"I have given my life to the study of
many strange things--facts which, perhaps,
the world would be better off nob knowing."
--Professor Van Helsing, in the
film Dracula (1931)

"... the ghastly paraphernalia of our
beneficent trade." --idem.

THE VARIETIES OF PRICE THEORY:

WHAT MICROFOUNDATIONS FOR MACROTHEORY?

by

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It has been a cliché for the last ten or fifteen years that macroeconomics stands in dire need of explicit microfoundations. For the purposes of this paper, this will be accepted as an unquestionably noble--if so far unproductive--sentiment. It expresses, of course, a complaint over the "split" between micro and macrotheory and a recognition of the unhealthy state of affairs of teaching these subjects as two independent, unrelated branches of economic theory. But frequently something more than this--or so one suspects--is intended to be conveyed when the cliché is aired, namely, the judgments that (a) microtheory is in good shape, (b) macrotheory is in rather bad shape, and (c) that it is up to macroeconomists to put the Humpty Dumpty of general economic theory together again by rebuilding their models with building-blocks that contemporary microeconomics provides. When the "split" is deplored, it is always the microunderpinnings of macrotheory that are found missing. To complain about the lack of a useful macro-superstructure on neoclassical microtheory would be rather eccentric.

Judgments (a) and (b) one may well share. There have been and remain good reasons for theoretical economists to accord more prestige to neoclassical microtheory than to macrotheory in the last 25 years or so. Since World War II, neoclassical microtheory has been a dependable source of new

problems and the mainspring of theoretical innovation. In contrast, it is hard to see that the central core of macrotheory has progressed much beyond, say, Modigliani's classic 1944 paper.[1] But--it does not follow that modern microeconomics does provide the building blocks that would do as foundation-stones for a more satisfactory and progressive macrotheory.

Consider the requirements that a macroeconomist would have to put on the microanalysis he could use: (i) the "microfoundations" would have to be simple and allow simplified, stereotyped descriptions of the behavior of individual agents; (ii) they should be applicable to the study of disequilibrium processes and not just to equilibrium states.

If, for present purposes, we can take it that "macroeconomics" has as its main subject the problem of the "coordination of economic activities" in the economic system--with particular attention, of course, to coordination failures--it is clear that the important problems of the field concern "disequilibrium motions" of the system. Providing explicit microfoundations for models of macroeconomic equilibria, we know how to do--thanks to Patinkin and others. But we have also learned from Patinkin's book[2] that the main yield of that enterprise is a crop of monetary "neutrality propositions" that teach us little about problems of unemployment or stabilization policy.

It seems to be commonly presumed that "microeconomics" is a single, unambiguous, relatively well-integrated body of analysis. But suppose that there were to exist more than one "variety of price theory"--of simple price theory--where, then, should macroeconomists go to borrow their microfoundations?

The main objective of this paper is not to answer this question but to show that it is a serious one with a multitude of rather difficult aspects.

I.

The conceptual differences between the great "neoclassical" system-builders remain of considerable interest and significance to us here and now. Their "common denominator", on the other hand, has with time become uninteresting and an obstacle to clear thought. The common denominator goes under the label of the "Marginalist Revolution"--portrayed as the simultaneous discovery of the first derivative of practically everything (followed, after decades of hard "neoclassical" work, in due course by the discovery of the second derivative of absolutely everything). This is a conception of the work of the "neoclassical" giants that irreparably trivializes their contributions in the eyes of a calculus-trained student generation.

How many distinct varieties of "neoclassical" price theory would we have to recognize? Surely, one ought to study at least the following "schools of thought":

(i)	Walras-Lausanne,	
(ii)	Marshall-Cambridge,	
(iii)	Menger-Austria,....	and then the important European hybrids
(iv)	Wicksteed-LSE, and	
(v)	Wicksell-Stockholm.	

But that is not the end, of course. Modern core-theorists are bound to insist on a place for the solitary Edgeworth, for example. And can we fit J.B. Clark and Irving Fisher into the above classification or should we add an "American school"?

This paper, however, will deal with only two of these varieties of price theory: the Walrasian and the Marshallian.

One caveat needs emphasis: the terms "Marshallian" and "Walrasian" will be used incessantly in what follows. They should not be interpreted as

referring specifically to the works of Alfred Marshall and Leon Walras. The attempt here is to sketch certain aspects of two approaches to price theory, of two "modes of thought", so that their presumptive analytical manifestations can be discussed.

A brief historical note, before we proceed: in the late 1920's, the Walrasian tradition was--despite Gustav Cassel's best efforts--practically dead. From a British vantage-point at least, Marshallian thought dominated all the civilized world.[3/] Leaving the Austrians out of it, what happened?

The Marshallian tradition was first softened up by The Trouble over Supply Curves that Sraffa initiated.[4/] For the Marshallian rank-and-file, certainly, simple Supply-and-Demand analysis was that to which allegiance was first of all owed. Supply-and-Demand analysis becomes difficult to do without the Supply curve--and during the 1930's Marshall's Supply curve disintegrated to be replaced by an increasingly "rich" (less coherent) taxonomy of possible "cases". Most demoralizing.

Next, the Keynesian Revolution was to Marshallians in the first instance a palace-coup-Cambridge headquarters taken over, from within at that, by the unfaithful.

Then followed Hicks' Value and Capital and Samuelson's Foundations of Economic Analysis[5/]--and a Walrasian revival with a vengeance. One is almost at a loss for words strong enough adequately to convey the combined influence of these two works on the development of Economics since World War II. Almost everyone, or so it seems, trained in the U.S. in the last 25 years or so has been trained in the Walrasian tradition of microtheory. If the term "neoclassical" means anything of consequence at all, in fact, it surely means

"Neo-Walrasian" in everyday professional parlance. And this latter-day Lausanne school has of course shown itself to have almost a natural monopoly on the recruitment of first-rate, influential talent in "pure" theory.

If it is justified to attribute the revival and subsequently increasing dominance of the Walrasian tradition in large measure to these works of Hicks and Samuelson, we are faced with a minor riddle, namely, that there is much about Marshall in both books and that it is by no means obvious that Marshall is made to play second "neoclassical" fiddle to Walras in either work. The answer to the riddle lies in the theoretical research programme adumbrated in Hicks' early works and explicitly proclaimed by Samuelson. Briefly, the main task set before theoretical economists was the unification of choice-theory in all its applications in terms of the mathematics of constrained optimization. In the last two decades, this research programme has paid off most handsomely.[6/] In retrospect, it appears, it gave the advantage to Walras over Marshall. Walras' general equilibrium model was built up from clearly defined individual conceptual experiments to market experiments that were as artificial as they were expedient. Marshall, in effect, worked "backwards" from conceptual experiments intended realistically to simulate actual markets--his first and main concern--to comparatively more rough-and-ready individual experiments tailored to serve the market theory. Under neo-Walrasian scrutiny, Marshall's choice-theoretical constructions proved irritatingly beset by not easily expunged errors and ambiguities (constant marginal utility of money income, consumer's surplus, the ceteris paribus conditions for the demand curve).

At the intermediate level, it is still common to teach a pretty ruthless mix of Marshallian and Walrasian price-theory. Best-selling texts will set

chapters on U-shaped cost-curves and on competitive general equilibrium more or less side by side without much of a hint to the student that, perchance, oil and water are being mixed. But, twenty-five years after Samuelson's Foundations, there is virtually no place left in an up-to-date, systematic and rigorous treatise for Marshall as a contributor to modern neoclassical microtheory.[7]

If Marshall took "short-cuts" in his treatment of individual choice-experiments, so did Walras in dealing with interactions of transactors at the market level. As long as the exploitation of the mathematics of constrained optimization remained the core of the working agenda of theorists, Walras' short-cuts did not come home to roost. It is only in recent years that, as the perceived reasons for wanting to do without tâtonnement assumptions have grown stronger, the problems inherent in his conceptualization of the coordination of economic activities have begun to become apparent.

To this crude sketch of how the Walras-Lausanne tradition has come to "sweep the civilized world" (if the expression may be pardoned), one note must be added. In the U.S., there has remained an active group of "recalcitrant Marshallians" centered at the University of Chicago. Its exception to the trend in economic theorizing noted above was first sounded by Professor Knight in his "Realism and Relevance in the Theory of Demand." The case against the Walrasian revival was spelled out in Friedman's review of Lange and the corresponding case for Marshall in the well-known paper by Friedman already cited. The price-theory texts by Stigler and by Friedman are probably the most consistently Marshallian of those widely used.[8] Neither the Trouble Over Supply Curves, nor the Keynesian Revolution, nor again the formal unification of choice-theory had any deep influence on the methodological

outlook of this group of influential economists. Their intellectual independence of these developments had its parallel in their continued respect for Marshall's achievement.

With regard to the "Chicago" case for Marshall, it is probably the case that few economists now retain a clear memory of the broader "realism and relevance" arguments. What has stuck and what students are still asked to learn is much more specific--the matter of demand-curves constructed with real income, instead of all other prices, as the ceteris paribus specification--i.e., the "income-compensated" demand curve associated with Knight, Friedman, Bailey, and Buchanan.[9/]

II.

The objective of our discussion in this paper, in the end, is to essay some comparative generalizations about Walrasian and Marshallian modes of analysis. Before any such tentative generalizations are let aloft like so many hot-air balloons, we had better anchor them to some specific exercises so that they will be less easily wafted away by readers impatient with "methodology." The choice of illustrative exercises below may seem odd and ideosyncratic, so that some preliminary remarks by way of explanation are in order. Firstly, the objective of comparing the Marshallian and Walrasian traditions dictates in part the setting of our examples--they should be standard topics within either literature. Hence, we will assume "atomistically competitive" markets in obeisance to Walras and focus on a single "industry" in deference to Marshall. Secondly, we scrutinize microtheoretical constructions with ulterior macro-motives. Knowing that all the simple models we are likely to find will be equilibrium models, the question to be kept in mind is "Can we throw out the market equilibrium condition and keep the rest of the

construction as microfoundations for macrotheory?" Thirdly, it is desirable to avoid entanglement here with the most recent Marshall vs. Walras Methodenstreit which in the main took place in a comparative static (equilibrium analysis) setting. That debate concerned the theory of demand and focused on the ceteris paribus assumptions underlying "the" demand curve. To dodge it, we will avoid dealing explicitly with household demand and, for the most part, proceed as if questions concerning the choice of ceteris paribus assumptions had commonly accepted resolutions.

