encyclopedia of Philosophy, we learn that the distinction between apriori and a posteriori is an epistemological one, i.e., it has something to do with knowledge. Apriori means literally "from what is prior" and aposteriori means "from what is posterior." The ideas have their source in Aristotelian philosophy. "A is prior to B in knowledge if and only if we can not know B without knowing A...It follows that to know something from what is prior is to know what is, in some sense, its cause" (p. 140).

This is where the discussion of apriorism must begin. A priorism is an epistemological theory of knowledge that like Popper attempts to confront and defeat the problem of induction. Contrary to Popper who assumes that knowledge is never certain, there is one thing that is certain to the apriorist and that is the logical structure of the human mind. Ludwig Von Mises, a strong proponent of apriorism, has written "Human knowledge is conditioned by the structure of the human mind. If it chooses human action as the subject matter of its inquiries, it can not mean anything else than the categories of action which are proper to the human mind and are its projection into the world of becoming and change" (p. 36).

Hence, the essence of apriori reasoning is that it aims "at a knowledge unconditionally valid for all beings endowed with the

logical structure of the human mind. Its statements and propositions are not derived from experience. They are, like those of logic and mathematics, apriori. They are not subject to verification or falsification on the grounds of experience and facts" (Von Mises, p. 32).

It is important to note that the classics never doubted the essential truth of the intial premises. To them, "introspection was universally regarded in the past, whatever may be the fashion today, as an empirical technique of investigation, and sharply distinguished from intuition or innate ideas" (Jacob Viner, p. 328).

One can readily see how the two concepts of apriorism and a-posteriorism found ready application in the field of economics. It seems reasonable that only those creatures with an innate knowledge of what it is like to be human and who themselves have to choose, could gain insight from the tenets of economic theory. There is no need to find quotes from early and even contemporary writers regarding the importance that being human is to understanding economics. The apriorist sees the necessity for the economist to be truly a renaissance man. No Martian could ever be an economist because no Martian could know what human action is like intuitively. And without intuitive understanding, we are without knowledge of ultimate causes. The goal of the apriorist is insight into the workings of human interaction. Insight is a feeling, it is a thought, it is not modernist objectivity.

It is this insight into the ultimate causes which separates the social scientist from the natural scientist. The esteemed natural scientist may have controlled experiments, but they can

say nothing about such ultimate causes as force and cause. Thus, despite the complexity of human action, the apriorists felt that their science could say things which the physical sciences never could. Not suprisingly, the apriorist concludes that "What a huge advantage for the natural scientist if the organic and inorganic world clearly informed him of its laws, and why should [economists] ignore such assistance?" (Weiser, p. 132).

According to Mises, the central tenet of economics (and the title of his book) originates from the apriori notion that humans "act." From here he reasons that they wouldn't act at all if they were perfectly satisfied. They also wouldn't act unless they possessed some type of reason which would allow them to equate means to ends. Also, action implies choice and choice implies choosing among possible means to achieve given ends. From this all the textbook dogmas can be quickly deduced.

What the apriorist economist argued was that things in the economic world are not always as they seem. Prices, markets, wages, and the rest were not studied disembodied from innate knowledge. They knew, as Shackle has said, that the source of human action is thought. "It is therefore a branch or application of epistemics, the theory of thoughts. Economics is concerned with thought about things, both directly, when business men consider the intended uses of their resources, and indirectly, when they consider and conjecture each other's thoughts about what to do with the resources entrusted to them" (1972, preface). Since economics, to the apriorist, is a branch of epistemics, and since epistemics is a branch of metaphysics, the core proposi-

tions of economics are irrefutable. In other words, the apriorists inserts truth into the universal statements and then deduces particular consequences from that.

The specific method of the apriorist was the formulation of ideal types, e.g. Mill's economic man. In his On Definition of Political Economy, Mill created the famous "economic man" "which makes entire abstraction of every other human passion or motive; except those which may be regarded as perpetually antagonizing principles to the desire of wealth, namely, aversion to labor, and desire of the present enjoyment of costly indulgences" (1967, p. 321).

Surely, Mill's conception of economic man appears rudimentary to the today's economist after one hundred and fifty years worth of refinement has been added to the concept. Yet an economic man is just as abstract as Euclidian geometry unless there is that epistemic correlation with the real world. The classical writers certainly recognized this. That is why they buttressed their theories with what has become known as tendency laws. Mill recognized that man doesn't always behave with cold reason (especially in the face of uncertainty.) Therefore, he supported efforts to verify the theory. And if the deductive consequences of the theory failed to coincide with actual fact, Mill said that "the discrepancy between our anticipations and the actual fact is often the only circumstance which would have drawn our attention to some important disturbing cause which we had overlooked." (1967, p. 332). The economist errors, he argued, "when he makes the wrong kind of assertion; he predicted an actual result, when he should only have predicted a tendency to

that result --a power acting with a certain intensity in that direction" (p. 333).

Thus, given premises assumed to be true on the basis of introspection, the economist need only be concerned with determining "the limits of [the theory's] application" (J.N. Keynes, p. 17). Hence, we have verificationism. Verification is consider a "defensive methodology" because it is difficult to supplant theories with new ones when the economist is caught between determining whether the discrepancy between theory and the facts is due to "disturbing causes" or whether perhaps the theory itself is incorrect. Mill writes, "in all cases where the decutive method is used, it [the qualification ceteris paribus] is present more or less," we must not "suppose theories overthrown, because instances of their operation are not patent to observation" (1884, p. 218).

One wonders if Mill and the verificationists have pulled a fast one on us. By switching from the inductive to the deductive via apriorism, has he evaded the Humean problem of induction? apriorism is after all an epistemological theory of knowledge that is irrefutable. It posits itself as the one exception to the problem of induction. Yes, reasoning about the nature of the human animal when only a finite number of them have been observed is in violation of Hume's proscription against induction. But at least in economics, the apriori assumptions of rationality, choice, and action do seem, well, a priori sensible. Frank Knight wrote that "we surely know these propositions better than we know the truth of any statement about any concrete physical

fact or event, whether reported by someone else or made by ourselves on the basis of our own experience, and fully as certainly as we know the truth of any axiom about mathematics or logic" (1940, p. 165). Sensible or not, the apriori is strictly speaking irrefutable. That is probably the primary reason why it has been replaced in the last 30 years as the "official" rhetoric of economics.

3.4 POPPER'S SYSTEM OF FALSIFICATION

Like the apriorists, Popper begins his theory of falsification with a certain metaphysical theory of knowledge. But, as will be seen, Popper doesn't contend that his falsification stands or falls on the validity of the metaphysical concept he calls realism. "Realism [he writes] like anything else outside logic and finite arithmetic is not demonstrable; but while empirical scientific theories are refutable, realism is not even refutable...But it is arguable, and the weight of the arguments is overwhelmingly in its favour" (1972, p. 38). Popper's point is that the starting point in the search for justifiable knowledge is not decisively important (1972, p. 104). Unlike Descartes, we needn't go about doubting until we find the indubitable. Hence, acceptance of the metaphysical notion of realism is not a necessary prerequisite to justify Popper's empirical rule of falsificationism. Popper writes that "I find it comparatively unimportant whether anybody believes in my [philosophy]" (p. 25). We can therefore bypass realism until the next question and move straightaway into the gist of the matter.

And the gist of the matter is the "problem of induction." Solve it, and you will have vindicated reason as a meaningful tool of human understanding.

As was mentioned above, the problem of induction begins with questions about how people come to believe what they believe, i.e., it is a question of epistemology. Why do people expect the sun to rise tomorrow morning? The commonsense answer is that we believe that the sun will rise tomorrow because it has done so in the past. But what is the justification, the philosophers have wondered, for the belief that the future will be just like the past? Or to rephrase the question, what is the justification for reasoning from the particular to the general. When Hume raised this question, he believed that it consisted of two separate problems, one logical and one psychological. According to Popper, Hume's logical problem is "are we justified in reasoning from repeated instances of which we have experience to other instances [conclusions] of which we have no experience?" (1972, p. 4). Hume's answer was "No." No matter how many repetitions we observe of a phenomena, there is no justification for reasoning from the particular to the general.

Hume's psychological problem was, again according to Popper, "Why nevertheless, do all reasonable people expect, and believe, that instances of which they have experience will conform to those of which they have no experience?" (p. 4). Hume's answer was that, through repetition, we become conditioned to the facts of life and thereby believe them. The devastating consequence of these answers is that we can't tell the difference between beliefs that are justified and beliefs that are not. The problem

of induction is then that "our knowledge is unmasked as being not only of the nature of belief, but of rationally indefensible belief ---of an irrational faith" (1972, p. 5).

Popper's first step towards the solution of the problem is to reformulate the questions above into objective terms. He does this because "only objective knowledge is criticisable: subjective knowledge becomes criticizable only when it becomes objective. And it becomes objective when we say what we think; and even more so when we write it down, or print it" (p. 25). Hence, "beliefs" become "statements" and "knowledge" becomes "explanatory theories." The next step for Popper was to assert a principle of transference whereby "What is true in logic is true in psychology" (p. 6). This meant that if he could explain this main problem of induction, then this would also show that our understanding needn't be built on irrational faith.

Popper then reformulated the logical problem in induction into three separate generalizations: 1) Can the claim that a theory is true be justified by empirical evidence? Popper, agreeing with Hume, answers "No." 2) Can the claim that a theory is false be justified by empirical evidence? Here, Popper answers "Yes." This is the genesis of falsificationism. 3) "Can a preference, with respect to truth or falsity, for some competing universal theories over others ever be justified by such 'empirical reasons'" (p. 8). Again, Popper answers in the affirmative by saying that we should embrace those theories which have so far resisted attempts at falsification.

There are a few important implications to these answers.

First, since the answer to the first question is negative, we can never justify truth. Popper's philosophy of science sought to defeat the notion that deductive reasoning can transmit truth in two directions. Previous to Popper, the consensus among philosophers of science had been that the distinguishing feature of a scientific argument was its logical form. This means that all universal statements must be hypothetical guesses or conjectures. Of course, the consequences of this are devastating for apriorism. By reformulating the Humean problem of induction into objective terms, Popper completely cut off any connection between aprior knowledge and truth.

The second implication follows from the first. It is the principle of empiricism. "Only experience can help us to make up our minds about the truth or falsity of factual statements" (1972, p. 12). Popper doctrine seeks to demarcate science from all other fields of inquiry. He writes,

"...I still take it to be the first task of the logic of knowledge to put forward a concept of empirical science, in order to make linguistic usage, now somewhat uncertain, as definite as possible, and in order to draw a clear line of demarcation between science and metaphysical ideas...We may distinguish three requirements which our empirical theoretical system will have to satisfy. First, it must be synthetic, so that it may represent a non-contradictory, possible world. Secondly, it must satisfy the criterion of demarcation, i.e., it must not be metaphysical, but must represent a world of possible experience. Thirdly, it must be a system distinguished in some way from other such systems as the one which represents our world of experience" (1965, p. 38,39).

Third, the method that is suggested by the reformulated questions is deductive logic by which the theorist should endeavor to construct tests for his hypothetical universal

statements. However, Popper concludes that there is no particular scientific form that can produce scientific knowledge. The path to truth is not certain, all that can be accomplished, he suggests, is that falsity be avoided. Hence, we have "falsificationism." Thus, the demarcation problem has been made clear: "But how is the system that represents our world of experience to be distinguished? The answer is: by the fact that it has been submitted to tests, and has stood up to tests" (Popper, 1965 p. 39).

Hence, Popper calls for methodological "conventions" by which theory will seek insight by trying to predict and that the scientist will test that theory by trying to falsify it. Mark Blaug in his book on methodology sums the methodological rules of the falsificationist. (The references given are from Popper.)

1. ...adopt such rules as will ensure the testability of scientific statements; which is to say, their falsifiability [1965, p. 49]
2. ...only such statements may be introduced in science as are inter-subjectively testable [1965, p. 56]
3. ...in the case of a threat to our system we will not save it by any kind of conventionalist strategem [1965 p. 82] [By this he means setting up a theory in such a way that it is very difficult to falsify.]
4. ...only those [auxiliary hypotheses] are acceptable whose introduction does not diminish the degree of falsifiability or testability of the system in question, but on the contrary, increases it.
5. Intersubjectively tested experiments are either to be accepted, or to be rejected in the light of counter-experiments. The bare appeal to logical derivations to be discovered in the future can be disregarded. [1965 p. 84]
6. We shall take it [a theory] as falsified only if we discover a reproducible effect which refutes the theory. In other words, we only accept the falsification if a

lower level empirical hypothesis which describes such an effect is proposed and corroborated [1965 p. 86].

7. ...those theories should be given preference which can be most severely tested [1965 p. 121].
8. ...any new system of hypotheses should yield, or explain the old corroborated regularities [1965 p. 273]
9. ...auxiliary hypotheses should be used as sparingly as possible [1965 p. 253]

These rules will be considered in detail as they relate to non-certainty theory in Chapter IV.

