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EFFECTIVE DEMAND FAILURES

by

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--There is no reason why the form of a realistic model (the form of its equations) should be the same under all values of its variables. We must face the fact that the form of the model may have to be regarded as a function of the values of the variables involved. This will usually be the case if the values of some of the variables affect the basic conditions of choice under which the behavior equations in the model are derived.

--Trygve Haavelmo<sup>1</sup>

My primary concern over the past several years has been with certain theoretical problems embedded in the Keynesian literature that I believe (of course) to be important, unresolved, and difficult. The ramifications of these problems are such that a few of us cannot hope to deal effectively and satisfactorily with them all. In order to enlist others in the work, one has to convince them that the problems in question have not been satisfactorily solved and have a high claim on their attention relative to other matters currently in professional fashion. To do so in this case, I believed and believe, requires a revision of recent macro-doctrine history. Economists who accept without qualification the inherited image of the Keynesian past will not share our views about either the present or the directions for the future. My writings in that vein seem to have brought off

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\* Professor of Economics, University of California, Los Angeles. This paper draws on work done together with my colleague, Robert W. Clower, for a book to be entitled The Coordination of Economic Activities. Still, Clower's responsibility for any stupidities or errors is somewhat less than 50%.

1/ Cf. A Study in the Theory of Investment, Chicago, 1960, p. 205.

a certain communication failure. Most readers have shown greater interest in the doctrine-historical themes, which should have been secondary, than in the agenda for fresh work in Keynesian theory that I attempted to advance.

In this paper, I provide a sketch, free of doctrine-historical subplots, of a number of issues and problems that stand in need of fresh attempts at theoretical modelling and/or empirical testing. Space, time, and dull wits combine to prevent me from treating any of the problems in detail and with rigor; I have chosen instead to convey to the reader how part of the working agenda for theoretical macroeconomics is perceived by someone with a "revised" perspective of the historical development of the field.

The broad theme of the paper is the theory of Effective Demand. In Part II, the theory of effective demand failures is examined, in turn, from the following vantage-points: A. The Theory of Markets and Money; B. Theories of the Consumption Function; C. Quantity Theories of Money Income Determination. Part I sketches the broader context within which I would presently view the theory, and is correspondingly informal and opinionated. Contrary to what seems to be current professional practice, I make my tentative opinions explicit. There are two reasons for doing so. First, one cannot today presume the kind of consensus in macroeconomics that makes it unnecessary to explain why one regards certain issues and approaches as more significant than others. Second, the reader may well want some hints as to the prejudices and biases that underlie what is to follow.

#### I.

In my opinion, the central issue in macroeconomic theory is--once again--the extent to which the economy, or at least its market sectors, may properly

be regarded as a self-regulating system. In what respects does it, or does it not, behave in such fashion? How well, or badly, do its "automatic" mechanisms perform? This issue, to illustrate, lies at the heart of two of the most prominent controversies in the field over the last decade: the Fiscalist vs. Monetarist controversy over income determination and aggregate demand management, and the controversy over the long-run stability of the Phillips-curve. The volume of writings on each of these continues to mount steadily with no clear-cut resolution in sight--in large measure because this central issue is not being effectively addressed.

The social problem to which the issue of the system's self-regulatory capabilities pertains we may term "the coordination of economic activities." The reference is to coordination of desired sales and purchases at the market level; "full coordination" for our purposes means simply that existing markets clear; it does not mean "efficient allocation." Our central question is to be put in that frame. Does the market system (as presently instituted in the U.S. or in Sweden. . .etc) tend to move "automatically" towards a state where all market excess demands and supplies are eliminated? How strong are those tendencies?<sup>2/</sup> The significance of these questions is not affected by the

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<sup>2/</sup> Note that if the conclusion from the proposed inquiry were to be that the system does tend towards establishing a state of full coordination, no laissez faire implications whatever follow. Our conception of "full coordination" omits most of the criteria for Pareto-optimality--it allows markets to be monopolistic or monopsonistic, sales or income taxes to be present, non-existence of organized markets for certain "goods" and other causes of external effects, and so on. Quite apart from all that, the system's homeostatic mechanisms might be so slow in their operation that policy intervention would be deemed desirable simply to speed up the (in themselves) "automatic" self-regulatory tendencies of the system.

The reader will have observed that the term "equilibrating" is eschewed here in favor of "self-regulatory", "homeostatic" or other more or less cumbersome circumlocutions. The reason, of course, is that our

admission that one deems the probability of actually ever observing an economy in a fully coordinated state to be zero.

When the issue is put in this very general, diffuse way and with reference to real-world systems rather than particular classes of models, modern economic theory can as yet provide no answer.<sup>3/</sup> And the message out of all the empirical work in macroeconomics of the past decades is very largely in the (casual) eye of the beholder. Yet, on almost all economic questions of major importance, systematic inquiry can only proceed on some presumption of what the answer is likely to be. Otherwise determinate results are unobtainable. Few, if any, major questions have the same answer independently of whether the entire system of markets "works" to coordinate activities or whether one or more markets fail to function as homeostatic mechanisms. This is most obviously true of macroeconomic issues,<sup>4/</sup> but applies as well to a host of problems to which most economists would affix the "micro" label.<sup>5/</sup>

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discussion moves in a realm of discourse where the "unemployment equilibrium" notion of textbook Keynesianism unavoidably insinuates itself. Since that is not a "coordinated" state, the stability properties asserted for it are not of the kind to which our central question is addressed.

The term "stability" is also better avoided because of its firm associations with certain classes of models. What should concern us is the dynamic behavior of actual economies--and we do not want to prejudge how that behavior is most appropriately to be modelled.

- <sup>3/</sup> The current boundary markers on this front are set out in K.J. Arrow and F.H. Hahn, General Competitive Analysis, San Francisco and Edinburgh 1972. Cf. esp., Chapters 13 and 14.
- <sup>4/</sup> A most familiar example concerns the consequences of an increased propensity to save as deduced (a) from a neo-classical growth model, or (b) from a simple Keynesian ("Paradox of Thrift") model. Most standard conceptual experiments in macroeconomics produce the same disturbing result--qualitative predictions for some of the important variables emerge with opposite signs from the two coexisting bodies of theory.
- <sup>5/</sup> Consider a social benefit-cost calculation for a labor-saving government investment project under the alternative assumptions (a) that the displaced

The researcher or instructor must then proceed on some presumption or other with regard to the self-regulating capabilities of economic systems--or find that he has nothing to say. The assumptions made may be backed up to some extent by broad and casual empiricism and by reference to scraps of rigorous theoretical results obtained for a variety of special, simplified cases. But ammunition to compel the agreement of a disbelieving colleague will be lacking. At the same time, the validity of the work that an economist does will ultimately hinge on whether his presumptions on this matter do or do not in some sense approximate reality. If they do not, his work is likely to end up on the scrapheap of forgotten intellectual games. The emotional stakes are high, while solidly based knowledge is at best fragmentary.

Briefly put, then, our situation is one of emotionally charged ignorance with regard to a central issue of the science. In such situations one expects a high incidence of technically qualified men rejecting out of hand the work of other, equally qualified men--or, indeed, spurning entire branches of current inquiry. A's models are but "meretricious games" to B. B's regression results only "meaningless numbers" to A. And one also expects, rather sadly, to hear charges flying that "the other's" work can only be understood by drawing the always tempting inference that it is the product of nonscientific, biased motives.