For the reasons given, then, we will be mainly concerned with the producer-side of a single atomistic industry producing a homogenous output and using only one variable factor. The analytical constructions of immediate interest to us are the schedules describing the supply behavior and derived-demand behavior of producers. To begin with at least, we will assume a one-to-one correspondence between amount of output and amount of variable input so that, in any given situation, there is really only one decision to be made about quantities and only the ratio of output price to input wage need to be considered. Convenience alone will dictate whether we focus on the supply of output or the demand for input; when the former, p will denote the relative price; when the latter, w should be interpreted as the reciprocal of the same price ratio.

The price-quantity correspondences portrayed by the usual "schedules" may be written à la Walras or à la Marshall:

$$q = \Phi(p) \qquad \text{or} \qquad p = \Psi(q)$$

The distinction between these two representations[10] will be the connecting theme for the sundry observations, questions, and reflections to follow. Yet--is there a meaningful distinction to be made? If graphed, the Walrasian and

Marshallian "schedules" will, on standard assumptions, be geometrically congruent in the (p, q) plane. In such cases, we are wont to treat the algebraic inversion of one into the other as a trivial (and obviously admissible) manipulation and to go back and forth between the two in our analysis without hesitation or embarrassment.

Hence, if we are going to try "to make anything of" this distinction, one question that will have to be dealt with is whether this use of the "inverse function rule" is safe. Can we depend upon "economic-conceptual equivalence" always being preserved when this manipulation is undertaken? Are the respective conceptual experiments underlying the two functions really "equivalent" -- for all settings, including disequilibria -- so that the treatment of one as the inverse of the other is always legitimate?

One had better hesitate before asserting that the answer "must be" in the affirmative. What basis would we have to adduce for such an unqualified assertion? While we now have a much more detailed and refined understanding of the construction of Walrasian excess-demand functions than some decades ago, the Marshallian demand-price and supply-price constructions have not received much attention during that time. Modern duality-theorems, for example, are not necessarily pertinent. They deal with <u>efficient</u> allocations (correspondence between equilibrium price and quantity vectors) and, furthermore, the conceptual experiments underlying them presume the existence of some central information-gathering and -processing agent operating, alternatively, on Walrasian auctioneer or on Gosplan-type principles. Nor can the literature on the stability of general equilibrium provide much reassurance -- most obviously because, so far, it has concentrated on pure exchange and little work has been done on systems with on-going

production. The processes investigated are iterations in the vector of prices confronting price-taking transactors who only make decisions on quantities -- which is to say that the system of equations for which the iterative procedure is to find a solution is comprised of functions of the Walrasian $q = \Phi(p)$ type. There can be no presumption that "it would make no difference" to include also processes iterating in produced quantities and relying on Marshallian $p = \Psi(q)$ functions. The presumption, in fact, would have to be that it does make a difference -- although how significant it will turn out to be is at this point a matter of conjecture only.

We could ensure ourselves of having two functions that are inverses of each other in a particular case, of course, by making sure that both embody answers to essentially the same individual experiment questions -- with, in particular, the same ceteris paribus conditions imposed on the envisaged respondent. (If the ceteris paribus conditions for the transactor were not the same, it is obvious that geometrically congruent schedules would not be obtained -- so that possibility we will ignore.) We could, for example, subject a hypothetical, competitive producer to two successive questionnaires: (i) "For the prices listed, indicate what quantities you would choose to produce?" and (ii) "For the rates of output listed, indicate what would be the minimum selling-price at which you would be willing to undertake the production of that output?" Providing the answers are honest (and barring arithmetical errors), this should give us two "invertible" functional relationships.

This, however, only tells us that if distinctions between supply-functions and supply-price functions possibly should sometimes be made,

we will not get at them by considering individual experiments "in isolation." We should, therefore, start by considering the wider context for use in which these constructions have been adopted, which is to say, consider the nature of the market processes in which the producer is assumed to be participating. Imperfectly competitive industry structures we have ruled out. But is it enough to know that we have a case of "pure competition" in order to have all the information about interactions in the market that is needed to model market behavior? If alternative conceptualizations of "competition" have to be considered admissible, we had better consider whether those inherent in the Walrasian and Marshallian traditions are the same before concluding that the "inversion problem" is no problem at all.

Starting at the most elemental level,[11] we have the following representations of the two basic "market mechanisms":

Walrasian homeostat	Marshallian homeostat
W:1) $q^d = D(p)$	M:1) $p^d = d(q)$
W:2) $q^s = S(p)$	M:2) $p^s = s(q)$
W:3) $\Delta p = f[D(p)-S(p)]$	M:3) $\Delta q = h[s(q)-d(q)]$

Since we are equally interested in the behavior in and out of equilibrium, the usual equilibrium conditions are here replaced with adjustment rules such that the market is in equilibrium when the adjustment rule yields "no change." Denoting excess demand by x and excess supply price by π, the adjustment rules should, at a bare minimum, have the following properties:

$f(0) = 0$	$h(0) = 0$
$f(x>0) > 0$	$h(\pi>0) < 0$
$f(x<0) < 0$	$h(\pi<0) > 0$

This is all very crude, of course, but the more obvious refinements would be made for purposes other than those that will here concern us. Assuring continuity, for example, will not be an object. Note that we refrain from allowing the possibility that our demand and demand-price, supply and supply-price functions may depend upon the first and/or higher time-derivatives of price or quantity[12/] or that our adjustment rules may depend upon time-integrals of past "errors." These simplifying assumptions are not made because they are sensible, but simply to narrow the range of problems confronting us.

At this point, we adopt a notation that carries an implicit warning against facile employment of the inverse function rule. We have: q = actual output rate; q^d = "desired" purchase and consumption rate; q^s = "desired" sales and output rate; (the latter two obtained by aggregating individually optimal quantities); p = "ruling" market price; p^d = the maximum price at which consumers are willing to absorb a given rate of output; and p^s = the minimum price required to induce producers to continue a given rate of output. Consequently, treating, say, M:2 as the inverse of W:2 would only be legitimate on the prior demonstration that the particular analytical context considered is one in which p and p^s, and q^s and q, respectively, have the same economic-behavioral interpretation.

The market has been likened to a thermostat often enough. Either one of the above homeostats is a bit more sophisticated, however. A thermostat adjusts temperature on the basis of observations on the discrepancy between the actual and a known "desired" temperature. In the case of an atomistic market we cannot assume that anyone knows (or would care if he did) what the equilibrium price or output rate would be.

So, the "error" governing price-adjustment is the discrepancy between the amounts demanded and supplied at the momentarily ruling price. Starting with p_o, a price "crié au hasard," we have an iterative process with successive adjustments forced by the inconsistency of quantity-choices at given p's:

$$p_1 = f[D(p_o) - S(p_o)] + p_o$$

$$p_2 = f[D(p_1) - S(p_1)] + p_1, \text{ etc. . . .}$$

Similarly, with the Marshallian picture of the feedback mechanism, we begin with a quantity "créée au hasard" and obtain adjustments to the output rate forced by inconsistent valuations of the actual output:

$$\dot{q}_1 = h[s(q_o) - d(q_o)] + q_o, \text{ etc. . . .}$$

For the standard case of downward sloping demand and upward sloping supply loci, the two homeostats yield the same "stability" conclusions. One reason for the mental habit of not "worrying" about what type of schedules we work with may be that this case is, indeed, so standard. Consider, however, one of the cases for which Marshall and Walras discovered that their respective stability conditions disagreed, say, the one in which the supply locus is downward sloping and intersected from above by the demand locus. A Marshallian iteration will home in on the solution represented by the intersection, a Walrasian will wander away from it.

Consideration of the anomalous cases, even when they are of little interest in themselves, is worthwhile in that it forces one to recognize that the Marshallian and Walrasian homeostats are not just alternative conceptualizations of "the market-mechanism." Rather, a complete account of a market-mechanism for a produced good will have to include "governors"

for both price and the rate of output, for the market is more complicated than a thermostat also in that there are these two variables to be controlled rather than just one. Such a complete account would, therefore, have to explain how the two homeostats controlling, respectively, price and output are "coupled." This "coupling problem" -- largely ignored in most accounts of "simple Supply and Demand analysis" -- may be resolved in various ways.

We will consider one such possibility, labeled the "fish-auction case," in some detail. The coupling that it postulates is basically due to Marshall and will, not surprisingly, yield a verdict for Marshall in the matter of the contradictory stability conclusions just mentioned. In the fish-auction case, the industry output of a homogenous perishable good is, on any given market day, a historically given datum -- the aggregate result of past decisions by individual producers. This quantity is then sold off in its entirety before the end of the market day -- to make things quite unambiguous, we assume that it is auctioned off in accordance with strict tâtonnement auction rules (no "false trading," etc.). The resulting market clearing price is then compared with the supply price for this output rate to determine whether, next, production is to increase or decrease. The new production decisions give a new industry output to be auctioned off. . .and so the iteration proceeds.[13/]

Although this market adjustment process leads to the same result as the Marshallian homeostat for the "non-standard" case introduced above, it is not the same mechanism as that described by equations (M:1)-(M:3). Our first version assumed that producers adjust output in response to the discrepancy between supply price and demand price but without explaining how any firm could know what the demand-price is. The present account

plugs this loophole at least by the auction-assumptions that ensure that the ruling market price reveals the demand price to producers.

Consider the respecifications and reinterpretations of our original equations entailed by this particular mode of coupling the two homeostats. Since "speculation" on the course of prices has been ruled out, equation (W:1) remains as before. The tâtonnement process is now changed into a search for the price that will clear the market of a momentarily fixed amount of output. Hence (W:3) should read $\Delta p = f[D(p) - \bar{q}]$. (M:1) has to be rewritten as a "reduced form" relating market clearing price, p^*, to output rate $p^* = d(q)$. (M:2) should properly be disaggregated into supply-price functions for individual firms with, obviously, a corresponding reformulation of (M:3). For the moment, this can be held in abeyance, so we retain (M:2) and rewrite (M:3) as $\Delta q = h[p^s(q)-p^*(q)]$.

