

PRICES, INFORMATION AND EXPLANATION IN NEOCLASSICAL ECONOMIC THEORY

by

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## ABSTRACT

The most widely used method of explaining economic phenomena in the twentieth century has been and still is a class of models known as neoclassical economics. These neoclassical models use as their foundation the principle that individuals are the elemental decision-making unit. Prices in neoclassical models are said to be informationally efficient as individuals need only know the vector of equilibrium prices in order to attain social coordination. Equilibrium prices are therefore claimed to be informative as they are seen to carry the necessary and sufficient information to assure coordination by individuals.

This thesis starts from the position that for any observation or theoretical statement to be said to have the logical property of informativeness one must first discuss learning methods employed by individuals. It is the learning method that will define what class of observations individuals will deem informative. It is shown in this thesis that for a large class of equilibrium neoclassical models, including those of the Rational Expectations variety, prices cannot have the logical property of informativeness attributed to them unless one also limits the class of learning models. The admissible class of learning models includes only those which guarantee coordination out-of-time. That is, neoclassical models cannot provide equilibrium explanations of phenomena in a finite period.

This thesis then introduces a larger class of learning methods which can be consistent with neoclassical models. It is shown that within this larger class of learning models there is at least one which is time-based. This is a learning method which is a form of Sir Karl Popper's 'conjecturalism'.

The choice of learning method is then made endogenous through heuristic argument. This endogenous choice allows individuals to change the way their learning takes place. This is in contradiction to current theorizing and provides the 'higher level' learning theory called for by F. Hahn in 1973. It is shown that only by allowing the endogenous choice of learning method will prices have the logical property of informativeness in a neoclassical model short of the 'long run'. This provides a neoclassical time-based explanation of short-run coordination success or failure.

DEDICATION

To the memory of my grandmother, Tena Eitens Lange, and for my parents who have waited a long time for this.

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## CHAPTER I : INTRODUCTION

There is at present no satisfactory axiomatic foundation on which to build a theory of learning, of adjusting to errors and delay times in each of these. It may be that in some intrinsic sense such a theory is impossible. But without it this branch of the subject can aspire to no more than the study of a series of suggestive examples.

Frank Hahn, [1982, pg.747]

This is a thesis about how economists attempt to explain social coordination. These theories of coordination must answer at least the following two questions. First, is sufficient information generated by individual and/or group interaction that coordination may be achieved? Second, is the generation and acquisition of this information consistent with the behavioral characteristics of individual agents posited by the particular theory under consideration?

Following a long tradition it is still common for most economists to treat the relative price system as the information sufficient for coordination. Usually, the property of "informativeness" is discussed as an inherent and unquestioned attribute of prices without reference to other considerations. In that light prices are treated as the summary statistic of an exchange process. Knowing the equilibrium values of prices is seen to confer upon individuals some knowledge of the structure of that exchange process. There is rarely, if ever, a discussion of the sense in which those prices are informative or what kind of information they

convey to the individuals whose behavior is being explained.

Any approach relying on an ill-defined concept has little chance of explanatory<sup>1</sup> success. Consequently, Chapter II will provide a firm grounding for the idea of "informativeness". If prices are to be informative, then it must be clear what informativeness and uninformativeness are.

Chapters III through VII comprise a critical evaluation of current neoclassical theorizing on the coordination issue; in these chapters five different treatments of the coordination problem are addressed. These chapters roughly follow the chronology of economic thought on this issue. Chapter III deals with the static general equilibrium coordination. Chapter IV investigates the so-called dynamic approach made popular by Samuelson and Hicks. Chapter V considers the attempt to exchange price flexibility for quantity adjustment in non-Walrasian models. Chapter VI examines the 'economics of uncertainty' treatment of coordination; and Chapter VII studies the rational expectations modelling of coordination. Chapter VIII develops a neoclassical approach to endogenizing the choice of learning methodologies. Chapter VIII breaks with the long tradition of imposing a learning method, expectations function or reaction function upon the agents whose behavior is being described. Finally, Chapter IX provides a summary and conclusions.

This opening chapter frames the problem situation as I see it. It is an outline of the issues which will be discussed at greater length in the following chapters.

In the mid-1930's F. A. Hayek [1937/1948] viewed the problem facing economists as one of explaining how groups of individuals may reach a coordinated state. Later writers, for example Hicks [1946] and Samuelson [1947], assumed the coordination problem was one of individual behavior only. Their view was that individuals are making the decisions which affect social outcomes. Their models consequently represent the behavior of individuals, taking the view that social theories are grounded in the attitudes and behavior of those individuals. This contrasts with the view which asserts that social theories must be grounded at least in part by the behavior of irreducible groups of individuals.

The view "that allows only individuals to be the decision-makers in any explanation of social phenomena" is known as Methodological Individualism [Boland, 1982, p.28]. Hereafter I will term this view "individualism". The alternative view, that groups must form the grounding for social theories, will be referred to as "non-individualism".

For economists, discussions of these and issues to follow take place within a dominant theoretical paradigm: neoclassical economic theory. Briefly, this theory is individualist in that it posits social outcomes as the result of individual actions only. Individuals take actions attempting to optimize personal utility functions subject to natural, or outside, constraints such as the quantity of resources and extent of technical knowledge. The lone social constraint is a set of prices determined in markets as a result

of individual interaction. These prices are treated as parameters by individuals when making choices [see Boland, 1982].

This question of social coordination is intimately related to the stability properties of models of social theories. While not explicitly phrased as such the issue of stability in the context of coordination was addressed by most theorists [e.g. Smith, 1776/1937, p.99; Walras, 1874/1954, p.528-529; see Blaug, 1968, pp.582-583] writing prior to the 20th century.

Hayek's pre-occupation with the issue highlights the absence of a satisfactory solution as of the mid-1930's. Exegetically, the "stability" question followed closely upon the heels of the "existence" or coordination question. For example, in both Walras and Smith the description of what an equilibrium is precedes any discussion of how equilibrium might be attained. Usually, the description of how the system once equilibrated stays equilibrated is much weaker in logical rigor. Similarly, Hayek followed Wald's [1936/1951] mathematically rigorous demonstration of the existence of equilibrium with his own questions on the conditions necessary for its attainment.

Hayek was less than sanguine about providing as rigorous a demonstration of systemic stability as Wald had of the existence of systemic equilibrium. However, Hicks and Samuelson were less reticent [see Negishi, 1962]. Samuelson [1947, Chaps. IX, X] described the price adjustment of any good as a response to demand/supply inequalities both in the market for that good and the

markets for all other goods. Further, he provided the conditions under which prices in all markets would converge to their equilibrium values.

Static equilibria and the exhibition of multi-market stability took on greater meaning through his Correspondence Principle [ibid p.258]. Samuelson argued that comparative statics, which is the comparison of two different static equilibria, and the "stability of equilibrium cannot be discussed except with reference to dynamical considerations." [ibid p.262].

Unfortunately, as will be shown in Chapter IV, Samuelson's price adjustment approach is inconsistent with individualism [Koopmans, 1957]. Koopman's point is that if everybody takes prices as given then who is left over to set them? Price taking is consistent only with the existence of the given equilibrium. Therefore, any dynamical considerations covering the move from one static equilibrium to another bear closer scrutiny. For, if this path is describing individual behavior and these individuals are price takers then equilibrium points only comprise the dynamic path. This denies any possibility of disequilibrium and hence any need to worry about how and when individuals learn of results which are less than optimal. The price changes described by the path, being shifting equilibria, convey no new information to individuals.

An alternative view of the dynamical path is one of claiming that the path relates two static equilibria by showing all the disequilibria passed through on the passage from the one equilibrium

to the next. Unfortunately, and this is allied to Koopman's point again, all those points of disequilibrium are inconsistent with individualism. This is because disequilibrium implies at least one individual's failing to optimize relative to the perceived set of givens. One of those givens is the set of prices determined in the markets. For those prices to change to the equilibrium prices where universal optimization prevails at least one person must be a price maker not a price taker. Yet, the price making behavior potentially denies the influence of other individuals; thus, the inconsistency arises between an individualist explanation and disequilibrium price adjustment [Arrow, 1951].

One can explain disequilibria relative to some optimum in patently non-neoclassical ways. For example, it may be claimed that individuals are not maximizing utility and /or profits [e.g. Leibenstein, 1979]. Instead, this view maintains that custom, convention or rules determine the outcome. A committed neoclassicist, however, may raise at least two points. The first, if the method of choice [e.g. custom, rule of thumb, utility maximization etc.] is a choice then is it optimal or not? The second, if it is an optimal choice to allow customs to determine outcomes then is not the objective function of the individual still one of the givens? It may be that the theorist has mis-specified the objective function, but the decision process is still one in the neoclassical mould. For example, firms could be seen as trading off extra monetary profit for more of another positive attribute or less of a negative one [e.g.



costs of decision making]. Consequently, a committed neoclassicist would claim that the 'disequilibria' explained are apparent and not real as the theorist has incorrectly specified the objective function(s) [Boland, 1981].

The problem of disequilibrium and individualism would not arise if theorists gave up either the attempt at explaining how a coordinated state comes about, or were willing to give up an individualistic explanation. Entering the 1960's it remained for someone to demonstrate that there was some way that individuals could acquire the knowledge of the system hence solving the coordination problem consistent with individualistic principles.

The first problem in any attempted demonstration which is to be consistent with individualism is to specify what constitutes "knowledge". Knowledge is that information which if known would facilitate social coordination. A related issue is what would be the minimally sufficient knowledge, or information for coordination. How little do people need to know for coordination to be possible? Thirdly, by what process can individuals acquire this minimum information and how can they guarantee what knowledge they have acquired is true?

Hayek [1937/1948, 1945/1948] and Koopmans [1957] argued that the minimally sufficient knowledge over and above the personal givens needed by each individual was the set of equilibrium prices. In other words, the price system in their view is "informationally efficient". This view won wide-spread endorsement from economists of

all persuasions.

For example, Lange and Hayek could both agree that a price system, whether they be shadow prices or some other form, is most efficient when compared to actual alternatives [see Layard and Walters, 1978, p.27]. The debate raged over who and how the prices should be calculated, central planners vs. individuals through the market, but both sides were in agreement that the minimally sufficient information for systemic coordination was (and still may be) seen to be the set of equilibrium prices.

For equilibrium to exist it is necessary that individuals are maximizing subject to their constraint sets including prices. Only at the equilibrium prices will all individuals be maximizing. Maximizing implies that individuals will attain that level of utility and/or profits they feel to be the highest obtainable given the constraints. If even one person is not maximizing - that is, given the constraints, he does less well in a utility or profit sense than he thought he would - then those cannot have been equilibrium prices [Boland, 1982].

How those prices are determined and acquired in a world where all individuals take prices as given remains the moot point. Traditionally the auctioneer as deus ex machina [Leijonhufvud, 1968] performed both roles - price maker and distributor; however, it was never presumed that in "real life" there actually was anyone filling that role. The story was used as a bridge to allow theorists' models a passage to equilibrium. The auctioneer costlessly, truthfully and

completely disseminated the necessary information to ensure coordination.

It has been in the attempt to model this acquisition process more realistically that theories of learning have become important. The two traditional approaches to the acquisition process which have gained much popularity (if measured by use) are a priorism and inductivism. Briefly, a priorism is the doctrine that individuals may, without sensory experience, have guaranteed true knowledge of the problem situation. Inductivism is the view that "asserts that any justification of one's knowledge must be logically based only on experiential evidence..." [Boland, 1982, p.14].

Right from the start of economic formalism which finds in David Ricardo its first great practitioner [Hutchison, 1978], the issue of acquisition is bypassed by presuming individuals have perfect knowledge a priori of all givens including prices. However, ignoring the problem of how this knowledge was acquired did not sit well even in the 1800's. T. E. Cliffe-Leslie [1888/1969, Chapter XVII] assailed Ricardo's a prioristic approach precisely on the issue of the extent of people's knowledge. Unfortunately, Cliffe-Leslie's approach, which is inductivist in nature, is explanatorily no better than Ricardo's despite its continued popularity [e.g. Hayek, 1937/1948; Stigler, 1961; Turnovsky, 1969; De Canio, 1979; Lewis, 1981].

I argued earlier that the development of equilibrium neoclassical theory took a particular course: descriptions of

behavioral relationships, proofs of existence, including the uniqueness or existence of multiple equilibria, and then stability analysis. As will be shown in Chapters V, VI and VII, current theorizing traces the same path with the mechanics of Temporary General Equilibrium theory [see Grandmont 1977, Drazen, 1980] built upon ideas put forth by Hicks [1965], Clower [1965] and Leijonhufvud [1968]. Formalization of these ideas and existence proofs were provided by Dreze [1975] and Benassy [1975, 1976]. Finally, stability analysis was quick to follow [Fisher 1978, Eckalbar, 1980]. One finds the same sequence in the very current micro-Rational Expectation literature [e.g. Radner, 1979, Blume, Bray and Easley, 1982].

Exegesis and increasing mathematical elegance aside, it is interesting to note that Hayek's original question remains unanswered. The later stability analyses of Fisher and Eckalbar share the non-individualistic properties of Samuelson's approach though the techniques used to derive the results differ. The stability analysis cited in Blume, Bray and Easley [1982] is based on inductivism indicating that infinite time is needed to ensure obtaining the rational expectation of how the economy is structured. In short, the models are more complicated, but Hayek's problem remains unresolved unless one wishes to forego individualistic explanations and/or forego time-based explanation.

The above is not meant as a systematic nor comprehensive review. Rather it seeks to provide a glimpse of how this particular

issue - the treatment of how individuals can know they are acquiring correct information - has been treated in some past and present economic literature. I have asserted (briefly) that there have been no alternatives to a priorism or the infinite long run inductivism that have been used by theorists to date. It should be recalled that within the neoclassical paradigm the only social (non-individual) information is the price system. All other information is personal embodied in the natural givens and utility functions. Why this matters is that it can only be from prices and personal information that anyone "learns" in current treatments. What they are learning is the structure of the economy. That is, how the price/quantity results are generated. The personal information is assumed to be unchanging over the period of stability analysis, otherwise it could not be distinguished if the system was unstable or if continual exogenous shocks (e.g. changes in personal givens) were creating the movement [Arrow, 1959b].

I have pointed out that for some theorists the question of how people acquire demonstrably true theories from the endogenously determined prices is not of interest. For example, following Ricardo, one may a priori ascribe to individuals perfect knowledge of the problem situation. Or, following Cliffe-Leslie, enough time could be allowed so that all observations of the (relevant) world are made. In the first case there is no need for market-generated information, in the form of endogenous prices, as they can be calculated privately by all. In the second case, time does not

matter. This forfeits any chance of explaining successful short run coordination.

Explicitly, my thesis is intended to counter the well-established claim [e.g. Hayek 1937/1948, 1945/1948 and Koopmans, 1957] that equilibrium prices are all the information needed to keep an economy in equilibrium once it has been attained. I will show that this feature of equilibrium prices, "informativeness", follows from an adherence to an outdated and incorrect view of knowledge acquisition known as inductivism and that contrary to this popular view equilibrium prices must be, by definition, uninformative. Further, I propose that making the system "dynamic" by the addition of exogenous learning methodologies which add (a) another personal given, or (b) adding an exogenous time rate of change of one of the natural, technological and/or utility function givens, can not add any time-based informative explanatoriness to the model. This approach continues to render prices uninformative despite this "dynamic" structure.

Finally, I will show that the claimed characteristics of prices, informativeness, can only be explained if one looks at allocation problems within what has been termed "...the Popper-Hayek program for explaining any rational dynamic process." [Boland 1982, p.178].

I offer a view consistent with neoclassical analysis and the Popper-Hayek program. This provides a 'point of view' about the ways one might think about prices and information in a neoclassical framework. Counter to much current theorizing [e.g. Grossman and

Stiglitz, 1980; Verrechia, 1982; Parks, 1978; Weiss, 1980] one implication of this study is that ad hoc defects in the price system need not be appealed to for coordination failure. Prices need not be 'sticky', 'noisy' and/or absent [e.g. "incomplete" markets] to explain the apparent lack of coordination.

I hope that the illumination of the informativeness or unformativeness question of prices and their necessity in any individualist model will lead readers to a fuller understanding of why current neoclassical theorizing has not added to our understanding of real-time, short-run coordination problems.

## FOOTNOTE

<sup>1</sup>By "explanatory", "explanation" and "explanatoriness" etc., I follow the definitions as discussed in Boland [1975, pps. 27-28]:

" An informative explanation is the 'explanation of the known by the unknown' [Popper, 1972, pg. 191]; and ... a description is the explanation of the known by the known; where 'explanation' is the name given to the logical relation of the explican to the explicandum [Popper, *ibid.*, pg. 192] and where "known" is the usual empirical sense ( although it need not be restricted to that ). One sense in which an explican will be an "unknown" is when ( necessarily ) one of them is a strictly universal statement ( e.g. "all men are mortal" ). "



## CHAPTER II

There is nothing arbitrary or unessential in analyzing the precise content of a notion in the various connections in which it is involved.

J. N. Keynes [1917/1973 pg.156]

In the previous chapter an outline was provided where it was argued that one of the longer standing and still current problems in economics is how one explains the generation and acquisition of information in a social framework. Notable by its omission is a specification of just what 'information' or 'knowledge' is. Information traditionally has been equated to discrete, sensory observations [e.g. Stigler, 1961; Ng, 1975; Milgrom and Stokey, 1982].

If one could take all observations through time and space of any given statistic these observations would then serve to define a so-called 'objective' distribution. Where 'objective' is usually taken to mean that this is the way the world 'really is' and it is only our inability to have all observations which prevents us from certain learning of that fact.

By contrast, 'subjective' knowledge or information is personal in nature and may be subject to different interpretation by different individuals. Thus, we find in Hayek [1937/1948, p.36] the

distinction made as follows: "It is important to remember that the so-called 'data' from which we set out in this sort of analysis, are (apart from his tastes) all the facts given to the person in question, the things as they are known to (or believed by) him to exist, and not, strictly speaking, objective facts."

It will be argued in this chapter that despite the popularity of such views one need not be limited to only these views of information.

'Informativeness' is a property of statements. These statements may be about empirical reality or theoretical knowledge. Of interest in this chapter will be the view individuals have of what constitutes potential information. The concern will not be how one might acquire what one believes will be informative or how one responds to information. It is assumed - for this chapter - that individual(s) have acquired what they believe to be a true, or at minimum the "best", theory of learning; within those theories we explore what statements or observations, if any, would be considered informative.

It quickly becomes apparent that the idea of a statement's informativeness cannot be divorced from the perspective (e.g. theory) through which it is viewed. However, this need not make information subjective (i.e. personal). As long as the theories are expressible in repeatable forms, e.g. words, the view of what is informative - given that theory - is objective [Boland, 1982, pg.179].

More specifically, it is important to specify a framework within which observations may be asserted to be informative or uninformative

as ultimately I wish to make statements concerning the informativeness of specific observations, those on prices. Informativeness is of concern because the absence of the property denies a role for response, or learning. It is the acquisition of knowledge which agitates for change. In short, one wishes to know if the claimed virtue of a price system, its informativeness, is consistent with the way economists model individual behavior towards learning.

The relationship between observations which are potentially informative and theories is important. Frequently, the treatment of observations, especially those obtained through sensory experience, is that they are theory-independent [i.e. a priori true]. Observations become synonymous with "objective data" or "objective facts". But, observations exist only as the result of theories that can be held. As N. Hanson [1965, pg.19] put it: "...seeing is theory-laden,". This view rejects notions which claim that there is a role for observations independent of any theory. Without at least one theory (i.e. the theory of observations) there can be no observations and without an observation there can be no potential for information.

There are several theories of learning or expectations formation available. Probably the best known would be inductivism in its various forms such as naive induction and conventionalism. Other theories are Popper's [1959] conjectural knowledge hypothesis and a priorism. It is through these theories of learning that I delineate which observation(s) are informative and which are not. Recall that in this chapter we are taking the theories of learning as givens. I

do not here critically investigate their implications other than for the above stated purpose. Further, there is no concern in this chapter about how and why people might change these theories [see Chapter VIII].

"Inductivism is the methodological doctrine that asserts that any justification of one's knowledge must be logically based only on experiential evidence consisting of particular or singular observation statements; that is, one must justify his or her knowledge using only verifiable observations that have been verified by experience."  
[Boland, 1982, p.14].