From a history of science point of view, none of this is novel, nor is it by itself unhealthy. It could be said of many episodes in diverse fields that

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labor will be reabsorbed into other employments, and (b) that it will be permanently unemployed. More to the point, perhaps, consider the benefit-cost calculator's utter impotence if he were completely ignorant about which assumption (or combination of the two) is applicable.

we now look back upon as the gestation period of major advances--but, one feels sure, also of many now forgotten controversies that produced only heat and no light in their time because the issues were never given a "soluble" formulation.

What seems to me unhealthy about the situation in macroeconomics is that the central issue does not occupy center stage. In the unending controversies to which it is critical, it keeps bobbing to the surface only as conflicting declarations of faith. I do not think it has been given "soluble" formulation. It is being avoided, I would infer, because the diffuse nature of the question--itself a result of past neglect--makes it very difficult to address it except in terms that (as here) fall short of present-day standards of precision and rigor in theoretical debate. Still, how can the profession go on for decades with this issue remaining out of focus?

The settled, conventional acceptance of having general economic theory split down the middle is, I believe, very largely to blame. Despite the several alternative ways that we have developed to make the gulf between microtheory and macrotheory seem plausible to new generations of students, the micro-macro distinction remains basically that between models with "fully coordinated" solutions and models where one or more markets reach such solutions only by chance. Both sets of exercises are referred to as "theories", but there could be no real-world economy for which both theories are true at once.<sup>6/</sup> One allows oneself the major convenience of static modelling

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<sup>6/</sup> In theory teaching, the schizophrenic pressure on young minds can be kept within tolerable bounds by dwelling on self-regulating systems on, say, Mondays, Wednesdays, and Fridays--reserving Tuesdays and Thursdays for the economy that "doesn't work that way." In the applied fields, micro and macro have to coexist. Example: the elasticity and absorption approaches to balance of payments theory.

by making one courageous decision (for each market). Either the market has demand-equals-supply equilibria only, or it has no tendency to eliminate excess demands at all.

A fully adequate characterization of the two alternative visions of what real-world market systems are like, to which neoclassical and Keynesian models give formal representation, would be space-consuming. A crude sketch of the two economic "cosmologies" might run as follows.

Assume that we can define a "fully coordinated" time-path for the economy. The first cosmology then attributes the following properties to the system. It tends to home in on the ideal path and, in the absence of disturbances, to stay on it. Shocks that displace it from the path will trigger immediate deviation-counteracting feedback control mechanisms. The larger the displacement, generally speaking, the stronger will be the homeostatic tendencies working to bring the system back.<sup>7/</sup>

According to the second cosmology, the system has no "automatic" tendency to home in on the coordinated path, would reach it only by chance--or through deliberate policy-intervention--and will not maintain itself on it if the path were reached. This system may settle down anywhere "between zero and full employment"<sup>8/</sup> with all servo-mechanisms idle. When displaced by shocks from a

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<sup>7/</sup> I.e., price adjustment velocities will be monotonically increasing functions of discrepancies between demand and supply in respective markets--where, in Clower's terminology, it is "notional" demand and supply that are measured. Similarly, adjustment velocities for rates of output and factor employments increase monotonically as functions of discrepancies between supply price and demand price--with these schedules also defined in notional terms. Cf. A. Leijonhufvud, "Notes on the Theory of Markets," Intermountain Economic Review, Fall 1970. The last statement in the text also assumes, roughly speaking, that excess demands and excess supply prices increase monotonically with the "displacement" from the equilibrium price- and output-vectors, respectively.

<sup>8/</sup> Cf., e.g., Robert Lekachman, The Age of Keynes, New York, 1966, p. 90.



previous position, moreover, the system will exhibit endogenous ("Multiplier") tendencies that, instead of counteracting the displacement, amplify it.

These, then, are in brief summary the two opposed visions of how a market economy behaves that we are saddled with. Both are firmly entrenched in the literature. The first goes back, of course, to Adam Smith and was the cosmology of economists for more than 150 years--if this was not your belief, you were almost by definition not an economist, but at best an amateur. Then, the Great Depression prepared the ground for a mass conversion of economists to the second. But the Keynesian Revolution did not quite succeed in making a clean sweep. The older view survived and has again grown in strength as the 1930's recede from memory and mass unemployment on that scale has failed to recur. The two are inconsistent images of the world but nonetheless manage to coexist--and in rather implausible comfort at that.

Clower's original venture into the uncomfortable no-man's land between Neoclassicism and Keynesianism<sup>9/</sup> sought to provide a microtheoretical foundation for the core concept of Keynesian theory--Effective Demand. For the contesting cosmologies, the ramifications of his success appeared at first rather one-sidedly in favor of Keynesianism. Solid microtheoretical respectability for the most important Keynesian doctrines seemed suddenly within grasp. At the same time, "effective demand failures" had to be perceived as an hitherto unrealized, pervasive malfunction of price-systems, casting grave doubt on the entire neoclassical vision of the self-regulating capabilities and modus operandi of market systems.<sup>10/</sup>

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9/ R.W. Clower, "The Keynesian Counterrevolution: A Theoretical Appraisal," in F.H. Hahn and F.P.R. Brechling, The Theory of Interest Rates, London 1965.

10/ I should add the following: It is, I think, true that the ramifications of Clower's contribution were seen as "one-sided" by those who focused

Time has by now allowed sundry second thoughts on the effective demand doctrine. The result is a less one-sided, more balanced perspective. Whether it is also a truer perspective remains to be seen. If I were to sum up my present views as a "cosmology"--neither more, nor less crude than the preceding ones--it would have the following outlines. The system is likely to behave differently for large than for moderate displacements from the "full coordination" time-path. Within some range from the path (referred to as "the corridor" for brevity), the system's homeostatic mechanisms work well, and deviation-counteracting tendencies increase in strength. Outside that range these tendencies become weaker as the system becomes increasingly subject to "effective demand failures." If the system is displaced sufficiently "far out", the forces tending to bring it back may, on balance, be so weak and sluggish that--for all practical purposes--the Keynesian "unemployment equilibrium" model is as sensible a representation of its state as economic statics will allow. Inside the corridor, multiplier-repercussions are weak and dominated by neoclassical market adjustments; outside the corridor, they should

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intently on the purely theoretical implications. Keynesian theory now had to be taken seriously by general equilibrium theorists; tâtonnement stability theorems had to be quoted at drastically reduced empirical values, etc.

My own work in the same vineyard, however, confused the picture no end for many people. By my attempts to explain and document in some detail the departure of "Keynesian economics" from the "economics of Keynes", and the prominence that I gave to purely doctrine-historical themes, I was in effect launching an attack on the scholarly repute of (conventional) Keynesianism--albeit from a totally different quarter. What this meant for its reception was foreseen in the review by Harry G. Johnson, "Keynes and the Keynesians," Encounter, January, 1970. (I didn't much like that part--but Johnson was right.)