Note that (W:2) was lost altogether in this shuffle. The variable q^s is eliminated from this market experiment. The fish-auction market mechanism works to coordinate activities without at any point requiring input of information on what quantities firms would like to supply at any given price.

The adoption of a particular coupling of the "mechanisms" regulating price and quantity must not be thought a mere "mechanical" procedure. It has to be based, even if implicitly, on a theory of market interactions for the case at hand. The theory behind the fish-auction mode of coupling dictates that the "downward-sloping supply locus" be interpreted as a supply-price function for a decreasing cost case. (If the only legitimate interpretation were that of a backward-bending supply function, the fish-auction coupling makes no sense at all -- and produces, of course, the

wrong inference about stability.) Ignoring pecuniary externalities, we may regard it as an increasing returns to scale case.

Increasing returns to scale for an industry that has not yet had time to gravitate to oligopoly or monopoly may not be worth much contemplation in its own right. But here it helps make a point. The decreasing supply-price schedule is a function which, naturally, can be inverted. Its inverse form, however, has no Walrasian interpretation -- Walrasian individual conceptual experiments will not produce a supply-function congruent with the supply-price schedule for the range in which production is not subject to diminishing returns.

The constant cost case may seem even simpler for that supply-price function just does not have a mathematical inverse. But that fudges the point which is rather that the Walrasian auctioneer will be in trouble trying to coordinate activities (if he does not come up with some new iteration procedure) whereas the Marshallian producers will obediently settle down to providing the equilibrium industry output rate, following the rule not to change output when excess supply price is zero and not worrying their heads with speculations over the determinateness or lack of it of "optimal" supply.

Two lessons are to be drawn from this sketchy discussion of "odd" cases that continue to apply, and should hence be retained, when we turn back (to stay) to comfortably "standard" upward sloping supply loci. (i) Mathematical functions are not lent economic meaning simply by naming their arguments "price" and "quantity." In order to have an assured economic interpretation they must be linked to some defined conceptual experiment. The increasing and constant returns cases illustrate

that the conceptual experiments defining Walrasian and Marshallian behavior "schedules" are not the same. This remains true for standard diminishing returns conditions. Under those conditions, the Walrasian and the Marshallian conceptual experiments both define functions that (a) can be inverted, and (b) happen to be congruent. Use of the inverse of, say, a supply-price schedule may then sometimes be helpful in solving some Marshallian comparative static exercise or other -- but one must then be on guard against the tempting inference that this mathematical manipulation changes the conceptual experiment into a Walrasian one. (ii) What makes the difference in the conceptual experiments defining the functions taken as descriptive of transactor behavior are the assumptions (explicit or implicit) made about interactions at the market level, or what was referred to above as the "coupling" of exchange and production. Assuming the usual case of diminishing returns does not, for example, change the central feature of our "fish-auction" model -- it still makes no use whatever of the Walrasian supply-function. In particular, one should note that knowing that "atomistic competition" is assumed does not give us enough information about the market setting in which individual behavior takes place to permit unambiguous interpretation of the "schedules" often portrayed as the result of purely individual conceptual experiments.

This second lesson may deserve some elaboration. The fish-auction model is presumably in accord with our notions of "pure competition." We have "innumerable" consumers and, on the producer side, a fishing fleet composed of "atomistic" (albeit non-nuclear) boats. No producer is a "price-setter," much less a "price-leader." Nor is any producer allowed even to express a reservation-demand. By assumption, the market "clears"

each day -- it will never close with either inventories of unsold fish or queues of unsatisfied buyers raising a stink.

Yet, even with its daily tâtonnement, this is not a Walrasian "competitive" market. Nor can we accurately describe the individual experiment characterizing the behavior of the representative producer in the terms that we are accustomed to use in Walrasian contexts. The reason is, of course, that this is not a tâtonnement with "bons" but a tâtonnement that only clears the market of the ex post catch of the fleet.

"Pure competition" has the textbook association of "the individual producer facing a perfectly elastic demand curve." In a Walrasian setting, the proposition that the producer "can sell all he wants to at the 'ruling' price" is a perfectly accurate verbal interpretation of this mathematical property of the demand function. In the Marshallian setting, this would have to read differently: "the producer can sell any quantity at the same price as other sellers." Note the consequent ambiguity of the term "price-taker." Neither the Walrasian nor the Marshallian producer "sets" price. But the Walrasian producer's decision problem is one of choosing his profit-maximizing output rate for a price of which he is given prior knowledge -- producing, as it were, on contract for the auctioneer. The Marshallian fisherman, on the other hand, can know only the recent history of fish price; in setting out to sea and in deciding how long to stay out and whether to cast the net one more time he cannot know what price the day's catch is going to fetch. It follows that, while "there is no excuse" for the Walrasian producer who fails to choose the profit-maximizing output rate, our Marshallian fisherman will often find that they have done so. Even when the fleet's

average earnings net of variable cost are fairly high, we might expect some boats to record losses and the owners of many more to regret that they did not produce a larger or smaller quantity than they did. Notice our use of ex ante language in discussing the Walrasian producer and ex post language for the Marshallian.

There is a corresponding ambiguity connected with the term "pure" or "perfect competition" here. Our Marshallian fishing industry is "not-imperfect," for none of its firms can vary the ratio of the price it receives relative to what other producers receive by its own quantity-adjustments. No firm has a downward-sloping demand-curve "of its own." But this is not the same as saying that the individual firm can take market price as a known constant or even as a variable whose values are totally unaffected by its own actions.[14/] In the present context, at least, there seems little reason why we should not get rid of the riddle caused by assuming that the partial derivative of market price with respect to the jth producer's output is viewed by j as strictly zero and the corresponding crippling association of the concept of "competition" with the "innumerable seller's" case.[15/]

Consider, then, the task of describing (or "explaining" or "predicting") the individual seller's behavior in this Marshallian setting. When charged with such a task, we are (nowadays) in the mental habit of automatically casting it as an ex ante choice-theory problem to be formalized, conventionally, as a constrained optimization exercise. In those terms, however, we are clearly one assumption short of having a soluble individual experiment -- which is rather odd since the description of our "fish-auction" market experiment seems complete enough. To describe the individual fisherman's behavior in ex ante choice-theoretical terms, his lack of knowledge about the price at

which he will be selling needs to be patched up somehow. One way out of this is to adopt some postulate or other about his price-expectation (with the proviso that his expected price is independent of his own planned output.) That it is not altogether easy to produce a satisfactory model this way is better conveyed by example than by extended disquisition. The simplest way of making the choice problem determinate is produced by the twin assumptions that the seller has static single-valued price-expectations ($p^e_{T+1} = p_T$) and treats the expected price as a certain prospect. These two assumptions bring equation W:2, the supply function, back into good standing -- and produce the "Cobweb" process in its strict form which, even when it does not blow up in our faces when checking with both the Walrasian and the Marshallian homeostat had assured us of stability, has producers constantly cursing the results of their decisions of yesterday but doggedly sticking with the same decision-rules today. And repairing this unfortunate piece of idiocy will require abandoning the use of steady-state schedules, such as W:2 as descriptions of short-run behavior.

A less naive (and even more complicated) way of completing the ex ante choice description starts from the recognition of the equally arbitrary and unnecessary nature of the assumption that $\partial p^e/\partial q_j = 0$. Abandoning that assumption, however, does not by itself present us with a well-defined (expected) marginal revenue function for the individual producer. What it does, rather, is give us our pick of a large set of conceivable games that the producer might play with more or less sophistication about the conjectural interdependence of his and other players' play. Essentially, he would regard the other producers, collectively, as one "large" opponent in a David vs. Goliath duopoly game of the general Cournot type. This line of reasoning, obviously, we had better abandon quickly.

If we shy away from pursuing these approaches, there remains the alternative with which we started in which the simple fisherman -- rather than taking Pontryagin for his prophet to give guidance in daily conduct sub specie aeternitas -- obeys a rule of the form:

$$q_{j,T+1} = h[s_j(q_{j,T}) - p^*(q_T)] + q_{j,T}$$

Given our earlier self-imposed limits on the degree of sophistication we will allow, this is not an unreasonable behavior-description to adopt. It is refreshingly free of teleological elements. As a rule-of-thumb imputed to the producer, it is not an obviously bad one for (ignoring, again, continuity problems)[16] if and when the market settles down to a steady state, $[p^*(q) = \hat{p}(\hat{q})]$, we should find the jth producer correspondingly homing in on his price equals marginal cost solution. Note that decisions are described as incremental -- an ex post reference to "what j is already doing" always enters into the "prediction" of what he will do. This entails a conception of market equilibrium as a state in which the "representative" transactor evidences constancy of observed behavior.

By the same token, the adoption of this mode of explaining behavior means that one refrains from the ambition to explain the decision at T as an "ab ovo choice." The Δq_j-adjustment rule is not here a result of applying the Maximum principle. Anyone who, in reading "Marshallian" market theory of this description, insists on attempting to interpret it as built up from individual choice experiments with well-defined representations in terms of the mathematics of constrained optimization is more or less bound to conclude that its "choice-theoretical foundations" are ambiguous, incomplete, or just generally lacking.

The "fish-auction case" used at tiresome length here is, naturally, a

very specialized one even in the broad "Marshallian" terms sketched here. We have stressed those aspects of it that make it different from a corresponding "Walrasian" model. Before abandoning it, we should note that this M-model still retains some familiar properties shared with standard W-models, namely:

(i) all decisions are represented as based on price-signals only; no quantity-signals are relevant -- no producer, for example, ever has to cope with "sales-difficulties;"

(ii) "no one sets price"; all agents whose behavior is accounted for are treated as price-takers.

III.

The construction of a Walrasian model follows a familiar, well-defined 3-stage sequence: individual experiments, aggregation, and "market" experiment.