For economists the idea and practice of using an inductive technique, acquiring knowledge or learning by induction, is far from novel. It is also well-known [Boland, 1978] that this view requires an infinite amount of time for the justification of one's knowledge. For example, if one had a theory which related prices to the price generating system one would have to view all singular observations [e.g. "the price of good  $i$  at time  $t$  in market  $j$  is  $p(ijt)$ "] at all points in time and all locations before one could guarantee the truth of one's inference from the information set [here the  $p(ijt)$  for  $i=1,\dots,n$ ;  $j=1,\dots,k$ ; and  $t=1,\dots,\infty$ ] to the price generating system.

The corollary to the view that induction can only guarantee success in the infinitely long run is that all the conceivable observation statements are necessary for successful completion of the proof of one's knowledge. Without all observations it cannot be guaranteed that there is complete knowledge of the system generating

the observations. This leads to the widely expressed view that, in general, the more observations one has the better off one is. More specifically, more information -- in the form of additional observations -- can never lower one's expected utility [Hirshleifer and Riley, 1979, p.1395].

The result is that in using inductive techniques to justify one's knowledge one will equate additional observations with no decrease (and usually an increase) in information as one is inferring the relationship from all the observations. Lack of one (or more) of the observations leaves the success of one's inference in doubt.

As a simple illustration consider someone using an inductive technique to infer the 'true' relationship between two variables  $p$  and  $q$ . Knowing the first  $n$  observations,  $p_1, \dots, p_n$  and  $q_1, \dots, q_n$  all fall on a straight line does not allow one to conclude that the true relationship is linear. That is, there is no guarantee that the next change in  $p$ 's ( $p_{n+1} - p_n$ ) bears the same relationship to the next change in  $q$ 's ( $q_{n+1} - q_n$ ) as it did previously. To guarantee the truth of one's knowledge of the relationship one must make every observation on the  $p$ 's and  $q$ 's.

The above view might be considered somewhat extreme. After all, it is well known that providing inductive proofs of strictly universal statements requires more than can possibly be delivered in a finite lifetime. As a result a "weaker" form of induction is often resorted to, Conventionalism. The aim of conventionalists "...is a choice of the 'best' theory according to conventional measures of

acceptable 'truth'." [Boland, 1982, pp.17-18].

A conventionalist approach may 'solve' the problem of induction by limiting the number of observations one must make to guarantee one's knowledge at some 'acceptable' level. Recognizing the problem with Induction -- that it cannot guarantee truth in a finite time period -- many conventionalists deny the truth of all non-tautological knowledge. As Solow [1956] puts it: "All theory depends on assumptions which are not quite true. That is what makes it theory." They instead search for 'better' or 'best' theories, where the choice criteria are often different for different problems, by collecting observations which add confirmation up to the acceptable level.

An informative observation is any that serves to bolster (as measured by the convention adopted) the theory. Since, from the conventionalist view, theories cannot possibly be true it is not informative for anyone to view an observation which is at variance with any given theory. If we already know the theory to be false (by definition) what does an 'outlier', i.e. refuting instance, tell us?

This conventionalist approach to learning differs from naive inductivism in that the naive inductivist may believe that the true theory will step front and center (can be successfully inferred) if only enough observations can be collected. For a naive inductivist all observations are informative. The conventionalist, denying theories can be true, searches only for a limited number of observations, determined by their conventions, and of those only the confirming observations are viewed as informative as they serve to

delineate the 'better' false theories from the 'worse' false theories. Since all theories are viewed as false a refuting observation carries no information.

This conventionalist solution only pushes the problem of induction back one step. For example, by what process does one choose the 'acceptable' level, or the required number of observations? This is the Problem of Conventions which may be stated as "...the problem of finding generally acceptable criteria upon which to base any contingent, deductive proof of any claim to empirical 'knowledge'." [Boland, 1982, p.18]. Most justifications of a choice of acceptable criteria involve reliance on an inductive argument at some stage. Consequently, conventionalism, while seeming to elude the problem of induction only hides it at the level of choosing choice criteria.

Using the previous example [see the Appendix to this chapter for further economic examples] it may by convention be acceptable to use the number  $k$ , where  $k$  is less than infinity, observations on the  $p$ 's and  $q$ 's to arrive at the 'best' relationship. The choice of  $k$ , as opposed to  $k-1$  or  $k+1$  etc., may now be open to question and its justification may put induction back into the picture.

With a conventionalist approach one need not appeal to every observation of  $p$  and  $q$  to arrive at 'acceptable truth'. However, confirming observations from the further  $k+1, \dots, \text{to infinity}$  observations are not uninformative in an absolute sense, but only relative to the cost of acquisition. The convention chosen dictates

one need only take a finite number of observations to arrive at a 'good enough' approximation to the true relationship between  $p$  and  $q$ . However, the conventionalist seeking to justify the choice of  $k$  as the acceptable number of observations may be interested in taking observations on the performance of his theories in order to provide a justification for the use of that particular choice criterion.

In order to justify criteria for 'acceptable truth' every theory generated by those choice criteria would have to be assessed. This implies inferring the true conventions by inductive routine. A process which takes as long and requires as many observations as if one were to infer the relationship of  $p$  and  $q$  directly. Of course, every observation on the relationship of choice criteria to resultant theory chosen would be informative by naive induction.

Consequently, the conventionalist approach does not get one out of the inductive woods. Most economists are so familiar with the above approaches to learning [see the Appendix to this chapter for a tiny subset of examples drawn from the economics literature] that it may seem strange if not downright unnecessary to introduce any further learning methodologies. However, if one were to assert that the above two views (and variants) comprised our knowledge of learning we would be left with the unpalatable choice between accepting the view that:

- (a) knowledge may be guaranteed true only at the expense of waiting an infinite amount of time, or the view



(b) that since all knowledge is false we can at best hope only to place false theories into categories, e.g. 'poor', 'fair', 'better', 'best', etc. based on some set of conventions.

To broaden the choice set comprising learning methodologies one needs to recognize that contrary to the conventionalist view not all theories (knowledge) need be false and contrary to the inductivist view we may provide deductive proofs of -- hence "guarantee" -- what we know to be true in the less than infinite long run.

This non-inductive approach to expectations formation will be referred to here as Conjectural Hypotheticism [Conjecturalism for short] and is best known through the works of Popper [1959, 1962, 1972]. Popper argues [e.g. 1962, pp.54-55] that we should accept conjectures (theories) only provisionally and since we cannot prove them true, if true, but can potentially show them false when false, we should seek refuting instances of our conjectural knowledge. This however, seems to admit a guarantee of our knowledge's truth status (i.e. its falsity) provided by finding observational counter-example(s) of the theory. This guarantee depends on the theory of observations being true, which, of course, it may not be. [Agassi, 1966].

Nor do we have to embrace the conventionalist position that all theories are false, which is itself a theory and if true contradicts itself. However, with conjecturalism, the only knowledge that we can guarantee is that we have observed contradiction(s) between theory and

evidence, and this only if we accept all observations as true statements. Theories not yet contradicted by evidence remain provisionally acceptable on a tentative basis. Failure to refute theories with past or present evidence does not impute to them any degree of truth, even though they may be 'useful' or 'practical' or highly corroborated, as refutation may take place in the future.

Here learning takes the form of error correction. We learn when a previously provisionally acceptable theory is confronted with any observation that refutes it. Thus, refuting observations are informative observations. Observations which do not refute the given theory being confronted cannot be considered informative.

I wish to abstract here from the issues [Popper, 1959, Sections 34 and 83] of the 'degree of corroboration' and the 'degree of falsifiability' of theories. While there may be ways of ranking (albeit loosely) the falsifiability of theories and hence the informativeness of refuting instances (e.g. if we thought a theory logically highly probable then a refuting instance might be in some sense viewed as more informative than a refuting instance of a theory we thought logically improbable) it is not an issue I will be considering. I consider, for the purposes at hand, that the demarcation of interest is solely between refuting and non-refuting instances of a given theory and that the former be considered informative -- it tells us that our theory is contradicted by evidence -- and the latter be considered uninformative.

Clearly, as a conjecturalist, if one is interested in

confronting theories with evidence one needs only one refuting instance to learn a theory is not true. This reverses the ranking placed on information and observations in the above inductivist and conventionalist programs. No longer is more information, here in the form of additional refuting instances, better than less. One refuting instance tells all. Rather than searching for additional confirmations of a model one is advised to search for one of possibly many counter-examples. 'More' is not necessarily better as once you have a refutation you need not continue collecting observations relevant to that theory. Additional refutations would not be informative as they will not reveal anything unknown.

The previous three views have the following implication for observational informativeness: naive inductivism attributes the property of informativeness to all observations, conventionalism attributes informativeness to confirming observations and conjecturalism attributes informativeness to refuting observations.

There is however one final theory of learning I wish to consider and that is a priorism. This is the view that one's knowledge may be guaranteed a priori without reference to observations. There need be no appeal to 'facts' or observations. Variants of this view would be that a priori knowledge comes from faith, authority and/or intuition.

If one were an a priorist clearly no observations are necessary to verify one's knowledge. One takes one's a priori given knowledge to be true. There are, consequently, no observations which might be deemed informative or uninformative. There is no reliance on

observation for any part of ones a priori knowledge.

Under a priorism in contradistinction to both the Inductive and Conjectural approaches neither empirical nor theoretical observations play a role. There is no need for verification nor conceivable refutation.

The relationship between one's theory of learning and how one treats observation statements, i.e. are they considered informative, will one learn from them, or not, has been the sole issue of this chapter. The argument has been made that observations may take two forms: they may be in accord with a theory or they may contradict it. Each type of observation may be informative or uninformative. Whether one views any specific observation as informative or not depends on the view of the nature of knowledge taken.

For example, it was argued that a naive inductivist, which comprises the view that the truth of knowledge may be guaranteed and that the truth springs from the facts (observations), will view all observations as informative. Alternatively, an a priorist, because of the belief that the truth of his knowledge is guaranteed by the yielding source, e.g. faith, views an appeal to observation as superfluous. Consequently, no observation would be deemed informative.

For naive inductivists: the set of all observations (temporally and spatially) is the information set:

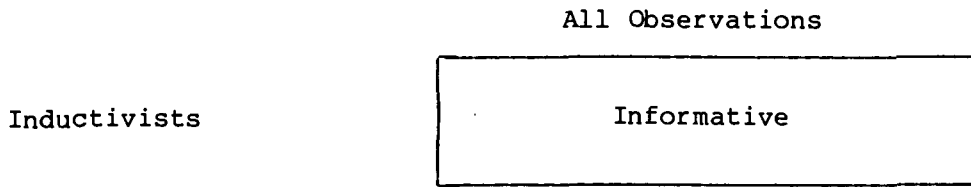


Figure 2.1

For A Priorists: the set of all observations is uninformative:

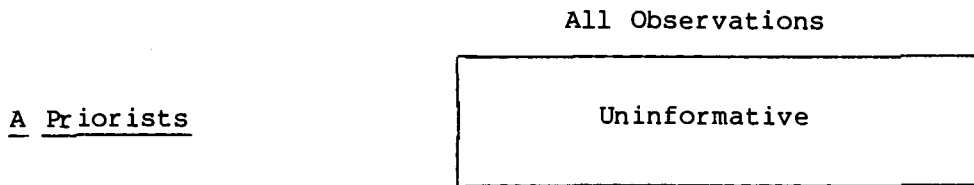


Figure 2.2

For Conjecturalists: The subset of all observations which refute a theory are informative and those observations which do not refute are uninformative:

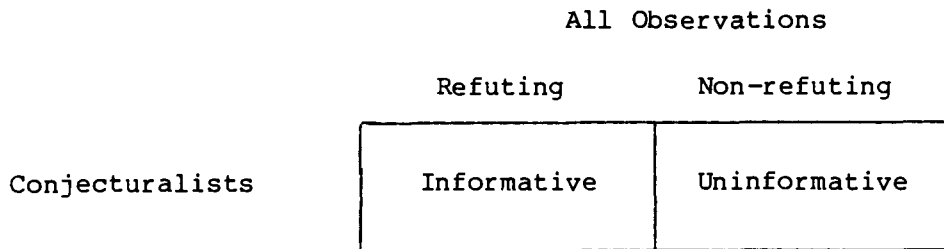


Figure 2.3

Finally, for Conventionalists: the subset of all observations which confirm a theory are informative and those observations which refute are uninformative:

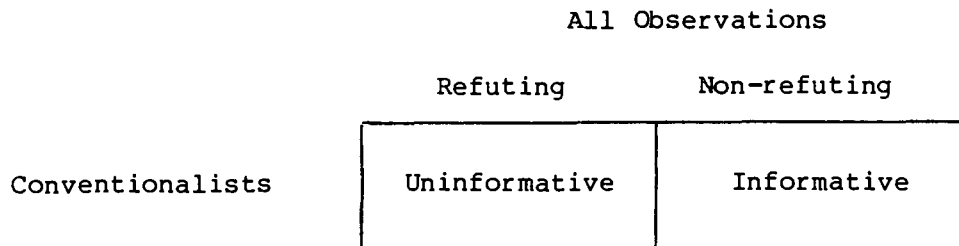


Figure 2.4

## CHAPTER III

Who walks with Beauty has no need of fear;  
 The sun and moon and stars keep pace with him;  
 Invisible hands restore the ruined year,  
 And time, itself, grows beautifully dim.  
Who Walks with Beauty D. Morton, [1886-1957]

From a wild weird clime that lieth, sublime,  
 Out of Space--out of Time.  
Dreamland, Edgar Allen Poe, [1845 st.1]

In Chapter II four distinct learning methodologies were introduced. Within a given methodology it was shown that one could specify which observations -- if any -- would be deemed 'informative' by anyone using that methodology. The purpose of this chapter is to use the previous definitions to show that observations on the prices solving static (that is point in time) general equilibrium (GE) models must be uninformative.

General equilibrium models are used by economists to explain individual and systemic behavior. Some economists [e.g. Hahn, 1973, pp.19 and 28] define equilibrium as an absence of learning and limit their equilibrium explanations to individual behavior which shows some 'regularity'. This is somewhat circular as an independent definition of 'regularity' is absent. I will mean by general equilibrium the view "...which presumes explicitly either that all markets are in equilibrium or that all individuals are maximizing. 'Equilibrium' [is] the continued existence of a stable balance [for stability one must give reasons, e.g. competition, why the balance, market supply

equals market demand, is not accidental] in the absence of any changes in the exogenous variables." [Boland, 1982, pg.81].

From Chapter II this implies that the task in this chapter -- showing that prices solving static GE models are uninformative -- is equivalent to showing

- (i) that the attainment of GE is consistent with a priorism, where no observations are informative;
- or (ii) showing that the attainment of a static GE sends price observations through the market(s) which do not refute conjectural hypotheses on these prices (confirmation tells a conjecturalist nothing);
- or (iii) showing that attainment of a static GE sends price observations through markets which are either (a) observations received after conventions have been satisfied (hence are superfluous and uninformative), or (b) observations which do not confirm the conventionalist hypothesis on these prices (refutation tells a conventionalist nothing).

The building blocks of the neoclassical general equilibrium framework [Boland, 1975] comprise three exogenously determined variables:  $D$ , market demand functions for the  $m$  outputs;  $R$ , the vector representing the  $n$  resource inputs, and a technology matrix  $A$  which specifies how inputs may be transformed into outputs. These determine the three endogenous variables:  $X$ , the vector of the  $m$  outputs;  $P$ , a vector of the  $m$  output prices; and  $V$ , a vector of the

n input prices. The task of the GE theorist is to show that there exist vectors of output prices, input prices and output quantities which are consistent with the initial endowments of resources, tastes and technology.

Walras [1874/1954] attempted to prove that preferences, inputs and technology were consistent with the market prices for inputs, outputs and quantities produced. His method of providing a 'proof of existence' or 'existence proof' was by a technique known as 'equation counting'. It was thought that if the number of independent equations was equal to the number of unknowns a solution of the unknowns would logically be forthcoming. However, it was well known by the late 1800's that equation counting could not be relied upon to prove the existence of a solution (i.e. the consistency among the endogenous  $P$ ,  $V$  and  $X$  and the exogenous  $R$ ,  $D$  and  $A$  variables). See, for example, H. G. Johnson [1971, pp.105-107].

Heavier mathematical weapons [e.g. fixed point theorems, see Varian, 1978] were brought to bear and the logical consistency of the endogenous and exogenous variables was shown to exist under certain conditions. This work was first done by A. Wald and many others were to follow [Boland, 1975].

Unfortunately, this mathematical method necessary to guarantee the 'proof of existence' of vectors  $P$ ,  $V$  and  $X$  consistent with the given  $R$ ,  $D$  and  $A$  logically denies provision of any time-related method whereby the existence can be shown to be non-accidental. As Clower [1975, p.10] puts it: "...this special case [long run neoclassical



equilibrium] is of no use for analyzing short-run disequilibrium adjustment processes; it is useful only for distinguishing between states of the economy that satisfy given criteria for long run equilibrium and states of the economy that do not." This does not make neoclassical economics timeless [Boland, 1978], however the 'equilibrium' proofs provided hold only at a point in time and there is nothing in the terminology or language of the proof which provides an explanation of attaining an 'equilibrium' in time. We only have proofs of consistency at a point in time (hence the use of the word 'static' at the beginning of the chapter).

Given the definition of an equilibrium above it is apparent that proving the consistency necessary via an existence proof is a very different matter than proving the existence of an equilibrium [Samuelson, 1947, Chapter IX]. Even though most theorists refer to the problem as one of proving the existence of an equilibrium. However, as we recall from above, equilibrium requires more than just market clearing (supply = demand in all markets); hence, more than just consistency or "balance" [Boland, 1982, pps.52-53]. "Equilibrium implies...(The) going (observable) price of a good is not an accidental price. It is not accidental because had it been higher or lower there would have been reasons for it to return to the balancing price." [Boland, op.cit.].

The resultant price vectors that 'solved' the GE system  $P$  and  $V$ , were said to be all the information "that is needed to keep all decision makers reconciled with a Pareto optimal state once it has

been established." [Koopmans, 1957, p.53]. But, to show that the balance 'established' is an equilibrium rather than an accidental balance of supply and demand in all markets one must provide some procedure consistent with the givens (in this case the vectors R and D and the matrix A) which motivates for individuals the chosen vectors P, V and X. However, this motivation is not inherent in the method of fixed point proofs.

Ironically, it is just this -- equilibrium -- of which "equilibrium" existence proofs do not prove the existence. Rather, they are "balance existence proofs" or "consistency proofs". This is not a semantic point. For, it is the failure to deal with the "reasons" for prices to move to their equilibrium values that distinguishes the "existence proof" approach.

Therefore, if one is to claim that the prices consistent with any balance are "equilibrium" prices one must also provide (or have provided) some theory of learning which explains why those equilibrium prices are not just "balancing" and/or "accidental" prices.

Given the success of the existence (consistency) proof approach how did theorists explain individuals obtaining the market determined prices in order to calculate their demands (D) and supplies (X)? That is, how do individuals in a static framework acquire the minimum information necessary for decision making? Further, how can individuals be assured that they have acquired the true price vectors, i.e. how can they be sure they will not be trading at 'false' prices?

According to Clower [1975, p.9] this information comes "...thanks to the freely provided services of a deus ex machina called 'the auctioneer'...the rationality of economic agents may be taken for granted, for price information is not only complete but also costless to obtain...". The guarantee that it is correct price information comes through an appeal to the source of the a priori knowledge in a subsidiary clause that prohibits "...trade until the equality of demand and supply is established for all markets. Hence during the process [the auctioneer adjusting prices to equate demands with supplies] endowments are constant." [van den Heuval, 1983, p.10]. Consequently, the only methodology consistent with both individualism and the guarantee ("proof") of the existence of an equilibrium short of the infinite-time long-run is an a priori methodology. This means that theorists have to implicitly or otherwise, e.g. figuratively through the auctioneer, ascribe to individuals enough knowledge (known a priori true) to assure an equilibrium. It is this knowledge - once known - that also assures the maintenance of equilibrium, if none of the exogenous variables change.

If sufficient knowledge is granted a priori then no observation will possibly contradict any individual's theory of the "world". This sufficient knowledge might take the form of the vector of equilibrium prices or entail complete knowledge of everyone's resource endowment, tastes and technology by everyone else and the computational ability to solve for the equilibrium prices and outputs. In either case the observation of the equilibrium prices

conveys no new or independent knowledge to individuals. They alight at the "correct" P, V and X combinations through any means consistent with a priorism. In fact, in the literature this process of trading at the true prices is given little consideration. Buyers and sellers are assumed able to find each other and carry out the equilibrating transactions costlessly.