The book was generally well received by anti-Keynesians (not by any means always for the wrong reasons). At the same time, some American Keynesians have read it as "just another Chicago-school-attack"--the emphasis on labor-market search behavior and on the significance of the structure of relative prices arouses, it seems, suspicion.

be strong enough for effects of shocks to the prevailing state to be endogenously amplified. Up to a point, multiplier-coefficients are expected to increase with distance from the ideal path. Within the corridor, the presumption is in favor of "monetarist", outside in favor of "fiscalist", stabilization policies. Finally, although within the corridor market forces will be acting in the direction of clearing markets, institutional obstacles of the type familiar from the conventional Keynesian literature may, of course, intervene to make them ineffective at some point. Thus, a combination of monopolistic wage-setting in unionized occupations and legal minimum-wage restrictions could obviously cut the automatic adjustment process short before "equilibrium employment" is reached.<sup>11/</sup>

## II.

### A. Theory of Markets and Money

Pre-Keynesian views of how activities are coordinated in market systems were based on two broad assumptions: (a) that price-incentives are effective in controlling the behavior of transactors (price-elasticities of excess demands are not zero throughout in any market, and, in principle, market-clearing solutions at non-negative prices exist for all markets); (b) prices are "free" to move in response to excess demands and supplies and will move towards their market-clearing values. The Keynesian model posed the spectre of a coordination failure of indefinite duration. How could that possibly be? Until relatively recently, the generally accepted answers interpreted

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<sup>11/</sup> It is generally true of homeostatic mechanisms, studied in other fields than economics, that their self-regulating capacities are bounded. Displacements so large that the system cannot "cope" are always possible. Is it farfetched to hypothesize that this is true also of economic systems?

Keynesian theory as necessarily denying either one or the other of the two broad assumptions--or both.<sup>12/</sup>

Arguments for denying the pre-Keynesian assumptions can be developed in a great number of ways, of course, and we cannot comment on all the versions. There are theoretical and/or empirical reasons for being at least "very uncomfortable" about all of the arguments I am familiar with. The inelasticity of saving and investment behavior with respect to intertemporal prices (interest rates) is an instance of denying (a). This denial has been argued, for example, on the grounds that theoretical reasons for assuming non-zero elasticities can only be derived from strong underlying assumptions of "rationality" and "foresight". But this argument won't hold water.<sup>13/</sup> Similarly, the one-time belief that these strict inelasticities had solid empirical support has not stood up. With regards to denials of (b), it is enough to point out that the prime test-case of Keynesian theory must be the Great Depression. But "rigid wages" due to monopolistic unions could only apply to a relatively small (and shrinking) proportion of the U.S. labor force at the time. And in fact money wages were not "rigid"--there is no more dramatic wage-deflation on record than that of 1930-33. And so on. The long survival and endless repetition of sundry arguments denying (a) and (b) appears in retrospect a product of psychological necessity: On the one hand, economists could not imagine the persistence of coordination failures on a large scale if both (a)

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<sup>12/</sup> For amplification of these remarks, cf. my Keynes and the Classics: Two Lectures, Institute of Economic Affairs, London 1969, pp. 24 ff.

<sup>13/</sup> Cf. Gary S. Becker, "Irrational Behavior and Economic Theory," Journal of Political Economy, February 1962.

and (b) had to be accepted as "true". On the other hand, the horrors of the Great Depression were impossible to ignore.

Clower's explanation of effective demand failures offered a release from this dilemma. Price-incentives may be effective in all markets and all prices may be "flexible"<sup>14/</sup> and a market system may still go haywire in its groping for the coordinated solution. Conditions are possible, and are not farfetched, under which some prices may show no tendency to change although desires to sell and to buy do not coincide in the respective markets. Not only that. Prices may be at their "right" (general equilibrium) levels, but amounts transacted may differ persistently from the desired rates of sale and purchase in some markets.<sup>15/</sup> And not only that. Prices that were at their GE values may tend ("automatically") to move away from those values so that the information disseminated by price changes is "false" and makes the coordination failure confusion worse.

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<sup>14/</sup> The main reasons for insisting on working out the theory of effective demand failures first while assuming atomistic markets and no institutional restrictions on price-adjustments are implicit in--but obvious from--the text. Another one is also of some importance: even when the main results are more easily obtained by ad hoc "rigidity" assumptions, that procedure had better be shunned--since it carries the false suggestion, "Break up the unions and monopolies (if you dare!)--and all will be well with the world!" The insistence on working with atomistic markets, etc., has been misconstrued by some economists as revealing an "anti-Keynesian bias" and what not. Macrotheory would become a positively charming occupation could we but regain that state of grace where one professional could understand the work of another even under the restrictive assumption that the latter can be trusted. . .

As pointed out at the very end of Part I, once the general outlines of the theory have been clarified, empirically verified "rigidities", etc. should of course be put back in.

<sup>15/</sup> Cf., e.g., R.J. Barro and H.I. Grossman, "A General Disequilibrium Model of Income and Employment," American Economic Review, March, 1971. Check the "B-solutions" to the various cases they consider.

It was surely inevitable that the early discussion of effective demand theory would focus almost exclusively on the newly discovered possibilities for system malfunction. The net result of this concentration on all the fascinating ways in which the system conceivably can go wrong, we now think, was to give a rather grossly exaggerated picture of the propensities of actual real-world economies to lose track and fail to home in towards a coordinated state. Second thoughts on effective demand theory suggest that the capabilities for self-regulating behavior of actual market systems are likely to be a good deal more "robust"<sup>16/</sup> --even though the models we were working with at the time were not robust at all, but instead extremely sensitive to changes in specification that explicitly incorporate the conceptual distinction between "notional" and "effective" excess demands.<sup>17/</sup>

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<sup>16/</sup> i.e., suggest the notion of "the corridor" within which market forces, as they were traditionally conceived, are strong enough to override the disorganizing tendencies arising from "trading at false prices", etc. Perhaps I had better make it explicit that this is not "in Keynes". Obviously, Keynes' theory envisages a world in which potential deviation-amplifying endogenous mechanisms are as strong (multiplier-coefficients as large) in the immediate neighbourhood of the "fully coordinated" state as they are far away from it.

I have not changed my mind on the significance of Keynes' contribution to economic analysis. I think it of fundamental importance. I have long since parted company with Keynes on many aspects of his economic theory, in the sense of beliefs about how real-world systems function. (One hates being otiose like this, but many economists simply have trouble drawing the distinction).

<sup>17/</sup> In this paper we will be dealing only with one branch of the effective demand doctrine, namely that concerned with effective demand failures among current spot-markets. Issues that belong to this branch include: (a) the possibility of persistent states of large-scale unemployment; (b) the "original" multiplier based on the simple consumption-income relation; and (c) the independence of model solution-states of conditions determining the supply of labor.

Point (c) is more brief than accurate. Note that, in standard Keynesian models, shifts in the supply of labor function that increase or decrease the excess supply of labor (without, however, making it negative),

The original modelling context for the discussion was that of a standard Walrasian model "without the auctioneer" (as I chose to put it). Two properties should be emphasized here: (i) all quantities appear as flows (albeit cumulated over the "period"), and (ii) none of the goods is explicitly given the singular attribute of being the only means of payment.<sup>18/</sup> We begin, thus, with a non-monetary "pure flow" representation of an economy.<sup>19/</sup> Within that setting, the consequences of letting individuals trade at disequilibrium prices ("false trading") were then examined. With the budget-constraints of a pure flow model, it is readily (albeit not immediately!) apparent that, if the transactor fails to realize his desired sales due to excess supply in those markets, he will not have the wherewithal to realize his desired purchases. If the

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never change the solution obtained. The changes in corresponding planned demands for other goods in the system are always treated as "ineffective."