The individual transactor is a price-taker making quantity choices. Recalling the ambiguity of "price-taker," we had better specify him as a "parametric price-taker." In a single individual experiment, given prices together with his endowment or production-possibilities give us his opportunity set; maximization of a utility or profit criterion function solves the decision-problem for the conditions given; the solution values for the quantity variables are associated with the fulfillment of characteristic individual optimality conditions; with inventory-holdings precluded (as before) by assumption, the optimal value minus the endowed value of a quantity-variable is said to represent the "planned" ("intended," "desired," "notional") trade in the good in question, call it the ith.

The transactions plan resulting from the solution to this constrained optimization problem we may term a "contingent q-plan" -- it is contingent

upon the price-vector in question being a market-clearing one and it involves only decisions on quantities.

The single individual experiment produces a point on the transactor's ex ante endowment (and technology) constant net demand/net supply schedule for good i. Repetition of the experiments for different values of p_i, keeping other objective and subjective conditions constant, defines the entire schedule.

At the second stage, individual schedules are aggregated to form what is usually termed the "market" excess demand schedule or, if the setting of the problem makes it reasonable and desirable, separate "market" demand and supply schedules. The procedure represents some sort of centralized "pooling" of information on contingent q-plans.

At the third stage, we find the value of p_i, denoted $\hat{p}_i$, for which $x_i = 0$. (In our "rigged" single-market illustration, we assume that the general equilibrium values for all other prices have already--somehow--been set.) The "market" experiment consists of finding the value of the price-variable satisfying the condition that the sum of contingent consumption plans equal the sum of contingent production plans.

On this familiar set of instructions on "How to Build a Walrasian Model on the University's Time," we have the following observations.

(1) The concept of "equilibrium" employed is one of "consistency of plans --in the aggregate." The equilibrium state obtains if and only if all individual optimality conditions are met.

(2) The distinction between individual and market experiments is perfectly clear and that between individual optimality and "market" equilibrium conditions equally unambiguous.

(3) Standard explanations of the "market" experiment have $\hat{p}_i$ "determined"

through an iterative procedure of the general type represented by our initial equation (W:3). This p-adjustment rule, like the Marshallian Δq-rule, is not derived from a constrained optimization experiment. Unlike the Marshallian one, however, it is not taken to represent behavior attributable to any set of transactors "in the model," but to be obediently carried out by an unaccountable "auctioneer." The account given of individual decision-making may hence be kept pristinely optimal and unmuddied by transitorily merely "satisficing," thumb-rule elements.

(4) The language appropriate to the model runs in ex ante terms throughout. The ex ante/ex post distinction might seem to parallel the clear individual/market experiment distinction above and the solution to the market experiment has, indeed, often been given an ex post interpretation. In general, such an interpretation of the equilibrium values is not warranted. They mark the consistency of plans in the aggregate. As demonstrated most forcefully by Ostroy,[17] the fulfillment of the "market" equilibrium condition does not ensure the realization of all individual plans. The model does not specify anything about the "logistics of trading"; consequently, it leaves us in the dark about how plans are to be carried out--and, more generally, about what observable behavior to expect.

(5) By the same token, the use of the term "market" is questionable--unless it be clearly understood that, in Walrasian contexts, the term is drained of the last dregs of common-sense association. There is no "story" appended to the model to tell us what will happen "if we depose the 'market administrator' and let buyers and sellers loose on each other directly."[18]

(6) The demand and supply schedules that summarize the alternative plans of the universe of transactors are generated by individual experiments (and

aggregation) antecedent to use of the "market" equilibrium condition. To the macroeconomist in search of a brand of microeconomics from which one might "throw out the equilibrium conditions and keep the rest," it might hence seem tempting to opt for Walrasian constructions and omit the third stage and utilize the results of the other two. But, as is now widely realized, things are not that easy. An equilibrium assumption of sorts (or, better, an equilibrium presumption) has gotten into the individual experiments by the back door--the fulfillment of individual optimality conditions yields q-plans contingent upon the parametrically given price becoming the market-clearing one.

In Fig. 1, let the Walrasian construction have the "same" short-run equilibrium as a corresponding Marshallian one at $(\hat{p}, \hat{q})$--although the equilibria do not have the same interpretation. For the Walrasian construction $(\hat{p}, \hat{q})$ is also the only "market clearing" point, while for Marshallian fish-auction models the entire DEd locus--the $p^*(q)$ locus--consists of potential market clearing solutions.

Two sets of restrictions define the $(\hat{p}, \hat{q})$ point. (1) Consumption, purchase, sale, and production quantities are all equal--albeit in an ex ante sense for the Walrasian case and (as we shall argue) in an ex post sense for the Marshallian. (2) The valuations of the marginal quantum made on the two sides of the market are equal--in the Walrasian case for any arbitrarily selected buyer/seller pair; in the Marshallian, the buyer may be picked at random but must be paired with the "representative" supplier in checking this condition.

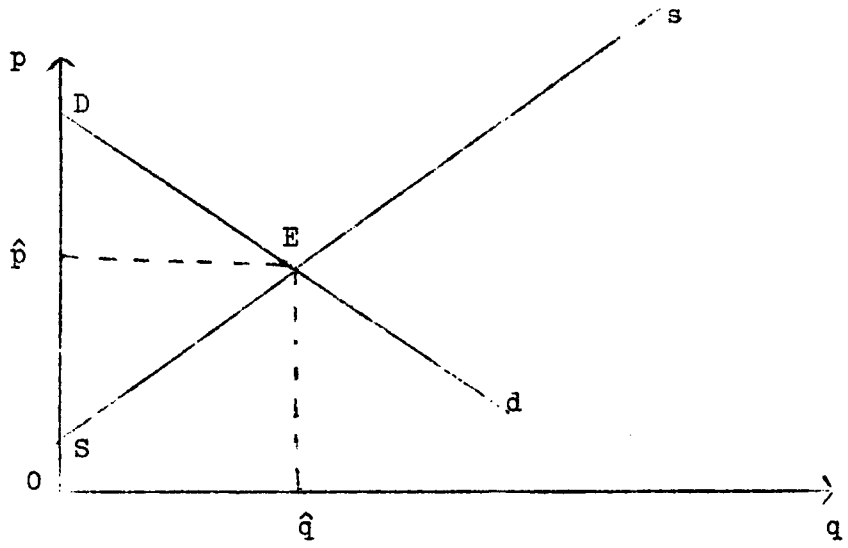

Fig. 1

We will have a general market disequilibrium theory when we are able to dispense with <u>both</u> sets of restrictions and still to handle the analysis in a reasonable rigorous and "potentially testable" way. Here we will not even comment loosely on what problems this task entails. Of the various, more or less contrived (but easier), special cases, the analysis of which may help us <u>en route</u>, we make note of two. One is again the fish-auction. It results, obviously, from relaxing the second set of restrictions while retaining the first. This enlarges the set of admissible solutions from the single short-run $(\hat{p}, \hat{q})$ equilibrium point to include all the "fixed-output, market day" market-clearing states represented by the $p^*(q)$-locus.

From a Walrasian point of departure, the corresponding first step towards a more general disequilibrium analysis is taken when we relax the first set of conditions and allow $q^s \neq q^d$, and consider developing the analysis of "fix-price market day" cases. From a purely formal standpoint, there will be a rather "rich" taxonomy of these--enough to occupy those so

inclined for some time to come. When market day states such that $p \neq \hat{p}$ are allowed, terminating the analysis at a point where nothing but statements about individually optimal q-plans have been derived becomes flagrantly inadequate. The rules under which "false trading" is supposed to be carried out have to be specified so that statements pertaining to observables can be made. With the competitive conditions (absence of coalitions) assumed in this paper, one such specification comes naturally, i.e., that "the short side dominates." When $q^s < q^d$, the suppliers rule; when $q^d < q^s$, the buyers have it. Wilfully abstracting from (possibly necessary) intermediate transactions, we conclude that we have thereby enlarged the set of admissible solutions to include all points on the locus DES. Thus the fix-price market day model predicts that the quantity transacted will not be observed to exceed E; the points on Ed, potentially observable according to the fish-auction scheme, are ruled out; those on SE, ruled out by the fish-auction, are rendered as possible.[19/]

Modelling of a case belonging to this family of short-side dominated fix-price cases requires a number of additional analytical decisions. When the supply side is the short one, assumptions have to be formulated about how buyers are rationed and about what relation amounts consumed might bear to amounts purchased, etc. When the demand side is short, analysis requires assumptions about the "rationing" of realized sales and about the relation of amounts produced to those sold. It would take us too far afield to comment on the complications that arise along this route of theoretical inquiry.

The point to be made here is that "false trading" will make just as much of a muddle of the choice-theoretical foundations of the standard Walrasian models as "false production" caused in our Marshallian cases. Consider a

Walrasian competitive producer. In the standard individual experiment, he "takes" the real wage, w/p, as parametric. His production possibilities show diminishing marginal physical product of labor. Using the optimality condition that the marginal rate of transformation of labor into output should be set equal to p/w, he calculates his desired input-purchase, production rate, and output-sale. In describing his planning in this way we attribute to him the belief that he won't find himself on the long side of either the output or the input market. His optimal q-plan is conditional on this belief being justified by events. Now, then, suppose he finds himself on the long side of the output market. Realized, observed magnitudes at variance with the expectations implicit in the standard choice-experiment cannot be introduced in the ex ante individual calculus without wrecking its original formulation. The conditions customarily postulated suffice to make the optimal q-plan determinate. "Just adding" another equation--an independently specified sales-constraint--would make the problem overdetermined. So, something has got to give. We may reasonably suppose that the producer experiencing an "effective" sales-constraint will give up the "notion" that he can expect to sell his optimal output. This means that the standard optimality condition is abandoned as a decision-rule. It does not mean that some other rule automatically takes its place.[20/] Thus, the choice-theoretical foundations for this disequilibrium case are no clearer than for the Marshallian cases previously discussed.

* * * * * *

We turn ther to the construction of a Marshallian model. The first proposition about it is that the Walrasian 3-stage sequence may not be followed.

To illustrate, consider the competitive firm's marginal physical product of labor schedule interpreted, alternatively, as a labor demand schedule and as a demand-price schedule for labor. Focus on an arbitrarily chosen point on it, say (w', l'). What are the respective conceptual experiments that define this point?