In these static models purporting to prove the existence of equilibrium and deriving the price vector consistent with that position in less than the "long run", the so-called "equilibrium prices" convey no knowledge not already known. This is because no observation whether it be on prices, quantities or whatever, can refute true knowledge. And this is what agents have a priori.

This completes the demonstration of proposition (i) that static GE prices are consistent with prices arrived at through a priori acquisition and hence are uninformative.

The second proposition, that static GE prices may be consistent with a conjectural acquisition method may be illustrated as follows. If individuals are conjecturalist they view all knowledge (theories) that is as yet unrefuted as provisionally true. Conjecturalists believe there is no way short of the long run to ever prove theories true, even if one or more is true. Thus, they search out evidence contradicting their theories in order to provide refutations -- if possible -- of currently unrefuted theories.

Consequently, a static GE may be consistent with all individuals conjecturing the equilibrium relationships amongst themselves and the

natural givens. This would lead to the conjectured set of prices being realized and -- in that period (instant?) -- every individual would be optimizing. Market signals (equilibrium prices) then fail to refute any conjectures. In some terminology these signals are said to "rationalize" the conjectures. [Hahn, 1978]. If even one individual had conjectured a different set of prices there would have been a failure of conjectured trades (e.g. someone could not have bought or sold as much as they conjectured) and the ruling prices would have failed to have been equilibrium prices.

Consequently, static GE prices may be (felicitously) consistent with universal conjectural "success". However, that "success" -- failing to refute conjectures -- fails to indicate the failure of one or more of the various theories (if they are false) used to form the conjectures. The equilibrium prices are uninformative to a conjecturalist seeking refutation.

Finally, to show that static GE prices are uninformative through a conventionalist glass it remains to prove proposition iii. Static GE prices -- if they are uninformative and consistent with a conventionalist methodology -- must (a) be superfluous in that conventional "success" has already been attained, or (b) fail to confirm an hypothesis if it is still being judged.

One might think, given what was said about conventionalism in Chapter II, i.e. its failure to divorce itself from induction completely, that it might be difficult to speak of conventionally acquired knowledge in a static framework. This would certainly be

the case if conventionalists did not sacrifice their definition of equilibrium for the expediency of finding the 'best' theory given the conventions.

If one as a conventionalist believes all theories false then one is going to see an equilibrium as including some failure to maximize on the part of one or more, if not all, of the individuals. That is, there may be an "equilibrium amount of disequilibrium" [Grossman and Stiglitz, 1980] present. Or, as Hahn put it [1973, p.28]: "The traditional notion of an equilibrium...requires the equilibrium action of agents to be consistent, whereas I have the weaker requirement that they not be systematically and persistently inconsistent."

This will imply that 'equilibrium' [one hardly knows how to distinguish conventionalist equilibrium from conventionalist disequilibrium without conventions on 'systematic' and 'persistent'] prices will fail to confirm anyone's theory of the economy. After all, by definition those theories of the economy are known false so one would never expect (except accidentally or in the long run) quite the same set of prices as one's theory predicted. This is proposition (iii b) above.

Alternatively, if one observed prices within the 'equilibrium neighborhood' then one might be tempted to say that the prices are informative: they do confirm the hypothesis -- do they not? However, these prices are within an equilibrium neighborhood the radius of which has already been determined by some set of agreed upon conventions prior to this observation. If that set of conventions is

already, ex ante, agreed upon then this observation must be superfluous. That is proposition (iii a). Consequently, static GE prices are uninformative.

This chapter sought to show that in economic models featuring a static GE solution the prices arrived at fail to be informative to individuals. This could be for three different reasons depending on the learning methodology assumed to underly the attainment of the equilibrium. Short of the long run (and I take static to mean 'at a point in time' not necessarily the last point in time) there are three of the four discussed learning methodologies possible: a priorism, conjecturalism and conventionalism.

For an a priorist the equilibrium prices are uninformative as they fail to add anything to his already perfect knowledge. For a conjecturalist the equilibrium prices are uninformative as they fail to contradict provisionally-held theories; consequently, there is no error needing correction. Finally, for conventionalists, the 'equilibrium prices' are uninformative as they either (a) fail to (except accidentally) confirm one's theory or (b) are superfluous confirmations of already accepted conventions.

This refutes the commonly held view [Hayek, 1937/1948, 1945/1948; Koopmans, 1957] that equilibrium prices in the static GE model are informative. Under no learning methodology that I have considered will they be so.

## CHAPTER IV

Time will explain it all.  
He is a talker, and needs no questioning before he speaks.

Aeolus, Euripides, [frag.38]

Learn, compare, collect the facts!  
Ivan Pavlov, [1936]

I have argued in Chapter III above that if agents are in equilibrium, there is no informational role for the price system. In equilibrium, individuals observe the price signals which are consistent with those they expect to observe given their actions. Consequently, they learn nothing new about the economic system. Immediately following the Wald paper which for the first time rigorously proved the existence of an 'equilibrium', was the address published in 1937 by F. A. Hayek which partially addresses the 'stability' or "how equilibrium gets reached" problem but goes much deeper. Hayek's concern [pg.39] was with who has knowledge of the givens -- "observing economists or...the persons whose actions he wants to explain" -- and [pg.33] "how that knowledge is acquired and communicated." This relates to stability through the demonstration that it is the "acquisition of the (true) knowledge of the givens or facts (constraints, etc.) [which] is essential for any (stable) equilibrium." [Boland, 1982, pg.105].

As Hayek [1937/1948, p.45] said: "The statement that, if people know everything, they are in equilibrium is true simply because that is how we define equilibrium. The assumption of a perfect market in



this sense is just another way of saying that equilibrium exists, but does not get us any nearer an explanation of when and how such a state will come about."

Walras recognized and addressed the issue of the stability of a competitive equilibrium [Blaug, 1968, p.582]. By stability he meant that in a multi-market system if the price for one good was not an equilibrium price adjustments would take place in that market and others until all markets were balanced, supply equals demand, and all prices were at their "equilibrium" levels. He demonstrated stability "fairly completely for the two-commodity exchange economy." [Takayama, 1974, pg.314]. The multicommodity solution did not come for a few years after Wald's work.

The more complicated issue of 'propositions about the acquisition of knowledge' [Hayek, op.cit., pg.33] eluded solution by Hayek. However, it was not an issue picked up by subsequent theorists. Instead, Hicks [1946] and Samuelson [1947] concentrated their efforts on proving the "stability" of multimarket systems. This is most frequently done [Samuelson, *ibid.*, Chapter IX] by appending a difference or differential equation which relates the time rate of change of the price of good  $i$  to the excess demand situation in the  $i$ th market. For example, the time rate of change of the  $i$ th price is a function of the excess demand (or supply) in that market:

$$(1) \quad \frac{dP_i}{dt} = f_i(D_i - S_i) ,$$

with  $f_i' > 0$  and  $f_i(0) = 0$ . This may be generalized so that the change in price over time for any good is a function of all markets' ( $i=1, \dots, n$ ) excess demand characteristics [see, e.g., Takayama, 1974, Chap. 3]. Making time explicit led Samuelson [op.cit., p.260] to refer to a "theory of dynamics" hence the idea of 'dynamic stability' where the stable equilibrium is reached through time.

Technically, the proof of "truly dynamically stable" equilibria seemed to complete the Walras-inspired task. What Walras had started, years before the appropriate mathematical tools were available [Takayama, *ibid*, pg.277], had finally been completed. Further, Samuelson [1947] derived much more than just a tradesman's satisfaction at seeing an earlier master's design successfully executed. To him the relationship between the 'static' equilibrium and the 'dynamic' path consistent with it took on higher meaning. It -- as the Correspondence Principle -- shows "how the problem of stability of equilibrium is intimately tied up with the problem of deriving fruitful theorems in comparative statics." [Samuelson, *ibid*, pg.258].

For those interested in:

- (a) the individual precepts underlying neoclassical theory [e.g. Koopmans, 1957],
- (b) the logic of equilibrium and price taking behavior [Arrow, 1959a],

and (c) the testability ('deriving fruitful theorems') of economic theory [Boland, 1977a] satisfaction was somewhat harder to come by.

Koopmans and Arrow make highly related points. Koopmans [ibid, pg.179] asks: "If, for instance, the net rate of increase in price is assumed to be proportional to the excess of demand over supply, whose behavior is thereby expressed?" Arrow [ibid, pg.46] remarks that "when supply and demand do not balance,...the individual firms are in the position of monopolists.". These criticisms attack the consistency of a viewpoint which would assert that the given time paths of prices are optimal for all producers and consumers. This denies universal utility/profit maximization, denies equilibrium and highlights the market power held by some individuals.

More precisely, during the adjustment period, to be consistent with neoclassical theory and individualism the specific price adjustment function must be shown to be the result of everyone's optimization plans through time. Otherwise some individuals, those on the 'short side', have market power (monopoly and/or monopsony elements) and the perfectly competitive neoclassical world does not obtain. However, if the price adjustment is consistent with each individual's optimization plan (hence prices move optimally through time) then any 'disequilibria' in market  $i$  at a point in time, defined as demand for good  $i$  at time  $t$  not equalling supply of good  $i$  at time  $t$ , cannot be real; it must be only an 'apparent' disequilibrium.

From the vantage point of earlier chapters if price movement for

all  $i$  markets  $1, \dots, n$ , is tracing out optimal price trajectories through time then these must be equilibria of a now larger economic model. One where the previously given (a priori) tastes, technologies and endowments sets,  $R$ ,  $D$ , and  $A$ , are augmented by the inclusion (a priori) of given time-rate changes in some or all of the original givens. For example, initial conditions may be given on the given tastes, technologies and endowments plus a 'change in taste', or technology etc., with respect to time differential specified.

What results is a neoclassical model where at least one of the givens is time dependent. The price trajectories are tracing through continual equilibria "representing" individuals continuously optimizing. If this is not what results, that is disequilibria are real, then both Koopmans' and Arrow's criticisms hold: those on the 'short side' of markets have price setting power, and Samuelson's theorizing is non-neoclassical, ad hoc and counter to his own proposed intent [Boland, op.cit.].

In an 'expanded' neoclassical model the price trajectories reflect changing equilibrium relative prices over time. However, as these prices reflect only the information already embedded a priori in the expanded givens set they cannot be said to be informative in their own right. Information neither necessarily nor sufficiently follows from relative price changes alone.

As shown in Chapter III what is viewed as informative depends on the learning methodology employed. Milgrom and Stokey [1982] generate results where relative price changes are "purely

informational" however, this result follows from their implicit use of naive induction. For example [p.18] their definition of "a fact or an event is common knowledge among members of a group if it is known by each of them, if each knows it is known by each of them, if each knows that each knows that each knows that it is known, etc." depends on induction for its guarantee. There is also appeal to conventionalism in their view of the necessity for all agents to "agree about how this information should be interpreted" [p.20]. One would be rash to deny that prices are informative (in some sense) in this framework, but the heroic role of infinite time in providing the guarantee of equilibrium should not be overlooked.

Negishi [see Takayama, op.cit., pg. 345, note 2] sought to make a behavioral adjustment rule like (1) consistent both with real disequilibria and neoclassical theorizing by postulating (a la the 'auctioneer') that the rule was capturing behavior of a 'market manager'. The manager was to be seen as the "incarnation of the competitive forces in the market". However, this solves nothing as Takayama [ibid.] points out. The problem of explaining the manager and his behavior is somewhat similar to explaining the creation and behavior of any social institution [e.g. the auctioneer or governments and their monetary and fiscal policies].

Neoclassical theories of social institutions comprise the view that "one can explain any institutional setting and its evolution as merely the consequence of the logic of choice,...,our understanding of institutions is merely another example of neoclassical analysis."

[Boland, 1979, pg.957]. This means that resort to a 'market manager' may be consistent with neoclassical economics only where the expanded situation includes the market manager's preferences and presumably where his job depends on responses consistent with the utility/profit maximizing decisions of himself and the others. Otherwise, the 'market manager' is an ad hoc, non-individualistic device inconsistent with the neoclassical view, where his behavior is not explained within the neoclassical view of institutions [Gordon and Hynes, 1970, pg.372].

Clearly, though, Samuelson's effort in his Chapter IX was in the spirit of providing a representation of how a market would work in moving from one equilibrium to another. That it is not clear who is changing prices or whose behavior is being described might conceivably be overlooked if it is the "incarnation of competitive forces" that are of interest. This is closely allied to Hayek's question of "how experience (in the market) creates knowledge" [Hayek, 1937/1948, pg.47].

Starting from an existing equilibrium in the market for good *i* (the arrival at which is unexplained) some exogenous shock -- say a change in a consumer's tastes -- creates a difference between the existing level of supply of good *i* and the new demand for it. Hayek's concern was with how the market will incorporate the new information (change in taste) and how other individuals will learn of this new information. For those of us interested in explanation the source of the change in tastes should also be specified.

Samuelson's characterization of the 'incarnation of competitive forces' is that the now different supply and demand levels create pressure on price(s). In equilibrium, prior to the exogenous shock, everyone had been optimizing; now, post-shock, at the old prices at least one person's plans to buy or sell a certain quantity must be thwarted. Someone's theory of price and quantity is being refuted. Someone will end up with a different than planned (or expected) composition of the goods available. This creates pressure on price. For example, a frustrated buyer may offer to pay more per unit or a frustrated seller may lower prices. This is where Arrow's [1951 and 1959a] criticism applies as if one buyer or seller alters the price it potentially denies influence to the other agents. These changing prices inform others of the change in market conditions (change in tastes) and they learn -- from prices -- that the market for good *i* has changed. It is this story that price-adjustment equations like (1) are meant to formalize. Yet this information may take an infinity of time to disseminate.

One should remember that price movements are consistent with many explanations. Which explanation will be chosen is an arbitrary choice of the theorist [Parkin, 1982, pp.423-428]. I have shown above that the simple observation of a non-zero price change over time could be due to 'real' disequilibria described by a system such as (1). Alternatively, price movements may reflect different points on a time path of equilibrium prices. I have argued such a system might arise from the inclusion of exogenous time dependent variables. In

such a case system (1) might be altered to a form like:

$$(1'): \quad \frac{dP_i}{dt} = g_i \left( \frac{dT}{dt}, \frac{dX_i}{dt}, T, R, A, X \right), \quad g_i(0) = 0$$

Where,  $dT/dt$  is a time dependent taste variable,  $dX_i/dt$  represents other possible time dependent variables and  $T, R, A$  and  $X$  are the initial values of exogenous variables.

The issue here is that (as Samuelson realized) as soon as one specifies an equilibrium concept one also has included explicitly or implicitly the disequilibrium dynamics underlying its possible attainment. However, not every equilibrium is consistent with the implied time path of its attainment. The famous explosive cobweb [e.g. Henderson and Quandt, 1971] model is the usual demonstration of this.

The common example consistent with equilibrium in the market for good  $i$  being defined as "demand equaling supply" implies that disequilibrium is given by "demand not equalling supply". Consequently a system such as (1) is postulated to capture the market "reaction" to the disequilibrium and its working towards a new equilibrium. The Correspondence Principle specifies additional restrictions on the relative slopes of supply and demand curves. Without these restrictions there would be no guarantee that the definition of equilibrium and the dynamic path would be consistent with one another. Unfortunately, these additional restrictions may violate the independence of individuals in the market as supply and demand decisions are implicitly made interdependent [Boland, 1977b,



p.76].

Every equilibrium concept, and the associated behavior assuring stability -- thus satisfying the Correspondence Principle -- gives rise to some system describing disequilibrium time paths like (1). The only exception would be the claim [Alchian 1950, 1969/1970; Coase 1960; Demsetz 1969] that there can never be a disequilibrium situation. This argument contends that relative to conditions of knowledge, costs and etc. the best or optimal, hence equilibrium, decisions are being made continuously. This approach has been characterized as "explaining disequilibrium away" [Boland, 1982, pg.63].

In such a view there is no need for a Correspondence Principle. For, if prices were moving predictably according to equation (1) profits could be made exploiting that knowledge [Gordon and Hynes, 1970]. The process of exploiting that knowledge would lead to equilibrium. Consequently, the equilibrium view is that any movement of prices is along an equilibrium trajectory (describable by a system like (1')) which should appear "random" denying any arbitrage possibilities. There is little use for providing definitions of equilibrium if disequilibrium cannot exist. Given that people want to maximize profit and utility, this view sees one's job as a theorist as finding and describing the optimality of the constraints leading to whatever situation is observed.

Not all theorists have taken this view. For those that do not an explanation of disequilibria will always be necessary. Resultant

price movements will be attributed at times to disequilibria between supply and demand due to divergence between plans and realizations. These movements will be describable by systems like (1). However, "most models which include time-differential equations only guarantee a solution in the long run." [Boland, 1982, pg.102]. It takes an infinity of time before price movements die out after an initial shock. This means that even though we are able to say that the changing prices are informative -- there are individuals (conjecturalists and naive inductivists) learning from them -- we are not able to say when the new equilibrium will be attained short of the infinite time "long run". Therefore, it is impossible to explain the attainment of any equilibrium state [where everyone must be right to remain in equilibrium].

As Hayek saw it [op. cit., p.34]: "...before we can explain why people commit mistakes, we must first explain why they should ever be right." Here, short of the long run, that explanation cannot be guaranteed. Nor is this a problem with Walrasian models alone. 'Stability' analysis [Fisher, 1978, and Eckalbar, 1980] on 'non-Walrasian' models [see Chapter V for references] faces the same dilemma. That analysis requires the use of a Liapunov function to 'assure' stability.

Equilibria are defined as "Liapunov stable if for any real number  $\epsilon > 0$  and any  $t^0$  there exists a positive real number  $\delta$  such that

$$\|x^0 - x^1\| \leq \delta \text{ implies } \|x(t; x^0, t^0) - x^1\| \leq \epsilon$$

for all  $t \geq t^0$  where  $\delta = \delta(\epsilon, t^0) \dots$

In essence it says that if  $x^0$  is sufficiently close to  $x^1$ , then  $x(t; x^0, t^0)$  remains bounded for all  $t$ ." [Takayama, 1974, pp.348-349].

Resort to Liapunov techniques to ensure stability raises the same questions encountered earlier when considering Samuelson's dynamics. Whose behavior is being explained? Are the price movements the result of individuals' optimization? Is there a guarantee in any time less than infinity that the system converges to equilibrium? The answers, as earlier, are all in the negative. Additionally, there cannot be a guarantee -- even in the long run -- of equilibrium. There is only a guarantee in the limit that a "neighborhood" of the equilibrium is attained [Negishi, 1962, pg.641; Varian, 1979, pg.271].

This, naturally, poses two questions:

(i) to claim such a system stable do we have to consider the entire neighborhood an equilibrium?;

and (ii) by what criteria (i.e. conventions) does one choose radii in order to define the concept of 'neighborhood'?

If one has to accept the neighborhood as being the equilibrium state then one has at the same time admitted -- as equilibria -- points of supply-demand inequality. Any definition of equilibrium must exclude something, but the above excludes neither equalities nor inequalities of supply and demand in the definition of 'equilibrium'. Similar problems arise when one claims an 'equilibrium amount of disequilibrium' [Grossman and Stiglitz, 1980, pg.93] or "...is

satisfied with concepts of equilibrium and optimum that are defined in terms of long-run statistical averages..." [Radner, 1968, pg.58].

If one chooses -- for the sake of the technique -- to admit an 'expanded' equilibrium, answering 'yes' to (i) above, then the answer to (ii) becomes crucial. How much disequilibrium is 'equilibrium' in nature? That is, how far should the neighborhood extend?

For the purposes of theory the distinction between equilibria and disequilibria becomes meaningless until one is also provided with some arbitrary distance function serving as boundary between the true supply-demand disequilibria and the "equilibrium" supply-demand disequilibria. For purposes of this type of explanation it must be the case that some price changes are just "too small" to worry about [e.g. see comment cited by Rothschild, 1973, pg.1283] if we are to adopt this Liapunov-consistent definition of equilibrium.

For the purposes of the current discussion, though, we need not agonize further over this issue. At stake here is the fact that Liapunov techniques suffer the same problems as response functions described by equation (1). They do not necessarily describe optimal price setting behavior, they do not describe who is adjusting price and they take an infinity of time to guarantee the establishment of even loosely defined equilibrium.