The other branch of effective demand theory pertains to intertemporal effective demand failures. More than half of my 1968 book dealt with topics in this area (Saving-Investment coordination; Wicksellian cumulative processes; the Keynes effect vs. the Pigou effect, etc.). Space will not allow second thoughts on this set of problems.

<sup>18/</sup> Clower's reasons for not considering a stock-flow system are given, op.cit., pp. 114-15. The reader who chooses to check this statement will find that the reasons still hold--for the analytical problem posed in that essay. He will also find, however, that the very same reasons would compel us to use stock-flow representations whenever the object is to derive theoretical inferences about the self-regulating capabilities of real-world systems.

When it has already been said that we deal with a pure flow model, it is naturally redundant to add that we do not have the stock-good, "money", in it. The reasons for nonetheless emphasizing the point will be apparent shortly.

<sup>19/</sup> Since the very simplest Keynesian models (the "45° Keynesian Cross", etc.) do not include the stock of "money", but deal simply in relationships among flows, and since these elemental models exhibit all the properties that one deems singularly and particularly "Keynesian", this seems "the way to go," all right. But. . . it turns out to be misleading.

actual price-vector at which trading takes place differs at all from the GE vector, furthermore, some markets must exhibit excess supply and, consequently, some transactors must necessarily fail to realize desired sales at prevailing prices. It follows that, as soon as you have a departure from the GE price-vector, the demands of some transactors must be sales-constrained--in the model stipulated.

Another point about the mental setting of these conceptual experiments is relevant: the consumption-income relation (from which the "multiplier" is obtained, for instance) seems the nexus of all the "singularly Keynesian", "obviously anti-Classical" model-properties that concern us here.<sup>20/</sup> It is natural, therefore, to concentrate on a particular case of the above conceptual experiment as the very archetype of it--namely, the case of a household failing to realize desired sales of labor and thus finding its consumption demand income-constrained.<sup>21/</sup> (Note that unsold labor-services cannot be

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<sup>20/</sup> Two notes: I say (i) "seems the nexus" because Keynes' Employment Function (General Theory, Chapter 20) expressed the analogous sales-constraint on the demand for labor on the business sector side of the market. But Chapter 20 is pretty far into that book. . . and elemental Keynesian models do not make use of either the Employment function or its converse, the aggregate supply-price function; (ii) "all the. . . properties that concern us here", because intertemporal effective demand failures we have ruled out of order (in fn. 17 above).

<sup>21/</sup> Beyond concentrating on income-constrained consumption demand of labor-suppliers at the expense of the sales-constrained labor demand of commodity-suppliers, the same considerations led me to focus on the search-behavior on the supply-side of labor markets at the expense of demand-side behavior. The latter deficiency I share with numerous authors (Stigler, Alchian, etc. . . good company!)

Barro and Grossman, op.cit., have two virtues in this context: (a) they give equal attention to the sales-constrained demand behavior of producers, and (b) in considering explicitly also situations of purchase-constrained supply-behavior, they forcefully remind us that false trading in inflationary situations must also be encompassed by the theory.

E.S. Phelps, "Money Wage Dynamics and Labor Market Equilibrium" in Phelps, et al., Microeconomic Foundations of Employment and Inflation Theory, New York 1970, models a system in which the demanders of labor do



stored for sale in the next period--again the case analyzed tends to direct one's attention away from stocks). The analytically crucial aspect of Keynesian theory seems thus to have been isolated: realized transactions appear as arguments in the excess demand functions of such systems, whereas they have no place in Classical models belonging to the Lausanne-school ("modern") tradition.<sup>22/</sup> The troubles with effective demand failures follow immediately--in the archetype case, we will have no effective excess demand for wage-goods in an unemployment situation even though "notional" household consumption demand exceeds current output. Wage-goods output is too low. . .but the servo-mechanisms of the market system are idle.

In this setting, all we thought we knew about the stability of market systems seems suddenly imperiled. Stability theorems proved for systems of notional excess demand equations apparently prove nothing, because notional and effective excess demands coincide only when the system is already in general equilibrium.<sup>23/</sup> Any trade at false prices might upset the apple-cart. Trade at false prices will surely take place if prices do not move instantly to their GE values when a disturbance occurs, etc. For models of the type considered, all of this is true--but getting a balanced perspective on its "relevance" is another matter.<sup>24/</sup> The income constraints derived from this

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the "groping," and demonstrates that it exhibits much the same qualitative properties as Alchian, myself and many others have gotten from "supply search" assumptions. The not-so-easy task remains of providing a model in which both sides of the market "grope."

<sup>22/</sup> Clower, op.cit., pp. 111-12, 119-20.

<sup>23/</sup> Clower, op.cit., p. 123.

<sup>24/</sup> To take just one example, I committed the following: "Income-constrained processes result not only when price-level velocity is zero, but whenever it is short of infinite." Leijonhufvud, On Keynesian Economics and the Economics of Keynes, New York 1968, p. 67. A number of other authors have followed suit.

type of model are "too tight."

Second thought, if not necessarily wisdom, starts from the observation that realized sales have been made to do heavy, in fact, triple duty in the above context. Realized sales appear (i) as a proxy for expected income,<sup>25/</sup> (ii) as a constraint on current purchases, and (iii) as a constraint on the demand-signals that may be currently emitted. These three ideas need to be kept carefully distinct.

(i) If realized income is expected, income expectations will be realized.

One assumes that sellers know beforehand what sales they will succeed in making. But, strictly speaking, this assumption makes sense only for that subset of the possible states of the system that are Keynesian "income-equilibria." Most of these states are, of course, coordination failures--dis-equilibria from a neoclassical standpoint. The assumption allows the analysis of some of the properties of such states with the aid of essentially static model-constructions--a convenience that recommends it for certain expository purposes. But it also precludes analysis of the recursive interaction processes that propel the system from one Keynesian income-equilibrium to another.<sup>26/</sup> The inherent limitations on what we might learn about the dynamic

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<sup>25/</sup> Following Keynes' procedure of collapsing, for convenience, short term expectations with realized results. Cf., The General Theory of Employment, Interest and Money, London 1936, pp. 50-51. But, apart from the issue noted in the text below, following Keynes here risks fudging another distinction as well, namely, that between Friedman's concepts of measured and permanent income--an aspect of the issue that we save for the next section.

<sup>26/</sup> Cf., Leijonhufvud, op.cit., pp. 74-5.