In constructing the Walrasian representation, we "call" the real wage w'. The producer takes this price as parametric and responds with a "conditional order" for l' of labor services. This is the amount of labor input that he has calculated as optimal. We may envisage him as exploring alternatives from l = 0 rightwards along the w'-line--he locates l' as the "top of his profit-mountain."

For the Marshallian construction, we might imagine ourselves instructing the producer to consider employing l' of labor. We could then put two alternative questions to him: (i) "What is the value of the marginal unit to you?" (ii) "What wage rate would you offer to pay for l' of labor services?" The answer to the first one is the one we want ("w'")--but it is less than obvious that it has any behavioral significance.[21] What then about the second question? It asks the producer to consider himself, momentarily, in a hypothetical price-setting situation. Obviously, w' is not in any sense whatever an "optimal" price to pay. If we envisage the producer contemplating alternatives along the l'-line, starting from (0, l'), we have him facing out from the profit-mountain, staring down an uninterrupted slope into a dark abyss of losses. How then do we get him onto the demand-price schedule, i.e., to take up a position at w', when any lower wage-rate would be preferable? Only by postulating that competition in the market makes it impossible for him "to get away with anything less." But in

generating the (w', l') point on the schedule this way, we can hardly claim to have used "choice theory" in any proper sense of the term; we have given the producer "no choice but. . . ." The mode of explaining behavior employed is more appropriately labeled "situational determinism"[22/] than "choice theory."

By the same token, what we have described is not a proper "first-stage individual experiment." We find ourselves unable to complete the description of the individual producer's behavior without relying, in effect, on a market experiment--for the "competitive process" invoked will not back our producer into the desired "situationally determined" corner until it has run its full course, getting laborers into their highest valued employments, making all producers pay the same real wage, etc., i.e., until <u>market equilibrium</u> has been reached.

Consideration of what would be the second stage of construction for Walrasian models leads to much the same conclusion, namely, that "one cannot really do it" if not allowed to presume that the third stage has, as it were, already been performed. In trying to replicate the Walrasian 1-2-3 stage construction in a Marshallian frame, we seem to swim ineffectually against the current of the stream of thought.

Aggregating for a Marshallian model would have us attempting to build up, say, the market supply schedule from the supply-price schedules of individual firms. A given industry output rate, q', is the sum of firm output rates, $q' = \sum_j q'_j$. On the market schedule, q' corresponds to market price p'. We might then suppose that the way to obtain q' is to assume that we give each firm time to adjust its output rate until its supply-price equals p' and that it is the sum of these <u>optimal</u> firm output rates, $q^*_j(p')$, that gives

us q'. Two observations on that procedure: (i) it treats p', for the sake of aggregation, "as if" it was the market equilibrium price; (ii) although there can be no Marshallian objection to conflating stage 3 and stage 2 as noted in (i), this procedure is still not the right one.

This is seen most easily if we turn to the market equilibrium stage and consider supply-behavior at the $(\hat{p}, \hat{q})$-point. In Marshallian analysis, to repeat, equilibrium is interpreted as constancy of observed behavior. But it is the output rate of the industry--of the population of firms--that is to be considered constant, not that of each and every producer. At $\hat{q}$, we have $\Delta q = \sum_j \Delta q_j = 0$. This condition merely requires that the output-changes of individual firms sum to zero. None of the actually existing firms need be at rest with the marginal cost of its on-going output rate equalling the equilibrium price. The "representative" firm is said to have this optimality condition met, but it is a statistical artifact and need have no actual counterpart among existing firms.[23/]

All other points on the Marshallian market supply-function must be interpreted in the same way as (aggregative) steady state points or "potential equilibrium points." Each (p^s, q)-point on the market schedule has the condition $\Delta q = 0$ attached to it. But if $p^{s'}$ were not to be an equilibrium price, "more firms" would be expanding than contracting or vice versa, implying $\Delta q \neq 0$ and hence $\Delta p^* \neq 0$. Each point on the industry supply locus is constructed "as if" the value of the price-variable associated with it was the demand price in market equilibrium, which is to say, "as if" the market demand schedule in effect passed through that very point. The Marshallian supply schedule is thus seen to be a mutatis mutandis construction with the assumptions made about market demand among the conditions "to be changed"

as the schedule is drawn point-by-point. The industry output rate is otherwise not analytically determinable; if we assume a ruling demand price that is not to be considered the equilibrium price for the sake of the exercise (so that the "representative firm" is not in an optimal position), the best we can do with supply is to assert that the industry output rate is historically given and that its rate of change is, in principle, analyticaly determined, e.g.,

$$\Delta q_T = \sum_j \Delta q_{j,T} = \sum_j h_j \left[s_j(\bar{q}_{j,T}) - p^*(\bar{q}_T) \right].$$

As we have already seen, a Marshallian demand locus of "potentially observable" market points--the $p^*(\bar{q})$-locus--is produced from a "purely hypothetical" aggregative demand schedule using a very similar procedure, although the critical assumption in this instance is one of "market-clearing" rather than short run equilibrium.

On this mode of analytical construction, we may then make the following observations.

(1) The concept of "equilibrium" employed is that of "constancy of observed behavior--in the aggregate." That the optimality conditions for all transactors be met is not a necessary, although it would be a sufficient, condition for equilibrium in a Marshallian market.

(2) In Marshallian statics, there is no clear distinction between individual and market experiments, nor--it would seem-could there be. In contrast to the Walrasian 3-stage sequence, the market experiment is in a sense primary in Marshallian analysis. The construction of the static supply-and-demand apparatus starts from the predetermined desiderata of producing two schedules summarizing the alternative steady-state possibilities for the two sides of the market. The supply schedule is to be traced out by shifting the demand schedule and plotting the market equilibrium point for its every

position; and vice versa for obtaining the demand schedule. From this conception of the static representation of the market, one then "backtracks" to propositions about "representative" individual transactor behavior consistent with it. The main object in context will be to check that the representative transactor "would not want to behave differently from what he is already doing." This does not necessarily require the description of a complete choice-theoretical experiment; checking that certain marginal conditions (e.g., marginal cost equals price) are met will often suffice. Note also the ex post character of this "backtracking" into individual experiment--the relevant optimality condition is fulfilled for the price and sales-rate prevailing. In a purely ex ante description of a Marshallian seller's behavior, in constrast, he is ignorant at least of either the price (as in the Walrasian fish-auction case) or of the rate at which he will sell (if a price-setter).

With market experiments built into the description of individual behavior in this way, it is perhaps hardly surprising that the terminology in common use often fails to make a clear distinction between individual optimality conditions and market equilibrium conditions. The equality of marginal cost and price, for example, may be called an "equilibrium condition for the firm." This is somewhat ambiguous. Spelled out in full, it should be "an optimality condition fulfilled ex post for the representative firm when the market is in equilibrium."

(3) As has already been repeatedly emphasized, the output-adjustment rule which is an integral part of the description of producer behavior is not derived from a constrained optimization experiment. For "non-representative" producers optimality conditions need not even be fulfilled in market equilibrium. Judged by neo-Lausanne criteria, this makes the choice-

thecretical foundations of the model seem incomplete or unclear. From a different methodological viewpoint, however, the same properties might be appraised as desirable traits that remove the theory from the realm of deterministic and/or teleological representations of human action.

From neither point of view need the arbitrarily specified Δq-rule be accepted as a "primitive" postulate of the theory with no possible underlying foundation. The neo-Lausanne economist will be tempted to convert it into an "optimal dynamic adjustment function" for a producer with given expectations. This, however, requires assuming that the producer may "consult Fate itself about its intended mockery of human ambitions"[24/] (and be advised, at least, about the odds). The alternative of a Δq-rule learned from past experience would be preferable to many others and, of course, more in accord with Marshall's preference for "biological," rather than "physical," analogies in economic theory.

(4) The language of the theory stresses "potentially observable" and/or realized, rather than planned, magnitudes throughout. In static or comparative-static applications, the interpretation of solution values for the model runs in ex post terms. Even in market equilibrium, the magnitudes "planned" or "desired" by non-representative transactors are left undefined (by the lack of ex ante individual experiments), so that the relationship of plans to realizations is unclear.

5 The Marshallian theory is in the first place a theory about behavior in markets. The theory, to repeat, is phrased in terms of potentially observable magnitudes and attempts roughly to simulate behavior in real-world market. Use of the theory requires prior specification of the "market form," to use Hicks' phrase.[25/] The fish-auction case of Section II is one particular such 'market form"--although one that, by presence of the market-day

tâtonnement, is not quite true to Alfred Marshall. The same market form will not do for all the markets of a macromodel; even if it is assumed that they are all "atomistic," it will generally be necessary to model each one separately before linking them up. For any given market, furthermore, the postulated market form brings institutional features into the model that may fit one historical context but not all. Thus, Hicks has for example argued that Marshall's treatment of manufacturing output markets was badly dated by the 1930's at the latest,[26/] requiring by then replacement with a model capable of handling price-setting market-day producer behavior. One common institutional feature of the various Marshallian market forms we can at least be sure of: it is clear enough that in the Marshallian market for the i-th good, good i is exchanged for money and that we do not have to be preoccupied with the first riddle raised by "institutionless" Walrasian constructions, namely, that all other goods may have to be considered "effective purchasing power" over good i.

(6) In the standard Marshallian short-run supply and demand diagram, both schedules must be interpreted, in Patinkin's terminology, as "market equilibrium curves." Since market equilibrium (or clearance) conditions have to be used in their derivation, these basic Marshallian tools would seem quite hopeless to a macroeconomist in search for a microeconomics that could be used without making equilibrium assumptions. And the Marshallian static construction is, of course, of no use in analyzing processes, except stationary ones. But things are not as bad as that. The general Marshallian theory deals with market interaction processes for which the static apparatus portrays the special steady state case. Whereas the "market form" presupposed in the fish-auction case, which has been our main vehicle for illustrating Marshallian analysis here, may not be the one we want for a macrotheory

applicable to contemporary economic systems, it should be feasible to provide a process-analysis for other market forms to fit within the general Marshallian frame.

The Marshallian approach has features that make it attractive to the macrotheorist. It deals, in principle, in observables. It promises escape from deterministic and teleological riddles. The ex post, historical cast of the process analysis, together with the property that each good exchanges directly for money only, makes it the natural tool for the analysis of "effective demand" problems.