As Boland [op.cit.,pg.102] writes: "To assert the existence of a long-run equilibrium when its attainment requires an infinite length of time is to imply either that time does not matter or that we have no explanation.". For explanation when time does not matter the

guaranteed success of people learning from prices is required. In this admittedly inductivist framework borrowed from Samuelson there is no way to provide a guarantee for the success of an inductive (naive or conventionalist) method short of accident or infinity [see Boland, *ibid.*, Chaps.1 and 4 and my Appendix to Chapter II.]. Hence, if the explanation requires infinite time then time cannot matter, that is, our explanation cannot be time dependent for success.

The converse, explanation when time does matter, requires the specification of a learning methodology which can guarantee one's knowledge true short of the infinite length long-run. This can be done as shown earlier in Chapter III. Unfortunately, that methodology -- a priorism -- is inconsistent with learning from market observations.

Price movements are consistent with any of the learning methodologies. However, the guarantee of stability, hence equilibrium, is only consistent with naive induction, a priorism and conventionalism. The system represented by equation (1) is an inductive routine where people are always wrong hence always learning from the disequilibrium prices. These prices are always informative to the inductivist. However, if that is the case we never have an explanation of why people would be right short of the long run. There is not a time-based explanation of equilibrium. Plans and actions will always be wrong short of the long run. There is no explanation of equilibrium prices shy of the in-the-limit long run. If the price movement is an a priori routine described by a system

like (1') people are always right, always in equilibrium. The price movements reflect the initial conditions and a priori specified changes in exogenous variables. There is no way people could be wrong. Prices, even though they are changing, are never informative.

There is no way that a conjecturalist methodology may be represented by systems like equations (1) or (1'). Those systems guarantee the truth of one's knowledge (i.e. through induction or a priorism) and in my view of conjecturalism one would deny that there is a guaranteed method of finding true theories. Consequently, any systems which lead inexorably to equilibrium whether through time or intuition (or faith etc.) must not be describing behavior consistent with conjecturalism except by accident.

There are no bounds on conjecturalist behavior, that is, one cannot specify auxiliary conditions which always imply prices rise when demand exceeds supply and that prices do not move when demand equals supply. Therefore, a time path of conjecturally formed prices might appear much more random than those given by equation (1). To that extent one might be tempted to ascribe (1') to conjectural behavior, where the arguments might be random functions of time. However, there need not be exogenous variables randomly distributed through time, e.g. tastes, which cause the movement in prices. A conjecturalist may change behavior even when anticipations are fully realized. That is, one may not act as before even when that action, under the same circumstances, was successful. This is because the

past may not necessarily be the true guide to the future.

It appears that there is little to be gained from employing stability analysis in the form of descriptions of price movements given by systems (1) and (1'). Such price movements if they are to be consistent with neoclassical individualism imply either

(a) prices and their movement are representable by the a  
priorism of equation (1') thus yielding no new  
information to individuals,

or (b) prices and their movements represented by equation (1)  
are systematically informative;

however if (b) holds there must be an absence of perfect competition hence not all individuals have an influence on prices. Left behind is the neoclassical world of individualist maximization.

More important, the additional cost of providing stability analysis through acceptance of the inductive view inherent in (b) is that there is no way -- short of infinite process -- to guarantee equilibrated plans and actions. Therefore, there is no consistent neoclassical method of explaining price levels or price formation in the first place. The cost of "informative" prices through this inductive price adjustment time-differential approach seems excessively high. It implies we have to accept induction as the only method by which people learn.

To paraphrase Hayek [op.cit., pg.34] looking at (1) as an inductive routine describes why people are wrong but never will explain how they could be right.

## CHAPTER V

Before I built a wall I'd ask to know  
 What I was walling in or out.  
Mending Wall, Robert Frost, [1914]

The previous two chapters may leave one (implicitly) dissatisfied with the non-explanatory role that equilibrium prices are assumed to play in most traditional neoclassical theorizing. Prices consistent with static -- point in time -- equilibria are consistent with three learning methodologies, but are uninformative (Chapter III). Prices, consistent with a guarantee through a priorism of attaining an equilibrium are also uninformative [Chapter IV]. Prices consistent with a guarantee through induction of attaining an equilibrium are informative, but an infinite amount of time is required before the truth is ultimately arrived at.

It was this certainty of knowledge which Keynes [1936, 1937] attacked in neoclassical theory. Much has been written on what 'Keynes Really Meant'. One very popular notion [Clower, 1965; Hicks, 1965; Leijonhufvud, 1968] is that Keynes' contribution is in the recognition that if prices are not performing their explanatory role, e.g. they are not infinitely flexible, then quantity adjustments, buildups or rundowns of inventories, must take place to complete the equilibrating process. Leaving adjustment up to



quantities, however, implies that some markets will not clear, supply will not equal demand at the ruling price, instantaneously and that the resultant quantity adjustments reverberate through time and markets (the 'multiplier' process) leading to temporary notional disequilibrium ('underemployment equilibrium' etc.).

Interesting as this approach has been, does it give any reason to believe that individuals may acquire and communicate their knowledge of the economic system through quantities more readily than through prices? That is, could this approach be more successful in answering Hayek's question than the analysis [i.e. that prices convey all the information necessary] it purports to replace?

Work in this vein continued at higher levels of abstraction throughout the 1970's [e.g. Benassy, 1975, 1976, 1982; Dreze, 1975; Hahn, 1977, 1978; Varian, 1977; also see surveys by Grandmont, 1977; and Drazen, 1980]. The main starting point was exogenous price rigidity and the resultant pressure on quantities to do the informational 'work' of generating an equilibrium.

While the initial idea of 'inverting price-quantity adjustment speeds' is appealing (if for no other reason than it seems to explain Keynes) there are several problems with the approach. These include

- (i) an explanation of how and why prices could be 'rigid' in the first place;
- (ii) if they can be rigid is this rigidity consistent with individualist (specifically neoclassical) principles, or is it imposed ad hoc?

There are basically two ways to assert rigid prices [by rigidity let me mean that prices are changing less rapidly than instantaneously and/or are completely fixed]:

(a) imposed from without [e.g. Benassy, 1982, p.9],

or (b) 'imposed' from within.

By imposed from without I will mean that the rate of change of prices is an exogenous given (e.g. technologically determined datum) and by imposed from within I will mean the rigidity is endogenously determined as a result of the maximizing choices of producer and consumer agents in the economy.

Before one can address these issues it may be helpful to start from a neoclassical representation of the issue(s). Without worrying about who is setting prices [Arrow, 1959a; Barro, 1972; Fisher, 1972] the neoclassical story says that it is the interaction of independent demanders and suppliers who through this interaction determine the market price and quantity transacted. The process (of price/quantity determination) takes place simultaneously and there is no unexpected increase or decrease in inventories. One could as well in these auctioneer-type stories think of quantities leading to price adjustment as prices leading to quantity adjustment. The reader will recognize the familiar Walrasian and Marshallian adjustment mechanisms here [Hansen, 1970]. That both adjustment mechanisms share a non-individualist ad hocery [Boland, 1977b] is secondary at the moment. The point to note is that (taking the familiar Walrasian neoclassical story) quantities adjust to prices until an equilibrium

is determined. Usually, the question of adjustment is moot as an equilibrium determination takes place out-of-time [Hicks, 1976].

Introducing the ad hoc price adjustment function equation (1) of the last chapter [Samuelson, 1947, Chap.IX] allows us (non-neoclassically) to speak of prices changing through time, therefore price movement, as a response to demand-supply inequalities. However, if supply and demand are unequal (relative to a point of equality) then inventory movement must be non-zero over some range of time. Again, the ranking of price/quantity 'velocities' does not necessarily lead to the automatic conclusion that quantity movement is zero per unit time and that price movement is infinite.

It would only be through an arbitrary fixing of quantities supplied (or demanded) that would allow (trivially) prices to be said to move faster than quantities per unit time. This may be a description of the (Marshallian) market period, but it need not be attributed to neoclassical theory in toto. Similarly, it is only through an arbitrary fixing of prices that one can ensure (trivially) quantities adjust (i.e. inventories change) faster than (fixed) prices.

However, before we look at rigidity as a matter of fact perhaps specifying how prices could be rigid should be done. Prices are terms of trade [Clower, 1975, p.15] between commodity pairs (where one of the commodities could be fiat money). Usually what is implicitly

assumed is that the goods are homogeneous, that exchange takes place centrally and simultaneously for all goods. If any of these assumptions are relaxed, then ceteris paribus something else adjusts to keep the terms of trade equal for any two goods. For example, if apples and oranges trade one-for-one we say the relative price of an apple is one orange and vice versa. Nominating a numeraire (say oranges) the price of one apple is equal to one. Let apples now become heterogeneous. Will we expect the relative (to the still homogeneous oranges) price to remain at one for all apples? The answer is that of course we would not. We would expect apples of 'low quality' to trade at a lower relative price (say 1/2 orange per one 'bad' apple) than apples of 'high quality'. The terms of trade, quality-adjusted relative price of apples, may still be one-to-one, but the relative price in terms of the numeraire oranges has changed. Some apples have a price of one and some have a lower price.

Suppose we had fixed the relative price of apples at one orange to one apple and then changed apple quality. One apple, good or bad, would be legislated to exchange for one orange (homogeneous quality). We would expect bad apples to "chase out" good apples (Gresham's "Law") and, as a result, traders of oranges coming to ask for other considerations on the non-legislated dimensions. They may, perhaps, ask for delivery of apples today and promise delivery of oranges tomorrow; they may make the buyer (paying with apples) pick up the oranges at a different site, and/or they may "squeeze" the oranges in order to surreptitiously alter (lower) their quality. In short,

unless one fixes every conceivable element subsumed by the phrase 'terms of trade' it is impossible to argue that simply by fixing relative prices one has made price (terms of trade) rigid.

But, one of the fundamental ideas behind quantity-constrained analysis denies the logical possibility of keeping the terms of trade rigid. That is, inherent in quantity constraints, where the 'short side' of the market 'rules', is the idea of some rationing scheme or perceived constraint etc. [Drazen, 1980]. Something is needed to inform buyers (if demand is greater than supply at the rigid relative price) that they cannot receive in total the quantity desired at that price. Similarly, sellers -- if supply is greater than demand -- need be told by some mechanism that some of them will be frustrated in their attempts to sell all of their goods (labour etc.) at the going price. It is these ration rules or constraints which alter the terms of trade (the prices) for the goods even though the relative prices are rigid. If the ration scheme is a simple "first come first served" rule it alters the terms of trade through a change in the transaction technology.

The argument here is that there is no way logically possible to make the terms of trade between goods rigid. Fixing every conceivable characteristic (e.g. quality, timing, location etc.) will not do the job. This is because if the constraints are to be perceived as binding their very 'binding-ness' alters the terms of trade (when relative prices are not rigid neither buyers nor sellers perceive constraint to trade). When buyers and sellers perceive

constraints the terms of trade have changed. If the quantity constraints are not binding (hence no change in the terms of trade) then relative prices need not be rigid in the first place.

If exogenous price rigidity is logically impossible (because for its success constraints have to be imposed which alter the terms of trade which by definition change prices) can endogenous price "rigidity" be possible. The answer [Alchian 1969/1970] is yes and, further, that too much price flexibility can exist. That is, there exists some optimal rate of price change or number of price changes per period and it need not be instantaneously keeping all markets cleared; consequently, the resulting apparent under or over-employment of resources is optimal and the ascribing of the 'employment' problem (which is apparent not real) to rigid prices is only a failure of researchers to specify the problem correctly.

It is not hard to come by examples of this type of approach. One need only introduce the costs of changing prices and the costs of holding inventories to generate a neoclassical model with finite price adjustment and quantity adjustment [see the appendix to this chapter] determined simultaneously. For our purpose it is of interest to know whether or not quantity signals should be any more informative than price signals. There seems to be no reason a priori to expect so.

To prove the existence of fixed-price quantity constrained equilibria [e.g. Dreze, 1975] it takes the same show of consistency as discussed in Chapter III. Once this has been demonstrated, that is

once balance has been attained in all markets, then the quantity signals are uninformative no matter which of the in-time learning methodologies (a priorism, conventionalism or conjecturalism) one chooses to employ.

The 'stability' analysis [e.g. Fisher, 1978; Eckalbar, 1980] which purports to guarantee an equilibrium suffers from the same non-individualistic approach as discussed in Chapter IV. As well, it introduces fuzziness to the concept of what an equilibrium is and what it is not. Further, it is analysis only consistent with induction or a priorism for its 'success'.

In short, there seems to be no advantage in attaching all importance to quantities in the explanation of how knowledge is acquired and communicated. In fact, it appears to be a route which represents a methodological sidetrack (if not derailment) as the fixing of relative prices does not and cannot ensure the fixing of the terms of trade between two goods. It seems difficult, if not impossible, to argue that prices -- terms of trade -- may be fixed. Consequently, while prices may not play the informational role in traditional neoclassical theorizing that has been ascribed to them this must be seen to be the fault of the learning 'dynamics' (induction and/or a priorism) embraced not in the nature of prices per se.

It is logically impossible to escape from prices whether they be explicit or shadow prices or in whatever form. Even if prices could be fixed, fixing all prices to force all adjustment onto quantities

would only explain true short run notional disequilibria as the result of non-individual (hence non-neoclassical) influence. Any fixing (or rigidity) consistent with neoclassical theorizing implies the disequilibria are only apparent, not real, as the rigidity is the result of optimizing by agents in the economy.

For our purposes the substitution of one signal for another is not a methodological advance especially when it seems to come at the expense of logical rigour.



## CHAPTER VI

As for a future life, every man must judge for himself between conflicting vague probabilities.

Charles Darwin, [1887]

In Chapter IV the existence of price adjustment was shown to be consistent with either disequilibrium -- where the movement is a response to an error which led to a supply-demand imbalance -- or with equilibrium, where the movement is consistent with plans being actualized through time. Empirical analysis, i.e. just observing price movement, cannot separate the issue further.

Theorists of both stripes could agree that a world with just one chain of future states was not a very likely picture of a world actual agents face. Though it could be. That is the degree of complexity is a matter of choice. We could choose to structure our roles in the system such that all uncertainty is absent. That we do not, indicates that the costs relative to benefits are perceived to be too high; as a consequence, "the economics of uncertainty" became identified with an approach due to Arrow [1964] and Debreu [1959] which attempts to formalize mathematically 'event uncertainty'. Where, as Hirshleifer and Riley [HR, 1979, pps. 1376-1377] define it, event uncertainty is the notion that agents are uncertain not about market prices but about endowments and/or productive opportunities. Goods now are specified not only as to date, location and physical characteristics but also their value (magnitude, size, etc.) given the 'event' or state. [Debreu, 1959, pg.99].

To set the problem out [following HR, op.cit., pg.1377] an agent needs to specify for every time period  $t = (1, \dots, T)$ :

(1) a set of acts  $a = (1, \dots, A)$ ,

(2) a probability function  $\Pi(s)$ , over the set  $s$  of states,

$$s = (1, \dots, S), \sum_{i=1}^S \Pi_i = 1,$$

(3) a consequence function  $c(a,s)$  showing outcomes under all combinations of acts and states, and

(4) a preference scaling or utility function  $v(c)$  defined over consequences.

Combining (1) - (4) above generates a consequence or "pay-off" matrix of dimension  $A \times S$  (at time  $t$ ):

	STATES				Utility of acts $u(a)$
	$s=1$	$s=2$	....	$s=S$	
$a = 1$	$c_{11}$	$c_{12}$	....	$c_{1S}$	$u_1$
$a = 2$	$c_{21}$	$c_{22}$	....	$c_{2S}$	$u_2$
.	.	.	.	.	.
ACTIONS	.	.	.	.	.
(a)	.	.	.	.	.
$a = A$	$c_{A1}$	$c_{A2}$	....	$c_{AS}$	$u_A$
Beliefs as to states	$\Pi_1$	$\Pi_2$	....	$\Pi_S$	

Table 6.1 : A Consequence Matrix

This differs from the certainty case discussed in Chapter III only in that the number of states is now larger than 1 (for each  $t$ ).

State contingent prices in equilibrium will be given by [HR, 1979, pg.1391]:

$$\frac{\Pi_s v' (c_s^i)}{\Pi_1 v' (c_1^i)} = \frac{P_s}{P_1} \quad \begin{array}{l} \text{for all agents } i = 1, \dots, K; \\ \text{for all states } s = 2, \dots, S. \end{array}$$

As an explanation nothing is gained over the model in Chapter III. There one had to explain at time  $t$  for the certain state  $S$  the  $m$  output prices,  $P$ ;  $n$  input prices,  $V$ ; and quantities  $X$  of the  $m$  outputs chosen as a function of the  $n$  resources  $R$ ;  $m$  market excess demands  $D$  and  $n \times m$  input-output (technology) matrix  $A$ . It was shown in Boland [1975] that such a model was uninformative.

The reason why static GE models do not provide informative explanations -- hence are uninformative -- is that the requirements for guaranteeing a solution and informative explanation are inconsistent. Informative explanation is the "explanation of the known by the unknown", this requires exogenously determined values ("unknowns") to explain endogenously determined values (the "knowns"). For proofs of existence using fixed point techniques explanatoriness requires only "logical entailments." [Boland, *ibid*, p.28]. These logical entailments do not try and cannot identify which set of variables is exogenous and which endogenous. All we know is that when the "proof" is demonstrated, the solution values of all of the variables are consistent with one another. At best we have a pure description.

Chapter III showed that equilibrium prices were perforce uninformative. This is because the only learning methodologies consistent with attaining equilibrium are those where either (a) enough information is provided each agent so that they may calculate prices, or (b) they are given equilibrium prices. These options can only be guaranteed by a priorism in the first case and induction in the second.

Adding additional states to the model does not change the basic technique necessary for proving existence [Radner, 1968, Sec.5]. The dimensionality rises but explanatoriness does not follow. One may look at the problem as one of showing  $S$  times (for every  $t$  in  $T$ ) that the  $R$ 's,  $D$ 's and  $A$ 's (RDA) are consistent with the  $P$ 's,  $V$ 's and  $X$ 's (PVX) given a probability structure  $\Pi(s)$ .

This means that in each state  $s$  the  $\Pi_s$ -weighted RDA and PVX values must be consistent. The only guarantee short of the long run that can be provided to assure knowledge sufficiency consistent with individualism and equilibrium is a priori true knowledge for all agents. It is of little surprise that Debreu [op.cit. pg.98] writes, "This ... allows one to obtain a theory of uncertainty ... formally identical with the theory of certainty...".

The unformativeness of both model and prices aside this approach deserves a closer look. It is in the provision of explicitly recognizing different events happening with some probability between 0 and 1 that is seen in some corners as an advance. However, there are questions which should be recognized

when the idea of stochasticism is introduced into one's view of the world.

There are two basic questions,

(i) where do the  $\Pi(s)$ 's come from?,

and (ii) what would constitute a refutation of a stochastic model?

The usual answer to (i) is that people learn through time, e.g. by having knowledge "passed on" to them [Lewis, 1981, pg.248] from previous generations. But this explanation is patently inductive in nature and cannot be defended as an explanation in time [see Chapter IV].

The answer to (ii) is simply that no observation could ever refute a stochastic model [Boland, 1982, pps.122-127]. Some states (events or observations) might be very unlikely, however their observance cannot refute the model as any observation taken in finite time serves to refine the probability estimate not 'refute' the stochastic approach. One may appeal to testing conventions to declare an observation so at variance with the model that the model is "refuted", but such an action calls for as lengthy a discussion of the 'correct' testing conventions. Testing conventions themselves are the product of an inductive procedure and as such are therefore still open to question [see Chapter II].

Any naive stochastic approach masquerading as a model of uncertainty relies totally on a priorism to push through equilibrium proofs. All the  $\Pi$ 's and RDA's for each state must be given a priori

to all agents to generate the equilibrium PVX's. Otherwise, some (non-individualistic) mechanism (e.g. the auctioneer) must be found to give the PVX's but this is a retreat from individualism.

As for the 'uncertainty' in a model depending on infinite

(a) time,

(b) computational ability

and/or (c) observations for its success:

"...whenever we can appeal to the Law of Large Numbers, we are justified in using the term 'practically certain'. This means, of course, that the uncertainty has somehow disappeared..." [Borch, 1968, pg.14]. The world of Arrow-Debreu [Debreu, 1959, Chap.7] is therefore "deterministic" [Nermuth, 1982, pg.29] and begs the information questions in the same fashion as the Wald-type model studied earlier.

If one argues that the  $\Pi$ 's are inductively acquired then one is arguing for an explanation where time does not matter. To paraphrase Boland [1982], either uncertainty does not matter because we must allow an infinity of time to acquire the true probability distribution; or we have no explanation of an equilibrium of individuals as we cannot explain how individuals acquire the probability distribution.