The collapsing of realized and expected income was a feature (albeit not a necessary one) of Clower's original treatment of the "dual decision hypothesis." It remains in use, e.g., in H.I. Grossman, "Money, Interest, and Prices in Market Disequilibrium," Journal of Political Economy, Sept./Oct. 1971, as well as in Barro and Grossman, op.cit.

behavior of such systems in this way are obvious. But there is also, I think, a not so obvious danger to the procedure. It rather invites making ad hoc assumptions about the information available to transactors in making the decisions that, in effect, the steady-state assumption dictates that they make.<sup>27/</sup> Having put price-rigidities and price-inelasticities to one side, effective demand theory seeks to explain coordination failures that arise through faulty communication among transactors. Communication takes place through market interactions. Hence, ideally, all statements of the type: "Transactor A is expected to behave in such-and-such a manner at date t, because he has good information on x, no information about y, and false information about z. . ." etc., should be justifiable with reference to a history of market interactions that would reasonably produce such a state of knowledge. This means tracing the recursive process. In my (no doubt overly jaundiced) view, therefore, steady-state constructions divert attention away from the fundamentals of the theory.<sup>28/</sup>

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<sup>27/</sup> I.e., much the same kind of ad hoc-ery that makes so much of neoclassical micro-theory useless to the macrotheorist. For the purposes of the "new" macroeconomics, models with assumptions of the type "sellers just know the demand-schedules they face" can simply not be trusted. It is not that such assumptions do not make sense in context. For the steady state they often do. It is that they avoid the question: How did they come to know? Since one does not know the process through which this knowledge was gained, one is hard put to start grappling with the questions that count (in macro). For example: In what circumstances would that process teach them some things "that ain't so".

<sup>28/</sup> Note that "imperfect information" is a rather misleading label for the theories developing in the area under discussion since it is likely to be understood as referring to "generalized probabilistic uncertainty." The rich literature on that topic from Knight through Arrow is not particularly relevant or helpful here. We are concerned with "incomplete information" in the sense of certain specific pieces of information missing--and missing for reasons inherent in the structure of the model. (The weakest postulate here might be: Nobody interacts directly with everybody).

Situations for which we assume "good information about x, no

(ii) Realized sales as constraint on purchases. As long as Say's Principle is the well-enforced law of the land, the model obviously must have this property. Nonetheless, it is easy to get one's bearings on the real world wrong at this point, particularly when focusing narrowly on the labor-selling, wage-goods-purchasing household experiment. In the pure flow model, realized sales are interpreted as current income (from the sale of labor-services). But, whereas it is obviously true that the value of purchases has to be financed by the value of sales, it is not at all true that they must be financed out of current income. A supplier of  $x$  does not have "current  $x$ " as his only source of funds; he can (a) sell "stored  $x$ ", and (b) other things.

The point here is as simple as it is important to our main theme. In pure flow models, realized sales have the interpretation of "income." Income constrains legal acquisition of goods directly. Any little "blip" in the realized income-flow must show up (100% at that) in purchases--the gearing between income receipts and expenditures being that tight. As soon as some market does not clear and false trading takes place, multiplier repercussions should necessarily be observed. This income constraint is too tight; it lures one to adopt an exaggerated view of the potential instability of real-world economies --stock-flow economies.

In stock-flow systems, the stocks act as "buffers" between physical inflows and outflows and between financial income and expenditure flows. Stocks of liquid assets--of cash balances, in particular--allow expenditures to be

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information on  $y$ . . ." etc., will sometimes produce inferences of "asymmetric behavior" that are apparently peculiarly offensive to theorists used to steady-state frames of reference. For an example, cf. A.A. Alchian, "Information Costs, Pricing, and Resource Unemployment," in Phelps et al., op.cit., p. 44n.

maintained when receipts fall off; indeed, they are maintained by traders exactly for the purpose of meeting such contingencies. Modern economies maintain, in normal times, an enormous, elaborate system of physical and financial buffer stocks.

Conclusions: (a) in such economies, we must expect the propagation of shocks impinging on flows to be heavily damped--as long as the shocks are not of greater magnitude than anticipated by transactors in making their decisions on the levels of buffer stocks to maintain; (b) such economies are, therefore, much more "robust" than pure flow models would suggest--within "the corridor;" (c) if disturbances are of unanticipatedly large magnitude, buffer stocks may be exhausted--at which point, the direct gearing of inflows and outflows of the "tight" income-constraint takes over. For such large displacements, effective demand theory in the version considered becomes a better guide to the behavior of the system than "Classical economics."

(iii) Realized sales as restriction on demand-signals. Consider a system in which each good may be traded against every other good. Let us have this "barter economy" in the midst of a most un-Walrasian false trading debauch: quantities actually transacted (and produced) are far below what they would be in a coordinated state; exchange ratios differ from the GE vector of relative prices;<sup>29/</sup> and resources are unemployed. For our archetypical case, we would explain: workers fail to sell their services, so their purchases of wage-goods

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<sup>29/</sup> Not redundant for several reasons the most important of which is that establishing the GE price vector is a necessary, but not sufficient, condition for "full coordination" of activities. It does not guarantee that transactors will find a way actually to execute all desired trades. This observation has been made the starting point of important work on the pure theory of money by my UCLA colleague, Joseph Ostroy. Cf. his "The Informational Efficiency of Monetary Exchange," (forthcoming). Also Ostroy and R.M. Starr, "Money and the Decentralization of Exchange," (forthcoming).

are constrained; producers fail to sell wage-goods, so their purchases of labor services are constrained, etc. Realized sales of labor (wage-goods) in this setting are in themselves realized purchases of wage-goods (labor); there is no intermediate "money"-commodity to separate sale and purchase. Labor services constitute direct "purchasing power" over wage-goods and vice versa.

What are the signals that the market homeostat would respond to in such a case? If at the going rate of real wages,<sup>30/</sup> the desired supply of labor exceeds employment, producers will ipso facto receive the signal that demand exceeds current sales and output; if the desired supply of wage-goods exceeds current sales and output, workers will ipso facto receive the message that demand for labor exceeds current employment. Note that, at a particular date, both statements could well apply at once<sup>31/</sup> --indicating, simply, that the ongoing rate of transactions does not exhaust the mutual gains from trade realizable at the going exchange rate. As these gains from trade come nearer to being exhausted, however, the market situation will clarify: it emerges either (a) as one of excess supply of goods and excess demand for labor or (b) as one of excess supply of labor and excess demand for goods. In case (a), real wages will tend up, in case (b) down. In either case, the observed volume of transactions, output, and employment will rise.<sup>32/</sup>

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<sup>30/</sup> For brevity, I fudge matters: the exchange ratio for any pair of goods is bound to vary depending upon what pair of traders are observed; inconsistent cross-rates and corresponding arbitrage opportunities are likely to abound in disequilibrium, etc.

<sup>31/</sup> Naturally, markets need not clear continuously in this regime. When shifts of the basic parameters occur, producers and workers will spend time searching the other side of the market for the best bargain, etc., etc.

<sup>32/</sup> At disequilibrium prices, the short side of markets is assumed to predominate in determining actual transactions.

Next for the contrasting case: a regime characterized by Clower's postulate "Money buys goods and goods buy money; but goods do not buy goods."<sup>33/</sup> Money and no other good is a means of payment. Obligatorily, money separates each sale from corresponding purchases. Suppose we find this system in the "same" type of disequilibrium as the one considered above. Offering to sell now means to ask for money in exchange; offers to buy are no longer "valid" if not backed by ready cash.<sup>34/</sup> If the desired supply of labor exceeds employment, producers will be aware of the excess supply in labor markets but receive no valid signal indicating that the demand for wage-goods exceeds output. If desired supply of wage-goods exceeds current sales and output, workers are not informed that demand for labor exceeds employment. Even if the ratio of money wages to money prices comes out as the GE real wage, we may be caught in the vicious circle where the unemployed cannot make their consumption demand effective until they have sold their services for money, and producers with excess capacity cannot bid for labor until they have sold their goods--which the unemployed do not have the cash to purchase, and so on.<sup>35/</sup> This failure of the markets to transmit messages about desired transactions from one side

<sup>33/</sup> R.W. Clower, "A Reconsideration of the Microfoundations of Monetary Theory," Western Economic Journal, December 1967, reprinted as "Foundations of Monetary Theory," in Clower, ed., Monetary Theory: Selected Readings, London 1969. Cf. pp. 207-08.