As far as at all possible we have avoided bringing into th discussion the vexatious ceteris paribus problems that the "Chicago Marshallians" have made such a central issue of in their advocacy of Marshallian over Walrasian modes of analysis. Even so, the differences between Marshallian and Walrasian constructions that we have tried to dramatize here bear a clear resemblance to some of the themes that Knight, Friedman, and Buchanan have elaborated around the, to them, central notion of the income-compensated demand curve. Although the perspective is different, the "realism and relevance" comparisons that these authors tend to draw have their counterpart, I think, in the juxtaposition of "contingent ex ante plans" and "potentially observable behavior" stressed here. The Chicago Marshallians are clear on the point that the static M-schedules should be interpreted as loci of "genuinely attainable" supply-equal-demand points.[27] And, as Yeager has observed, their "insistence on genuinely attainable alternatives" carries with it the relinquishing of Patinkin's illuminating distinction between individual-experiments and market-experiments."[28]

Knight's classroom question "How can one price change?" is a piece of

Chicago oral tradition that got into print in the course of the debate over income-compensated versus "Slutzky-Hicks" demand-curves. What is the point of the question? Several, not necessarily incompatible, interpretations are possible. Buchanan[29] recalls the question as the starting-point of an argument to the effect that demand-curves, generated by shifting a parametric price and asking what the optimal amount demanded would be for transactors whose endowments are held constant, are not analytically "useful"--i.e., not "realistic and relevant." The most straight-forward interpretation is simply that Knight wanted to impress on his students that price is an endogenous variable, and that no valid comparative static experiment can be conducted by asking what the "effects" would be of a shift in an endogenous variable, nor can we conceive of a valid general equilibrium comparative static experiment affecting only the endogenous variables in the one market of the system leaving all others constant. While valid, these points are equally as applicable to Walrasian as they are to Marshallian constructions. On yet another interpretation, however, the question does indeed become something of a pons asinorum into Marshallian price-theory. The two static Marshallian schedules are market equilibrium curves. If they intersect at $(\hat{p}, \hat{q})$, we may not use the construction--as we might a Walrasian one--to try to deduce what would happen if price was set, for example, at $2\hat{p}$. The assumption that price is not at the market-clearing level contradicts the assumptions used in constructing the two steady-state reduced forms.

In the interpretation advanced in this paper, this point is the crux with regard to use of Marshallian static constructions. It is quite distinct from the issues having to do with the choice of ceteris paribus conditions. The latter pertain to the relation between partial and general equilibrium experiments. With regard to that problem, I believe the only thing to do is to turn

back to the position of earlier Marshallians and limit the use of partial equilibrium tools to exercises where they will yield a "tolerable approximation" for the purposes at hand. When general equilibrium repercussions must be recognized to make interactions between the supply and demand curve exceed the "secondary order of magnitude," general equilibrium constructions should be used. Neither utility-constant demand-curves nor any other once-and-for-all choice of ceteris paribus assumptions will yield a foolproof partial equilibrium apparatus for the entire class of relevant general equilibrium experiments.[30]

By making the ceteris paribus issue the focal issue of their discussion of the relative merits of Walrasian and Marshallian analysis, the Chicagoans obtained, in my opinion, a skewed perspective on the latter.

IV.

Before concluding, we should note that the Marshallian method of analytical construction is--via Keynes--[31] deeply entrenched in macroeconomics and that horrible confusions result when these constructions are not recognized for what they are. Keynes' Marginal Efficiency of Capital schedule,[32] his Aggregate Supply-Price schedule and his Employment Function are all "market equilibrium curves."[33] The latter two are constructed such that, for a system assumed to have but a single output and labor as the only variable input, they happen to be "congruent" with Walrasian aggregate supply and labor-supply functions generated by "calling" alternative real wage rates, finding the optimal supply of output and demand for labor of individual competitive firms, and aggregating. This congruency invites misinterpretation which, in turn, will pose a dilemma: one cannot at the same time operate with, say, a Walrasian derived demand for labor and recognize "sales-constrained"

behavior on the part of firms.[34] Faced with this dilemma, Patinkin recognizes that Keynes' involuntary unemployment must be associated with "sales-difficulties" and concludes, therefore, that it must be interpreted as "off-curve behavior;"[35] Grossman sees a "completely unambiguous" Walrasian labor demand schedule in Keynes' MPL-locus and concludes that, therefore, Keynes could not possibly have had his firms experience sales-difficulties.[36]

To stress simply that Keynes took his price-theory from Marshall will not suffice to get us out of this predicament, for the Marshallian "market form" we have dealt with here is the fish-auction in which sales-difficulties never occur either. In my earlier work, I suggested that one should interpret Keynes as having reversed Marshall's ranking of price and output adjustment speeds.[37] But this gives us a "fix-price market day," on which we have to expect sometimes to observe combinations of real wage and employment "off" the MPL-locus--a contingency for which, it is clear, Keynes did not attempt to provide an analytical representation. But it is equally clear that his firms are not assumed to act on "called" prices while ignoring the possibility of running into sales-difficulties; on the contrary, "the behaviour of each individual firm in deciding its daily output is determined by its <u>short-term expectations</u>..."[38] How do we resolve this? Keynes' writings certainly are not much help. One will search them in vain for a coherent account of firm behavior in the very short run.[39]

We could go half-way. Instead of reversing Marshall's ranking we could consider equalizing price and output adjustment speeds. The fish-auction and the "short-side dominated" W-case considered earlier both obey <u>two</u> "chronometrics" at the same time. In the fish-auction, for example, we have

$\Delta p_t = f[D(p_t) - \bar{q}_T]$ and, then, $\Delta q_T = h[s(q_T) - p^*(q_T)]$. The t-subscript here might be interpreted as indicating the "minutes" on a 24-hour clock,[40/] whereas the T-subscript is geared to the "dates" of an open-ended calendar. The short-side dominated model likewise has a double chronometric but the reverse one. Two distinct, "coupled" chronometrics do have advantages, permitting as they do the use of "market day statics" to approximate the dynamics of cases where either the velocity of price or that of output is "very high" relative to that of the other.

Keynes makes sense, although his explicit analysis is sadly incomplete, if we interpret him as having the adjustments of both output-price and output occurring on the same time-scale (metered, say, by "minutes") with money-wage adjustments on a different "slower" chronometric ("weeks"). Adopting such a procedure forces one to relinquish some of the simplicities of the fish-auction case and of the Walrasian <u>tâtonnement</u> case considered in Section II. Thus, (i) quantity-signals must be allowed to influence behavior, (ii) the idea that price-movements are governed by quantity "errors" and output movements by valuation discrepancies (equations W:3 and M:3) must be reconsidered, and (iii) the task of setting prices must be turned over to agents "in" the model. The most reasonable inference to draw about Keynes' largely implicit theory of firm behavior is, I believe, that he treated firms as obeying the following two rules in "disequilibrium":

$$(K:1) \quad q_{j,t+1} = s^e_{j,t+1} = k_j[\bar{s}_{j,t} - \bar{q}_{j,t}] + \bar{q}_{j,t}$$

$$(K:2) \quad p_{j,t} = MC_j(q_{j,t})$$

where q_j is the j-th producer's output, s^e_j his expected and $\bar{s}_j$ his actual sales, and p_j the price <u>he</u> sets. Within the market day, output is brought

into line with expected sales and price adjusted to equal the marginal cost of the new output rate. But, having begun by asserting categorically that it is these short-term sales expectations that count and that "actually realized results...will only be relevant...in so far as they cause modification of subsequent expectations"[41], Keynes once again begged off from tackling the "intricacies" of analyzing market day behavior. The apparent promise of an ex ante analysis of producer-decisions geared to expectations is left unfulfilled as Keynes in a few brief pages deftly shifts his ground to a Marshallian short run affording all the ex post conveniences of such steady-state analysis. The trick lies simply in imposing the conditions:

$$(K:3) \quad s_T^e = \bar{s}_T = \bar{q}_T = q_T^s$$

i.e., that expected sales, realized sales, actual output, and "desired" output are all equal for all the "dates," T, to which the explicit analysis is confirmed.[42] Note that, as is usual in M-type analysis, we lost track of individual transactor behavior in going to the short-run market equilibrium analysis. Nonetheless, it is the marginal cost pricing rule, (K:2), that makes the last one of the conditions, (K:3), more than simply an ad hoc assumption. Fulfillment of the "middle" condition (the equality of sales and output) requires $p_T = \hat{p}_T$, of course. Thus, the model predicts that actually observed "T-dated" combinations of the real wage and employment will be found to lie on the marginal productivity of labor locus.[43] Yet, it is still a model where the behavior of firms is geared to effective and not to "notional" demand.

Market day events are left in limbo by this. Previous sections have alerted us to the complications that an attempt at rigorous analysis would have to cope with if one abandons at once both the restrictions imposed in

ordinary static supply and demand analysis, namely, that "desired quantities" as well as "marginal valuations" are equal on the two sides of the market. Keynes is able to cope simply because he wafts these complications aside. The most serious one thus neglected is that of the unplanned changes in inventories that would normally occur in the process of bringing actual output into line with sales. Clearly, if unsold inventories have to be taken seriously, the combination of rules (K:1) and (K:2) will no longer do, for the producer cannot get rid of his unwanted inventory as long as he obeys them

Cost-based pricing rules are kept in ill-repute in our textbooks. The simple--almost surely too simple--(K:2)-rule may be less offensive than most since it is obvious that standard marginal conditions will be met once the firm gets output into line with sales. But this "excuse" is hardly the point. What should be said in its favor is that it is the simplest (rather than "best") example of cost-based pricing strategies that have "reasonable" steady-state consequences. In dealing with price-setting, competitive firms out of industry equilibrium, the alternative to using a Δq-rule of the (K:1)-type, together with some cost-based pricing strategy, is to model the firm as a "transitory monopolist"[44/] trying to keep track of the position and elasticity of a negatively-sloped demand-curve for his own output--which live its shadowy existence only when the industry is out of equilibrium--in order to capture what he can of these most evanescent monopoly profits. To pursue this line of analysis, one has to make assumptions about the information possessed by the firm that seem most unpalatable, even when dressed up as stochastic expectations.