Popper recognizes the fact that once induction is recognized as non-existent, meaning that truth will never be ascertained with finality, this might discourage some from research. But, he argues, the theories which the researcher will be most interested in are those that have so far resisted efforts to falsify them. His interest stems primarily from the fact that some of them just might be true. In the case of competing theories (which explain the same thing), the doctrine of falsification urges the researcher to set up "crucial experiments" whereby the constructed theories might be put to the severest possible test that only one of the competitors could possibly survive.

As a theory passes successive tests of falsification, it becomes corroborated to a greater or lesser degree. All theories should at any time carry with them "a concise report evaluating the state of the critical discussion of a theory, with respect to the way that it solves its problems; its degree of testability, the severity of the tests it has undergone; and the way it has stood up to these tests...Corroboration is a report of past performance...it is essentially comparative:...one can only say

that the theory A has a higher (or lower) degree of corroboration than theory B...[the report] says nothing whatever about future performance or about the reliability of the theory" (p. 18). These reports are essential to determining whether or not a theory is being protected by immunizing strategems which minimize testability. Also, they suggest the necessity for the methodological rules presented above.

3.5 THE MODERNIST NOTION OF OBJECTIVITY

Recall Keynes' argument for a distinction between positive economics, normative economics and the art of economics. It should be immediately noted that all Keynes met by "positive" was that the positive economist should "stand neutral between competing social schemes. [Theory] furnishes information as to the probable consequences of given lines of action, but does not itself pass moral judgements, or pronounce what ought or what ought not to be" (p. 13). And this determination of "what is" is precisely what Popper's falsificationism promises to do.

The desire for a positive economics can be claimed to be satisfied by Popper's falsification doctrine only because Popper's metaphysical theory of realism mentioned at the beginning of this chapter provides for an explanation of the existence of objective knowledge. To the falsificationist, there are three distinct worlds of knowledge. Imre Lakatos, a philosopher whose ideas are used often to justify the positivist case, describes the theory as follows: (Note that Lakatos uses the term "demarcation" where we have used the term "falsifica-

tion.")

"...The 'first world' is the physical world; the 'second world' is the world of consciousness, of mental states and, in particular, of beliefs; the 'third world' is the Platonic world of objective spirit, the world of ideas. The three worlds interact, but each has considerable autonomy. The products of knowledge; propositions, theories, systems of theories, problems, problemshifts, research programmes live and grow in the 'third world'. The producers of knowledge live and work in the first and second worlds.

"...all demarcationists agree on some important points. They hold that the question of whether a theory is pseudoscientific or not is a question about the 'third world.' Hence, for demarcationists, a theory may be pseudo-scientific even though it is eminently 'plausible' and everybody believes in it and it may be scientifically valuable even if it is unbelievable and nobody believes in it. A theory may be of supreme scientific value even if no one understands it, let alone believes it. Thus, the cognitive value of a theory has nothing whatever to do with its psychological influence on people's minds. It matters not whether the theory lures them into intensive belief and vehement commitment, nor whether it induces the euphoric (second-world) mental states of the human mind, Belief, commitment, understanding are states of the human mind. They are inhabitants of the 'second world'. But the objective, scientific value of a theory is a 'third world' matter. It is independent of the human mind which creates or understands it" (p. 109).

To the falsificationist, it matters little where the hypotheses to be tested are to come from. In Lakatos's terminology, they must spring from the second world into the third. But as long as the theory survives tests of falsification, it doesn't matter if the theories were written on tablets at the foot of Mt. Sinai or whether the concepts they postulate are observable or not (1984, p. 31).

In contrast to the falsificationist, the apriorists discussed above also have their own theory of knowledge. Like the falsificationist, the verificationists have a particular epistemological theory to back up their approach. Also, like the

falsificationist, this theory is metaphysical in nature and is therefore open to verification or falsification. It is merely intended to throw light on the method of apriori. In a 1940 article entitled "What is Truth in Economics," Frank Knight explained the verificationist's theory of knowledge. Like Lakatos, the verificationist delimits knowledge into three different spheres. The first is knowledge of the external world, "including both the plain man's knowledge of everyday reality and the physical scientist's knowledge of his primary data of observation." The second world consists of "the truths of logic and mathematics." And the third world, the world where economic problems lie, is the knowledge of human conduct" (p. 155). The reader will immediately note that the verificationist has no world of "objective spirit" of the type spoken of by Lakatos regarding human behaviour.

The task of the philosopher of science is then to "demarcate" the realm of knowledge into that which is scientific and that which is not. Once this demarcation criterion has been established, all scientists worthy of the name should seek to formulate theories within these bounds. On the one hand, we have the apriorists who say that the science of human behavior must be built on certain fundamental principles. On the other hand, there is the falsificationist position which says that the assumptions of theory have no basis in fact apriori. The only way to approach truth is to build objectively falsifiable theories.

Fortunately, it is not necessary for us to choose from these competing methodological schemes. As the second chapter shows,

economics has chosen the Popperian route. And this is not surprising. The power and persuasive of Popper's argument can not be denied. How is it possible to exaggerate the fact that knowledge need not be built on irrational faith? Popper's conception of truth makes its realization endlessly elusive. Truth is the limit we hope to ceaselessly approach while we recognize we'll never get there. It is a concept much like infinity. Truth is a word that is used to define God. It makes good sense to postulate a limit to objective knowledge; the answers to the problem of induction given in the past are surely unsatisfactory. Mill's idea that one could predict future behaviour if one could only know the exact circumstances of the case implies universal determinism. The implications of that case are too horrible to even contemplate.

Popper has speculated that the reason why the problem of induction took so long to solve was because the thinkers were seeking to justify that which is inherently unjustifiable. The seeker after truth is left with a clear choice: either all knowledge is based on irrational faith (because induction is non-demonstrable) or, all knowledge is of a tentative sort which coincides to a greater or less degree to the truth. Only time and repeated efforts at falsification can show how close we have come to that which is the goal of all science: Knowledge. In the next chapter, we will use the rules derived here to evaluate the status of the research in agricultural non-certainty.

CHAPTER IV.

THE MODERNIST APPROACH TO ECONOMIC NON-CERTAINTY

4.1 WHAT IS A SCIENTIFIC RESEARCH PROGRAM?

Up to this point the discussion of scientific methodology has been a bit one-sided. The preceding two chapters focused attention on just one half of that philosophy of science which stands as the source of the accepted methodology of economics. But it is erroneous to perceive modernism as little more than a "how-to" model for researchers in economics. On the contrary, philosophers like Imre Lakatos have also emphasized potential of the modernist precepts as tools by which previous scientific work might be evaluated. As Blaug writes "For Lakatos, methodology as such does not provide scientists with a book of rules for solving scientific problems; it is concerned with logic of appraisal, a set of nonmechanical rules for appraising fully articulated theories" (p. 35). It is this potential of modernism as an historical evaluation apparatus that we seek to exploit here.

Lakatos's "set of non-mechanical rules" have been presented and discussed in the previous chapters. The task at hand is to fully "articulate" the modernist theory of economic non-certainty. In other words, now that the necessary groundwork has been laid, we can leave the philosophy of science behind (temporarily)

and focus on the modernist theory of risk and uncertainty.

The strategy to be used in evaluating the proliferation of theories that incorporate risk is to attempt to define what Lakatos has termed a "scientific research program" or SRP. This chapter seeks to outline the predominant characteristics of the non-certainty research program (NCRP).

But look first at the defining features of a scientific research program as defined by Lakatos. An SRP, in Blaug's words, is a "cluster of more or less interconnected theories" (1980, p. 36). Hence, it is not necessary to consider risk theories in a piecemeal fashion. We should identify what those lines of interconnection are. According to Lakatos, the SRP is built around a "hard core" which is treated as irrefutable by "the methodological decision of the protagonists" (1978, II, p. 50). The sacred cows of the program rest here in the hard core. For instance, an SRP's hard core might hold the methodological rule that whenever events of type A are to be studied, methodological procedure C (and never procedure D) will always be invoked. This is what Lakatos calls the positive and the negative heuristic. Thus, it is the hard core where all the do's and don'ts of the research program are stored.

Wrapped around this hard core is what Lakatos has termed the "protective belt." This belt contains, in Blaug's words, "the flexible part of the SRP, and it is here that the hard core is combined with auxiliary assumptions to form the specific specific testable theories with which the SRP earns its scientific reputation" (1980, p. 36).

While there is usually very little change over time in a

SRP's hard core, the protective belt is constantly changing as new tests and theorems are devised and executed. As should be clear from the previous chapters, the method by which SRP's are appraised is the modernist criteria. For a program to be "progressive," the adjustments in the auxiliary assumptions which have resulted from attempts at falsification must predict "some novel, hitherto unexpected fact" (Lakatos, I, p. 33). Conversely, a SRP is said to be "degenerating" when "ad hocery" prevails. Namely, if the auxiliary assumptions must be adjusted to fit the data, then the passage of time and the continued efforts at falsification are whittling away at the theory and it is "degenerating."

The central question then is to determine whether or not the NCRP is progressing or degenerating. But this question can't even be addressed until a proper understanding of the SRP is gained. Research programs don't operate alone and untouched in a void of theoretical thinking. There is first of all a subject matter, a raw material, a "phenomena" that the theory seeks to explain. In this case, the raw material happens to be so vast that it literally encompasses the full scope and breadth of economics. If economics is, as so many authors have insisted, a study of the causes and consequences of human choice, then the non-certainty research program must be built around a particular conception of the nature of choice. By investigating the modernist perception of choice, the essence of the NCRP hard core can be uncovered.

Secondly, an appreciation of a SRP's significance requires a

consideration of the competing SRP's which also seek to explain the same "phenomena." In a manner similiar to the way in which Newton's theory replaced Kepler's and was itself replaced by Einstein's, a Popperian score of the hits and misses of the NCRP (i.e., record of attempts at falsificaiton) is hopelessly relative unless there is a means of comparision with other programs which attempt to explain the same thing. Ideally, the competing SRP's would theorize about exactly the same bundle of subject matter. But there is often a problem, especially in the social sciences, with the comparision of research programs that are not fully commensurable. In Lakatos's scheme, according to Blaug, "A particular SRP is judged superior to another if it accounts for all the facts predicted by a rival SRP and, in addition, makes extra predictions as well, some of which are empirically confirmed" (1980, p. 37). However, it is often the case that an SRP will imply more than its rival in one way and less in another. In such instances, the evaluation of rival theories can become something like comparing apples and oranges. Moreover, the very existence in the first place of the non-certainty research program suggests that economists were, at the very least, uncomfortable with the theoretical limitations of the orthodox theory which is based on perfect certainty. But why the move to non-certainty based research? Had the certainty-based micro program, to use Lakatos's terminology, begun to "degenerate"? Was this because the assumption of perfect certainty was just too unpalatable for even the sturdiest of the modernists to stomach?

These are the questions that this chapter seeks to address.

But the point of the questions is to get to the hard core of the NCRP, albeit indirectly. As Arrow has argued, there are two basic sources for a theory of behavior under uncertainty. In the following two sections the analysis reaches for an understanding of the NCRP by 1) coming to grips with our "subjective sensation of lack of knowledge about the future" and 2) considering the non-intuitive sources of the NCRP by confronting "the existence of economic and other phenomena in human affairs which can only be explained on the assumption that the actors were adapting to a situation of uncertainty" (1959, p. 12).

4.2

WHAT IS CHOICE?

To be presented in this section is nothing less than an a priori conception of choice. By this is meant that the ideas discussed are assumed as intuitively knowable to human beings; objective substantiation is not required. With this philosophical stance, there is no need to worry about whether or not the concepts discussed are testable by predictive experimentation. Like the nineteenth century apriorists discussed in Chapter III, we shall for the moment assume that the essence of choice (which is to say the essence of all economic behavior) can be understood by looking within to the intuitive feelings that originate such behavior rather than looking outside of ourselves to the objective manifestations of human decision.

From a purely modernist posture, therefore, what follows should be considered as "non-scientific." Recall that the essence of falsificationism is the notion that all hypotheses, a

priori derived or otherwise, are conjectural. Strictly speaking now, the modernists should, by their own "official rhetoric," have absolutely no desire to build up a theory of behavior under conditions of risk unless the incumbent theory of decision under conditions of certainty has begun to degenerate.

While such degeneracy may indeed have been occurring, there are indications that there is more behind the origination of the theory of non-certainty than just a professional dissatisfaction with the predictive power of certainty theory. As Arrow was quoted above, a primary source for a theory of behavior under uncertainty is "the subjective sensation of lack of knowledge about the future." Also, consider the striking similarity in the way that the opening pages in NCRP literature typically begin. Almost always there is an appeal to our apriori(!) sensations of uncertainty. For example, John D. Hey in his Uncertainty in Microeconomics opens his book with a familiar exhortation about the pervasivity of uncertainty.