The implication that there is no uncertainty about market prices is that equilibrium prices, given each possible state, have already been arrived at and are known by all. The only process remaining is to make the terminal choice of action under fixed probability beliefs

over all states and times. As hinted at above [e.g. Borch, 1968] this is a relatively uninteresting problem.

Capturing the idea that probability beliefs as to states may be made endogenous -- rather than be fixed relative frequencies arrived at as the end result of a long-run process -- allows the introduction of informational actions. Informational actions allow the deferral of a final decision while the agent awaits or actively seeks new evidence which may reduce uncertainty as to the future state(s) [HR, pg.1378]. That is, the agent may 'learn' something which alters his view of the probability distribution,  $\pi(s)$  of future states.

The  $\pi(s)$  distribution becomes a function of incoming messages or signals which are acquired any number of 'different' ways, e.g. casual observations, systematic search, advertising, talks with others, [HR, pg.1378; Nermuth, 1982, pg.57] all of which amount to sampling in a statistical sense.

This allows a probability distribution to be constructed relating the distribution of the set  $s$  of states to the set  $m = 1, \dots, M$ , of messages. This is formally referred to as an 'information structure' [Nermuth, *ibid.*, Chap.1] and may be represented by:

	MESSAGES			
	m=1	m=2	...	m=M
s = 1	q <sub>11</sub>	q <sub>12</sub>	...	q <sub>1M</sub>
s = 2	q <sub>21</sub>	q <sub>22</sub>	...	q <sub>2M</sub>
...				
s = S	q <sub>S1</sub>	q <sub>S2</sub>	...	q <sub>SM</sub>
	S $\sum_{s=1}^S q_{s1}=1$	S $\sum_{s=1}^S q_{s2}=1$	...	S $\sum_{s=1}^S q_{sM}=1$

Table 6.2 : The State-Message Space

For finite state-message spaces the Cartesian product  $S \times M$  is also finite. A theory may be thought of as a conjecture regarding the relationship of any state  $s$  to the message space  $m$ . A model is a conjecture as to the specific nature of this relationship. For example, the theory may be that  $s = f(m_1, m_2)$ , the model that  $f$  is linear and the coefficients on  $m_1$  and  $m_2$  are between 0 and 1.

For finite  $S \times M$  the maximum number of theories represented by the structure in the discrete case are:

$$\#s \left( \sum_{x=1}^{\#m} \#m C_x \right).$$

Where  $\#s$  denotes the number of states in  $s$ ,  $\#m$  the number of independent messages in  $m$  and  $x$  is the number of messages from  $m$  to be considered. For example, for state-message space given by  $s=3$ ,  $m=3$  there are 21 possible theories being represented. Each  $s$  may be a function of all three messages, each  $s$  may be a function of two  $m$ 's (3



combinations each), and each  $s$  may be a function of one  $m$  (3 combinations each). In short:  $\#s=3$ ,  $\#m=3$ ,  $x=1,2,3$  yielding:

$$\begin{aligned}
 3\left(\sum_{x=1}^3 {}_3C_x\right) &= 3({}_3C_3 + {}_3C_2 + {}_3C_1) \\
 &= 3\left(\frac{3!}{3!(3-3)!} + \frac{3!}{2!(3-2)!} + \frac{3!}{1!(3-1)!}\right) \\
 &= 3(1 + 3 + 3) \\
 &= 21.
 \end{aligned}$$

The set  $m = 1, \dots, M$ , bears closer scrutiny. For example, in the world of Hayek, the price system is the only necessary social (non-personal) knowledge an agent needs. Therefore, given his own 'knowns' provision of prices leads to the revelation of market 'beliefs' as to the coming state. However, as we have seen Hayek was less than sanguine about how such an equilibrium would be reached.

Consequently, informational activity is the broadening of the message space to include information on others' actions, endowments, etc. As mentioned above this message acquisition -- informational activity -- is happening prior to the terminal decision date. Underlying each of these descriptions of a message must be a theory of how that message and state are related.

Thus, we have the familiar explanations of such phenomena as advertising and education -- in general 'signalling' [see Von Neumann and Morgenstern, 1944/1953, pps.50-54] -- as rational behavior when agents hold theories which indicate e.g. higher quality goods and inputs are associated positively with these attributes of advertising and/or educational attainment. Hence, holding that theory one expects

a state reflecting higher prices and quality for those goods and inputs if one sees beforehand that advertising/education 'signalling' behavior.

The information structure in the above form is perfectly general and allows for the representation of several common structures. The structure represents perfect or complete information if the message space is equal to the state space  $M=S$ , and for every  $s = 1, \dots, S$ , the 'correct' message  $m=s$  is observed with certainty,  $q_{SM}=1$ . With proper row-column transformations the information structure is an identity matrix signifying that the observance of a message  $m$  corresponds with certainty to the coming of state  $s$ . For perfect certainty this determines the values of the  $\Pi(s)$ . The  $\Pi(s)$  distribution takes the identical shape of the underlying distribution of the message distribution  $m$ . Determining the  $m$  distribution then would follow the same procedure where messages -- if they are not the elemental unit -- could be related to some underlying atomic or sub-atomic unit. One is obviously engaged in an infinite regress arguing in this fashion. Resort to information structures does not explain certainty -- it just shows more clearly the re-naming routines involved between the distributions of states, messages and more elementary 'causes'.

The case of no information may be represented by a message space of only one member. That is, there is only one message observed with certainty, independent of the true state  $s$ . Consequently, the message  $m$  conveys nothing about the possible state.

The only guarantee one would have in order to believe in an

information structure whether it be one of perfect certainty or its converse or in between -- would be if one believed one's knowledge a priori true or if one believed one had infinite time and observations to validate inductive proofs. This being familiar ground I shall not churn it further.

The potentially interesting case is when one wishes to revise one's probability estimate of forthcoming states. This requires either more time to acquire more observations on states thus potentially changing the  $\Pi(s)$  relative frequencies or an appeal to additional messages prior to revelation of a state.

Wishing to revise one's estimates implies one is unhappy with the current model of the theory underlying the message-state relation not the theory itself. Presently, economic theorists utilize two basically similar model revision techniques: classical and Bayesian statistical estimation [Intriligator, 1979; Savage, 1954/1972].

Classical technique is basically one of appealing to current sample evidence to derive either relative frequencies or more generally the estimates of the parametric relationship between, in this case, state  $s$  and messages  $1, \dots, M$ . Bayesian technique differs only in that past (prior) information is augmented by current data. In the limit they converge, as the current sample size becomes larger the lower is the importance of the prior data [Holloway, 1979, pg.323].

Recalling Bayes' Theorem [and our discussion of it from the Appendix to Chapter II] and re-labelling:

$$\Pr(\text{state}=s | \text{message}=m) = \frac{\Pr(m|s) P(s)}{\Pr(m)}$$

$$\text{and } \Pr(m) = \sum_{s=1}^S \Pr(s) \Pr(m|s).$$

If one had absolutely no prior evidence but believed states  $s = 1, \dots, S$  and messages  $1, \dots, M$  were the only possibilities then the first-round estimation is done by assigning the prior distribution over  $s$ ,  $\Pr(s)$  uniformly. This gives equal probability to every state  $s$  equal to  $1/\#s$ . Observance of sample information weighted by the prior yields the posterior distribution,  $\Pr(s|m)$ . These are the elements  $q_{SM}$  in the information structure matrix.

Once 'enough' data has been considered one is left with a set of models of the underlying theories of state-message determination.

That is, the  $q_{SM}$  are defined 'precisely' enough. Consideration of more sample data refines the  $q_{SM}$ , that is, refines the model.

However, this says nothing about theory revision [Boland, 1977c].

The construction of state-message spaces allows for model revision but not theory revision.

To this point the question of how and why observations are acquired has been ignored. The first task was to show the limit put on the number of theories and the non-congruence of model vs. theory revision inherent in the use of information structures carried forward from neoclassical theorizing. Presumably agents seek out additional

data (information) due to dissatisfaction with their current information structure [Stigler, 1961].

If information were costless there would be no conceivable limit to observations. Agents could derive the 'correct' relative frequencies. If they have misspecified the state-message space i.e., suppose a message is omitted (they hold the wrong theories), there will not be a "perfect information" solution. As defined earlier that is where each and every  $s$  is associated with a single and different  $m$ . However, given their theories with costless information the models are "perfect". There is no further room to maneuver. There is no underlying Bayes' Theorem for theory revision, that is throwing out some  $m$  and/or  $s$  and adding others.

Information is most often not costless. This leads to the obvious treatment of information as a commodity transacted through markets. However as Arrow [1962/1971, pps.150-152; also Newman, 1976] points out the returns to information are not fully appropriable, information is subject to increasing returns in use and information is not a divisible commodity.

Other lacunae include (1) the logical problem of primacy of information prices. If the information market provides -- at a price -- information on other prices (through state likelihoods) then what market provides information on the information market? This problem is very much like the logical problem of how a market for market forms would arise and what characteristics it would have [Arrow, 1969; McManus, 1975]. (2) How does a seller of information "exhibit his

wares" without giving them away?

This chapter deals with "the economics of uncertainty" as it is traditionally presented. The following points are of interest: the Arrow-Debreu representation of uncertainty is logically equivalent to certainty theory, therefore state-contingent equilibrium prices are uninformative since they are uninformative under certainty; expanding the information structure and suspending time, allowing non-terminal acts, fixes the number of conceivable theories; the revision process revises models not theories; revision processes are inductive hence their success depends on unlimited time. However, even unlimited time will not generate true theoretical knowledge if the information structure omits the true theory ab ovo. Finally, treating information as bits transacted through the market while consistent with inductivist methodology takes us out of a neoclassically consistent framework as there are increasing returns (non-convexities) and non-divisibilities introduced among other logical problems.

## CHAPTER VII

He hath indeed better bettered expectation  
 Than you must expect me to tell you how  
Much Ado about Nothing, Shakespeare  
 Act I, Sc.i, l.15

In Chapter VI the basic neoclassical model of uncertainty was presented. There, as in earlier Chapters, the results are (i) that equilibrium prices are uninformative, (ii) disequilibrium (as opposed to changing equilibrium) prices inform at least one agent of the incorrectness of his model and (iii) the process of specifying a 'disequilibrium dynamic' in effect makes price movements describable as equilibria through time thus all that is being described is "dynamic" equilibrium behavior. This is because the appending of any price adjustment equation or system describing price movements implies an 'optimal' adjustment process. This makes all disequilibria only apparent not real and denies an informative role to prices.

Under event uncertainty the dimension of the problem was increased to reflect recognition that, looking into the future, agents admit the possibility of more than one mutually exclusive event. I argued that the finiteness of events (states) and messages fixed the number of conceivable theories an agent could hold, that there was no a priori reason to believe that the true theory was included in the given set, and that as a result any revision -- whether Bayesian or Classical in nature -- is only of models not theories.

This last point is important enough to warrant closer

consideration. Imagine the following situation: a single agent with demand given as a function of price:  $D=f(p)$  and facing one of two possible states in the following period, H, 'high supply' and L, 'low supply' respectively. Given a single message,  $m=H$  for high supply, or  $m=L$  for low supply; let the beliefs as to states be given by:

		message	
		H	L
state	H	1/2	1/2
	L	1/2	1/2

There is a uniform prior distribution over the probability of event  $s=H,L$  given message H,L.

The expectation of the equilibrium price,  $p$ , in states H and L is given by inverting the demand function once the expected supply,  $x$ , is known:  $E(p) = f^{-1}(x)$ . Since the priors are uniform the expectation of supply is given by:  $x = 1/2 (X_H + X_L)$ .

For the agent once the problem is specified there are explicitly only two theories of prices (through states): one generated by the relationship of state H to  $m=H,L$  and one generated by the relationship of state L to  $m=H,L$ . Initially, to capture the idea of no information, uniform priors were assigned which specified the two 'general' theories:  $\Pr(S=H \mid m=H \text{ or } m=L)$  and  $\Pr(S=L \mid m=H \text{ or } m=L)$ .

The four models of these two theories are given by:

$$\Pr(s=H \mid m=H) = 1/2 \quad , \quad \Pr(s=L \mid m=H) = 1/2$$

$$\Pr(s=H \mid m=L) = 1/2 \quad , \quad \Pr(s=L \mid m=L) = 1/2.$$

The only "learning" for the agent is to compare the priors (the specification of the models) to experimental evidence (or



'experience'). This is Hayek's 'learning from experience'.

Observational data lead to the 'correct' model of the theory.

Suppose the following  $n$  observations are of  $m=H$  and  $s=H$ , then surely  $\Pr(s=H|m=H) \geq 1/2$  and  $\Pr(s=L|m=H) \leq 1/2$ . The parameters of at least two of the models should change in the indicated direction; how great the change depends on the review process used. As more and more sample data is assessed -- i.e. in the limit -- the "true" models are revealed.

In the case of perfect information -- certainty -- the result may be revealed as:

		message	
		H	L
state	H	1	0
	L	0	1

That is  $\Pr(s=H|m=H) = 1$  etc.

Under perfect information the two prices given the two states given a message  $m=H$  or  $m=L$ , follow from:  $E(p) = p = f^{-1}(x)$  for  $m=H$  and  $m=L$  respectively. Clearly,, if  $m=H$  implies  $s=H$  with certainty,  $E(x) = x_H$  and  $p_H = f^{-1}(x_H)$ . Similarly, when  $m=L$  implies  $s=L$  with certainty,  $p_L = f^{-1}(x_L)$ . See Figure 7.1.

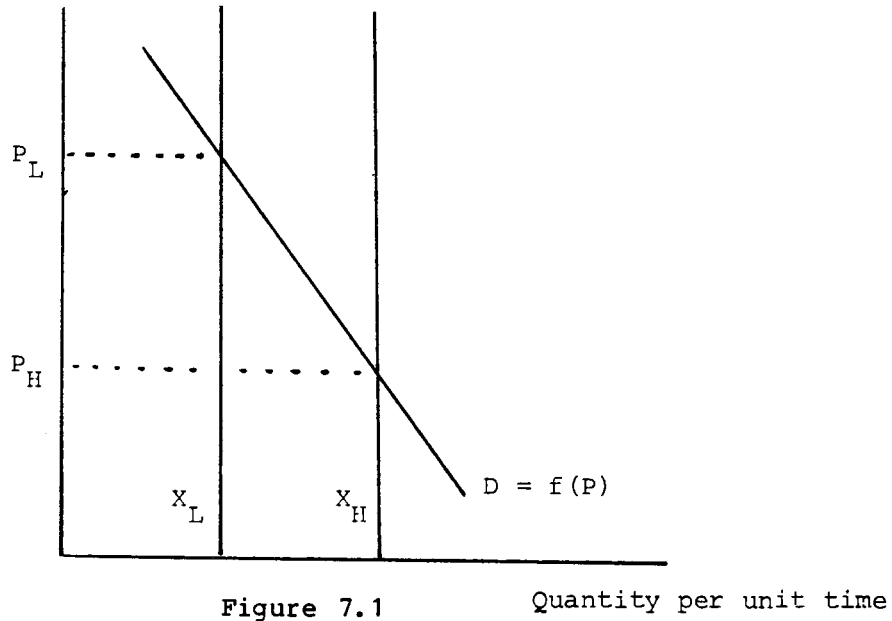


Figure 7.1

Quantity per unit time

Certainty, by its nature yields no surprises. Clearly, some conditional probabilities may differ from 0 and 1 even in the limit. In the limit one could ascribe any deviation from 0 or 1 to a false theory, however as an agent dealing through time one never knows if and when the limit has been reached. Consequently, viewing non-zero and non-unitary probabilities in time (not in the limit) may be attributed to one of two causes: (a) poor data collection, and/or computation, leading to model misspecification which, even though the underlying theory is true, leads to less than certain probabilities; or (b) false theoretical knowledge.

An example of (a) would be mistaking message L as H, expecting  $p = p_H$ , encountering  $P_L$  and attributing 'message' H to state L. An example of (b) might be that in addition to being a function of price the demand is unknowingly also a function of some parameter  $z$ . Consequently, if  $m=H$  and simultaneously (the unobserved)  $z$  changes

shifting  $D$  to  $D'$  then  $m=H$  might mistakenly be associated with  $p=p_L$  and, mistakenly state  $L$  will be thought to have obtained. See Figure 7.2.

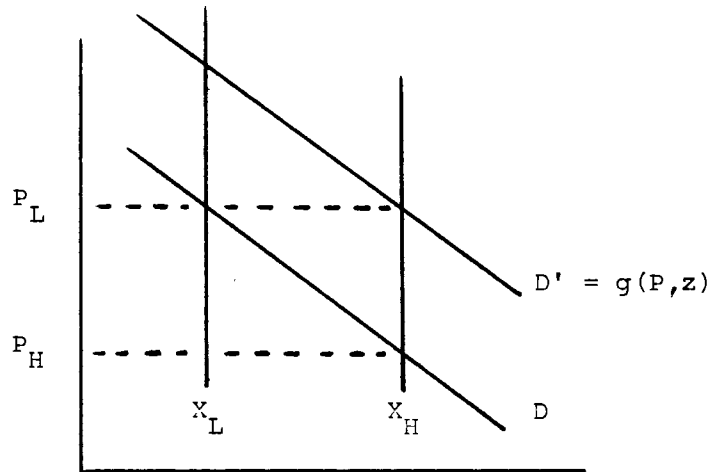


Figure 7.2

This will also confound  $m=H$  with (perceived) state  $L$  so the information structure will be less than perfect.

Both introduce a semblance of 'randomness' into the world as perceived by the agent who believes his theory to be true. Seeming refutations may be blamed on poor data collection technique, computational errors, and/or lack of data. The problem may even be seen as one of not having the various moments of the stochastic process calculated with enough precision. As pointed out earlier [see Chapter VI] this insulates theories from ever undergoing revision. The reason why theories need insulation, though, is not because actual agents never attempt theory revision, but because the finiteness of the message-state space limits the theories an agent may hold.

The 'economics of uncertainty' dealt with in Chapter VI and above might be summarized in the following way: agents' models (or expectations) of states are a function only of their own information which once the non-terminal acts are completed lead to a trading or terminal act where Walrasian 'higgling' leads to state-contingent prices.

This analysis has been extended in a fundamental fashion [see e.g. Jordan and Radner, 1982]: agents may also condition their expectations on what they think other agents know. The only way any agent  $i$  has of finding out what others know, short of collusive or other patently non-competitive behavior, is through a theory of how other agents' information becomes embedded in prices. Then, a reading of the price vector should reflect, given the true theory, what others think they know about the coming state, hence allow agent  $i$  to revise his estimate of state likelihoods. This revision, of course, may feed into other agents' expectations and cause a further change in prices. An equilibrium of such a process would be one where the market clearing prices do not contradict agents' forecasts of prices, their forecasts of others' forecasts ad infinitum [Radner, 1979; HR, 1979]. This recognition that agents may revise beliefs upon viewing endogenously determined magnitudes -- most often prices -- has been termed the Rational Expectations Hypothesis (REH).

What one notices in much of the recent microeconomic REH literature is a refreshing self-awareness that it is a research program that has no chance of success if one's aims are to "...save

rational expectations as an appropriate equilibrium concept in economics and to establish its consistency with the standard general equilibrium models..."[Allen, 1982a, pg.245]. Of course, much of this candor is due to the proofs of the non-existence of informational equilibria under non-pathological conditions [Green, 1977; Kreps, 1977; Grossman and Stiglitz, 1980]. The response to these proofs has been instructive.

Radner [1979] showed the 'generic' existence of rational expectations equilibria. The term 'generic' means that results (in this case existence): "are true for 'almost all' -- but not for all -- possible specifications of (the) model. ...Intuitively, genericity means that if (a result) is true for a certain parameter configuration, then it remains true if the values of the parameters are changed a little; and if (the results) should happen to be violated, then an arbitrarily small disturbance in the parameters is enough to restore its validity. To the extent that there is always some inaccuracy in the data (errors of measurement, etc.) one may therefore assume (the results) to be "always satisfied" [Nermuth, 1982, pps.186-187].

From the standpoint of what is being explained -- equilibrium prices -- it would seem that the resort to generic proofs comes at the cost of admitting the same arbitrariness admitted earlier in stochastic models [Chapter VI]. There it was shown that a retreat to stochasticism insulated a model from refutation. With "genericity" the results are assumed to be "always satisfied" even though for some

specification of the model they are not. The only further comment is that for any individual we as theorists are arbitrarily specifying that his model is a true description of the equilibrium process when the actual prices he faces are different than he conjectured they would be. This is an implicit retreat to stochastic equilibria where the theorist forfeits the process of agents' learning -- in fact we make him ignore refutation -- to provide some description of an equilibrium.