The general approach exemplified by (K:1) and (K:2) has some other attractive features. It is clear, for once, who sets price. When the

competitive industry is in equilibrium, all of its firms should be approaching the same price. But when that equilibrium is disturbed, say, by a decline in demand, the dispersion of prices charged should, it is reasonable to suppose, open up--the incidence of declining sales would not be evenly distributed, the Marshallian firms may not have the same cost-functions, and their reaction-speeds may differ. Only as the industry homes in on a new steady state would the dispersion of prices again shrink back towards a uniform "market price." For macroeconomists who share the hunch that search for price-information is an important part of the plot, therefore, this type of model provides the right habitat for their buyers. To graft search-behavior onto a Walrasian construction in a reasonable manner is not so easy.

One more note on Keynes. His reputation for competence in elementary price-theory has declined somewhat further than he deserves as Marshallian modes of reasoning have gone out of fashion. His chapter on "Expectation as Determining Output and Employment," which we have considered here, illustrates the difficulties that non-Marshallians will have with his "microfoundations" particularly well. The "expectation" that is to be taken as given in explaining the short-run behavior of the firm, he alternately refers to as an "expected price" (p. 47) and as "expected sale-proceeds" (pp. 47, 50-51) and in yet other places it appears to be "expected sales volume" (in labour-units, e.g., p. 282). A reader who tries to make sense of this as an ex ante choice-theoretic experiment must despair--what can you do with a writer who cannot discriminate between his p:s, q:s, and pq:s? By now, the reason for this sort of thing should be clear. There is at any time "a large overlap between the effects on employment of the realized sale-proceeds of recent output and those of the sale-proceeds expected from current input";[45] in

constructing Marshallian short-run behavior schedules, the overlap between expected and realized magnitudes must be treated as complete, for the schedules are not defined for conditions other than those that make "constancy of observed behavior" reasonable. If that is the analytical context, consequently, one is at liberty to substitute realized (ex post) for expected (ex ante) magnitudes and hence to speak of expectations as if they referred to "points" rather than "schedules." The same failure to understand Keynes' analytical constructs as being of the "Marshallian" type underliesthe complaints about the General Theory that, ever since to so-called "Saving-equal-Investment" debate, have been the most familiar and persistent, namely, that Keynes can be depended upon (i) to be consistently obtuse about ex ante/ex post distinctions, and (ii) to give his readers trouble by failing to distinguish between schedules and points on schedules.

V. Concluding Remarks

The caveat made in the beginning had better be re-emphasized in closing: this has not been a doctrine-historical investigation of the works of Leon Walras and Alfred Marshall. To avoid mislabeling the product, footnote references to their writings have been eschewed altogether, even in spots where it is obvious that propositions stated here are fully in accord with the patron saint in question. What we have done, essentially, is to discuss the kind of analytical constructions that seem "to come naturally" if one starts from a prior decision to build with "quantity-into-price" building-blocks and to compare them with the--nowadays more familiar--modelling consequences of beginning with "price-into-quantity" constructs. The "quantity-into-price" approach is associated with Marshall, the "price-into-quantity" with Walras, Pareto, Slutzky, Hicks, Samuelson and the modern

general equilibrium theorists. With the understanding that the terms "Marshallian" and "Walrasian" mean little else here, our usage will hopefully be acceptable.

One doctrine-historical theme of the essay is meant to be taken seriously --albeit, at this point, as a hypothesis rather than established thesis. Even as a hypothesis it is not meant to be understood as a single-factor explanation for there are obviously other strands to the important development to which it pertains. The hypothesis is that the widespread acceptance of the research programme propounded in Samuelson's Foundations--and for which the influence of Robbins,[46] Hicks, Lange and others had helped prepare the ground--in large part explains the displacement of the Marshallian tradition in favor of the Walrasian. The call to exploit the mathematics of constrained optimization systematically and to unify economic theory on that basis switched the focus of theoretical attention from market interaction processes to individual choice problems--from the strong to the weak points of Marshall's structure.

It is a bit tempting to speculate that the temper of the times has in some way favored Lausanne over Cambridge. Analytical schemata set a stage for tales of a like genre. Constancy was a Victorian virtue, consistency a Gallic vice. Marshallian plots are strong on action and have actors with robust motives of the sort that pre-Freudian authors would naturally capitalize: "Precaution, Foresight, Calculation, Improvement, Independence, Enterprise, Pride and Avarice..(or) Enjoyment, Short-sightedness, Generosity, Miscalculation, Ostentation, and Extravagence."[47] Steadfast shopkeepers compete in the market on center stage, enjoy profits and suffer losses, surrounded by stolid householders out for the thick gravy of Cardinal Utility.

The plotting and planning of the dramatis personae should be inferred from their behavior on stage; a good play keeps it largely from view in the wings--self-introspection in public being, after all, rather disgusting. Neo-Walrasian plots have as much action as an existentialist play and characters sicklied over with the pale cast of ordinal utility. With whatever patience can be mustered, one watches individuals, spooked by non-convexities and prone to nightmares over discontinuities, torture themselves with Hamiltonians and such to draw up plans for every conceivable contingency from here to eternity--and never act to either lose or gain.

Be that as it may, we end up, of course, in no position to choose the "right" microfoundations for macrotheory. That neither has a sole claim to "usefulness" is suggested by the theory of effective demand failures to which the distinction between "notional" and "effective" excess demands is central. Notional magnitudes are derived from a Walrasian ex ante construction; the determination of effective magnitudes requires reliance on Marshallian ex post realizations. In analytical construction the theory of effective demand is basically Marshallian, but the diagnosis of effective demand failures depends on the juxtaposition of "notional" Walrasian and "effective" Marshallian states.

Casting judgment is premature equally because of our omissions. Imperfectly competitive market structures have not been allowed to intrude here. The Austrian tradition and the two European hybrids in which it enters have not been let into the running.[48] The Stockholm School,[49] in particular, should deserve a second look for its efforts at constructing theoretical structures to carry both planned and realized magnitudes along in process-analyzes. But, apart from all omissions, methodological "tastes" and

research-strategical "hunches" will in any case be the deciding factors to those who work in the field. And we have gained no new methodological gospel to preach to them.

There are some minor revelations, however, that may deserve to be reflected in the liturgy of the subject. "Disequilibrium," for example, although we used it in the beginning, is a slippery term when we have more than one equilibrium concept in circulation. Partial (general) process analysis may be the more suitable counterpart to partial (general) equilibrium analysis. If tâtonnement must be recognized as but a chapter of the theory of market dynamics, the residual "non-tâtonnement" is probably too broad a term to be useful. It also appears that rather than calling for "micro-foundations" to be supplied, we ought to make up our minds whether it is "choice-theoretical" or "market-theoretical" foundations that we most urgently require.

In lieu of a grand conclusion, finally, a resolution: Let us be done with the term "neoclassical theory."

FOOTNOTES

*) Professor of Economics, University of California, Los Angeles. I am obliged to Armen Alchian and Robert Clower for extended discussions, to Meyer Burstein for remarks in letters of some years ago that have festered until now, and to Harold Demsetz, Stephen Ferris, Jack Hirshleifer, John McCall, and Joseph Ostroy for comments. But this is not the paper these gentlemen would have had me write.

1) Franco Modigliani, "Liquidity Preference and the Theory of Interest and Money," Econometrica 1944. It is hard to imagine us having had a macroeconomic debate of much vigor over the last fifteen years without the role played by Professor Milton Friedman. But Friedman distinguishes his position on the basis of "judgments about empirical regularities" rather than modelling innovations. Thus, the core of Friedman's analytical structure in his "A Theoretical Framework for Monetary Analysis," Journal of Political Economy, March/April 1970, and "A Monetary Theory of Nominal Income," ibid.,March/April 1971, is still much the same as Modigliani's in 1944.

2) Don Patinkin, Money, Interest and Prices, 2nd ed., New York 1965.

3) For a contemporary perspective from a different vantage-point, however, consult Wicksell's 1924 review of the reissue of Menger's Grundsätz: "...he was successful in establishing a school...whose doctrines spread over the whole world, and for a period of fifty years set the course of all work and discussion in theoretical economics, and to some extent in fiscal theory, too." Cf. the translation in Knut Wicksell, Selected Papers on Economic Theory, E.Lindahl ed., London 1958, p. 193.

4) For a perceptive review of this development, cf., G.L.S. Shackle, The Years of High Theory, Cambridge 1967, Chapters 3-6.

5) J.R. Hicks, Value and Capital, Oxford 1939. P.A. Samuelson, Foundations of Economic Analysis, Cambridge, Mass. 1947.

6) For economics since World War II, this has been the "progressive research programme" in the sense of Lakatos. Cf., e.g., Imre Lakatos, "Falsification and the Methodology of Scientific Research Programmes," in Criticism and the Growth of Knowledge, Lakatos and Musgrave, eds., Cambridge 1972.

7) K.J. Arrow and F.H. Hahn (General Competitive Analysis, San Francisco 1971 give four references to Marshall in their Name index. One is a misprint, the other three run in the vein "...as is well know since the days of Marshall...." This piece of trivia is not mentioned with critical intent; on the contrary, the point is that this is what will happen to Marshall when one does one's Walrasian theory carefully.

"We curtsy to Marshall, but we walk with Walras," observed Friedman 25 years ago. The curtsies are curter than they used to be! Cf., Friedman "The Marshallian Demand Curve," Journal of Political Economy 1949,

7 Cont.) reference to the reprint in his Essays in Positive Economics, Chicago 1953, p. 89.

8) Frank Knight, "Realism and Relevance in the Theory of Demand," Journal of Political Economy 1944; M. Friedman, "Lange on Price Flexibility and Full Employment: A Methodological Criticism," American Economic Review 1946; George Stigler, The Theory of Price, 3d. edn., New York 1966; Friedman, Price Theory, Chicago 1962.

9) For further references and a most useful critical review of this literature, cf., L.B. Yeager, "Methodenstreit over Demand Curves," Journal of Political Economy 1960.