"Uncertainty is everywhere; it pervades every facet of life. Uncertainty affects everyone. From the cradle to the grave, we are all confronted by uncertainty; however hard we may try to avoid it, the problem of taking decisions in partial ignorance of their consequences remains ever-present" (p. 3).

But modernists should have no concern for such "second world" ideas! If uncertainty is indeed a subjective concept, then the modernist should deal with it only in its objective, "third world" manifestations. Apparently they realize this since such apriorist notions quickly vanish after the first few pages of the the typical NCRP book or journal article. Still, our intuitive

notion of choice appears to be the primary source of a theory of behavior under uncertainty. Yet it is odd that the causes of this "uncertainty" are hardly ever speculated upon.

Consider as another example the following quote from the opening pages of Halter and Dean's book on uncertainty, "Decision making is the central coordinating concept of any organization, whether it is a family farm business, a giant industrial complex, or a government agency" (p. 1). Certainly the authors are correct, but they never tell us why decisions are so important! Perhaps this is because modernists aren't supposed to speculate about things so metaphysical; they are supposed to make predictions. Yet these "speculations" are crucial to the task of uncovering the essence of choice. And uncovering the essence of choice is crucial to understanding the significance of the "hard core" of the NCRP.

If we are going to gain an initial understanding of the essence of choice, it is necessary to move outside the modernist camp to one writer mentioned earlier, G.L.S. Shackle. Shackle is often termed a "subjectivist" by the mainstream NCRP. What this means is that Shackle usually tries to finish up what the NCRP leaves off in the first few "subjectivist" pages of their literature. Long after the NCRP has turned to the business of making economic predictions, Shackle continues to think about the causes and the meaning of human choice. It is his scheme of thought that we shall exploit here. In what follows, Shackle and his thought will be quoted liberally. There are two reasons for this: 1) Most economists are unfamiliar with his writing, and 2)

The essence of choice is one topic that is so intuitive, so apriori, that in the interest of clarity of presentation, its description is best left in the hands of a man with the literary acumen of Shackle.

Shackle's 1969 book, Decision Order and Time in Human Affairs represents what he calls in the preface a "final attempt on my part to communicate my scheme of thought." Since 1949, Shackle has been the gadfly of the modernist economic method. His many books which elucidate and elaborate on his conception of decision testify that he is a man with a cause. His near anonymity among members of the NCRP research community shows that he is a man alone. Shackle's determination (his 1979 book Imagination and the Nature of Choice shows that the "final" book has not yet arrived) and persistence stem from what he sees as self-evident fundamentals of the human condition.

Shackle's first fundamental is apriorist reasoning. He writes "I seek to show that the essential nature of choice is discernable in men's most direct, inescapable and imperious intuitions" (1979, p. vii). Though Shackle doesn't advertise the fact that he is an apriorist, his conception of choice defies that possibility of a predictive approach to the truth. Again and again, he says that choice is an origin, a beginning, an uncaused cause. He writes,

"Decision means literally a cut; and this I take to be the most essential aspect of its meaning in our spontaneous, intuitive, everyday and almost universal usage, betraying our attitude to our life and the human condition and our apprehension of the essential nature of that life as a process of creation" (1969, p. 1).

His phrase "life as a process of creation" is sufficient testament to the fact that Shackle's economic man is a "playwright of history." The choice that he (and we) are interested in is one that "makes a difference." And as Shackle writes, choice of this type requires an explicit recognition of the nature of our existence.

"At the outset, in contemplating 'choice,' we have to make an election of policy. We can suppose men to be uninvolved in the architecture of their own history, save as enforced dwellers in it. If history was determined in every particular in its whole stretch of finite to infinite extent, at some source outside of that history, at some one-for-all creation, men's thoughts and acts are merely items amongst those particulars...Choice in this deterministic view can be nothing but the name of an illusion" (1979 p. 6).

Thus we have the first fundamental of choice, and that is that it must be real. To use Shackle's term, choice must be "non-illusory." Non-illusory decision requires a conception of history that is non-determinant.

Shackle's second fundamental of choice strikes at the heart of the problem of uncertainty. It recognizes the fact that the human animal lives and acts in the present, the "solitary moment." Confined by time to the present and facing a future that is not predetermined, man is inherently confronted by a state of "unknowledge" about the content of the future. In short, he faces uncertainty. But Shackle's point is that decision, as we intuitively perceive it, would not be "decision" at all if the future was foreknowable. He writes,

"Decision can only take place when several distinct and mutually exclusive acts appear to the individual to be available to him. If, for each available act, he sees one and only one outcome, and if also he assumes that an act necessar-

ily has an outcome, and if further he can order all the outcomes (one for each act) according to his greater or lesser desire for each, then we say that his choice amongst the available acts will not involve decision, but will by contrast be a mechanical and automatic selection of that act whose outcome he most desires" (1969 p. 4).

Hence, Shackle's second fundamental of choice is that it is "non-empty;" its content is created by the existence of uncertainty. Immediately, then we can see that the notion of decision under what we would call perfect certainty is not, in this scheme, decision at all. Choice is not choice without uncertainty.

Shackle's third fundamental of choice is centered around the notion that the uncertainty which the chooser faces is bounded. Yes, the future is not foreknowable, but that doesn't make it random:

"In a cosmos lacking order, that consistency of nature that we think of as cause and effect, a cosmos in which no act placed any constraint whatever on the character of the sequel, choice amongst acts would be pointless" (1969 p. 4).

If the universe is random then not only is choice "pointless," but it is powerless as well. Thus, the fact that we do perceive an order in the cosmos, a rationale for existence, and a possibility of purposeful living means that choice is "non-powerless."

When a chooser faces a decision under conditions of bounded uncertainty, he considers a set of mutually exclusive possible outcomes. But where do these mutually exclusive hypotheses come from? Are they inputted directly from the events of the past? No, Shackle argues that the source of the potential actions of the chooser is the imagination.

"Choice cannot be made among facts, they have been chosen, or have chosen themselves. Choice requires rivals. Choice is choice of a course of action able to be followed by a desired sequel in the evolution of history to come...Each [rival possibility] must aspire to the occupancy of one and the same stretch of time-to-come, yet all must coexist in the chooser's choosing thoughts, his present thoughts. Only thoughts, not facts can possess this double essential capacity. Choice is necessarily made amongst works of thought, of imagination. Choice is made among thoughts originated by the chooser" (1979 p. 12).

So it is the work of "inspiration" that creates the entities from which the chooser must select. These "imagined, deemed possible" rivals are what makes choice the creative act that it is. It is because the rivals need not be grounded in events of the past that genuine novelty and innovation become possible.

Therefore, it is only when choice is non-illusory, non-empty, and non-powerless that it can possibly take on the meaning that we intuitively associate with it. Choice under these conditions becomes the origin of human action:

"[Choice's] vital nature is commitment. Choice is a resolve, a moral and not merely an intellectual act. Choice erects a structure of intentions, any abandonment of which will be hurtful to the chooser in some degree. In the act of choice, the chooser in some degree stakes his own self-esteem" (1979 p. 15).

In all of this we recognize that what the chooser is looking for, the point of the effort of imagination and the strain of decision, is a good state of mind. It is exclusively through the process of choice that men can achieve such well-being.

In a very short space, we have attempted to bring out the essential nature of choice. Though one man's view only, Shackle's concepts have rarely been challenged on the level which they are presented here. Certainly no one doubts the existence

of genuine uncertainty; though perhaps too few economists have thought about the causes of this subjective sensation of unknowledge on which Shackle has built his conception of choice. For example, note how the following quotation from Hey's uncertainty book clashes in no way with Shackle's conception of choice.

"We are concerned with an individual who is confronted with a set of choices, one, and only one, of which he must eventually choose; the crucial feature of his choice problem being that he does not know, in advance of making the choice, what will be the actual outcome of any particular choice" (p. 38).

Armed with Shackle's analysis, we can fill in the holes of the Hey interpretation. We know the origin of the choices that the chooser faces --the imagination. We also know that the source of his state of unknowledge about future outcomes --the non-determinacy of the cosmos. And we know why the individual chooses --to achieve a good state of mind. It is doubtful Hey himself would quarrel with this interpretation. Thus, the general scheme of choice as non-illusory, non-empty, and non-powerless is not a component clinging exclusively to any particular research program's hard core. Later we shall see just where and why the MCRP parts company with Shackle; the point here has been to simply attempt to get a grip on the essence of choice. Shackle's scheme appears persuasive (and generic) enough to be accepted for the time being as a building block of non-certainty theory.

Finally, why was it important to "get a grip on the essence of choice?" Why come to terms with "our subjective sensation

of lack of knowledge about the future?" The answer is deceptively simple. Modernist or not, economists had trouble justifying to themselves and others the use of the classical notion that men have perfect foreknowledge about the future. Even if this assumption is applied for purely operational purposes (i.e., making predictions), the whole notion of perfect certainty just wasn't very persuasive. In Hey's words,

"Consider the original motivation for work in the economics of uncertainty --that the conventional certainty theory assumed too much. In particular, objections were voiced about the amount of information that the individual agent (in certainty theory) was assumed to have. For example, the theory of demand assumes that the agent knows the prices of all goods, and his tastes (both now, and in dynamic theory, in the future). All modern, liberated economists threw up their hands at this, and shouted 'No way; the informational requirements are too great!' and set to work with a will, producing the 'new' microeconomics" (p. 232).

We conclude, therefore, that despite the modernist notion of strictly conjectural hypotheses, one of the original sources of the modernist NCRP is a non-modernist ideal. A conclusion such as this was the aim when the introduction of this paper aspired to uncover the "real" rhetoric of non-certainty research. Later sections will seek to uncover more of the real rhetoric of the NCRP in a similar fashion.

4.3 NON-INTUITIVE SOURCES OF THE NCRP

Like the previous one, this section seeks to uncover the economist's originating impulse to build an economics of uncertainty. But the sources of the NCRP presented here, and there are four general categories to discuss, are unique in that they

can not be intuitively perceived. Like our apriori beliefs, the objective phenomena of the physical world which can be seen, heard, felt, and spoken about have also worked to convince economists that there was a need for a theory of uncertainty. Economists observed phenomena which could not be explained by traditional certainty-assuming theory. The first of these categories is the existence of rule-governed behavior that is non-optimizing. The second is the existence of economic profit in the pure sense. The third are the manifestations of a non-constant marginal utility of money function. And the fourth is the phenomena of liquidity.

Periodically, economists have recognized particular features of the economic landscape that simply could not exist in a world of perfect certainty. For the most part, these phenomena are easily recognized and may be summarily dealt with. For example, the existence of gambling and insurance provide facile, objective testimony to the existence of uncertainty.

But there are more subtle indicators of uncertainty in the real world. Without doubt, perfectly certain decision-makers would seek to optimize utility by equating marginal utility with marginal cost. But there is evidence which suggests that the decision-maker oftentimes will employ standard pricing rules which do not approach the $MC = MR$ equality. Robert Heiner, in a recently published article, argues that people's behavior settles itself into a behavioral regularity that is not even an approximation to the optimizing level. Consider the three examples Heiner gives. The first is the publishing history of books designed to show people how to win at blackjack in gambling

casinos. At first, the biggest selling books emphasized complex card counting techniques. From a statistical perspective, card counting is definitely the "optimizing" way to go. However, owing to the tremendous difficulty facing the player in executing the card counting procedures, these books have of late faded from the best-seller list. Presumably, the sizable economic profits netted by the casinos during this period indicate that card counting is to the average player a hard-working way to lose a lot of money. More recently, the most successful books teach a more rigidly structured method which, though mathematically inferior to card counting, is implemented because it is a strategy within the competence of the player.

Heiner's second example is the recent phenomena of the Rubic's Cube. There are over 43 trillion different initial combinations from which the unscrambling process may begin. Experts at solving the cube do not choose the process which minimizes the number of moves (even though the "cube races" are often timed). Rather, the rules established for solving the cube are largely independent of the initial scrambled position even though all the information needed to optimize is costless to observe (all you have to do is look at the cube).

Before proceeding to Heiner's final example, we should note that the essence of the examples given above is that the decision-maker consciously restricts the use of information that is available. In the case of the blackjack books, the decision maker chooses not to count even though such information would be useful. In the case of Rubic's cube, the decision-maker attempts

not to determine which one of the 43 trillion combinations his version of the cube is. This restriction of information that is readily available is the first clue that agents are not attempting to optimize. Heiner now ties this in with the work of Herbert Simon and the "satisficing" approach which Simon has developed over a number of years. Simon has found that decision-makers are repeatedly found to "systematically restrict the use and acquisition of information compared to what is potentially available" (Heiner, p. 564). Satisficing is a means used by the agent to achieve a desired state; it is not an attempt by the agent to optimize.