Allen [1982a,b] takes a slightly different approach. Realizing explicitly that the old concept of equilibrium must go [Allen, 1982a, pg.245] she instead studies "strict rational expectations approximate equilibria." [Allen, *ibid.*]. Of course, any appeal to approximationism meets with the (previous) criticism of what behavior could conceivably refute a model of this form.

REH approaches are popular because they embody the notion that different traders have different information either about the items to be traded or the coming state. Of course, the ex ante provision of information differentials implies the provision of market power across agents. Allen [*ibid.*] and Nermuth [*op.cit.* pg.3] recognize this. There is no attempt to explain the choice of "how much" of a specialist to become. Information differentials -- as illustrated by the small amateur traders vs. large, experienced institutional traders [Allen, 1982b, pg.20] -- may arise as a result of choice, hence be a competitive rather than non-competitive result. Further, these information differentials must be infra-marginal not differentials on

the margin. In equilibrium everyone has to have the same information as it is embedded in the equilibrium prices. If market power is a matter of arbitrariness on the part of the theorist we have then imposed a non-competitive, hence non-neoclassical, structure.

Theoretical arbitrariness alone explains the result.

Border and Jordan [1980] and Hellwig [1982] derive existence results as a function of allowing conditioning on past but not current market prices. This lag in forming expectations (learning) is a function of "processing" [Hellwig, pg.281] and "publishing" [Border and Jordan, pg.396] delays and the second set of authors feel it to be "unnatural" [ibid.].

However, if an agent believes his information to be superior to others then he will pay for the opportunity to exercise its use prior to revelation by the market, i.e. utilizing it infra-marginally. That is, agents will pay to have market forms which preserve their perceived information advantage until they have acted upon it. Consequently, rather than being exogenous, the type of markets and transactions an agent makes should be endogenously determined [Hurwicz 1959, 1973]. This may mean that one might never expect to see a neoclassical equilibrium (where all individuals have the same information -- i.e. market equilibrium prices) exist. This implication is very similar to the result of Milgrom and Stokey [1982] where they show that differential private information will lead to a "no-trade" result if everyone has rational expectations. That is, if one knows that people only trade to better themselves and 'everyone

knows everyone knows' etc., then willingness on the part of someone to trade with you must mean he knows something you do not and therefore you would do better not to trade at all rather than allow the other person to utilize their private information at your expense. Trading is therefore not an equilibrium phenomenon.

Most real world market forms may be looked at as different ways of allowing agents to exercise their information. English auctions differ from Dutch auctions and sealed-bid forms of transactions are different than verbal bidding. The necessary assumptions to make are either (a) that different goods have different information revelation characteristics or (b) that different individuals believe they have "better" information than others. Hence different goods may be traded through different market forms.

The above descriptive comments are aimed more at motivating a more 'natural' reason for past-price conditioning than at providing a more general theory of markets, one where the market forms are themselves endogenous. From a theoretical point of view the problem while interesting, calls for the existence of a market for markets and the ascribing ad hoc of a priori information attributes across commodities and/or information detecting abilities (or desires, e.g. risk aversion) across agents.

Market forms aside, Border and Jordan [1980, p.396] conclude that the "general existence of an equilibrium...cannot be assured for any nontrivial previous market data." Nontrivial meaning in their sense a function which is not a constant function. This result



contrasts with Jordan [1977,1982] where conditioning on current prices allowed existence to be shown for non-constant functions as well. However, if agents condition on current market variables, it is harder to explain the information difference.

When the information difference collapses and when the noise is small it becomes very hard to explain equilibria. Under these circumstances as Hellwig [1980, pg.491, also see Verrechia, 1982] remarks: "...the communication process simply is not well defined." This is a situation where one is back "explaining" price formation through the a priori provision of true theoretical knowledge or non-individualistically via the auctioneer.

Work on the stability of rational expectations has fallen into two main frameworks [see Blume, Bray and Easley, [BBE], 1982]. The difference between the two is whether agents' likelihood functions over the structure of the economy are correctly specified or not.

It is not surprising that results in the first case are 'positive'. Both Friedman [1979] and Frydman [1982] among others [see BBE op.cit.] find that in the limit using correctly specified likelihood functions leads to stable rational 'learning'. Frydman's result depends on agents knowing the 'average opinion' [Frydman, *ibid.*, pg.654]. The average opinion is defined as the average of forecasts formed by the other agents. The computation and dissemination is done by a non-market agency.

Friedman [1979] studies a case where expectations do not affect the observed variables and those variables agents wish to predict.

In effect the relationship between predicted and observed is exogenous and, with correct likelihood ratio, the agent needs only wait for the evidence to lead him to the correct expectational form. However, the a priori knowledge requirement is "extraordinarily demanding" [BBE, pg.314]. As well, the long run nature of the results moves Friedman [1979, pg.40] to conclude the results non-surprising and Frydman [pg.664] "that the possibility of convergence...appears to be remote."

When using models where the likelihood functions, i.e. structure of the economy, are not correctly specified negative results follow. Blume and Easley [1982] show that with a finite number of learning models even in the long run the 'wrong' model might be stable if the correct model is not in the finite set [parallel to the argument in Chapter VI]. Other results [e.g. Bray, 1982] show convergence but rely on long run induction [as does all the work cited in BBE, op.cit.] implying that these are all time independent explanations.

In a paper not cited in BBE, G. Fuchs [1977, pg.316] sums up the rational 'stability' question thusly: "...The interpretation of our result then appears more clearly: it means that, unless a very high level of global information is attained, attempts by the agents to learn something useful by comparing their forecasts to the actual dynamics have no chance to succeed...it exhibits how limited probably are the possible outputs of dynamical learning procedures."

When rational stability results work it is because agents know a priori much more than they practically can, and/or because the results follow from the use of limit analysis.

## CHAPTER VIII

...behold, I cast myself in faith upon conjecture, not knowing the facts...

Edwin Abbott, Flatland, pg.105

I was gratified to be able to answer promptly, and I did. I said I didn't know.

Mark Twain, Life on the Mississippi, Ch.6

The different models of neoclassical economics looked at to this point share common features. Early equilibrium work featured a given set of endowments and preferences, then showed that there existed at least one price vector which was consistent with an 'equilibrium' of the system. The 'economics of uncertainty' generalized that approach by adding multiple future possible states and a given probability distribution over their likelihood, and exhibited existence results. REH theorists added an awareness that any one agent's likelihood function may depend on the other agents' likelihoods and showed, given a set of response mechanisms, the 'generic' existence of equilibrium.

The common view shared is that agents may somehow know their givens to be true. That is, the view that equilibrium existence is assured by individuals' knowing with certainty the set of givens. This fundamental belief as to the truth status of agents' knowledge and their relationship to their knowledge -- i.e. that they know they can know it with certainty -- is a result of the theorists' epistemology. This particular epistemology, the theory of the nature of knowledge, viz. that guarantees of the truth of our knowledge exist, leads these theorists implicitly to ascribe methodological

positions, theories of learning or the knowledge acquisition process, to agents which are either a priorist or inductivist in nature.

A priorism is the epistemological view that knowledge is guaranteed by resort to faith, intuition, or authoritarian dictat. This depends on higher and higher order a priorism or successful inductivism to ensure that the source, e.g. authority, is knowledgeable. Inductivism fares little better in that it assures the above epistemological view by appeal to a long run collection of 'facts' which ultimately -- or in the limit -- leads to guaranteed knowledge. Any deviation from equilibrium is dealt with by receiving new knowledge either directly or through an exogenously supplied and guaranteed correct inductive response mechanism. This "...view which considers knowledge or its acquisition to be exogenous will not permit an explanation of the endogenous dynamics of a rational decision process." [Boland 1982, pg.185]. Finally, it was on this basis that I argued in Chapters III, VI and VII that the role of equilibrium prices could not be considered informative as they always send the same redundant signals as the underlying exogenously provided true knowledge. In Chapter IV I argued that if prices are 'disequilibrium' prices leading agents to respond in an equilibrium-response fashion these prices might only be considered informative when one's view is that learning is a process that has an end. That is, one can know with certainty that one knows the truth and that guaranteed knowledge is the result of collecting the necessary 'facts'. Enough 'facts' in the form of enough changes in prices ultimately leads to equilibrium and therefore to the truth.

I have also argued above that in any framework employing inductive technique, based upon the above belief in knowledge and learning, the only modification done by agents is to models of the originally specified -- i.e. given -- theories. These theories include individual's views of their endowments, preferences and possible future states of the world. I argued this on the basis that existence work becomes murky at best when sets of information signals or messages are infinite dimensional [Radner, 1979, pg.677]. Attempts at loosening this constraint [e.g. Allen, 1982a,b] perforce resort to employment of non-neoclassical structures.

These issues might be considered academic at best by some, however I will show in this Chapter that the methodology employed by individuals may have implications for their observed behavior. To explain why any individual chooses any given learning theory over others requires on the part of the theorist assumptions about how one learns about, or from, learning. This requires assumptions about the relationships between the aims, truth status, epistemology and methodology of agents. This will lead to effects on the allocation of resources. For example, to illustrate the behavioral implications of the neoclassical inductivism discussed above consider the following familiar representation of individual trading (see Figure 8.1).

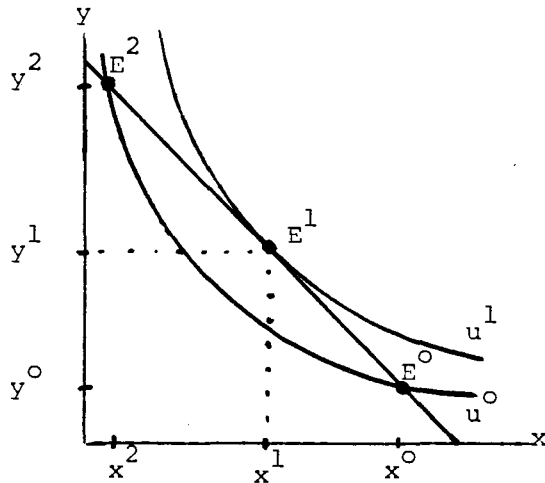


Figure 8.1

The individual perceives his problem situation to be one of maximizing utility  $= u = u(x,y)$  subject to the resource endowment constraint,  $x^0, y^0$ , and given market prices  $P_x$  and  $P_y$ . The usual story has the optimization worked out such that  $x^0 - x^1$  of good  $x$  is traded for  $y^1 - y^0$  of  $y$  and a reallocation of resources takes place through trade such that the individual moves from  $E^0$  to  $E^1$  and attains a utility level  $u^1$ .

As mentioned earlier [Chapter III], such trading usually takes place out-of-time. Making the move time-based requires a response or trading rule. One rule consistent with inductivism would be the Edgeworth process where individuals only make utility-improving trades [Takayama, 1974, pg.344]. To guarantee the move from  $E^0$  to  $E^1$  will require allowance of an infinity of time as only then have we the assurance that enough utility improving comparisons can be made via trading to guarantee  $u^1$  as the utility maximum. If one traced the excess demand function for  $x$  through time it would appear as if the (negative) excess demand for  $x$  is slowly being eliminated through utility-improving trades. Prices do not change as I am using the

neoclassical assumption that the individual perceives himself too small relative to the market to affect prices especially when trades made are infinitesimally small. Of course, he may be wrong about this. Graphically, the excess demand for  $x$ ,  $ED(x)$ , as a function of time might look like (Figure 8.2):

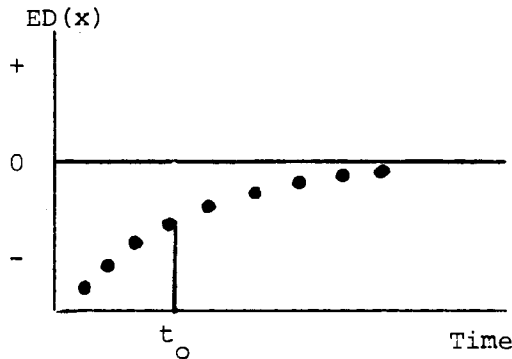


Figure 8.2

However, following the Edgeworth Process does not rule out oscillations in the excess demand for  $x$  as utility increasing trades may be made by "bouncing" between  $E^0$  and  $E^2$  before settling at  $E^1$ . This excess demand pattern may look quite different than the above pattern. See Figure 8.3.



Figure 8.3

While the explanation of this choice of method occurs at more length later in this chapter one possible reason for the different

choices e.g. small trades versus large trades is offered here. If one aim is to learn about the world around (the givens) and one perceives oneself as having no influence on the givens then one may be indifferent ceteris paribus as to large or small trades. However, if one perceives one's actions as affecting the givens and that the larger the trade the larger the effect, then large trades may be viewed as making learning about the position of the givens more difficult. Small changes may be conjectured more preferable as it may be thought small changes will change the givens little if at all. Consequently, one can worry about learning about the givens and not worry about the confounding problem of learning about how the learning technique alters the givens.

In contrast to either of these inductive trading rules, an a priorist, always believes himself at the utility maximizing point,  $E^1$ . The excess demands through time would always be zero. Shown graphically in Figure 8.4,

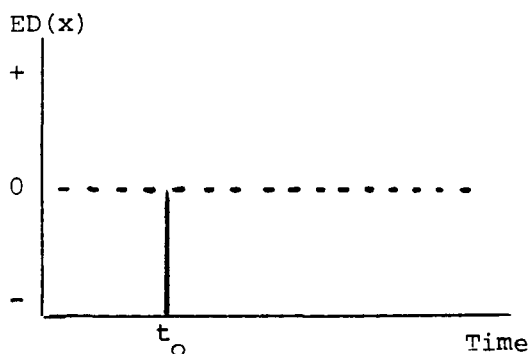


Figure 8.4



If one goes a step further [following Samuelson, 1947] and assumes this a representation of the market then the excess demands can be associated with prices such that Samuelson's price adjustment equation follows. That is,  $ED(x)$  greater than zero might be seen to imply a rising price of  $x$  through time and vice versa.

Note that even at this stage we would expect to see different market behavior and different resource allocation depending on methodology. If an inductivist individual makes "large" utility increasing trades overshooting  $E^1$ , as opposed to "small" trades homing in on  $E^1$  asymptotically, the time path of resource holding will be quite different. Significantly, both individuals are inductivist following the same general rule: "improve utility", however there is more than one method to improve utility. Depending on the method chosen resource allocation will differ markedly. The explanation of this choice of method will be the main task of this chapter.

Dissatisfaction with these views of learning that say truth can be guaranteed led Keynes [1936, summed up neatly in 1937, pps.214-215] to deny the information base necessary for correct decisions to be made about the future. This work was kept alive by followers [e.g. Richardson, 1959,1960; Shackle 1952, Leijonhufvud, 1968] and continues. Too often the starting point is that an agent's knowledge can not be true, however the baby need not be thrown out with the bath water. A decision-maker's knowledge need not always be false -- it may be true -- the decision maker must only be unsure as to the truth status of his knowledge at the time he is acting on it [Boland 1981].

The approach of appealing to aggregate constructs taken by Keynes [1936, Chapter 3] and some followers is patently non-neoclassical. If the only available theory of learning is the inductive one which implies knowledge can be guaranteed and that acquisition of knowledge is assured only in the long run then Keynes' wholesale change towards the non-individualist short run might be the price one has to pay to solve the problem. However, that price is much too high for most neoclassical theorists. Consequently, learning methods are put beyond explanation and are supplied as an exogenous given.

Fortunately there is another theory of learning which is amenable both to the idea that one might act correctly -- but knowledge that one is cannot be guaranteed, ex ante, even in the long run -- and to neoclassical economic explanation. [see Boland 1982, Chapters 10,11, and 12]. This view is most closely associated with the Austro-English philosopher of science, Sir Karl Popper [1959,1962,1972] and I have earlier and will here refer to it as Conjecturalism [Popper, 1972, Chapter One].

Popper [Boland, op.cit., pps.168-169] views learning as a process without end as "all explanatory theories involve unverifiable universal statements, learning in the more traditional, positive sense (verifying true explanations) is impossible. In this sense, one could never justify one's attempt to learn on the grounds that the ultimate end is possible." This interpretation of Popper leads to the position that evidence ('facts') -- if we can accept its truth

status [Agassi, 1966] -- while not useful for proving strictly universal statements of the form "All X are Y" is useful in the refutation of these universal statements hence refuting the theories that they help comprise.

Universal statements which take the form "All X are Y" cannot be proven true without reference to at least one other universal statement as otherwise a person would have to see all X at all points in time to ensure, given the truthfulness of the observation, that they are indeed Y's. This cannot be guaranteed in any finite time period. Refutation, when the statement happens to be false, comes somewhat more cheaply. One need find only one X that is not a Y to refute successfully the strictly universal claim that "All X are Y". Therefore [Boland, op.cit., pg.168] "...we can still learn by correcting our errors. Discovering one's errors is definitely a positive step - as long as one does not reserve the idea of a positive step only for a step leading towards a justification or an inductive proof."

Some philosophers have made just such an error when discussing Popper's work. Their argument is that refutation like verification is inductive in nature because if one continually seeks refutation or falsifications then each new theory must be closer to the "truth" than the previous refuted theories [Putnam 1974/1981]. Therefore falsificationism is mistakenly viewed as the march towards ultimate guaranteed truth by virtue of eliminating all but the last theory in the set of possible theories which by reductio ad absurdum must be

true.

Popper rejects this view on the basis that the set of conjectures -- theories -- is infinite in dimension. The removal of any finite subset, the refuted theories, from an infinite set of theories does not leave a finite set. Consequently, the remaining set of theories is always infinite in dimension and no appeal through the back door to induction may be made. Learning is always in time as there is no long run to which a system moves. "For Popper, science is a social institution that is pointing in the right direction even though it is readily admitted that it never reaches the goal at which we might think it is pointing" [Boland op.cit., pg.169].

This Popperian conjecturalist view that learning, which is finding the falsity of one's knowledge, can only be guaranteed through refutation then leads Boland [ibid, Chapter 11] to outline how one might 'put Popper on the neoclassical agenda'. An individualist explanation of short run neoclassical equilibria and disequilibria may then be possible. This avoids the twin problems of explaining coordination non-individualistically, e.g. as Post-Keynesians are wont to do, or describing them as optimal short run responses along or towards the long run equilibrium, as neoclassicists are wont to do.

It has been shown in earlier chapters that it is this very same appeal to a priorism and/or inductivism which renders equilibrium prices redundant in an informational sense. It may be by 'putting Popper on the agenda' that one might restore to the neoclassical price system the logical property of informativeness that has been claimed

for it all along.

To be consistent with neoclassicism as put forward by Hayek [1937/1948] one need only retain the view that it is individuals only who make decisions [Boland *ibid*, pg.177]. The conjecturalist view and neoclassicism then require that [Boland *ibid*, pg.178]:

- (1) all knowledge is presumed to be essentially theoretical; it is possibly true but we cannot prove its truth status.
- (2) Everyone's knowledge is potentially objective, capable of being stated in words or other repeatable forms.
- (3) What any agent does at any point in time depends on his knowledge at that time and the logic of the situation in which the knowledge is used.
- (4) Agents' behavioral changes result from changes in their knowledge as well as from intended or unintended changes in their situation.

It is this program (1) - (4) above as set out by Boland that I will refer to as 'neoclassical conjecturalism'. It is a blending of the neoclassical economic tenets of Hayek and Hicks and the epistemological and methodological conjecturalism of Popper.

Graphically, we might show conjecturalism as a series of moves from  $E^0$  which bear no systematic relationship to one another. Consequently, the excess demand functions for X and Y need not show any systematic patterns. (See Figure 8.5). A conjecturalist may not stay at  $E^1$  if indeed he gets there as he may continue to test

combinations away from  $E^1$  (at any distance) to try and find refutations to his theories of the givens.

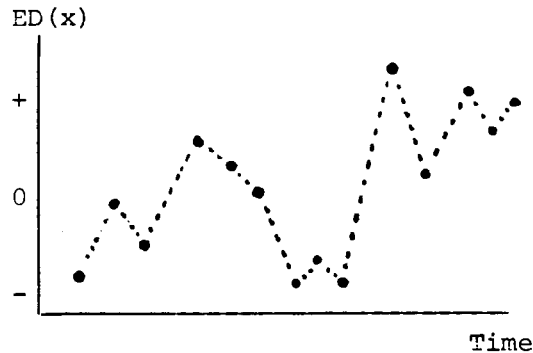


Figure 8.5

If the individual's actions do affect prices then the prediction on the observed pattern of prices will be that they, too, will appear less systematic than under inductivism or a priorism. There is no guarantee that a conjecturalist's excess demand for a good will fall with price rises or vice versa. The wish to test theories may outweigh some utility considerations. This suggests what will be shown shortly: that the choice problem needs to be expanded to include the choice of learning technique.