<sup>34/</sup> Suppliers who are sold out and face a queue of dissatisfied customers with cash in their pockets will increase their orders and raise prices; they will do neither when observing lines of starving, out-of-pocket unemployed. (Use of Say's Principle in the construction of the model precludes analytical consideration of acts of mercy).

<sup>35/</sup> This is a Barro-Grossman "B-solution" again. The Barro and Grossman paper does not stress the distinction between constrained purchases and constrained demand-signals developed in this sub-section, however.

to the other is what we mean by the phrase "effective demand failure."

The non-clearing market states for both our barter and our money system may be described in virtually identical terms, stressing the simple logic of the equal-value-in-exchange requirement (Say's Principle): since transactors do not succeed in selling more than they do, they cannot be buying more than they do. . .etc. But such descriptions of the states are analytically incomplete<sup>36/</sup> or it would be clear what happens next, namely, the first ("barter") system homes in toward a coordinated state, the second (monetary exchange) system does not.

Since it carries a reminder of this analytical fact, we now prefer the term "cash-constrained" to our earlier usage of "income-constrained" (behavior, process, etc.). For similar reasons, it is sometimes necessary to distinguish between situations of "deficient aggregate demand" and those characterized by "effective demand failure," and not treat "effective demand" and "aggregate demand" simply as synonyms.

#### B. Theories of the Consumption Function.

This section can be brief; the main ideas I have stated elsewhere,<sup>37/</sup> and their underpinnings have just been discussed in some detail.

I referred above to Keynes' simple consumption-income relation as the nexus of all the "singularly Keynesian, obviously anti-Classical" properties

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<sup>36/</sup> In the sense that, in mechanics, a description giving only, say, the mass and space-coordinates for a body at a given point of time would be incomplete in omitting information about its (directed) velocity. Cf. the earlier discussion above concerning analysis of non-clearing market states using standard steady-state tools.

<sup>37/</sup> Keynes and the Classics. . . , pp. 42-45.



of standard macro-models, including the Multiplier and the independence of consumption demand from labor supply. One reason for calling Keynes' consumption function "anti-Classical", we recall, is that it makes the consumption of households depend, not on utility maximization constrained by prices and endowments, but on "realized sales of factor services" (current income).

The so-called "postwar forecasting debacle" in the U.S. was attributed in large part to use of this function to predict the consumption component of aggregate demand.<sup>38/</sup> Later work on the problem--some of the best work we have seen in macroeconomics--produced the "modern consumption functions" of Modigliani-Brumberg-Ando and Friedman. Theirs is the seminal work, but there have been many important contributions both before and since.

It is impossible to do this literature justice in short compass. Ignoring operational empirical issues altogether, the main theoretical theme is this: current consumption is to be predicted, not from current income, but from what I will call "perceived wealth."<sup>39/</sup> If we treat wealth--defined as the present

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<sup>38/</sup> My own hypothesis to account for why the U.S. economy did not lapse back into "great depression" is simple, perhaps naive: it was put back into "the corridor" through the huge balances of liquid assets that war finance allowed the private sector to accumulate and then insured against a new departure from it by pre-Accord monetary policy which, to put it favorably, made certain that the well would not run dry again.

If this hypothesis be "true" (whatever that might mean with reference to a generalization so broad), the conclusions drawn from the forecasting debacle caught only part of the trouble. They were, in effect (1) that what is nowadays the standard Keynesian textbook model was all right, but (2) that its consumption function needed repair. The results of that diagnosis seem to have been a "very nearly" neoclassical consumption function stuck into a Keynesian model.

It just might be that that peculiar combination won't fit any state of the world, be it inside or outside "the corridor".

<sup>39/</sup> How large a step back towards pre-Keynesian theory this represents does not seem to have been generally appreciated. Although there are some fairly subtle conceptual problems, the wealth concept used here is not all that different from "the value of the endowment" concept that appears in

value of current and expected future income--as the main determinant of consumption, and define income as the rate of change of wealth, it is clear that no stable relationship between consumption and income can be predicted for short time periods. Even for consumption and income flows cumulated over, say, a calendar year, the influence of income on consumption should be relatively weak and unreliable. What then remains of the "Keynesian nexus"? A low marginal propensity to spend on consumption goods<sup>40/</sup> means weak multiplier effects, ineffective fiscal policy, etc., etc.

The transmogrifications of "wealth", in this context, are about as numerous and difficult to deal with as were those of "realized sales" in our earlier discussion.<sup>41/</sup> We note just two: (1) wealth represents a "subjective

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intertemporal neoclassical constructions.

For reasons partially adumbrated in the preceding footnote, these theories do not go all the way back to neoclassicism, but occupy in effect a curious halfway house. Keynes' preoccupation with "involuntary unemployment" states of the system allowed him to split the traditional model of household behavior down the middle, separating the consumption decision from the labor supply decision. Income is not the result of household choice, but "involuntarily" determined. Traditional determinants of the labor supply decision, etc., may then be ignored and the consumption decision treated as determined by income. Modern consumption function theory has not put the theory of household behavior back together again. The usual generalization of Keynes' current income to (the present value of) current plus expected future income still treats future wage-income as parametric to the household's consumption-accumulation decision.

<sup>40/</sup> Admittedly not the same thing as consumption--but we can't go into that. For an up-to-date assessment of empirical work in the Friedman branch of this literature, cf. M.R. Darby, "The Permanent Income Theory of Consumption---A Restatement" (forthcoming). Darby shows that "the econometric procedures which have been utilized in the estimation of permanent income have biased upwards the estimates of the weight of current income. . ."

<sup>41/</sup> This is true quite apart from the fact that the human capital component consists largely of income from "not yet realized" sales of labor services. This helps account for the "imperfect" market in loans secured by human capital collateral which is of importance to our argument, but we cannot enlarge upon it here.

estimate of maintainable living standards," and (ii) wealth constitutes a (presumably objective) intertemporal constraint on expenditure.

Suppose for the moment that the effect of a change in the level of current income (over an undefined, but not indefinite "period") on "wealth" in both senses were of the second order of smalls. Consumption should then be unaffected, and secondary (multiplier) effects on aggregate demand should not be observed. Consequently, no effective demand failure would be observed in our archetypical case. But suppose next that a drop in income impinges on a household whose balances of cash and other highly liquid assets are zero; the household has no liquid "buffer stock" at the date of impact of the disturbance. Still its "wealth" is, by assumption, unimpaired. In what ways could it finance a maintained level of consumption? I suggest that the empirically relevant opportunities for so doing can be described as "distress" sales of non-human assets and "distress" borrowing against future income prospects.<sup>42/</sup> Either avenue of action would, if taken, (a) reduce "wealth" in sense (ii), and (b) reduce "wealth" in sense (i) by more than is avoidable by simply cutting current consumption until income starts once more to flow at its "permanent" rate. It appears that situations may occur for which "wealth (ii)" is not the relevant constraint on expenditure.