Perhaps a farm manager could be shown via a computer simulation of his farm that his current crop plan is non-optimal. He might reply, "Well, that's the way I've always done it." Even though farmers seem to be more afflicted by stochastic elements than others, their behavior in many ways is more structured as a response to this variability. Agriculture is the definitive example of environmental variability; the weather, the insects, the inelasticity of demand for food, etc. all create a bitterly hostile environment for the traditional, all-knowing economic man. But farmers aren't optimizers in that most of their behavior appears to be rule-governed. They plant about the same time every year. They plant in years where it would probably had been better if they hadn't even bothered.

Heiner's point is that the economic world is full of examples of demonstrable non-optimization. And the existence of uncertainty is presumed to be the reason why. Certainty theory predicts that agents will always use all of the available

information. Thus, the empirical phenomena which demonstrates that agents don't always use all the information at their disposal to optimize is our first objective source of a theory of uncertainty. Heiner concludes that

"The above examples suggest that allowing flexibility to react to information or to select actions will not necessarily improve performance if there is uncertainty about how to use that information or about when to select particular actions. Thus an agent's overall performance may actually be improved by restricting flexibility to use information or to choose particular actions" (p. 564).

Our second objective source of the theories of uncertainty is a somewhat theoretical one given by Frank Knight in his 1921 book Risk, Uncertainty, and Profit. Though Knight is most often mentioned for the distinction he drew between risk and uncertainty (a distinction we shall shortly return to), a more important argument is the one he makes for the relationship between uncertainty and profit. In a world of perfect certainty, each factor of production would know precisely what the value of its marginal product would be (in the case of stochastic phenomena, each factor would know the expected MVP). Thus, each factor would demand payment for the value of services rendered and consequently, there could be no profit. He writes

"In every [conceivable] case, the necessary and sufficient condition of a perfect, remainderless distribution of the product of industry among the agencies causally concerned in creating it, in addition to perfect competition itself, is that the change can be anticipated over the period of time to which producer's calculations relate. Where the results of the employment of resources can be foreseen, competition will force every user of any productive resource to pay all that he can afford to pay, which is its net specific contribution to the total product of industry" (p. 172).

Knight's theory is not without its detractors. But his example illustrates what might be called a second source of theories of uncertainty. Whereas Heiner's empirical phenomena centers around the difficulty faced by humans in solving complex problems, Knight's example is based on the lack of foresight discussed in the previous section.

A third non-intuitive source of theories of risk and uncertainty are the objective manifestations of risk preferences. To be sure, risk preferences are intuitively palatable. But our concern here is with the objective consequences of such preferences. Perhaps the oldest example of risk preferences is Bernoulli's 1738 memoir of the St. Petersburg Paradox. (Much of the following discussion is taken from Blaug's Economic Theory in Retrospect, 1978, p. 347,348). The nature of the paradox is this: A coin is tossed until heads appears, if heads appears on the first toss, A pays B \$1; if heads appears for the first time on the second toss, A pays B \$2; if heads appears on the third toss, A pays B \$4, and so on, always paying \$2ⁿ⁻¹ for each nth toss if a head appears. Now the question is: What fee should B be willing to play for the privilege of playing the game assuming that the coin tossing is "fair." A fair game is one in which the player is never asked to pay more than the total mathematical expectation of success, that is, the actuarial value of the gamble, at each stage of the game. The expected gain or loss of income from a "fair bet," therefore, always equals zero. The mathematical expectation of success is

on the first toss; (Prob. of heads)(\\$1) = (.5)(\\$1) = \$0.50
 on the second toss: (P of heads)(P of heads)(\\$2) = \$0.50

on the nth toss: (P of heads)ⁿ x \$2ⁿ⁻¹ = \$0.50

Since the probability of heads is .5 (or 2^{-1}), the mathematical expectation of success for each toss is always 50 cents. Since the total expectation E is the sum of the expectations at each stage of the game, $E = \$0.50 + \$0.50 + \$0.50\dots$. The sum of this infinite series is infinitely large and so B must pay A an infinite sum of money for the privilege of playing this "fair game." Since people are clearly not willing to pay an infinitely large stake for anything, much less for a gamble, the assumption that people act as if they were maximizing expected income produces a contradiction.

The important conclusion to be drawn from this presentation of the St. Petersburg Paradox is that there is no constant relationship between income and utility. Risk preferences exist which distort human action away from the maximizing level. This phenomena has long been recognized by economists. Adam Smith in his effort to explain relative wage differentials argues from casual analysis of lotteries and insurance to show that, according to Blaug, "people tend to overvalue uncertain gains and to undervalue uncertain losses, that is, he assumes as a matter of course that people are 'risk-lovers' (1978, p. 49).

As economic theory became increasingly codified into a comprehensive allocative system, it became necessary, in order to demonstrate the notion of "consumer sovereignty" (where what

is demanded the most in society is what gets produced) to assume that the relationship between utility and income was constant. Only through a constant MU of income could the price mechanism be demonstrated to faithfully transmit people's wants and desires to the production of commodities. Marshall in his Principles recognized that "a pound's worth of satisfaction to the ordinary poor man is much more than a pound's worth of satisfaction to the ordinary rich man" (p. 130). However, Marshall brushes this objection aside with the words

"On the whole however it happens that by far the greater number of events with which economics deals, affect in about equal proportion all the different classes of society; so if the money measures of happiness caused by two events are equal, then there is not in general any great difference between the amounts of happiness in the two cases" (p. 131).

To say that the marginal utility of money is constant is equivalent to saying that a person has no risk preferences. A risk-neutral person is one whose disutility from, say, a 10% reduction of income would equal the utility of a 10% increase in income. But as the St. Petersburg paradox illustrates, this is rarely the case; and there are other examples. Doll and Orazem list three important manifestations of risk non-neutral behavior in agriculture.

The first of these is diversification. According to Doll and Orazem. "diversification means growing two or more products in an attempt to avoid the yield and price uncertainty of a single product" (p. 252). Yet there is a cost to diversification; the decision-maker ordinarily forgoes a certain amount of profit in order to protect himself from down-side loss potential. That we

see diversification in agricultural is objective evidence of non-certainty. Arrow writes,

"In a world of certainty there would never by any reason to hold more than one kind of asset bringing in constant returns, in particular no more than one kind of financial asset. The obvious presence of diversification requires explanation in terms of any theory of uncertainty" (p. 18).

Secondly, farmers are observed taking out crop, hail and fire insurance against the possibility of large losses. Though the insurance premium is a fixed cost that is wasted if the disaster never materializes, those who take out insurance evidently prefer a certain small charge (comparatively) and no chance of a devastating loss to no charge and the small chance of a large loss.

Third, the existence of futures markets as hedging tools demonstrates again the risk preferences of farmers. Doll and Orazem write that "the farmer may remove all or part of the future uncertainty cloaking an enterprise by signing a contract with an outside party" (p. 253). But hedging has a cost; farmers wouldn't pay that cost unless non-neutral risk preferences existed.

Of the three general categories of objective manifestations of uncertainty presented thus far, rule governed behavior, profit, and risk preference, there is a fourth to add. Liquidity. John Maynard Keynes in his General Theory was the first economist to explain the existence of liquidity as a response to uncertainty. In fact, of his four motives to hold cash (income motive, business motive, precautionary motive, and the specula-

tive motive) the latter two are unexplainable under conditions of perfect certainty (p. 170).

We can generalize Keynes's thought to non-financial assets as well. Hart, as cited by Arrow, has argued that capital equipment designed to provide for an uncertain future tends to have operating costs that vary as little as possible over a wide range of output. Arrow comments that "This indeed is part of a general principle that under uncertainty the optimal policy is not, in general, the same as the best policy corresponding to [conditions] of perfect certainty" (p. 19). We might consider this phenomena to be a sort of "liquidity of cost" where the agent seeks to keep flexibility in production high.

The existence of inventories is perhaps a more obvious form of liquidity. No one would hold them if they knew for certain what the future demand would be.

But the Keynesian notion of liquidity has more dramatic implications than inventories and capital flexibility. In the Keynesian system, unemployment and depression are themselves manifestations of uncertainty. Given the volatility of investment, Keynes showed that unemployment equilibrium is possible. And to Keynes the volatility of investment would never occur in a world populated by Mill's perfectly certain economic man. By comparing Mill's "economic man" to the Ford Model T automobile, Shackle colorfully describes what Keynes' interpretation of uncertainty meant to the economy as a whole.

"Keynesian economics, the economics of unemployment and depression, found the Model T economic man to be quite useless. He had to be redesigned with a new high-power but very erratic and unreliable engine called expectation and a

new set of brakes called liquidity preference, and a petrol tank called income with a carburation system called consumption which had a very large leak called saving, which if too large, slowed the machine down until it could no longer carry its full load of unemployment, unless expectation could be tuned up to a very high marginal efficiency of capital" (1966, p. 124).

Thus what many have called the Keynesian revolution was a revolution in the first place because of his recognition of the existence of uncertainty. We shouldn't have to look beyond the employment and inflation statistics for our non-intuitive evidence which has prompted economists to build a theory of uncertainty.

The four manifestations of uncertainty discussed here are not autonomous nor is the list complete. This section merely sought to capture some of the major non-intuitive elements that helped to originate theories of uncertainty.

4.4 GENERAL STRUCTURE OF DECISION ANALYSIS

Now that the dual justifications for a theory of uncertainty has been considered, the next step is to discuss the general structure of the decision process. What follows are the general characteristics of any theory of decision --even decision under uncertainty. Thus, what is presented in this section does not constitute the "hard core" of the NCRP. This is the "generic" structure that few disagree with. This section is the final building block that needs to be set in place before proceeding to the "hard core" of the NCRP.

It should be said first of all that there is a particular type of business decision to be focused on here. Section 4-2

discussed decision in its most general form. The emphasis of decision theory is on what Margaret Wray has called "the entrepreneurial decision." We assume that, in her words,

"The entrepreneur has, typically, certain acquired skills which he uses to form judgements about the credibility of certain outcomes, when faced with the problem of choosing between alternative courses of action in the face of uncertainty. These skills result from the entrepreneur's business experience in the particular sphere of industry or trade in which his firm operates" (p. 120).

Wray contrasts the entrepreneurial decision with what she calls the "managerial" decision which are "those made as part of the routine of any well-established and efficiently run business" (p. 122). At its core, the entrepreneurial decision is unique. It is in Shackle's terminology, a non-divisible, non-serializable trial. By non-serializable, Shackle means "to exclude any act which...lacks all individual importance and is a mere anonymous item in a long series of trials all made under similar conditions" (1958, p. 35).

A possible example of a "serializable" decision is the case of the stock-broker on commission who spends his entire day "cold-calling" in search of business. The broker doesn't really care who he sells to; his only concern is that at the end of the month the bottom line will be satisfactorily profitable. Each individual call lacks uniqueness, it is thus a serializable trial. Now suppose that the broker knows that he on average sells stock to one out of every 35 people that he talks with and that on average he talks with seventy people everyday. A divisible decision would be one where the broker would be asked to predict how many

sales he would make in a day. Such a prediction would be calculable on the basis of objective probabilities; $[= (1/35)(70) = 2]$, hence we would call it a divisible decision.

This restriction of discussion to non-divisible, non-seriable decisions means that the only choices that interest us are those which are "creative, capable of injecting something essentially new into the stream of events" (Shackle, 1958, p. 108). A good example of this "entrepreneurial" decision is the "real life" one that faces the western Kansas farmer who at present is irrigating his crops. In recent years the farmer has had to pay more for less water as the Ogallala Aquifer, at least in this region, appears to be drying up. Soon it may no longer be feasible to irrigate and the farmer may have to switch to dryland wheat farming. But when, if ever, should such a switch be made? A major decision like this one will require all the expertise that the farmer can possibly muster. It will be an entrepreneurial decision at its most extreme. Decisions like these are what concerns the choice theorists. And the general structure of decision as it is presented below is intended to model such choices. According to Cohen and Cyert there are five elements of the basic decision problem (p. 290). The first of these is that the individual has a number of possible alternative actions. Let a^* be this vector of all the possible alternatives from which the decision-maker might choose.