Conjecturalism may also provide the explanation of how an individual might learn about changes in the givens. For example, consider a change in tastes. It is an assumption of neoclassical theory that indifference curves are convex. This assures the uniqueness of the preferred choice. This convexity need only be local, not global. Suppose an individual's tastes change such that his indifference curves change from  $U^0$ ,  $U^1$  to  $U^{00}$ ,  $U^{11}$  (see Figure 8.6). How would the a priorists and inductivists ever find out about

their change in tastes? If they are at  $E^1$  there will be no test consistent with their methodologies which will reveal the change. This is because small moves away from  $E^1$  will be utility decreasing hence  $E^1$  will not be moved away from. A conjecturalist, always searching for refutations and not bound by a convention only to trade to improve utility, may discover the taste change more quickly than the others. Of course, he may not be observed to stay at  $E^{11}$  if he attains it.

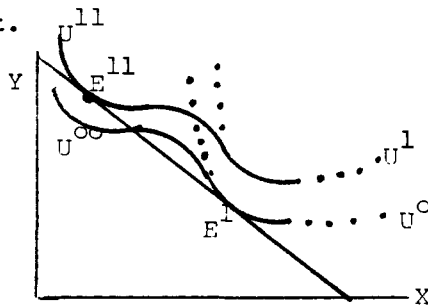


Figure 8.6

Conjecturalism denies an end zone reached after a 'long run' so all problems and learning must be done in the 'real-time' short run. 'Real-time' as errors and responses will be the consequence of agents acting on their knowledge to that point. That knowledge is comprised of the agent's theories of the givens which include preferences, endowments, the structure of the economy, prices etc. All knowledge being conjectural these theories may either be true or false. Change is motivated by two things:

- (i) changes in givens either exogenous or endogenous,
- and/or (ii) acting on false knowledge which leads to unintended consequences.

How the agent responds to unintended consequences or counter-examples of at least one theory of the givens depends on the theory of knowledge held, the aims of the individual and the theory of the problem situation. Learning will only result when the additional information -- the unintended consequence -- forces a change in one's knowledge, i.e. one's theories of the givens which may include how one learns.

As Frank Hahn [1973, pp.20-21] put the matter: "The concept of the equilibrium action of an agent here proposed is such that if it is in fact the action pursued by the agent an outside observer, say the econometrician, could describe it by structurally stable equations. When the agent is learning, however, then there is a change in regime so that one would require a 'higher level' theory of the learning process. Such a theory is not available at present."

It is towards such a 'higher level' theory I hope to move in this chapter. To explain why any individual chooses any given learning theory over others requires on the part of the theorist assumptions about how one learns about, or from, learning. This requires assumptions about the relationships between the aims, truth status, epistemology and methodology of agents. These form what one might, in Hahn's terminology, refer to as a 'higher level' theory of the learning process.

One possible way of drawing these points together is to say that an agent with conscious aims and objectives faces a situation which he perceives calls for a decision at that time. These aims may follow



from the view of the problem situation. Common aims include solving practical problems, cataloguing alternatives and/or learning for learning's sake. The agent is 'armed' with

- (a) a specific view of the nature of knowledge, that is, does it matter if his knowledge is true or not.
- (b) theories -- his knowledge at that time -- of the givens including his view of the problem situation;
- (c) his responses to the perceived outcome(s) of any decision made and action taken;

and (d) a view of his role in society, can some givens be changed by the actions of one person? (see Newman, 1981, for more on this point).

I will concentrate on the first three of the above points. It is the conjunction of these three inter-related views on the importance of truth status, whether or not knowledge can be guaranteed true or false in any real-time period (the agent's epistemology), and how knowledge, whether conjectural or not, is acquired (the agent's methodology) which defines an agent's theory of learning.

One may go further in an attempt to formalize these ideas.

Let  $\#t$  represent the possible number of opinions one might have regarding the importance of the truth status of one's knowledge. Clearly, there are at least two views:  $t_1$ , that the truth status is important, or  $t_2$ , that it is not. In this case  $\#t = 2$ .

Let  $\#e$  represent the possible number of opinions of whether or not there is a method for guaranteeing the truth status of one's

knowledge. There are at least four non-mutually exclusive opinions:

- $e_1$ , knowledge can be guaranteed true,
- $e_2$ , knowledge cannot be guaranteed true,
- $e_3$ , knowledge can be guaranteed false,
- $e_4$ , knowledge cannot be guaranteed false.

In this case  $\#e = 4$ .

Let  $\#a$  represent the number of ways knowledge may be acquired.

There are four that have been discussed in earlier chapters:

- $a_1$ , naive induction, seek all observations
- $a_2$ , a priorism, seek no observations
- $a_3$ , conventionalism, seek confirming observations
- $a_4$ , conjecturalism, seek refuting observations

In this case  $\#a = 4$ .

If these were the only possible views on truth status ( $t$ ), epistemology ( $e$ ) and method of acquisition ( $a$ ), there would be 32 potential theoretical learning schemes, i.e. "higher learning theories". This follows from the multiplicative product of the set of  $t$ 's, ( $t_1, t_2$ ), set of  $e$ 's, ( $e_1, e_2, e_3, e_4$ ) and set of  $a$ 's, ( $a_1, a_2, a_3, a_4$ ). For this example, let that product be represented by  $(\#t)(\#e)(\#a)$  or  $(2)(4)(4) = 32$ .

As an example let me outline two possible schemes.

Conjecturalism combines the views that truth status matters ( $t_1$ ), that false knowledge can be guaranteed false ( $e_3$ ), and that this may be done through seeking refutations ( $a_4$ ). In terms of the notation introduced above conjecturalism is defined as the triple ( $t_1, e_3, a_4$ ).

Another scheme, induction, would comprise the triple representing the views that truth status matters,  $t_1$ , knowledge can be guaranteed true ( $e_1$ ), and that one may do this by seeking all observations ( $a_1$ ). Consequently, in the above notation, induction is defined as the triple  $(t_1, e_1, a_1)$ .

Other well known learning theories fall out of the above array. As a final illustration, Instrumentalism [see Boland, 1982, Chapter 9], which is the view that while the truth status of one's knowledge is unimportant, inductive techniques may be used to acquire knowledge that works "well enough", may be represented by  $(t_2, e_2, a_1)$ .

Very often one's methodology,  $a$ , depends on one's epistemology,  $e$ , and one's epistemology on one's view of truth status,  $t$ , so the actual number of consistent triples may be much smaller than suggested above. However, if all three sets are independent, the above number of triples represents combinations one might consider as solutions to learning problems.

One could identify with a triple other well known 'isms' e.g. pragmatism, skepticism, a priorism and conventionalism. However, my task at hand is not to explicate every possible learning theory (I, for one, doubt that 32 even scratches the surface of the number of possibilities), but to show that there exists more than one and that the choice of one at any time should be explained not assumed.

Each of the 32 triples represent a learning process or theory. That is, each specifies an agent's epistemological and methodological views of his world. The question of how an agent might choose any

learning theory, the 'higher level theory' in Hahn's terminology, requires exploration before any claim of having provided an explanation of the learning process may be made. If one can go some way towards providing an explanation of that choice then some distance towards an endogenous learning process will have been covered.

To remain faithful to the espoused conjecturalism in this chapter requires me to conjecture that the choice of a learning model may be made any of a great number of ways. One of the many possible ways is consistent with a classical method of optimizing an objective function expressed over the aims of the individual. In addition to the traditional aims of utility maximization over goods space additional aims may include learning for learning's sake, cataloguing alternatives or solving practical problems.

The space over which an individual has preferences now may be extended beyond goods/characteristics space into the realm of choosing the technique through which planned optimization is to be carried out. When time does not matter this extension is unimportant as it is costless, time is not money, to arrive at the utility optimum. When time does matter, as indeed it must, the choice of the learning method employed to test one's theories of the givens against reality must be explained. For, it is the potential failure of one's theory of learning to lead to the perceived optimum, in some time short of infinity, which may force the individual to change learning theories. Further, the choice of learning theory and changes in it may explain resource allocation at any point in time.

As an example of this heuristic consider an individual wishing to maximize utility in goods space as quickly as possible. His aim may then be seen as one of solving the practical problems: how do I reach the true utility maximum in goods space, given my budget, in as short a time as possible? This objective may lead to the choice of a learning theory where truth status is deemed important ( $t_1$ ): it is the true utility optimum over goods space which is desired. The epistemology entailed is that the truth can be guaranteed,  $e_1$ , and the methodology chosen may be to seek all observations,  $a_1$ . If the individual had different aims, e.g. to catalogue the utilities associated with the goods space which would reveal to him his utility function over goods, then he would choose a different learning theory, that is a different triple  $(t,e,a)$ .

As in any optimization problem the aims may not all be maximized simultaneously. For example, trying to reach the true utility optimum in goods space may not be attainable in any time short of the long run. An individual would need a guarantee that the consumption bundle chosen is indeed the best of all goods bundles. But, there are an infinite number available in the usual neoclassical representation of choice. Consequently, the time taken to test all bundles may stretch to infinity. The "practical problem" will go unsolved in any time short of infinity. This unintended consequence of the individual's choice may lead to change. The ambitiousness of his joint aims "to reach the true optimum in as short a time as possible" may make solving the problem in real time impossible. Many

choices for change are open. Aims will change. He may decide time does not matter and relax that constraining aim. Or, he may decide to reach the highest utility level possible in a given time period which is fixed and less than infinity. In either case, those changes in aim, as reflected in changes in (t) or (e) or both, will lead to a change in the learning method chosen. Learning is thus endogenous.

If, as a consequence of changing aims, truth status is now deemed of secondary importance or unimportant,  $t_2$ , and beliefs of truth guarantees in finite time are lacking,  $e_2$ , then the method chosen to solve the practical problem may be to make a smaller number of discreet trades in order to increase the utility from commodities within some finite time.

The implications my view has on the allocation of scarce resources should be scrutinized. The traditional explanation of resource allocation is that individuals facing known endowments and price constraints maximize an objective function. Going back to Figure 8.1 they start at  $E^0$  and go to  $E^1$ . The prices, endowments and preferences determine or explain the choice of  $E^1$ . Had any of these been different a different optimum would have been chosen. This happens out of time. To make it time dependent one must, as I did earlier, introduce some sort of trading or learning technique. If we compare the four individuals already introduced [see Figs. 8.2, 8.3, 8.4, 8.5] having the same endowments, prices and preferences but each choosing a different commodity bundle at any given time how are we to explain it? Traditional theory says they would all make the same

choices at the same time or obscure the question by resorting to stochastic prediction. However, in my heuristic, the observable difference in allocations chosen are due solely to the different methodologies chosen. That is, the choice of learning techniques may provide the complete explanation of why people in the same circumstances make different choices, thus rendering the retreat to stochastic choice unnecessary.

Another way of saying this is to say that a change in learning technique (e.g. methodology) can explain the change in resource allocation. If an a priorist [Figure 8.4] at time  $t_0$  became an inductivist of the first type [Figure 8.2] that change alone could explain the observed change in the allocation of resources. As just shown that change in method may come about due to a change in aims.

This is obviously not the only approach one might postulate and to paraphrase Frank Hahn [1973] one would be dull indeed if one could not find problems with it. However, it is one conjecture on how agents choose a learning theory with which to deal with situations they face. Such a choice mechanism endogenizes the choices of epistemology, methodology and views on truth status, as a function of individual's aims and beliefs about the world. The higher learning theory that is the triple  $(t,e,a)$  is not arbitrarily chosen by the theorist which has been the case heretofor.

It has been argued in earlier chapters that the theorists' arbitrary choice of a learning process that includes any value for "e" that admits the possibility of guaranteeing one's knowledge, e.g. a

priorism, and/or the conjunction of that with a methodology that assured it (inductivism) rendered a price system redundant. Enough information had to be specified ex ante (known a priori true) so that the "dynamics" (exogenously given) were uninteresting. They were not conceivably false.

It remains to study the importance of prices in the neoclassical conjecturalist framework. The answer we arrive at hinges on the Popperian view that while the truth status matters we may never know our knowledge to be true when true, but only false when false, hence our methodology should be one of seeking refutations. In any neoclassical model it is prices which are the sole potential conveyor of social information. All other givens, while they may be objective, are not necessarily observable by all. That is, other givens are private information. Consequently, if an agent is to have a theory refuted it must be because the prices that rule in the marketplace, which determine his budget constraint, are not consistent with the result he expected. This constitutes a refutation of at least one of his theories of the givens.

The conjecturalist agent's response to a counter-example to a theory -- seeking a different theory -- represents learning and we conclude that, in this sense, prices were informative in time. These prices and the known quantities (private information) are used by the agent to falsify his theories of the givens if they are false. If the prices are consistent with the currently held theories the agent has not learned and the conclusion is that prices were uninformative.



I have argued earlier that the inductivist bent of current theorizing plus the exogeneity accorded message-state space does not allow for theory revision, only model revision. In the conjecturalist framework the refutation of a theory e.g. an agent's belief that a given observed message  $m$ , indicates something about the coming state,  $s$ , allows for the removal of that theory and the replacement with any other. One may change methodologies. One is not straight-jacketed to considering only a given set of theories which are kept even when they perform poorly. One may now remove refuted theories and replace with as yet unrefuted ones. Knowing that the methodology or any theory of the givens may be false and hence need changing does not unfortunately guide one to the knowledge of which should be changed.

This inability to attribute an unintended result to a specific cause, and instead being forced to attribute it to the conjunction of many possible causes, is sometimes referred to as the 'signal extraction' problem. Usually an agent is unable to extract the necessary information on what has changed (say supply or demand) simply by viewing changes in a signal, e.g. price, because there are two or more influences on that signal. This leads to the conclusion, for example, that prices, as a signal, are 'noisy' and it is this problem with prices which is seen to deny a competitive equilibrium [Stiglitz, 1977].

The signal extraction view misses the reason for the inability of theories to exhibit single-price, price-taking equilibria short of

the long run. By attributing the problem to price changes which reflect more than one possible underlying change the problem is viewed as one of underdetermination, and the consequent lack of coordination is attributed to a failure of the price system.

However, the fundamental problem is not that prices are 'noisy', or 'sticky' but that our knowledge of at least the givens can only be conjectural. An unintended consequence does not reveal which part of our conjectural knowledge is false. Prices need have no 'noise' at all; that is, as a signal they may refute unambiguously by constituting a counterexample the view of the economy an agent holds.

It is the response to this refutation that is in some circumstances not explicable in the mathematics of equilibrium. For the mathematics of equilibrium deny change. However, some agents may change methodology, some may change a theory of one of the givens, some may change nothing at all.

In contrast to a strictly mathematical representation, a less deterministic view of learning recognizes that the inability of an agent to guarantee the truth of even one of his theories, even when it is true, places any concept of equilibrium which is consistent with time-based guarantees in jeopardy. If one, as a theorist, wishes to embrace a conventionalist, inductivist or a priorist methodology then one may elude that problem. However, guaranteeing equilibrium coordination through methodological straight-jacketing may be too expensive. Remember that a conventionalist view sets the limits (by convention) on behavior which determines an 'equilibrium', an

inductive technique provides a long run guarantee of equilibrium and a priorism guarantees the knowledge necessary for equilibrium.

To close this chapter I wish to [following Boland, 1983] argue that yet another reason for theorists to recognize many different views of knowledge and methodology is that decisions based on them will usually lead to very different patterns of observed behavior. Changes by an agent in his choice of methodology will be 'changes in regime' making description by 'structurally stable' equations difficult if not impossible. This result leaves one suspicious of any attempts to treat 'learning' as a problem tractable solely by mathematical technique which would require the reaction or learning function to be already specified as one of the unchangeable givens of the problem.

For those of us unwilling to admit induction, conventionalism, and a priorism as the only possible learning methodologies, the explanation of behavior will rely on learning taking place in time with no guarantee of universal coordinating success. Only then will price signals generated in the marketplace lead to changes in method which reflect changes in knowledge and learning.

## CHAPTER IX

## Summary

The current superstition that only the measured can be important has done much to mislead economists and the world in general.

F. A. Hayek, [1975, p.20]

One of the earliest claims for neoclassical economics is the information role of prices in coordinating economic activity. Market prices as the joint -- common -- signal lead to coordination by clearing markets through the conveyance to any single individual the information, expectations and actions of all the other agents. Prices aggregate individual information and present a summary to market participants of the opportunities and terms of trade available to them [e.g. Hayek 1937/1948, 1945/1948].

The corollary to this view of prices is that any non-equilibrating price will lead agents to change behavior so that ultimately the resulting market imbalance is eliminated and equilibrium is re-established. This process is one where the market participants 'learn by experience' to coordinate in the optimal way.

The above view, which is the prevailing orthodoxy, usually depends implicitly for its explanatory success - that is deriving the endogenous solution values by reference to the exogenous variables (among them prices) - on one of two implicit but well known theories of learning which are false. Further, these theories, a priorism and

inductivism, have been known to be false for centuries.

The argument of this dissertation has been that the resort to false theories of knowledge as underpinnings of neoclassical explanation has rendered the role of prices superfluous. That is, prices cannot logically play the role attributed to them if agents have resorted to successful a priorism and/or inductivism as has been guaranteed by most theoretical studies including the more avant-garde rational expectations literature.

Chapter II provides a general overview of what it means for a statistic to be "informative" given an epistemology or theoretical view of knowledge. Starting from the commonly held view that learning takes place when information is received and that learning implies a change in behavior or actions, it is vital to know just what constitutes information. The determination of informational status was shown to depend on the particular epistemology. For example, an individual following an inductivist epistemology will consider verifying instances of a theory "informative" while a person who does not share that epistemological view may not consider the same observations informative. Several epistemological views and their implications for informativeness were briefly surveyed.

Chapter III puts forth the view that models of static equilibrium render prices uninformative. This is done by demonstrating that no matter what the methodology employed a static equilibrium result relies on prices which create no consequences which inform agents. Equilibrium prices do not confirm a conventionalist's

theory, they do not refute a conjecturalist's and they are redundant for an a priorist.

Chapter IV looks at the informativeness of prices when so-called disequilibria adjustments are being made. Perhaps the most familiar is price adjustment described by an excess demand relationship where price rises if excess demand is positive and falls if excess demand is negative.

It is shown that unless these price adjustment functions are the result of an agent's or agents' maximizing behavior, which they typically are not [Rothschild 1973], they must be either ad hoc, non-individualistically provided [Arrow 1959a] or otherwise non-neoclassical in nature. Clearly, if the theorist provides the 'dynamic', exogenously relying on a priorism, an agent's behavior in following that dynamic cannot be said to be exhibiting disequilibrium behavior or response. The set of givens known true has just been augmented.

If one truly believes learning to be taking place through the 'dynamic adjustment to equilibrium' then one must also believe in the epistemological view that knowledge may be guaranteed true. In this case, one of inductive search specified by the price-adjustment relationship, prices are again uninformative. For, if the agent knows the path to equilibrium -- which he must for stability results to follow -- and the adjustment path is being traversed then at every point the agent knows the size of the 'disequilibrium' which implies he must know (a) where he is, and (b) where he will end up. That

this adjustment path is optimal may be shown by arguing that too-rapid moves to 'equilibrium' may be more costly than slower moves [Alchian 1959].

The chapter concludes with the result that either 'dynamics' are ad hoc, sub-optimal and non-neoclassical or they are paths of equilibria being traced through time. Disequilibria are apparent, not real, and in both cases the informational content of prices is nil.

Chapter V briefly explores quantity signals as information substitutes for prices. First, a case was made that a logical paradox is created by attempts to 'fix' prices. Then it was shown that in a neoclassical framework resort to quantities requires more knowledge a priori true than was the case with prices. That is, agents have to know a ration scheme as well as the quantity signal. The argument that quantities have to adjust because prices are in some sense less than infinitely flexible is addressed and shown to rely on either ad hoc rigidity or mis-specification of the problem. Price adjustment need not be infinitely fast to be optimal and consequent quantity adjustment is also optimal. Apparent disequilibria whether they be labor and/or capital unemployment may be actual equilibria when adjustment costs are positive [Alchian 1969/1970]. Only infinitely costly price adjustment or ad hoc 'rigidity' removes the necessary price flexibility. Any other perceived rate-of-adjustment speed as long as it is positive cannot ex ante be considered sub-optimal even in the presence of resource 'unemployment' without

reference to adjustment costs.