10) Sir John Hicks has labeled them the "price into quantity" approach and the "quantity into price" approach. For the purposes of this paper, this usage would be more accurate and less likely to mislead; but it is hard to do without adjectives, so "Walrasian" vs. "Marshallian" is what it will have to be. Cf., J.R. Hicks, A Revision of Demand Theory, Oxford 1956.

11) The next few pages repeat material from my "Notes on the Theory of Markets," Intermountain Economic Review 1970. In particular, the notion of the "coupling" (of two feedback loops) used below is explained in this paper.

12) For an introduction to the problems lying in this direction, cf., T. Haavelmo, "Hva kan statiske likevektsmodeller fortelle oss?" Nationalø-konomisk Tidskrift 1958. A translation, "What Can Static Equilibrium Models Tell Us?", will appear in Economic Inquiry (Journal of the Western Economic Association).

13) The fishing conditions presupposed are somewhat unusual in that each boat-owner can decide in advance exactly how big a catch he will bring in on any given day. The catch is assumed to be a single-valued function of the amount of labor hired for the day.

The tâtonnement assumptions simplify the demand-side--and it is with the supply-side that we want to deal. But they are, of course, not true to Marshall. Cf., J.R. Hicks, Capital and Growth, Oxford 1965, Chapter V:4.

With production plans by assumption eliminated from any influence on price-formation, the present context is one in which we may well take our lead from Arrow and Hahn, op. cit., pp. 324-25: "...the idealization of postulating an auctioneer is not an obviously illegitimate shortcut through these problems..." etc.

14) Cf., H. Demsetz, "Industry Structure, Market Rivalry, and Public Policy," in J.F. Weston and S.I. Ornstein, eds., The Impact of Large Firms on the U.S. Economy, Lexington, Mass., 1973, pp. 71-75.

15) And no reason why we should not get out of teaching contrived riddles of the sort: "The essence of perfect competition is that neither buyers nor producers recognize any competitiveness among themselves; no direct competition among economic agents exists." C.E. Ferguson and S.C. Maurice, Economic Analysis, Homewood, Illinois 1970. As Demsetz points out, if

15 Cont.) the individual producer assumes no rivalrous response, he should behave as if he faced a demand curve with the same slope as the industry demand curve and not as if demand to him was perfectly elastic. Cf., also Demsetz, op. cit., pp. 73-4: "It is misleading to claim that rivalry is absent in highly atomistic market structures when what is probably meant is that competitive behavior does not require rivals to know who it is that their behavior harms or benefits. Intensive competition may not require such detailed knowledge but it does require rivalrous responses to price, quality, or output changes that otherwise would yield profits."

This same ambiguity in the theory of competitive supply is also the crux in the discussion caused by C.J. Goetz and J.M. Buchanan, "External Diseconomies in Competitive Supply," American Economic Review, December 1971. Cf. the "Comments" by Hay and McGowan, Nichols, and Shepherd and the "Reply" by Goetz and Buchanan in ibid., September 1973.

16) Piscator non facit saltum. Cf., G.L.S. Shackle, Epistemics & Economics, Cambridge 1972, Chapter 28, "Marshall's Accommodation of Time," esp. section 2.

17) J. Ostroy, "The Informational Efficiency of Monetary Exchange," American Economic Review, Sept. 1973.

18) The phrase is Haavelmo's, cf. op. cit. For a lucid and most useful exposition of the inadequacies inherent in Walras' treatment of exchange activities and some of the problems caused modern monetary theorists thereby, cf. P.W. Howitt, "Walras and Monetary Theory," Western Economic Journal, Dec. 1973.

19) We obstinately stick to discussing the Old Industrial State--so, either way, points on Es are ruled out.

Yet another case is found in Friedman's Price Theory, Chapter 2, where the set of points bounded by the price-axis and DES is said to comprise all the "pertinent or observable" ones. The analysis is, however, unclear for two reasons: (i) While Friedman's supply-curve is defined as a Marshallian supply-price schedule, his demand-locus "represents the maximum quantity that buyers would purchase per unit time" at given prices--apparently neither an M-curve nor a W-curve. (Walrasian conceptual experiments define the function, not a set with the function as its boundary). (ii) "For a more precise statement it is necessary to make some assumptions concerning institutional arrangements" (op. cit., p. 17). Without these assumptions made explicit, it seems impossible to infer what the market interaction rules would be that would limit the potentially observable points to the area indicated.

20) Cf., Leijonhufvud, "Keynes' Employment Function: Comment," History of Political Economy, forthcoming.

21) Note that, if we were dealing with a Marshallian consumer's demand-locus (for, say, tea) at this point, we would have a harder time still, for then the answer to the first question would be coded in "me own utils"--a message that no one in the market would know what to do with. It would

21 Cont.) have to be translated into the language of the market--into a marginal demand price. Having learned that the fellow values the 10th pound of tea at "12 utils cardinal", we would seek the key to this code by asking him for the value he attached to his marginal shilling. Suppose his answer is "6 utils cardinal". We then compute his marginal demand price for the 10th pound to be 2s/lb.

22) This useful term is due to Latsis. Cf., Spiro Latsis, "Situational Determinism in Economics," British Journal of Philosophy of Science 1972.

23) Cf., e.g., M.L. Burstein, Economic Theory: Equilibrium and Change, London 1969, Appendix to Ch. 1, para. 4.

24) G.L.S. Shackle, "Keynes and Today's Establishment in Economic Theory: A View," Journal of Economic Literature, June 1973, p. 519.

25) J.R. Hicks, Capital and Growth, Part I, esp. Chapter V, "The Method of Marshall," pp. 52-56.

26) Ibid., p. 74. Cf., also, pp. 78-80.

27) Cf., e.g., J.M. Buchanan, "Ceteris Paribus: Some Notes on Methodology," Southern Economic Journal, Jan. 1958, p. 264: "In general, demand curves are useful only because they allow some predictions to be made regarding the effects of changes in supply. Hence each point on a demand curve must represent an attainable equilibrium between demand and supply." While agreeing with Buchanan on this aspect of the interpretation of Marshallian demand constructs, the search for a demand curve that could be methodologically prescribed as the "only useful" one seems a quest with an impossible goal.

28) Yeager, op. cit., p. 59.

29) Buchanan, op. cit., p. 263.

30) For the present author's previous entanglements with income-compensated demand curves and the methodological arguments used to buttress their advocacy, cf., On Keynesian Economics and the Economics of Keynes, New York 1968, Chapter IV:4, and "The Backbending Supply Curve of Labor: Comment on Buchanan," History of Political Economy, Spring 1973, esp. n. 11, p.266.

31) Although constructed à la Marshall, Keynes' schedules are not simple "quantity-into-price" curves, but rather (i) "quantity-into value" constructs with (ii) employment substituted for the "quantity" and the "value" deflated by the money wage. Thus the dependent variable ends up having the dimension of a quantity (denominated in "labour units"--General Theory, p. 41) which, perhaps, increases the risk of having the constructs confused with Walrasian ones.

32) Cf., my On Keynesian Economics..., Chapter III:3.

33) The author who has most consistently and insistently been right on this is Professor Paul Davidson. Cf., esp., his "A Keynesian View of Patinkin's Theory of Employment," Economic Journal, Sept. 1967, and also the references there given (p. 560, n. 2) to earlier papers by himself and others on the aggregate supply-price function.

34) Cf., above, p. 28 , and Leijonhufvud, "Keynes' Employment Function: Comment."

35) Patinkin, op. cit., Chapter XIII.

36) H.I. Grossman, "Was Keynes a 'Keynesian'? A Review Article," Journal of Economic Literature, March 1972.

37) E.g., On Keynesian Economics..., Chapter II:1.

38) J.M. Keynes, The General Theory of Employment, Interest and Money, London 1936, p. 47.

39) Chapters 5 and 20 of the General Theory are about all the help one can get. In A Treatise on Money, London 1930, Keynes simply and openly refuses to get "too far into the intricate theory of the economics of the short-period" (Vol. I, p. 161).

40) The model, of course, dictates that the market-clearing price be found before midnight--or whoever uses it will turn pumpkin.

41) General Theory, p. 47.

42) General Theory, Chapter 5:II.

43) We are concerned here only with interpreting Keynes' argument. For explaining the world as it is, reversing Marshall's ranking of adjustment speeds is, I still think, the more promising tack to take.

The property of Keynes' model considered in the text elicited the first serious critical attack on the General Theory that Keynes found hard to cope with. It was launched on strictly empirical grounds. C.F., J.T. Dunlop, "The Movement of Real and Money Wage Rates", Economic Journal Sept. 1938, and L. Tarshis, "Changes in Real and Money Wages," ibid., March 1939. The latter issue also contains Keynes' rather lame reply, "Relative Movement of Real Wages and Output."

44) Suggested by Arrow in "Towards a Theory of Price Adjustment," The Allocation of Economic Resources, M. Abramowitz et al., eds., Stanford 1959, and pursued by D.C. Cogerty and G.C. Winston, "Patinkin, Perfect Competition and Unemployment Disequilibria," Review of Economic Studies, April 1964. Cf., Leijonhufvud, On Keynesian Economics..., pp. 75f.

45) General Theory, p. 51.

46) Lionel Robbins', The Nature and Significance of Economic Science, London 1932, defined Economics as concerned with the implications of "Scarcity" and, therefore, as a field of inquiry starting logically from the ever-present necessity of choosing among alternative allocations of scarce "means." The book has been so influential that its once challenging thesis will seem almost platitudinous to today's students. For that very reason, it should be recognized as an important part of the story of how choice-theory became the predominant--indeed, all but exclusive--"paradigm" of modern theoretical economics.

47) This particular list is from the General Theory, p. 108. Keynes may not have been "pre-Freudian" exactly, but his Essays in Biography nonetheless show him as mercifully unspoiled by that sort of thing.

48) For good reason. To clarify how these "neoclassical schools" relate to the two discussed here is no mean task. Cf., for example, J.M. Buchanan, Cost and Choice, Chicago 1969, in which the Wicksteed-LSE and the Marshall-Cambridge cost-theories are put on collision course.

49) The Stockholm School's most important effort in price-theory was unfortunately never translated and is now apparently impossible to find even in the Swedish edition: G. Myrdal, Prisbildningsproblemet och föränderligheten, Uppsala 1927.