The second element of the decision problem is to whittle down a^* to all those alternatives which are "the behavior alternatives that the agent actually considers" (p. 290). Let "a" be the vector of all the admissible alternative actions. Thus, referring

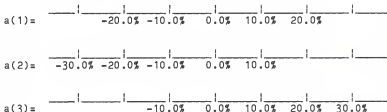
to the example above we might have:

- a(1) = continue to irrigate
- a(2) = stop irrigation and switch to dryland wheat
- a(3) = sell the farm

Third, there are a number of possible "states of nature" which might occur depending on what action is taken. Let the vector s represent all of the states of nature which the decision maker thinks to be possible. For example,

- s(1) = the water supply dries up
- s(2) = the water supply stabilizes
- s(3) = the aquifer literally overflows with plenty of cheap water for everybody

Fourth there is a payoff function which represents in dollars or utility the "range" of gain or loss which might result from each possible alternative action. For example,

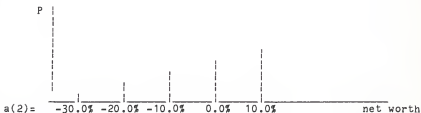
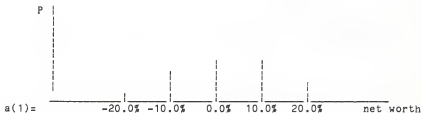


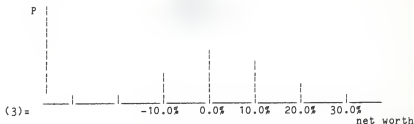
The chart above illustrates the of range of possible changes in the farmer's net worth in response to each possible alternative choice and each possible state of nature. If the farmer chooses to continue irrigating [choice a(1)] then the possible changes in his net worth (depending on what happens to the aquifer) will range from a loss of 20% to a gain of 20%. Thus, the consequen-

ces "c" of any action (in this case, the change in net worth) is a function of the action taken and the possible states of nature. Hence,

$$c = c(a,s)$$

Fifth, and most importantly, in order to make his decision, the farmer must attach some sort of weight to the possible states of nature. If "s" was a single value, then a situation of certainty would be the case. But since the farmer doesn't know precisely what will happen to the water level in the aquifer, he must attach some sort "possibility distribution" to each state of nature. These weights of the states of nature are then translated into the payoff function to determine the "possibility distribution" for each change in net worth. Perhaps the situation might look like the following.





The possibility distribution adds a vertical dimension to the range of possible outcomes. At this point, the specific properties and nature of the possibility distribution are purposely left vague. Of course, there is no reason why the possibility distribution needs to be discrete; it could just as easily be continuous. In any case, the essence of the decision problem is that for each action, the decision-maker faces some sort of distribution of outcomes that can be translated into dollar and cents (or utility) terms. It is from these distributions that he must eventually choose. This general structure of decision forms the basis of all theories of choice.

4.5 THE HARD CORE OF THE MODERNIST NCRP

The hard core of the modernist NCRP is built around, of course, the philosophy of Karl Popper and the falsificationists. Of overriding concern, therefore, in the formation of this hard core is that the system of decision theory be "operational." By operational, the modernists mean that theory must make unambiguous predictions that are capable of falsification.

Recall that the hard core of any social research program is

intended to serve as the backbone to the entire research program. The assumptions given in a hard core are sacred; they are never directly tested. The "protective belt" is where a research program undergoes specific application and testing. Hence, for our purposes, there is no need to be concerned with the experimental and empirical particulars of the program. The hard core contains the most general instructions (ala Friedman's filing system of human behavior) on how research should consider, model, and predict behavior under conditions of uncertainty.

There are essentially three elements which together compose the hard core of the modernist NCRP:

1. That people have risk preferences.
2. That these preferences are rational, measurable, and capable of predicting behavior under risk.
3. That decisions are made under conditions of risk rather than uncertainty.

Since most of the groundwork for these assumptions has been laid in previous sections, the following discussion of the three elements will be fairly brief.

1. Risk preferences. Traditional micro theory, with its assumption that the utility for money curve is linear, prohibits the possibility that people have risk preferences. In Section 3.4 the phenomena of the St. Petersburg Paradox was presented to demonstrate that given two risky choices, each with the same expected value, the decision-maker will often prefer one alternative to the other. Traditional micro theory, with its linear utility curve, would have predicted that a decision maker would

be indifferent to two choices that both carried the same expected value. By assuming that the slope of the utility for money curve is non-constant, the NCRP makes a radical break with neo-classical micro theory. In fact, if there is one element of this hard core that separates the NCRP from traditional micro theory, the postulated existence of risk preference is that element. And in the modernist vien, the NCRP contends that if people have risk preferences, then their behavior under conditions of uncertainty is impossible to predict, and hence theories impossible to falsify, unless those risk preferences are known.

2. Risk Preferences Made Modernist. In their 1944 book, Theory of Games and Economic Behavior, von Nuemann and Morganstern developed the necessary theoretical apparatus so that risk preferences might be elicited from simple gambling games in which the individual would be asked to choose among several risky prospects. With three simple and intuitively palatable axioms, von Nuemann and Morganstern made possible the prediction of behavior under conditions of risk. The axioms are as follows (from Anderson et al., p. 67-68):

a. Ordering and Transtivity. This axiom states that people are either indifferent between two risky prospects, or they prefer one or the other. Thus, if a person prefers risky prospect A to risky prospect B, and if B is preferred to risky prospect C, then A is preferrred to C.

b. Continuity. This axiom says that if a person prefers A to B to C, then a probability number P exists which would make him indifferent between B for certain, and a gamble yielding A with a probability of P and C with with a probability of (1-P). In

perhaps clearer form:

continuity implies
indifference
B <-- between --> P(A) + (1-P)(C)

c. Independence. According to the independence axiom, if a person prefers A to B, and X is any other risky prospect. Then in a lottery between A & X and B & X, the first lottery will be preferred provided that the $P(A) = P(B)$.

From these three axioms, what is called Bernouli's principle is deduced. If a person follows all of the above axioms, then a utility function exists which associates a single real number (a value of utility) with any risky prospect. From Bernouli's principle, the following properties exist. First, if risky action A is preferred to risky action B, then the utility from A is greater than the utility from B. Secondly, the utility gained from A is obtained by calculating the expected utility of A in an identical manner as calculating expected probability of A except that now all the possible outcomes of A are converted into utility terms. Third, the scale on which utility is defined is, like temperature, arbitrary. Thus, Anderson, et al. write,

"There is thus no absolute scale of utility and, tempting as they may be at times, comparisons of utility values between individuals are quite meaningless. Similiarly, it makes no sense to speak of one prospect yielding, e.g., twice as much utility as another prospect to a person. We can only say that one prospect exceeds the other in utility" (p. 68).

Thus, the utility function is said to be unique up to a positive linear transformation.

Assuming that a person follows the foregoing axioms, then a

utility function which describes the person's risk preferences can be derived in a variety of ways. The particular methods available need not detain us here; such empirical methods are considered to be part of the protective belt and hence are open to debate. What is not open for discussion is whether or not people actually do follow the axioms. Like the certainty theory which the NCRP seeks to replace, the assumption that men are capable of purely rational behavior under conditions of uncertainty is not challenged.

But reason alone is not enough to satisfy the modernist demand for theories with predictive power. In addition, it must be assumed that the individual is a maximizer. Thus, when calculating expected utility in a perfectly rational way, the NCRP assumes that the decision-maker seeks to maximize expected utility. Whereas the certainty theory assumes that under conditions of risk that the agent will select that action with the highest expected value, the NCRP postulates that he will take that action which carries with it the highest expected utility. Let us summarize this element of the hard core with a "repeat" of quote given earlier by John Hey in his Uncertainty in Microeconomics,

"...We start with a set of axioms, which appear attractive in the light of our intuitive notions of 'rational behavior.' On the foundation of these axioms we construct our theory, a theory that will enable us to characterize the behavior of any individual who obeys the axioms, and, more importantly, a theory that will then enable us to predict how that individual will act in new situations" (p. 26).

3. Risk=Uncertainty. This is the last and most important assumption of the modernist NCRP. In 1921, Frank Knight made the

distinction between risk as a quality that was "susceptible of measurement" and uncertainty which was inherently non-quantifiable (p. 19). In other words, risk prevails when probabilities can be calculated for possible outcomes and uncertainty dominates when they can not. For several reasons, however, the modernist NCRP assumes that Knight's distinction between risk and uncertainty is irrelevant.

First, since the primary business of the modernist is the making of predictions, the hard core of any modernist research program must contain an apparatus which, given the proper inputs, will generate unambiguous predictions. Though there are several extant strategies for decision under uncertainty but none of them has made much impact because, in Hey's words,

"...the decision procedure [under uncertainty] is not so simple. Indeed, there is no universally agree 'best' procedure (nor, because of the nature of the problem, is one ever likely to be found; perhaps this, more than any other reason, is why economists have steered clear of its analysis)" (Hey, p. 43).

After walking through the various decision strategies under conditions of uncertainty and showing how each could lead to different decisions in the same decision situation, Hey exclaims, "No wonder economists prefer the world of risk to that of uncertainty!" (p. 44).

On the other hand, decisions in the world of risk are relatively simple; all the decision-maker needs to do is to calculate the expected probability of the possible outcomes and transform these expected values into expected utilities via his risk preference function. Such an approach is eminently

rational and the result is always unambiguous. Given a situation of risk, the rational thing to do is to maximize expected utility.

Additionally, just as the axioms of the von Neumann-Morganstern utility theory made complete characterization of a person's utility function possible, the use of probabilities in decision making allow for great analytical dexterity in the characterization of possible outcomes. Most importantly, the use of the probability calculus requires that the sum of the probabilities attached to the outcomes of any one action be unity. With this axiom, means and variances of the different distributions can be compared and an optimal strategy may be selected. Probability is a logical concept that posits a particular relationship between a proposition and a body of evidence. With the power to calculate inter-action comparable expected values, the decision-maker ends up possessing essentially the same power that he had under the old theories of certainty, the power to maximize. And the modernist NCRP assumes that he does just that.

Given the necessity of a probability calculus, the modernist NCRP has adopted the stance that the probabilities formed by decision-makers are not objective; they are personal, subjective probabilities. The reason for this is two-fold: First, according to Anderson et al.,

"...Unfortunately, all the physical laws, properties, and interactions appropriate to defining the occurrence of the states of nature in real world decision problems can never be known. Thus [the concept of objective probability] must fall by the wayside in searching for an operational notion for decision analysis" (p. 4).

It would simply be impossible for the scientist to calculate all the probabilities for future events. It is much easier to let the agent do it instead. Besides, there are some nice ethical overtones which ensue when the decision-maker himself makes the probability judgements. For one thing, it keeps the decision in the hands of the decision-maker. Subjective probability is then defined as "a personal concept of probability. The degree of belief that an individual has about a proposition...is his subjective probability for it" (Anderson, et al., p. 18). In all, these writers conclude that "Subjective probability is the only valid concept for decision making, just as decision making is the only valid concept for probability" (p. 18).

As in the case of the utility functions derived above, methods have been developed to elicit subjective probability distributions from decision-makers. Often lottery type question techniques are employed that can be used to generate either continuous or discrete probability distributions.

This then is the hard core of the modernist non-certainty research program. Let us sum up the essence of the program by briefly considering the irrigation problem presented in the last section. Recall that the farmer needed to respond to the increasing cost and decreasing availability of water for his irrigation system. Via the modernist NCRP, the farmer would first calculate the subjective probabilities in terms of dollar outcomes for each of the three actions he is considering (wait, stop irrigating, or sell the farm). Then, with his utility function, the various dollar outcomes would be converted into

utility values. From these utility and the associated probabilities, the farmer would choose that action which would maximize his expected utility. Of course, there is no reason that the decision made will turn out to be the best. However, it will be a rational from the perspective of this research program.

Finally, if it strikes the reader as odd to picture the farmer laying awake at night trying to calculate the slope of his utility for money function, then the "spirit" of the modernist NCRP will have been misread. What the modernist method strives for is not descriptive accuracy, but predictive accuracy. Again, in Anderson's words,

"We do not wander through life with our minds packed with numbers that we have identified as degrees of belief or subjective probabilities, but we certainly make decisions as if such numbers exist. To make our analysis explicit, we must determine these numbers. Because they must be formulated or judged, subjective probabilities are also known as judgemental probabilities" (p. 18).

What it appears that Anderson is trying to say in the above quotation is that prudence, or judgement, plays a role in decision making. In the next chapter, we will consider just how prudent the NCRP is by the standards of the methodology which gave this research program birth, modernism.

CHAPTER V.

THE REAL RHETORIC BEHIND THE MODERNIST NON-CERTAINTY RESEARCH PROGRAM

5.1 A PRESENTATION OF "TEST" RESULTS

If this thesis were of the traditional quantitative sort, this chapter might be titled as a presentation of "test results." In an analogous way, this chapter represents a compilation of all the discussion that has preceded it. Namely, if we accept the idea that modernism is indeed the "official" "model" of scientific research, and if the basic tenets of modernism were accurately presented in Chapter Two, then this chapter gives the results of our mental "running" of the modernist model with the hard core of the NCRP used as input. A successful "run" would be one where the NCRP could be shown to be conscientiously adhering to the methodological rules of modernism. In such a case, the conclusion would be that the "official" rhetoric of the NCRP is the "real" rhetoric as well. However, if our "run" demonstrates that the NCRP is inherently incompatible with the modernist tenets, then we would be forced to conclude that modernism is, despite its reputation as the "official" rhetoric of economics, not the "real" rhetoric of the NCRP. In such a case, the search for the real rhetoric could begin. We might turn the idea of modernism on itself, rephrase the question, and ask, "Just how well do the tenets of modernism predict the content of the non-certainty

research program?"