Chapter VI shows that the approach represented by 'economics of uncertainty' does not elude the problem of non-informative prices. The learning process over state likelihoods is represented by either a classical or Bayesian statistical procedure. The process is exogenous and the 'learning' follows an optimal path. The problem is identical to that discussed in Chapters III and IV except that the dimensionality increases to represent the possibility of multiple future possible states. An algorithm is provided to calculate the possible number of theories of the possible states given the messages. Since the state-message space is finite, so is the number of possible theories. 'Learning' inductively according e.g. to Bayes' theorem results only in model revision, not theory revision.

Chapter VII looks at the above program in light of the recent micro-rational expectations literature. Non-existence proofs have caused theorists to move away from definite explanatory attempts to forms of "approximationism" or "genericity". I argue that in these frameworks prices will never be informative if equilibrium and disequilibrium are arbitrarily specified and the learning process exogenous.

Learning in a "rational world" is again treated as an inductive process with a pinch of a priorism thrown in. One set of studies asserts correct models (likelihoods) are known to be held and shows that in the limit the process converges. This logically is the same as the procedure discussed in Chapter IV. The other set of studies



does not presume correct likelihoods known, but cannot guarantee convergence. One problem with this approach which is a carryover from the 'economics of uncertainty', is that since the number of theories is finite and fixed -- there is no process for theory refutation and acquisition. Individuals never necessarily operate in a framework which includes the theory that is true but unknowably so. The only 'learning' is with respect to revising models of theories known to be false.

Within that constraint agents may inductively acquire very good models, but even the best model of a false theory will be refuted by some observation. The appeal to "approximate truth" then is a conventionalist necessity to determine on an ad hoc basis when the model is 'good' enough.

Finally, Chapter VIII provides an interpretation of Karl Popper's non-inductivist knowledge acquisition process when learning is the recognition by an agent that one or more of his theories are false. This requires a framework where refuted theories may be removed and new theories introduced.

The Popper-Hayek program [see Boland 1982], which I term Conjecturalism, is then shown to comprise a neoclassically acceptable way of providing endogenous learning dynamics. That is, the agent's actual methodology, theory of the knowledge acquisition process, is a choice variable. Any theorist's imposing a methodology whether it be, for example, conjecturalist, inductivist or a priorist, will never allow for endogenous dynamics hence informative prices.

A general framework is provided showing the dependence of the methodology on an agent's theory of the truth status of his knowledge and his epistemology. These variables are part of a larger choice problem and are in turn functions of the agent's aims and perceptions of the problem situation. There is a feedback from action determined by the methodology which create intended as well as unintended consequences; the latter resulting in unforeseen gains and losses. These unforeseen gains and losses then may change the agent's view of the importance of his knowledge and may lead to changes in methodology.

The ability to have an endogenous methodology then places the learning process squarely in the middle of any explanation of change. Examples were given which show that the learning methodology of an individual may explain the observed pattern of resource allocation. This also returns to the price system the role claimed for it, but implicitly denied it in much neoclassical work. When dynamics are endogenous, movement is characterized by theory replacement. This is done as the price signals refute at least one of the individual's theories. The individual consequently undergoes unforeseen wealth or utility changes. These changes then under certain methodologies lead to abandoning one theory and acquiring another.

Information -- as prices -- has a true role to play in a conjecturalist model. The view that the truth status of knowledge is at best conjectural leads one to a position of accepting refuting instances, e.g. unforeseen prices, as reason for an individual

changing at least one theory of his tastes, endowments, view of the economic structure and method.

In this dissertation it is the recognition that any individual may change any of his theories including his methodology as a result of viewing prices which imparts to the price system an informative role. If methodology is held fixed then learning in a real-time sense cannot take place. In addition, the endogenous change in methodology may also lead to different resource allocation patterns for the same individual in what appears to be the same situation.

If this argument is correct one need not hold to the view that 'failure to coordinate' is due to 'noisy prices', or 'lack of information' or that 'equilibrium' need be viewed as an indistinct arbitrary concept. Coordination in the sense of a mathematically describable and mathematically stable equilibrium may be a false hope held out by the reliance on false methodological positions. Holding those views of knowledge, e.g. conventionalism, admits coordination but at the expense of giving up learning in the sense of refutation leading to the change of at least one of the agent's theories. This must surely be too high a price for those theorists who claim to be tackling problems associated with the 'economics of information' and the informational content of economics.

## Appendix to Chapter II

Examples of the inductive approach are not hard to find in Economics. The following are meant only as a representative sample.

**Example One: Price Adjustment Models**

In general, any approach which guarantees a solution only when time is taken to the limit is inductive. Take Samuelson's [1947, pg.263] formalized interpretation of the process whereby supply and demand are equated by a changing price:

$$(1) \quad \dot{p} = \frac{dp}{dt} = H [q(d) - q(S)] = H[D(p,a) - S(p)], \text{ where}$$

$$H(0) = 0, \text{ and } H' > 0.$$

$q(D)$  and  $q(S)$  are quantity demanded and supplied respectively.  $D$  is a function of price (as is supply) and a taste parameter,  $a$ .

Equation (1) can be expanded (higher terms being omitted) about the equilibrium price  $p^0$ : (2)  $\dot{p} = \lambda(Dp^0 - Sp^0)(p - p^0) + \dots$ . To guarantee an "equilibrium", defined as demand equals supply, one shows the time path of price,  $p(t)$  to be approaching the equilibrium price,  $p^0$ . This approach may be either from above, below or both (oscillations). This demonstration cannot be made for time less than infinity. To show this: Let price at time  $t$  be given by the solution to (2) above:

$$(3) \quad p(t) = p^0 + (p - p^0)e^{-\lambda(Dp^0 - Sp^0)t}.$$

To guarantee the stability of the system, that is that  $p(t)$  approaches the equilibrium

price  $p^0$  from the initial price,  $\bar{p}$ , hence equating supply and demand, one need show that the second term on the right-hand side of (3) goes to zero. This is only possible when either (a) the initial price,  $\bar{p}$ , is equal to the equilibrium price,  $p^0$ , this might happen accidentally, but fortune smiling does not a proof make, or (b)  $e^{\lambda(\cdot)t}$  goes to zero, which is guaranteed only when  $t$  goes to infinity,  $Dp^0$  being strictly less than  $Sp^0$ .

Consequently, infinite time is necessary to guarantee the attainment of an equilibrium. One needs every observation from the initial time period into infinity to guarantee the result. If one did not need all the observations then the result could be guaranteed short of time going to infinity. Other than happy coincidence, accident and/or infinity this guarantee will not be forthcoming.

#### **Example Two: Distributed Lag Models**

The concept of permanent income due to Friedman [e.g. see M. B. Johnson, pp. 94-95] has become widely used in generating consumption related individual economic and aggregate results [e.g. reaction to taxation policies].

The value of aggregate permanent income in any period,  $t$ ,  $Y_p^t$ , depends linearly on the preceding value of permanent income  $Y_p^{t-1}$ , and current measured income  $Y^t$ :

$$Y_p = (1-\lambda)Y_p^{t-1} + \lambda Y^t. \quad \text{This holds for every previous}$$

period:  $Y_p^{t-1} = (1-\lambda)Y_p^{t-2} + \lambda Y^{t-1}$

$$Y_p^{t-2} = (1-\lambda)Y_p^{t-3} + \lambda Y^{t-2}$$

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·  
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$$Y_p^{t-N} = \lambda \sum_{n=0}^N (1-\lambda)^n Y^{t-N} + (1-\lambda)^{N+1} Y_p^{t-N-1}.$$

If one wished to guarantee the truth of the value of permanent income today one would have to provide all observations (through the indefinite past) on past values of permanent income and measured income. That is, one would calculate permanent income with  $n$ , the number of past periods being considered, going from 0 to infinity. Omission of any previous observation(s) denies an airtight guarantee as to the correctness of the value derived.

### **Example Three: Learning as a Bayesian Process**

Bayesian 'learning' [e.g. Turnovsky, 1969; Radner, 1979; Lewis, 1981; Blume and Easley, 1982] incorporates the idea that [Intriligator, 1978, pg.601]:

$$P(B_i | A) = \frac{P(B_i) P(A | B_i)}{\sum_{i=1}^n P(B_i) P(A | B_i)}$$

Which is interpreted as the probability of the  $i$ th hypothesis  $B_i$ ,

given event A. According to the theorem that probability is arrived at by dividing the product of the prior probability of  $B_i$  and the likelihood of A given  $B_i$  by the sum of all products of likelihoods and prior probabilities. The technical points need not detain us further.

What is useful to note from the above equation is that the elements,  $P(B_i)$  and  $P(A|B_i)$ , in the numerator are relative frequencies. That is, the ratio of the number of times something occurs (the  $i$ th element of B) to the total possible number,  $n$ , of possible occurrences  $\sum_{i=1}^n B_i$ . This means that any and every observation on B (and A) is informative - in the sense that more observations yield "better and better" relative frequencies. Learning in a Bayesian fashion would continue as long as observations could be collected. More observations are better than less as they add information. In Bayesian learning models there is a one-to-one correspondence between observation and information. Note that it need not apply solely to empirical observations as the prior probability  $P(B_i)$  allows for specification by any means. The value of the prior probability need not be assigned solely by appeal to empirical observations. It is important to add that this very general Bayesian approach includes many familiar expectations hypotheses as special cases. Among them is the adaptive expectations scheme [Turnovsky, 1969].

**Example Four: Rational Expectations**

This example [from Mussa, 1978, in Frenkel and Johnson, 1978, pp.58-59] embodies the spirit of the Rational Expectations Hypothesis [REH]. "Suppose the stock of domestic money which domestic and foreign residents are willing to hold is given by:

$$(1) \quad m(t) = \lambda e(t) - \eta \cdot \pi(t) + \delta(t), \quad \lambda \text{ and } \eta > 0,$$

where  $m(t)$  is the log of the stock of domestic money at time  $t$ ;  $e(t)$  is the log of the exchange rate;  $\pi(t)$  is the expected rate of change in the exchange rate,

$$(2) \quad \pi(t) \equiv E_t(e(t+1) - e(t))$$

where  $E_t(\cdot)$  indicates the expectation at time  $t$ , based on information available at that time, and  $\delta(t)$  summarizes all of the influences on the willingness to hold domestic money other than  $e(t)$  and  $\pi(t)$ .

Substituting (2) into (1) and solving for  $e(t)$  yields:

$$(3) \quad e(t) = \left( \frac{1}{\lambda + \eta} \right) \cdot [m(t) - \delta(t) + \eta E_t(e(t+1))].$$

It is necessary to know the current expectation of  $e(t+1)$ . We impose the assumption that expectations are formed "rationally".

Specifically, asset holders are assumed to know (or act as if they know) equation(3) and apply it to next periods' exchange rate; thus

$$(4) \quad E_t(e(t+1)) = \frac{1}{\lambda + \eta} \cdot E_t [m(t+1) - \delta(t+1) + \eta E_{t+1}(e(t+2))].$$

Using the fact that  $E_t(E_{t+1}(e(t+2))) = E_t(e(t+2))$  and applying the same procedure iteratively to  $E_t(e(t+2))$ ,  $E_t(e(t+3))$ , etc. it follows that:



$$\begin{aligned}
 E_t(e(t+1)) &= \left(\frac{1}{\lambda+\eta}\right) \cdot \left\{ E_t[m(t+j)-\delta(t+1) + \frac{\eta}{\lambda+\eta} (E_t(m(t+2)-\delta(t+2)+\dots))] \right\} \\
 (5) \qquad &= \left(\frac{1}{\lambda+\eta}\right) \sum_{j=1}^{\infty} E_t[m(t+j)-\delta(t+j)] \left(\frac{\eta}{\lambda+\eta}\right)^{j-1}.
 \end{aligned}$$

Notice the form of (5). To guarantee the expectation at time  $t$  for the exchange rate in time  $(t+1)$  all future observations must be considered. Instead of considering all observations back through time, successful rational expectations requires observations forward through infinite time. An inductive program.

#### **Example Five: Conventions**

Probably the best known set of conventions are those which have been developed for statistical and econometric testing. Economists are familiar enough with these tests, e.g.  $R^2$ ,  $t$ ,  $F$  etc., that much more need not be said. The following gives an idea of the prescriptive use of a testing convention: "for large degrees of freedom (e.g.  $n-k > 30$ ) the  $t$  distribution is approximately the same as the normal distribution, and in this case a general rule of thumb is that if the  $t$  ratio exceeds 2 then the coefficient is significant." [Intriligator, 1978, pg.130, emphasis added].

The continued appeal to conventions [e.g. using a 95% confidence interval when hypothesis testing] must be done following some faith in induction.

**Example Six: A Priorism**

One might think that a priorism is a relic of older economic analysis and rarely, if ever, appealed to currently. One would be wrong. The REH revolution is a mixture of inductivism [Example Four above] and a priorism [Boland and Newman, 1979].

Referring again to Mussa [1978, pg.59]: "We impose the assumption that expectations are formed 'rationally'. Specifically asset holders are assumed to know (or act as if they know) equation (3) and apply it...". But, equation (3) is nothing more than the "relevant economic model" governing the economic results. How agents are "assumed to know" this information can only be through a priorism.

As Lucas [1975, pg.1138] writes: "On the one hand, it is easy to postulate agents and market institutions which ignore or foolishly waste information...It is equally easy to postulate 'efficient securities markets'...: the result is a static general equilibrium model...one must avoid both extremes." That the Rational Expectations Hypothesis embraces the latter "extreme" through a priori knowledge provision cannot be considered an acceptable method of learning.

## Appendix to Chapter V

An example of price rigidity which is endogenous as a result of profit/utility maximization or cost minimization may be illustrated in the following fashion.

For producers let profit  $\pi$ , be equal to total revenue  $PQ$ , minus total costs  $TC$ . Where total costs for the period  $T$  between price changes (measured e.g. in days, weeks, or months) are comprised of four terms:

(i) price adjustment cost,  $\$A/T$ , where  $\$A$  is the one time cost of changing prices and  $T$  is the length of time those prices rule;

(ii) an inventory holding cost of  $\$BT$ ;

(iii) a wage bill,  $\$wL$ ,

and (iv) a capital cost,  $\$rK$ .

$\$A$  is the cost of each price change, when divided through by the length of time before a new price change is deemed necessary,  $T$ , one gets the cost of each price change per period,  $\$A/T$ . The longer the period  $T$ , the lower the average cost of any price change.

Conversely,  $\$B$  are the per unit inventory holding and disbursement costs. The total inventory costs for the period when inventories do all the adjusting (i.e. the period when prices are not changing,  $T$ ) is given by  $\$BT$ .

One solves for the optimal time period between price changes,  $T$ . The inverse of this,  $1/T$  yields the optimum number of price

changes in any period  $T$ . However, if  $T$  is measured in (e.g.) weeks and the optimum  $T$  is calculated to be 4 (weeks) then the optimum number of price changes per year would be 13.

Maximizing profits with respect to the time period  $T$ :

$$\begin{aligned} \text{MAX } \pi &= PQ - (\$A/T + \$BT) - wL - rK \quad (\text{I assume the amount} \\ \text{s.t. } T & \qquad \qquad \qquad \text{bought and produced is} \\ \frac{\partial \pi}{\partial T} &= \$A/T^2 - \$B \quad \text{independent of the} \\ & \qquad \qquad \qquad \text{number of price changes)} \end{aligned}$$

$$\frac{\partial^2 \pi}{\partial T^2} = \frac{-2A}{T^3} < 0$$

Solving for optimal  $T$ ,  $T^*$ :

$$(\$A/T^2) - \$B = 0$$

$$A/B = T^2$$

$$\sqrt{A/B} = T^*$$

$T^* = 0$ , that is the optimal price adjustment is instantaneous if and only if price adjustment is costless or when inventory costs are infinite. When inventory costs (per unit) are falling the optimal time between price changes grows longer, i.e.

$$\frac{\partial T^*}{\partial A} = \frac{1}{2\sqrt{AB}} > 0 \quad \text{and} \quad \frac{\partial T^*}{\partial B} = -\frac{1}{2} \sqrt{A \cdot B}^{-3/2} < 0$$

There is a trade-off between price adjustment costs and inventory holding costs. The resolution of an optimal  $T^*$  solves both the price adjustment and quantity adjustment problems simultaneously.

EXAMPLE TWO: Let the transactions technology (how one sells units of a good, either through price or quantity adjustment) be, in general, a

function of the number of quantity changes,  $\delta Q/\delta t$ , and price changes,  $\delta P/\delta t$  per unit time. That is, one may sell (or buy) depending on the number of price changes necessary per period to accommodate the transactions and/or one may use quantity changes per period to sell the given amount (at a given price), hence inventory may rise or fall.

Let  $\partial P/\partial t \equiv C$  and  $\partial Q/\partial t \equiv D$ . For a fixed number of transactions, the trade-off between price and quantity adjustments is given by

$$\begin{aligned} d(\text{transactions}) = 0 &= \frac{\partial f}{\partial (\frac{\partial P}{\partial t})} d\left(\frac{\partial P}{\partial t}\right) + \frac{\partial f}{\partial (\frac{\partial Q}{\partial t})} d\left(\frac{\partial Q}{\partial t}\right) \\ &= \frac{\partial f}{\partial C} dC + \frac{\partial f}{\partial D} dD \\ - \frac{\partial f}{\partial C} dC &= \frac{\partial f}{\partial D} dD \\ - \frac{dC}{dD} &= \frac{\partial f/\partial D}{\partial f/\partial C} \end{aligned}$$

which gives the marginal rate of technical substitution between the number of price changes per period  $t$ , and the number of quantity changes per period.

To find the optimal number of price and quantity changes let the total cost (TC) of price and quantity changing be given by

$TC = \$AX + \$BY$  where  $X$  and  $Y$  give the number of price and quantity changes respectively. Then,

$$\frac{dC^*}{dD^*} = -\frac{B}{A} = \frac{d(\partial P/\partial t)}{d(\partial Q/\partial t)}$$

will give the cost minimizing tangency for transacting a given level of units. This also determines the optimal price and quantity flexibility. Again, only if \$A is zero (or \$B infinite) will the optimum number of price changes go to infinity for any level of transactions.

Graphically:

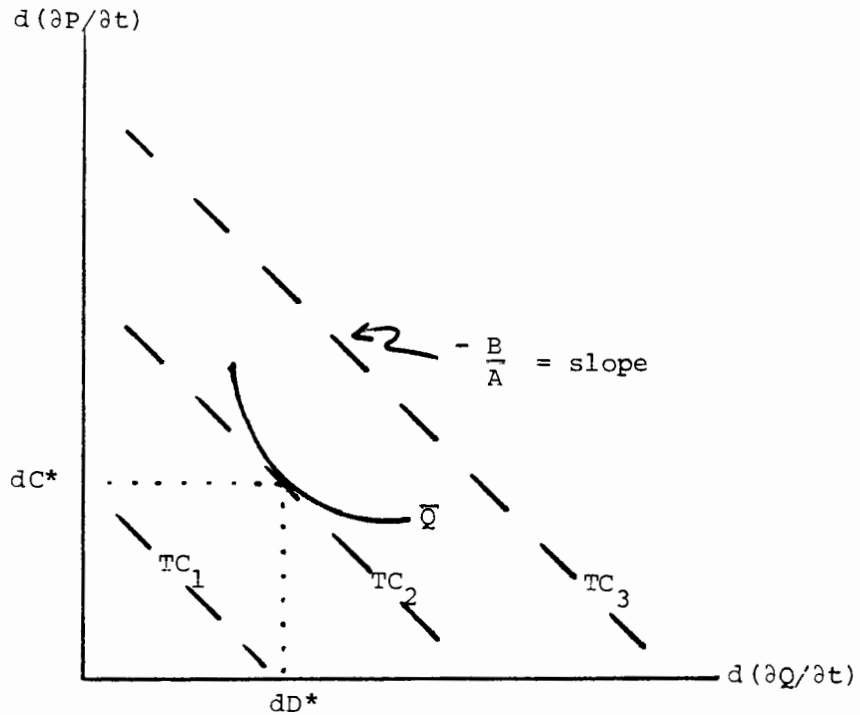


Figure 5A.1

where  $TC_1$ ,  $TC_2$ , and  $TC_3$  represent different total cost levels for fixed \$A and \$B.

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