In such situations, the system exhibits effective demand failures. Its self regulating capabilities are drastically reduced. With cash constraints operative, at least on households, further disturbances will trigger deviation-amplifying multiplier processes--an opportunity for effective, pump-priming, fiscal action.<sup>43/</sup>

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<sup>42/</sup> For (badly needed) amplification, see my LSE lectures, op.cit., pp. 43-4.

<sup>43/</sup> Cf. Leijonhufvud, "Keynes and the Effectiveness of Monetary Policy," Western Economic Journal, March 1968.

When would we then expect to observe effective demand failures, sizeable multiplier coefficients and the rest? In brief, when liquid buffer stocks have been squeezed out of the system. This takes a "large displacement"--an unanticipatedly large displacement.

We "supposed for the moment" that it was legitimate to discuss whether multiplier effects can occur while holding permanent income constant. It is now clear that this simplifying assumption has to be given up at the same time (if not sooner) that cash income constraints become binding. We are considering a hypothetical situation in which the transactor has been subjected to an income reduction of larger magnitude and longer duration than he anticipated in planning his liquid asset holdings. This necessarily means a drop in income of a magnitude and duration such that he must revise downwards the subjective estimate of his permanent income. He could not otherwise consistently (a) regard it as wholly "transitory," and (b) not have ensured himself of a buffer stock of liquid assets, credit lines, and unemployment compensation rights larger than our illustration supposes.

Outside the corridor, therefore, effective demand failures come to dominate the dynamic motion of the system due to two factors: (i) the exhaustion of liquid buffers, reinforced (ii) by dysfunctional<sup>44/</sup> revisions of permanent

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<sup>44/</sup> "Dysfunctional" in the sense of being self-fulfilling prophecies of future incomes below the incomes that would be earned could the system be returned to the "ideal path." For a turgid elaboration on this sort of thing, cf., my On Keynesian Economics. . . , Chapter IV, Section 5.

Note that the same sort of expectations-revision will apply to the earnings of corporations. When this happens, the present value of equity shares in them will--even if evaluated at the "natural rate" of discount-- fall below the market price required to call forth the rate of investment needed for the system to return to the "ideal coordination time path." Cf., my "Keynes and the Effectiveness. . .", op.cit.

The point is worth enlarging upon. Within the corridor, transactors that either over- or under-estimate the present value of earnings from

income expectations.

A final note is in order under this heading. At this stage, it is clear that our theory implies a variable width of the corridor. Transactors who have once suffered through a displacement of unanticipated magnitude (on the order of the Great Depression, say) will be encouraged to maintain larger buffers thereafter--until the memory dims. . .

C. Quantity Theories of Money Income Determination

On the topics treated so far, I have enjoyed the customary psychological comforts of an author: I know more than I have said. In this section, I will say more than I know<sup>45/</sup>--but there will be less of it. In any case, something needs to be set down under this heading to round out the picture this paper has tried to present.

Since Quantity Theories, ancient or modern, usually do not specify equations for the so-called "real sector," it is clear that they are essentially mute on the subject of effective demand failures. But such failures do not seem to fit in. The Quantity Theory approach to income determination and

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assets in prospect along the "full coordination" time path will, in Knightian fashion, suffer losses or forego profits and tend to be weeded out. This means that transactors who persistently act on "socially dysfunctional" evaluations have a low (private) survival probability.

The opposite tends to be the case outside the corridor. This conclusion applies symmetrically to inflationary and deflationary "large displacements."

I am indebted to Prof. Henry A. Latané for forcing my attention onto this issue.

<sup>45/</sup> An inequality that promises to be reversed by Mr. Daniel Benjamin's work on the topic of this section. Given the modelling difficulties and severe data availability problems, he risks investing in an "unpublishable UCLA doctoral dissertation." I am indebted to him for this splendid display of risk preference--as well as for numerous helpful discussions.

effective demand theory can be brought to confrontation by focusing on the multiplier implications of the latter.<sup>46/</sup> Modern Quantity Theory, in its elemental form, predicts income from three equations: (1) a money demand function with income as the independent argument; (2) a money supply equation stating that the money stock is exogenously determined; and (3) a "demand equals supply" equilibrium condition. The Quantity Equation is nowadays invariably interpreted as the reduced form of a system of this general description. To allow Keynesian disturbances to displace it from equilibrium, this closed system has to be opened up; we do so by putting the interest rate in as another unknown in the money demand function. The result is a "variable velocity" model.

Now, assume a "decline in the marginal efficiency of investment." The impact effects are an excess supply of commodities and an excess demand for securities. The latter is eliminated by a fall in the interest rate. This increases the amount of money demanded as of the initial income, thus producing an excess demand for money (equal to the "remainder" of the excess commodity supply). Standard quantity theory reasoning now applies: income must fall until the excess demand for money is eliminated. Utterly Keynesian so far.<sup>47/</sup>

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<sup>46/</sup> . . . or by listening to my colleague, Earl Thompson, to whom I am indebted for bringing about the confrontation in my own mind.

<sup>47/</sup> The version of Keynesianism enshrined in the textbooks invariably assumes a stable aggregate demand function for money. That assumption is as crucial to that construction as it is to any quantity theory. Since it is, to say the very least, unclear that this was the original idea, it might be better to refer to IS-LM models as "variable velocity quantity theories."

But that is it. The adjustment process should stop right there. To restart it would take another shock to create a new excess demand for money. There is no suggestion here of an ensuing deviation-amplifying multiplier process or of the system's ability to recover being impeded by effective demand failures.

The Quantity Theory could be made to accommodate the possibility of "cumulative processes" in various ways, e.g., (i) by making the money supply endogenous in such fashion that the "first-round" decline in income does not remove the excess demand for money; (ii) by assuming money demand dependent not only on "steady-state" income but also on changes in income in such a way that short-run "ratchet effects" are obtained. These alternatives I leave aside.

The revival of the Quantity Theory has been accompanied by much inconclusive debate about the proper operational definition of "money." Which assets and how many should be aggregated in measuring "M"? I have nothing conclusive to add to that debate; the following discussion leaves the choice to the reader. There has been virtually no discussion of the other aggregation problems. Granted that stable demand functions for money exist for individual transactors, how confident can we be of the existence of a stable aggregative money demand function?<sup>48/</sup>

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<sup>48/</sup> The reason why the question has not been raised even by fervent anti-monetarists is provided, I think, in the preceding footnote. Anyone who is tempted to grasp upon the (empirically unsubstantiated) argument that follows as a new weapon against monetarism should be forewarned that it is double-edged. If there were to be sizeable aggregation errors in one of the functions of the standard model, there must a fortiori be corresponding "instabilities" elsewhere in it.

The converse double edge to Friedman's insistence on the instability of the multiplier is, cuttingly, pointed out by F.H. Hahn, "Professor Friedman's Views on Money," Economica, February 1971.