But as in the natural sciences, any test or run requires a control group that can be compared with the experimental group. In this case, the control group is the standard neoclassical research program. The essential elements of the neoclassical hard-core, for our purposes, are 1) that people have perfect certainty about future prices, and 2) that people, because they are risk-neutral, maximize profit rather than expected utility.

What this chapter argues is that the "run" was not successful, that the NCRP has, in comparison with the certainty research program, failed the modernist methodological test. Specifically, in view of the methodological standards of modernism set forth earlier, there simply is no other way to grade the research in non-certainty than to classify it as a "degenerating" research program.

Ordinarily, such a claim would require a meticulous review of the results of the empirical testing which has been done within the "protective belt" of both of the programs. However, in this case, the need for such an exhaustive review disappears because the case to be made against the NCRP is a *prima facie* one. In other words, by the very content of its hard core, the NCRP violates the most essential of the modernist dogmas. Section 5.2 and 5.3 expand on this argument.

Given this, Section 5.4 argues that the "real" rhetoric of the NCRP is not modernist at all, but rather is a somewhat twisted version of Mill's verificationism. Recall that verificationism is built on *apriori* reasoning. Thus, it is possible (where under modernism it is not) to criticise the *apriori*

validity of the hard core of the NCRP. Section 4.4 is the vessel which contains this criticism.

5.2 A PRIMA FACIE CASE AGAINST THE MODERNIST NCRP

Translated directly from its Latin root, the phrase "prima facie" means "at first sight." By saying that a prima facie case is to be made against the NCRP, we mean that the ways in which the NCRP violates the codes of modernism are so obvious, so flagrant, that there is really little need to push our way into the protective belt of the program to see if the modernist tenets are upheld. We can tell that the tenets are ignored "at first sight."

The discussion in Chapter II mentioned Friedman's filing system of human behavior. Recall that that filing system is considered by the modernist method to be mere tautology unless epistemic correlations can be made with the real world to show that the filing categories are meaningful. To this end, the modernists have built up a set of methodological rules which were designed to maximize the corroboration between theory and the objective world. Thus, when the argument is made that the NCRP is degenerating, or, that it fails the modernist test, what we mean is that, in comparison with the certainty research program, the objective links between the NCRP and the real world are very weak. Let us recall those rules given in Chapter III which most apply to the hard core of a research program:

1. The theories which should be given preference are those which can be most severely tested.

2. The effects which falsify theories must be reproducible.
3. Any new system of hypotheses must yield the same old corroborated regularities.
4. Scientific statements must be inter-subjectively falsifiable.

The following discussion will take in order each of these rules and compare the performance of the modernist hard core with the hard core of the certainty research program. Let us recall now the hard core of the NCRP:

1. That people have risk preferences.
2. That these preferences are rational, measurable, and capable of predicting behavior under risk.
3. That decisions are made under conditions of risk rather than uncertainty.

First, which is more severely testable, that people have risk preferences or that they are risk-neutral? Concurrently, is it easier to falsify a theory based on incomplete knowledge or a theory based on perfect knowledge? Of course, the answer is that it is always easier to assume for testing purposes that people don't have risk preferences and that they are perfectly certain of the outcomes of the decisions that they make. Once we admit the possibility of risk preference, the need arises for an entire set of auxiliary hypotheses or premises that detail the way in which the preferences are to be elicited. And once uncertainty is postulated, a whole set of axioms must be assumed that describe a person's behavior under uncertainty. As Popper has remarked, what we are interested in is the empirical content of a theory. Those theories should be preferred which prohibit the

most from happening (1965, p. 121). What the necessary auxiliary assumptions of the NCRP do is to allow a wider range of corroborating outcomes than would be allowed by assuming that all people are risk-neutral and perfectly certain.

For instance, suppose a researcher sought to measure the impact that a new farm program would have on wheat farmers in Kansas. If the researcher assumes that people are both risk neutral and possessors of perfect information, then the behavior of the entire class of farmers can be predicted on the basis of objective information about farm conditions. On the other hand, risk aversion coefficients and subjective probability distributions must either be elicited or estimated in order to make falsifiable predictions within the NCRP. In the case where the predictions made under assumptions of risk are not verified, it is often unclear whether the theory itself is in error or whether one or more of the auxiliary hypotheses or premises are in error. It is a modernist rule that the theories which are most easily falsifiable are those with the smallest number of auxiliary hypotheses. Clearly, in comparison with certainty-assuming theory, the NCRP is plagued with more auxiliary hypotheses and is therefore less capable of severe testing.

Secondly, are the effects which falsify the theories of a research program reproducible? In the case of certainty research, the answer is 'yes', and in the case of the NCRP the answer is 'no'. Because of the fact that we are dealing with non-divisible, non-serialable decisions, non-certainty research assumes that economic decisions are unique, that the probability

distribution that happens once, will never occur in the same form again. Thus, only if a person could make the same decision under the same set of circumstances more than once could the falsifying effect be reproducible.

On the other hand, with certainty research, the falsifying effect is reproducible because there is no risk preference or probability distribution that is supposed to change as the situation changes. In all cases, the individual is just as certain about the outcome of a choice as he was about the outcome of a previous choice. Hence, certainty theory produces a reproducible falsifying effect because all that is required to falsify the theory repeatedly is repeated demonstrations that the individual is not acting under risk-neutral or with perfectly certain knowledge.

Third, does non-certainty research explain all the old corroborated regularities that have been established with certainty research? The most obvious of these corroborated regularities is the law of demand. Under conditions of perfect certainty (and with the auxiliary assumption that the substitution effect outweighs the income effect), standard micro theory predicts that the quantity demanded of a commodity will fall if the price increases.

But under conditions of uncertainty, it is easy to conceive of an example that doesn't imply this regularity. When uncertainty exists it is always possible that the prediction of future prices may be incorrect. And if the prediction of future prices is incorrect, it is possible that the decision-maker will error and end up purchasing more of a commodity even if the price has

increased.

Consider an example about hamburger and its price. Now hamburger is cheap enough so that we can safely assume that in the case of certainty theory that the substitution effect would outweigh the income effect. We could thus unambiguously predict under certainty theory that more hamburger would be purchased if the price were to fall. However, no such clear prediction is possible under uncertainty theory unless we know the probability distributions of all the economic actors for the future price of hamburger. It is conceivable that they would purchase less rather than more on the assumption that the price was going to continue to fall and that better bargains could be had by eating hot dogs now and waiting a little longer to buy some hamburger. The perfectly certain economic man is not plagued by such indecision nor is he prone to error. Thus, at least in this example, the law of demand is predicted with fewer restrictive assumptions (or, immunizing strategems) under certainty theory than under the NCRP.

Fourth and finally, the modernist methodological rules require that theories be inter-subjectively testable. It is this rule which, as long as the NCRP adheres to the notion of subjective probability and subjective risk preferences, makes the strongest prima facie case against the non-certainty research program. Recall that the essential core of Popper's system of falsification rests on objectivity, not subjectivity. In his Logic of Scientific Discovery, Popper's solution to the Humean Problem of Induction is wholly founded on his transference of the

problem from a subjective one to an objective one. He writes that "Now I hold that scientific theories are never fully justifiable or verifiable, but that they are nevertheless testable. I shall therefore say that the objectivity of scientific statements lies in the fact that they can be inter-subjectively-tested" (p. 44).

Let us make certain what Popper means by "inter-subjective" testability. Popper gives the example of the chemist who claims to have made a discovery in his own laboratory at home. If for some reason the chemist is the only one who can make the effect happen, then Popper argues that a scientific discovery has not been made. Only when another chemist produces the same effect can something objective (and hence scientific) be said about the effect because two subjects have at once witnessed it (1965, p. 45).

A more dramatic example is the one that Popper quotes from Winston Churchill's autobiography. Popper praises the following as "the philosophically soundest and most ingenious argument against subjectivist epistemology that I know" (1972, p. 43):

"...here is this great sun standing apparently on no better foundation than our physical senses. But happily there is a method, apart altogether from our physical senses, of testing the reality of the sun..[When astronomers predict an eclipse], we have got independent testimony to the reality of the sun. When my metaphysical friends tell me that the data on which the astronomers made their calculations were necessarily obtained originally through evidence of their senses, I say 'No.' They might, in theory at any rate, be obtained by automatic calculating machines set in motion by the light falling upon them without admixture of the human senses at any stage" (1947, p. 115).

Churchill's example is similar in many ways to the examples

given in such methodology texts as Friedman's "The Methodology of Positive Economics." In the same way that a calculating machine could conceivably predict an eclipse, so could a calculating machine verify that "the expert billiard player was calculating the angles of all of his shots" or that "the leaf positions itself in order to maximize the amount of sunlight it receives." Such calculation machines create the possibility of an inter-subjectively testable falsifying test. If the machine revealed that the leaf was not positioning itself in such a way that would maximize its reception of sunlight or if the machine revealed that the pool player was not making shots on the basis of the laws of geometry, then both theories would be inter-subjectively falsified.

Of course, Churchill's calculating machine is not necessary to meet the modernist requirement of inter-subjective testability. The point is that more than one of us should be able to observe the deductive consequences of a theory. But Churchill's calculating machine does make the problem rather clear-cut; for instance, could a calculating machine record the objective behavior of people under conditions of perfect certainty? Yes. Since certainty theory assumes that all actors are perfectly certain, there is no reason to treat choice as an individual matter. Our calculating machine could then record the phenomena of people engaging in voluntary exchange. In other words, the machine could observe the easily recognized characteristics of market activity, prices and quantities. Hence, the machine could inter-subjectively verify whether or not the economic laws of the market place were in fact laws at all.

Likewise, in the case of decision under objective probability, where the decision-maker is assumed to be risk neutral, the calculating machine could easily observe whether or not the agent obeyed the axioms of probability theory.

But, once risk preferences are allowed and subjective probability distributions admitted into the theory, the task of inter-subjective falsification becomes immensely more difficult. The real problem lies in the conversion of a subjective thing, degrees of belief about the likelihood of various possible outcomes, into an objective thing, probability distributions. At several points in this translation from the subjective to the objective, there is room for error.

Recall that the method used to derive the probability distributions is based on simple gambling games of chance. The risk preferences that are supposed to be elicited are the ones which apply to decisions that the agent makes in the real world. But since the gambling games are hypothetical, "There must always be some doubt as to how well a subject can succeed in answering hypothetical questions as if they were real-life actualities" (Dillon, p. 53). Also, perhaps some people have a psychic bias for certain probabilities. Or, how is it possible to separate the "love of gambling" from true risk preferences?

Such as these are some of the problems involved with the translation of the subjective to the objective. Hence, it is not surprising that writers have urged that,

"interviewers need to be sympathetic to a slow respondent or to one experiencing difficulty. A few helpful words to make a hypothetical situation more subjectively realistic are

often useful. However, an interviewer must take care not to intrude his own preference into the questioning" (Anderson et al., p. 69).

But there is a great problem which emerges when the interviewer needs to assist the subject in objectively forming his risk preferences and subjective probabilities. In a very real sense, the interviewer becomes the "co-creator" of the preferences and probabilities with the agent. It is thus entirely possible that two different interviewers will derive two different sets of probabilities and preferences from the same agent. Anderson writes "As for any subjective probability, different judgements will be made by different analysts. It is an empirical question whether such differences (which tend to be small) also result in different decisions" (p. 43).

But back to Churchill's calculation machine. Such a machine would be incapable of subjectively sensing the difficulty that a subject might be having in answering the questions. And even if it could be programmed to detect some of the distortions involved in the translation from the subjective to the objective, the personality of the man who originally programmed the machine would be indelibly stamped on the preferences and probabilities that were derived. What is required to meet the modernist criteria of inter-subjective testability is therefore that the same agent be quized by a number of different interviewers to insure that the results are consistent. However, even with multiple interviewers (assuming that the subject's patience holds out), there is the persistent danger of what Leamer has been termed "ad hocery." By ad hocery is meant the fitting of a

particular set of data (in this case, answers to questions) to a particular model. With a few questions and some probability paper, a subjective probability distribution can be estimated for just about anything. The point of the inter-subjective testability requirement is, as Popper points out, to test that distribution:

"I mean that observations, and even more so observation statements and statements of experimental results, are always interpretations of the facts observed; that they are interpretations in the light of theories. This is one of the main reasons why it is always deceptively easy to find verifications of a theory, and why we have to adopt a highly critical attitude toward our theories if we do not wish to argue in circles: the attitude of trying to refute them" (1972, p. 107, note).