At this oint, we must take a drastic short cut. I hate being mathematical about it, but consider the equation:

$$(1) \quad MV = PX,$$

where PX stands for aggregate expenditures on final goods. Instead of the modern interpretation of the equation as a reduced form, we adopt an old-fashioned one: V is taken to represent the "average propensity to spend on final goods out of money balances." We will assume (a) that stable underlying money-expenditure relations exist for all transactors, and also (b) that, within the ranges relevant to our conceptual experiments, all these relations are linear with zero intercepts. For each transactor, the average and marginal propensities to spend out of money balances (APSM and MPSM) are equal and constant.<sup>49/</sup>

We have k relations of the simple form

$$(2) \quad m_j v_j = P x_j, \text{ where } v_j = \text{APSM}_j = \text{MPSM}_j.$$

What assumptions would justify replacing these individual functions, k in number, with equation (1)? The usual first two lines of defense of such aggregations are: (i) that it is permissible to assume that all of  $v_j$ 's are of equal

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<sup>49/</sup> Readers already uneasy with this short cut may fortify themselves by interpreting these individual money-expenditure schedules as stock-flow equilibrium loci of the type constructed in G.C. Archibald and R.C. Lipsey, "Monetary and Value Theory: A Critique of Lange and Patinkin," Review of Economic Studies, 1958. This type of construction will not support our argument below, however, beyond the point where income account and capital account transactions are separated.

In my argument from this point on, I have been greatly fortified by the theoretical investigations of Peter Howitt and by numerous discussions with him during his 1971-72 stay at UCLA. Two of his papers are particularly relevant: "Stability and the Quantity Theory" and "The Short Run Dynamics of Monetary Exchange" (both to be published). The finite-time dynamics of Howitt's model in the latter paper exhibit "the corridor"—for reasons that, while formally more formidable, are at bottom the same as those loosely sketched below.



magnitude so that there can be no distribution effects; (ii) that, although the  $v_j$ 's differ, the proportional distribution of the  $m_j$ 's can be justifiably assumed constant over the time-period and population of transactors studied. I will suppose that we can agree that (i) is "obviously untrue." I will then infer that the implicit justifying assumption for the aggregation over transactors, in Quantity Theories generally, is of type (ii).

We may now proceed directly to the analytical possibility that intrigues me. Note that what follows definitely is a special case--and it takes the conjunction of the following, separate assumptions to produce it. Following Gurley and Shaw,<sup>50/</sup> we divide all transactors into two groups, referred to as "deficit" and "surplus" units, respectively. We assume (a) that, over the period that we focus on, migrations between groups do not occur; units are allowed to proceed, at most, to the boundary line of running a balanced income-account budget; and (b) that, on the average, the  $v_j$ 's of the deficit group exceed those of the surplus group. This completes the setting of our special case. It implies the following: if we were to trace all income account transactions in the system, while ignoring those on capital account, we would observe a net cash flow from the deficit group to the surplus group. If that was all there was to it, we should--vide assumption (ii)--observe total expenditure on final goods declining in the system. Since the money stock is held constant, this means that observed average velocity declines. Consequently, I enlarge upon the earlier inference concerning the implicit aggregation-justifying assumptions of the Quantity Theory: it assumes that capital account

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<sup>50/</sup> E.g., their "Financial Intermediaries and the Saving-Investment Process," Journal of Finance, May 1956.

transactions (sales and purchases of existing assets as well as credit transactions) occur so as to offset continuously the tendency of net flows on income account to "upset" the given proportional distribution of cash. For brevity, I refer to these offsetting capital account transactions as "cash reshuffling".

We come then to the point: when should we expect monetarist income predictions to "break down" (and do worse relative to simple Keynesian multiplier predictions than "normally")?<sup>51/</sup> Even omitting supporting argument, the answer is, I think, clear: during episodes when conditions in asset and credit markets are so "abnormal", that normal cash reshuffling processes are likely to be seriously impeded.<sup>52/</sup> Again, we would look for "large displacements" removing the system from the corridor. The most obvious possibilities involve the same conditions as those that we previously suggested should be present when effective demand failures occur. Consider the household. The tight cash/income constraint becomes binding when its income earners have been unemployed long enough to exhaust savings deposits and rights to unemployment compensation --and, of course, the availability of credit on "reasonable terms." In that situation, its MPSM should also be high relative to transactor units with healthier balance sheets.<sup>53/</sup>

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<sup>51/</sup> Cf., M. Friedman and D. Meiselman, "The Relative Stability of Monetary Velocity and the Investment Multiplier in the United States, 1897-1958," in Stabilization Policies (Commission on Money and Credit), Englewood Cliffs, N.J., 1963).

<sup>52/</sup> Here we can only hint at financial instability themes developed by Professor H.P. Minsky in numerous contributions.

<sup>53/</sup> Interference with the cash reshuffling process could also occur in inflationary situations. Consider, for example, "disintermediation" phenomena consequent upon nominal interest rates piercing legal ceilings. The widely discussed disintermediation problems in the U.S. in the late 1960's apparently coincided with previously relatively reliable monetarist velocity equations producing predictions later found to be over-estimates. I am

I suggest that those well known conditions of the 1930's that have been widely interpreted (by Keynesian writers) as attributable to a static "liquidity trap" property of a stable aggregative (excess) demand for money function are at least equally well accounted for by the hypothesis just outlined. If we can assume that open market operations are transactions between the central bank and units with below average MPSM, monetary policy will be atypically "ineffective" under the conditions assumed. In the same conditions, fiscal policy should be atypically "effective" even if unaccompanied by injections of money. It can be so because of the opportunity to borrow from low-MPSM units and to channel the funds through the budget discriminatively into the hands of high-MPSM units--in a situation where the normal endogenous reshuffling mechanisms are inoperative.

A final proposition regarding the corridor: Cantillon-effects will be strong and relatively long-lasting outside, weak and evanescent inside the corridor. <sup>54/</sup>

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indebted to Sam Peltzman for this observation.

For those taking the fashionable literature on "Optimal Monetary Growth" seriously, there just might be a warning here. The policy suggestion has been made that the rate of return on money balances "ought to" be manipulated into equality with the rate of return on real capital. As pointed out to me by Earl Thompson some 5 or 6 years ago, this entails killing off all intermediary institutions, since no margin between borrowing and lending rates will remain to live on. More generally, it completely eliminates the incentive for surplus units to lend--just letting the cash pile up brings the same return. And so on and so forth. The social optimality of this sort of things escapes me, but I must confess to having ignored much of the literature on the subject.

<sup>54/</sup> I define "Cantillon-effects" as occurring whenever the effects on aggregate money income of increases in the money stock are found to depend upon the route by which the injection of money takes place.

III.

Hopefully, the various themes of this paper will be seen to form an intellectually coherent theory. Rigorously consistent, it is not; solid empirical support, it does not have. As advertised in the beginning, it is more than anything else an agenda for--and invitation to--needed modelling and empirical work by those who find it plausible enough to be worth pursuing. Whether anyone outside UCLA would find it at all plausible, I have no idea. Labels are much in fashion in macroeconomics. To label this as "UCLA macro-theory" would be to implicate too many entirely innocent colleagues. But it will not have escaped the reader's attention that many an issue controverted these many years between various other "schools" is rather defused if the theory outlined here is judged provisionally acceptable.