Thus, the conclusion to this rather long discussion on testability is that, yes, it is possible to create an experimental design whereby a person's subjective probabilities and risk preferences might be observed in a manner which minimizes the distortion inherent in the translation process. The key, however, to inter-subjective testability is whether or not the predictions of behavior made by the calculation machine or by the army of interviewers actually are verified by the subject's behavior. However, there is no doubt that certainty theory is far more easily testable than the theory which assumes that people have risk preferences and that they construct probability distributions to aid in their decision process.

So, in sum, how does the empirical content of the NCRP stack up against the content of the certainty research program? What this section endeavored to show was that on four crucial counts 1) severity of possible tests, 2) reproducibility of falsifying

cases, 3) explanation of the old corroborated regularities, and 4) susceptibility to inter-subjective testing, the certainty research program clearly dominated the NCRP in terms of empirical content. But what this section presented was merely a prima facie case against the NCRP. If it would turn out that, despite its limitations, the NCRP had stood up better to tests of falsification than the certainty program, the conclusion reached here would need to be revised. Hence, what is now required is an epistemological review of the NCRP. Just how hard have the researchers in the NCRP worked to attempt to refute their theory? And how successful have they been? These two questions are, finally, the ones which will determine how much scientific knowledge is actually contained within the non-certainty research program.

5.3 THE EPISTEMOLOGICAL STATUS OF THE NCRP

The last section attempted to ascertain the empirical content of the NCRP by comparing it with the empirical content of the neo-classical research program. An understanding of the empirical content of a set of theories is important because it gives us a prima facie indication of the degree of falsifiability of a research program. Comparing the empirical content of two different theories is a lot like comparing the water-holding capacity of two different sized glasses. The analysis of the previous section concluded that the cup labeled certainty research is capable of holding more liquid than the cup labeled non-certainty research.

Now that the size of the cups has been defined, this section seeks to ascertain just how full they really are. And the liquid which modernists are interested in pouring in each cup is knowledge, scientific knowledge. Recall that what knowledge is supposed to serve as is a tentative representative of the truth. Truth is, after all, that golden ring towards which men constantly reach. Thus, we should seek to push our knowledge closer and closer to the truth. The modernists have given us methodological rules which provide the hueristic, or "how to" assistance that they claim will push our knowledge of economic behavior closer to the truth. If our theories possess at least some empirical content, and if they pass the crucial falsifying test, then those theories can be said to be corroborated. Thus, the theories which we build become vessels of knowledge.

The central question of this section might now be asked: Just how much scientific knowledge does the modernist NCRP contain? The only way to answer this question is to answer another question first: "Has the modernist NCRP been put to the falsifying test?" The answer, which will be defended throughout the course of this section, is "No." The modernist NCRP has not been put to the test; it is a vessel, but by the present nature of its construction, it is a cup incapable of holding knowledge.

Two facts about the NCRP lead us to this conclusion. The first is that the program has avoided the falsifying test by opting for a normative approach. Thus, where it might be possible to refute the theory if agents where to behave in a way that made it apparent that they were not maximizng expected

utility, the NCRP argues that whether or not the agent behaves in this way is immaterial, but the fact is that they "should" behave in this way. From their preface of their book Agricultural Decision Analysis, Anderson *et al.* write,

"The approach to risky choice that we follow is a conditionally normative and logical one...Given the decision maker's goal, the approach indicates which alternative he ought to take...[The steps of the decision theory] amount to no more than the processes followed by managers in making risky choices, processes that are usually attempted in intuitive fashion. However, many risky decisions are too complex and important to be handled satisfactorily by intuition. Decision analysis, by its formal procedure, enables a manager to better insure that his risky choices are in line with his preferences and beliefs and that full value is extracted from the information that is available to him" (p. ix, 12).

Why has the NCRP opted for the normative approach over the positive, falsifying one? There seem to be two answers to this question. First, the normative approach was chosen because the amount of knowledge that could be gained by testing a few people's response to risk is very small. Not much can be learned by studying an almost infinitesimal fraction of all the decisions that are made daily. If one person turns out to be a utility non-maximizer, then all we know about is that one person, and that particular decision.

It is important to recognize that certainty theory doesn't suffer from this same drawback. When people have identical risk preferences and are perfectly certain, there is no need to study the economy by studying one individual at a time. And since the assumptions of perfect certainty lead to many of our theories of the market, economists can learn a great deal by dealing with entire markets at once.

The second reason given for the normative approach is that

the axioms of von Neumann-Morgenstern utility theory (Ordering, Transitivity, and Independence) are so intuitive as to defy the need for testing. Again from Anderson, et al.

"...We emphasize the remarkable nature of the expected utility theorem. It says first that if a person accepts the perfectly reasonable axioms..., this necessarily implies the existence of both a utility function that reflects his preferences...and a subjective probability distribution. Second, it says that he should choose between risky prospects to maximize expected utility. If you accept the axioms, you must also logically accept the criterion of maximizing expected utility" (p. 69).

The most important implication that follows from this personal, normative approach is that economics is transformed from an objective science to a moral science. Our intuitive beliefs about whether or not people follow the expected utility axioms are just that, beliefs. Without a means to verify those beliefs, the potential for scientific knowledge vanishes. Suddenly economics becomes the study not of the actual, but the ideal. Subjective probability forces us to take a bold leap from the objective third world, to the subjective second world of beliefs and opinion. And scientific knowledge is decidedly not a part of this second world. Scientific knowledge can only exist in a world where these conjectures about future events may be criticized and tested. When the interviewers take to the streets to determine people's probability distributions, what they gather has no potential whatever for scientific knowledge unless the predictions generated from the von Neumann-Morgenstern axioms are allowed to be capable of falsification.

This fact leads a to a curious conclusion: The very method

which has given it birth, modernism, by its own standards, must condemn the NCRP on the grounds that it's current contribution to man's stock of objective knowledge is not likely to be great. The use of the normative approach on the grounds that the von Neumann-Morganstern axioms are a priori valid is about the least most critical way of examining the truth-content of a theory.

Finally, let us consider a possible objection that the reader might make to what has been presented here. One might argue that any appraisal of the epistemic status of a group of theories is impossible unless actual test results are given which show that either the falsification attempts have not been made, or that the theory has failed to predict behavior. And this is a valid objection. If studies exist whereby 1) risk preferences are estimated, 2) subjective probability distributions for all relevant states of nature are estimated, 3) decisions are predicted, and 4) those predictions are compared with the actual decision made by the subject in a real world "entrepreneurial" situation, then the strength of the arguments in this section will have been diminished. However, in order to make this knowledge useful for general economic research, it would also be necessary to show that one decision maker's preferences and subjective knowledge can be representative for a whole group of agents in similar circumstances. Additionally, it would be required that the empirical study show that these preferences remain consistent over time. At this time, no studies have been found by the present writer which contain each of these crucial elements.

Thus, it is only with these rather demanding provisos that

the NCRP can be shown to have filled its theories with objective knowledge. But the trend in research appears to be moving away from such descriptive (positive) accuracy, and more towards the normative approach as typified by the Anderson quotations given above (see Drèze, p. 11). But this can only be a tentative conclusion.

Nevertheless, the implications of this tentative conclusion are profound. Most importantly, it appears that the methodology of modernism, though it may once have been, is no longer the official rhetoric of the NCRP. And thus, the search for that official rhetoric can begin.

5.4 THE NCRP'S REAL RHETORIC: DEFINED AND CRITICIZED

The discussion to this point appears to have lead to a cross-roads. If the art of rhetoric is the art of probing what makes a strong argument, then by modernist standards, the NCRP makes a weak scientific argument indeed. In fact, modernism seems to be pointing the research in decision-making in a perverse direction. We start out wanting to gain knowledge about the way people make decisions under conditions of uncertainty, and we end up being sent right back to assuming that men make decisions under perfect certainty. One of these two opposing forces --modernism and the NCRP-- has got to give.

In fact, Karl Popper, the modernist founder, has written of the notion of subjective probability that "this theory is incredibly naive" (1972, p. 79), and later, he says that "This subjective interpretation of the probability calculus I have

combatted for thirty-three years" (p. 141).

That the research in risk and uncertainty continues to expand into new fields, and to convert new followers, should be evidence enough that the criteria for a strong argument in the NCRP are not the modernist dogmas. This realization that a whole research program is proceeding merrily on its way oblivious to the condemnation it deserves from the modernist viewpoint, immediately suggests the most important question this thesis could ask: "Just what is the real rhetoric behind the research in agricultural non-certainty."

Suprisingly, the above is a question that doesn't get asked very often. The allegiance to modernism has served as sort of an insulator that has protected the economics of uncertainty from the hard question of rhetoric. Adherence to Popper's three worlds of knowledge created a situation where the plausibility of the basic assumptions of a theory could not be questioned. Since these hypothesis were assumed to be conjectural, predictions were all that mattered. But we have shown that, at least in non-certainty research, that these falsifying predictions are extremely difficult to generate.

Once non-certainty research is loosed from the burden of the modernist dogmas, a whole new range of argumentative possibilities come into view. A theory can be criticized from top to bottom without reference to falsifying tests. No longer must the theory be "operational" in the sense that it generate predictive results. Let us therefore release the hard core of the NCRP from the authority which the modernists dogmas gave it and consider

the value of the research program in relation to the source ideas which gave it birth: How well does NCRP explain our subjective feelings about what choice really is? Can the NCRP explain the existence of profit, liquidity, depression, and rule governed behavior? These are the questions which the following will seek to address.

Getting a grip on the "real" rhetoric behind the NCRP is not a difficult task. First of all, as a sort of graceful nod to modernism, it is held that any "persuasive" theory of uncertainty must be "operational," that is, it must make unambiguous predictions. Secondly, "persuasive" NCRP decision theory always assumes that the individual adheres to the von Neumann-Morgenstern axioms. This is a convenient assumption since it is only by accepting these axioms that an "operational" theory is possible. Anderson et al. write that "if you accept the axioms, you must also logically accept the criterion of maximizing expected utility. Moreover, the theorem implies a unified theory of utility (preference) and subjective probability (degree of belief)" (p. 69).

We can see that the "real" rhetoric of the NCRP is apriori based! There should be no doubt that truth is being inserted into this theory (the epistemic correlation) via the assumptions. It is Mill's verificationism all over again, but this time with a couple of twists. The first twist is, of course, that Mill never would have assumed that we could gather enough information about a person to predict his behavior. The second twist is that, in reality, the economic agent is facing conditions of perfect certainty. Mill made things much more clear, his economic man

was unabashedly certain. But with the NCRP, a little bit of digging is required to uncover the perfectly certain man beneath the non-certain exterior.

There are essentially three separate arguments which have been made to show that the NCRP's economic man is just as all-knowing as Mill's man. The first argument applies to the axioms and the last two apply to the notion of subjective probability.

Essentially, what the basic axioms of the von Neumann-Morganstern theory assume is that the individual can order his preferences and that these preferences are consistent. These requirements are routinely considered to be fairly easy to swallow, until we realize that the axioms are also assumed to apply to commodities which the agent has little or no experience with. It is assumed that the agent is capable of attaching a utility value to a commodity that he has never purchased. Jacques Drèze, the originator of this argument, writes that,

"...a consistent decision-maker is assumed always to be able to compare (transitively) the attractiveness of acts, or hypothetical acts and of consequences as well as the likelihood of events. These requirements are minimal, in the sense that no consistency of behavior may be expected if any one of them is violated; but they are very strong, in the sense that all kinds of comparisons are assumed possible, many of which may be quite remote from the range of experience of the decision-maker" (p. 11).

Only a perfectly omniscient decision-maker can see what he has not seen, and know what he does not yet know. But these axioms require that he be able to do these things.

Subjective probability distributions are also something that the decision-maker allegedly forms. The essential character of

the distribution is that it follows the laws of objective probability. Most importantly, the sum of all the probabilities for any possible act is always one. Hence, perfect certainty about the outcome of a decision would assign that outcome a probability of 1. Hence, this is why Shackle has termed the probability calculus a distributional uncertainty variable. As more possible outcomes are imagined for a given action, the number one gets split up like a pie, and a little piece of certainty is handed to each one of these hypothesis. Thus, the probability of a certain event happening is really determined by the power of the decision-maker's imagination; the more possible outcomes he can think of, the lower must be the probability of each of the other possible outcomes (see Shackle, 1969, p. 48).

Additionally the distributional uncertainty variable requires (again taking the argument from Shackle 1969, p. 110) that the list of the possible outcomes be specific and complete. What the decision-maker must do is imagine a complete set of all the possible things that could happen, assign each of the possibilities a probability, and juggle them around to make sure that the probabilities sum to unity.

In this scheme, the probabilities work as a team; mixing together in such a way so as to yield the perfectly certain sum of 1. But sooner or later, the time will come that the decision maker will no longer be able to put off his decision until he has imagined all of the possible states of the world. Realizing that he could go on forever imagining what might come, the subject is forced to make one last hypothesis that means "some other possibility that I haven't yet imagined." Shackle calls this

last hypothesis a "black box" (p. 110) because the decision maker has no idea of its content. But by the rule of the distributional probability, he must assign a probability to that empty box. He must predict the probability of something happening that he can't even imagine!

Perhaps, therefore, we should argue that the economic man of the NCRP is even more omniscient than Mill's economic man. Mill's man only needed to know what will happen. But the NCRP's economic man needs to know everything that could possibly happen!

Another argument, this one advanced by Popper, speaks of the possibility that anyone could really be certain about anything. He gives the following example: Suppose that your hand is in your pocket and someone asks you how "certain" you were that there were five fingers attached to your hand. Assuming that yours is a normal hand, and assuming you choose to form a subjective probability distribution about the question, you would probably assign the probability of one to indicate your perfect certainty that your hand is full of five fingers. Now suppose that, paraphrasing Popper, "that the life of your best friend should depend on the truth of the proposition. You might (and you probably should) take your hand out of your pocket to make doubly sure that you hadn't lost one of you fingers miraculously" (1972, p. 110).

Popper's point is that there can be no such thing as perfect certainty, even in our subjective beliefs. Certainty, he argues, is a relative thing which depends on experience and the seriousness of the problem situation.

If a distributional uncertainty variable really implies certainty, as the above arguments suggest, can we still come to terms with the phenomena that suggested the need for an uncertainty of economics in the first place? Have we made any improvement over the old all-knowing economic man? In the conclusion to his book on the economics of uncertainty, John Hey asks this same question,

"But what does the agent need to know in the new uncertainty theories?: the probability distribution of the prices of all relevant goods, the probability distribution of his income, and the probability distribution of his tastes (both now, and in the yet-to-come integrated dynamic theory, in the future). Is this an improvement?"

"Consider also the optimization problems that economic agents are supposed to be solving. Most of these problems are so complicated that the economic theorist who publishes the model has probably spent several months finding the solution ...These optimization problems are so complicated that the "as if" methodology is stretched to the breaking point. Are we seriously suggesting that we are modeling economic behavior? Have we not gone wrong somewhere? (p. 232).

As Heiner has observed, the method of the NCRP has been to consistently upgrade the capabilities of the decision agent (p. 563) to the point where we are forced to conclude that in fact this agent faces no uncertainty at all. But despite all of this, can the NCRP better our understanding of things which prompted us to invent uncertainty theory in the first place?

What about our subjective sensation of choice? In the last chapter, we termed choice a creative act whereby the individual uses prudence to make a decision for which he is not sure of the final outcome. Historically, prudence was called the highest virtue and a rare gift because making decisions is such a

difficult task. But the NCRP doesn't associate decision making with virtue, rather, good decisions are supposed to come to those who are the best calculators of expected values. Suddenly the art of decision decision doesn't seem to mean as much as it once did before the intrusion of the distributional uncertainty variable. Suddenly decision is no longer the injection of something novel into the course of history, but instead, it is a "best bet."

Additionally, the NCRP carries with it the assumption that the decision-maker is an optimizer. How does this jibe with the instances of rule-governed behavior? Recall the black-jack and the rubic's example's given earlier. No doubt the NCRP's economic man would be a card-counter since that is the best way to maximize expected value. No doubt this decision-maker would be able to determine which one of the 43 trillion possible combinations of the rubic cube his particular version was. After all, if he would "prefer" to solve it in fewest possible moves, then he must certainly have the capability to do so.

Finally, what about profit, liquidity, unemployment, and depression? Would the maximizer of expected utility living in a world with other maximizer's ever encounter these phenomena? It appears that a world populated by these maximizer's would experience these real world occurrences because of one last quirk in the NCRP's decision theory that has yet to be mentioned. Specifically, even though we have shown that the requirements set up by the theory essentially mean that the decision-maker really possesses the power of perfect certainty, there is no guarantee under the NCRP that he will actually make the right decision. He

could guess wrong. Mistakes are possible. He needs to have no objective justification for the beliefs that he holds; the only requirement is that his preferences be consistent. But this leads us to a paradox from which I fear there is no escape; how could a man powerful enough to divide up certainty into all of the component possibilities ever error?

5.5

CONCLUSION

In sum, it appears that the non-certainty research program has caught itself in an intractable dilemma. On one hand, despite the fact that the program strives after predictions of human behavior, the empirical content of the program is very small. This has forced most economists to take the normative approach to non-certainty decision analysis. The problem with the normative approach is that it virtually precludes efforts at falsification. Without efforts to falsify, there can be no approach to the truth. Hence, by the rules of modernism, the theory holds precious little truth.

On the other hand, when we consider the real rhetoric of the program, we find that it is not persuasive at all. Our critique of the assumptions which was made possible by the removal of the modernist mask of the program, revealed that uncertainty doesn't exist, that essentially the decision-maker is assumed omnipotent enough to estimate all the possible states of the world. He is, when he has assigned the probability of one to a particular outcome, absolutely, positively certain that that outcome will occur. Yet, despite these godly traits, this decision-maker is

prone to error and to make mistakes. It is an interesting combination indeed, but it is hardly a persuasive one.

CHAPTER VI.

CONCLUSION

What this concluding chapter seeks to accomplish is more than a presentation of a mere abstract of the thesis given in the past few pages. Hopefully, from what has been said before, the most immediate implications of this study will already have been justified. Rather, let us look at the conclusions reached from our examination of the rhetoric of non-certainty theory to see if some more far-reaching consequences can be drawn.

If there is one central conclusion that this paper has sought to justify, it is that what has really destroyed the persuasiveness of the non-certainty research program is their adherence to an "official" rhetoric or methodology. It is not so important that the "official" methodology in this case happened to be Popperian; the important thing is that when an official rhetoric exists, there is little incentive to test the persuasiveness of scientific argument.

In the case of non-certainty research, the consequences of this adherence to an official rhetoric were profound. Namely, the modernist insistence that theories be "operational" (or, capable of making predictions) essentially implied that the powers of the economic man be even more omniscient than the powers of the old Millian economic man which the NCRP sought to

replace. This new economic man needed to, in order to form truly consistent preferences and logically sound probability distributions, be able to imagine all the things which could possibly result from his decision. Yet despite this remarkable ability, the man was still capable of making mistakes.

What we are left with is an odd paradox. And the source of the paradox which lead to an omniscient man who is still prone to error is the assumption that this man must maximize. Of course, the reason why he must be assumed to maximize is that it is only by the assumption of maximization that operational predictions can be derived.

However, the analysis showed that the amount of scientific knowledge capable of being gained through these operational predictions is extremely limited. In fact, when the NCRP shifted its focus to a normative perspective, the possibility of scientific knowledge absolutely vanished. It was this reality which lead us to the conclusion that perhaps Popper's system of falsification was possibly not the "real" rhetoric of the research program. Once that rhetoric was questioned, the odd inconsistencies mentioned above began to surface.

What is perhaps the most suprising conclusion to be drawn from this analysis is that modernism, by its very nature, violates the spirit of Karl Popper's methodology of falsificationism with impunity. The origin of the break between Popper and modernism appears to have occurred when economists decided that the only part of Popper that mattered was the making of predictions. But this is not at all what he meant. Predictions are meaningless unless they are capable of testing something,

unless they have the potential to falsify some theory. But in the case of non-certainty research, it is extremely difficult to falsify any theory via predictive experimentation. Thus, we find Popper himself criticising the notion of subject probability and risk preference.

The real essence of Popper's methodology is not prediction, but critical thought. It is only through the criticism of theory that theory becomes persuasive. Thus, the persuasive power of our rhetoric must be constantly challenged. The modernist NCRP failed for this very reason; they insulated themselves from rhetorical criticism. By building increasingly complex models of human behavior, the modernist NCRP postulated the one thing that Popper argued was patently impossible, certainty. Thus, if modernism must fall, there is no reason why Popper's thoughts must fall with it.

In Chapter II, when the origin of economic method was discussed, we reached the conclusion that part of the reason why such a methodology as modernism came into being was the fear of what was termed "truck-driver" economics. Namely, since economic issues are ones that affect everyone deeply, and are also ones upon which everyone thinks of themselves as expert, the modernist methodology created a convenient "closed shop" that held all of the "truck-driver's" arguments at bay. By arming their discipline with giant main-frame computers, and requiring that "acceptable" economic research be within the reach of only those who possess daunting abilities in statistics and mathematics, the modernists "solved" the problem of "truck-driver" economics by

eliminating, prima facie, any possibility that the truck-driver could ever contribute anything meaningful. It is for this reason that McCloskey labelled the modernist method as "arrogant and pretentious" (p. 490).

But perhaps such an arrogant and pretentious method could be tolerated if it produced some meaningful results. But this analysis indicates quite the opposite; little knowledge has been gained by the adherence to one method and the disposal of the truck-driver. Simply put, modernism has lead us no closer to an understanding of human behavior under conditions of uncertainty than the certainty theory it seeks to replace.

But to abandon an official methodology in favor of a system which is consciously critical of its criteria for strong argument requires a very important proviso: Men must be assumed to be reasonable. Without the assumption that each man is earnestly searching for the truth, economic conversation becomes pointless. This judgement of reasonableness is one that each reader must make for themselves. However, what this paper has attempted to show is that the costs of assuming reasonableness away can be very high. In the end, we may have no choice but to trust the integrity of those with whom we converse on matters of economics.

Finally, what suggestions present themselves for the the future research in agricultural non-certainty? Recall the example of the western Kansas farmer facing an imminent shortage of water with which to irrigate his crops. Certainly the technical matter of the future water supply should be left to the geologists to determine. And this analysis suggests that the use of a distributional uncertainty variable is incapable of either

providing the scientist with objective knowledge, or of providing the farmer any real assistance in making his decision on how to act. Should we choose to search for insight of the character of the decision which the decision-maker faces, it is clear that a non-distributional uncertainty variable will have to be used. But such insight is inherently generic in nature; the character of the decision which this farmer faces is no different from the character of the decision which faces the businessman who is contemplating the purchase of new manufacturing machinery. We should like to say something a bit more specific about the problem which the farmer faces.

It seems that the only alternative, since we are not experts in geology, and we recognize that predictions of this farmer's behavior are, if not impossible, then meaningless; is to opt for a classificatory scheme. How have producer's in the past responded to situations where the supply of a crucial input suddenly decreased? What have been the consequences of these responses? And finally, how well does the situation of the farmer parallel these historical cases? The number of questions which present themselves is limited only by the imagination of the economist. And what is so exciting is that these research possibilities only become possible when we loose ourselves from the burdens of an "official" rhetoric.

In all, the situation of the entrepreneur appears to be no different from the situation of the economist. Our imaginations create the future. And just as it is folly to predict scientific discoveries that haven't yet been made, so to it is folly to

predict decisions that have not been made.

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THE RHETORIC BEHIND THE RESEARCH
IN AGRICULTURAL NON-CERTAINTY

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ABSTRACT

The classic definition of rhetoric is much different than the sense with which the word is used today. The ancients considered rhetoric to be a fine and honorable word; rhetoric was the art of persuasive argument. This thesis, using the old definition of the word, considers the rhetoric of the current economic research in agricultural non-certainty. Specifically, the paper seeks to identify and criticise the standards of persuasive argument currently used in this research area.

In order to identify what the standards of persuasive argument are in the Non-Certainty Research Program (NCRP), it is necessary to first come to terms with what has been called the "official" rhetoric of economics. Called "modernism," the official rhetoric insists that good arguments in economics always contain theories which attempt to predict the behavior of individuals. The value of a theory is then the success it has in predicting individual behavior.

Hence, this thesis seeks to identify the essence of modernism and to evaluate the adherence of the NCRP to its methodological rules. First, we show that modernism is indeed the "official" rhetoric of economics. Additionally, there is some speculation as to why an "official" rhetoric might be considered necessary in a discipline like economics. Secondly, a chapter goes into detailed discussion of the philosophical foundations for modernism.

When the NCRP is judged by the standards of modernism, the analysis shows that it fails this test convincingly. Moreover, despite its vaulted reputation, modernism is shown to be, at least within the NCRP, just a cover for the "real" rhetoric of economics. We show that this real rhetoric inserts truth a priori into the assumptions of the theory. Given this philosophical stance, the justification emerges for a criticism of those assumptions, especially the dual notions of expected utility and subjective probability. The thesis concludes with a compendium of criticism of these two ideas.

Finally, the concluding chapter suggests that further research, rather than attempting to predict behavior, should opt for what has been called a classificatory scheme. Rather, than attempting to predict future occurrences, theory should identify links of similiarity between the current problem situation and a scene in the past where economic agents faced similiar circumstances.