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ALTERNATIVE APPROACHES TO MACROECONOMICS:
METHODOLOGICAL ISSUES AND THE NEW KEYNESIAN ECONOMICS

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ABSTRACT

While recent alternative approaches to macroeconomics have all begun with the presumption that macro-economic behavior ought to be derived from micro-economic foundations, they have differed in their views concerning the appropriate micro-foundations. This paper explores some of the key methodological issues, including those concerning the use of representative agent models, choices in parameterization, problems in aggregation and modelling adjustment processes and speeds, the imposition of ad hoc assumptions, such as that of instantaneous market clearing, and alternative approaches to validation of proposed theories.

The paper summarizes the basic questions with which macro-economic theory should be concerned. Focusing on the labor market, it explains why New Keynesian Theories provide a better explanation of the observed phenomena than do alternatives.

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Alternative Approaches to Macroeconomics
Methodological Issues and the New Keynesian Economics¹

Joseph E. Stiglitz

I. Introduction: Two Views of Capitalism

For two centuries, economics has presented two divergent views of the capitalist system. One, focusing on its less benign aspects, earned our profession the reputation as the dismal science: it saw the capitalist economy as characterized by (among other evils) periodic recessions and grave inequality. The other, following along the tradition of Adam Smith, emphasized its efficiency. Smith's vision was, of course, far broader than the view reflected in the Fundamental Theorem of Welfare Economics; for Smith stressed--as have most non-economist advocates of market economies since--the market's striving for innovation.

¹ An earlier version of portions of this paper were presented as the Fisher-Schultz lecture, at the European meetings of the Econometric Society, August, 1987, at a meeting on Alternative Approaches to Macroeconomics, held at Siena, November, 1987 and as a lecture at Oxford University. Financial support from the National Science Foundation and the Hoover Institute are gratefully acknowledged.

Section V of this paper is partly based on joint work undertaken with Bruce Greenwald and Andrew Weiss, to whom I am greatly indebted. Parts of the joint work have previously been reported in the papers by Greenwald and Stiglitz cited in the references, Greenwald, Stiglitz, and Weiss (1984); and Stiglitz and Weiss (papers cited in the references).

I.A. Three Alternative Resolutions

There is an obvious intellectual tension between these alternative views. The Smith view dominated in microeconomics, where one of the central theorems was the Fundamental Theorem of Welfare Economics, a formulation of Adams Smith's "invisible hand" conjecture concerning the efficiency of competitive markets; while the problems of unemployment were the focus of a quite separate sub-discipline, macroeconomics. The tension between the two approaches has been addressed in three different ways.

(a) The Neoclassical synthesis. The most popular view during the quarter of a century following World War II was Samuelson's neoclassical synthesis, which held that, once the government stabilizes the economy, the old "classical" laws prevail--the basic insights of Smith still hold. The neo-classical synthesis was put forward as an assertion: it was believed because economists wanted to believe it; they did not want to discard what they had previously learned (in spite of the fact that one of the first lessons they teach their students is that by-gones are by-gones). But is there any scientific basis for the neo-classical synthesis, which claims that, once the government corrects the problems posed by unemployment, the economy behaves efficiently? Is there any reason to believe that market failures come only in big doses, rather than the alternative hypothesis, that depressions and recessions are but the most obvious symptoms of more pervasive market failures; and correcting the most obvious symptom is by no means equivalent to correcting the underlying problem? The great advantage of the neoclassical synthesis was that it allowed these two disparate views

to be bound within the same text. The conventional practice, followed in most colleges and universities, of having different instructors teach the macroeconomic and microeconomic sections absolved each instructor of having to reconcile the seeming inconsistencies in the two approaches; and the natural tendency of students to compartmentalize what is taught in different courses meant that--even should they have noticed the disparity in the approaches-- most students felt little if any intellectual tension.

The lack of intellectual foundations for the neoclassical synthesis has led, during the past fifteen years, to two new attempts to reconcile macroeconomics and microeconomics.

(b) Neoclassical Models. One set of approaches drops the "synthesis" from neoclassical synthesis. It does not try to combine Keynesian insights with neoclassical analysis, as Samuelson tried to do. It argues that all that one needs to understand macroeconomic activity is the neoclassical model. In effect, it attempts to make macroeconomics more like microeconomics. Currently, this view is most ardently represented by the real business cycle theories, which claim that the economy's fluctuations can be explained by exogenous "shocks" to its productivity. In the late seventies and early eighties, new classical economics, emphasizing the importance of rational expectations, dominated. ²

(c) New Keynesian Models. The other approach attempts to change microeconomics, to make it possible to derive from "correct" microeconomic

²Later in this lecture, I shall comment more extensive on these two approaches.

principles commonly observed macroeconomic phenomena. To accommodate microeconomics to macroeconomics, it introduces doses of imperfect information, imperfect competition, and adjustment costs. It looks for a variety of explanations of wage and price rigidities; it seeks to explain both the causes and consequences of these rigidities--consequences that extend beyond the macroeconomic fluctuations to which they may give rise.

I.B. The New Keynesian Approach and Objections to the Neoclassical Approach

Though different economists disagree about the relative weight that they assign to the variety of forms of imperfections (imperfect risk and capital markets, imperfect information, imperfect competition, adjustment costs), New Keynesian economists all agree that these "imperfections" exist, are important, and, together, can account better for macroeconomic phenomena--and the effect of government policy--than the alternative "perfect market" models.

The objection to the "perfect market" models, however, is broader than just its inability to explain macroeconomic phenomena. As is by now well known, the conception of the market economy embodied in the Arrow-Debreu paradigm is basically inconsistent with that associated with modern industrial economies in which innovation plays a central role and in which entrepreneurs and business managers are constantly engaged a process of obtaining information, to enable them to make decisions to adapt to the constantly changing economic environment.³

³See, for instance, Stiglitz [1987a, 1987b, 1990a, 1990b].

Economics is, or is supposed to be, an empirical science: how could economists' views be so divergent? Were these so called scientists studying the same economy? Were they--or should I say, are we--simply ideologues looking for justifications for our political biases, or, no less worse, technicians, taking the assumptions provided to us by our ideologue brethren, and exploring their consequences, trusting that the models we are analyzing bear some semblance to the world, because we have been told so by others! (Indeed, it is curious that macroeconomists took hold of the perfect markets microeconomic model just as microeconomists began abandoning that model, recognizing the pervasiveness of, for instance, informational imperfections and the absence of perfect risk markets.) These are among the ruminations which have occurred repeatedly to me as I have been engaged on a research programme over the past two decades, attempting to explore systematically the problems posed for capitalist economies by imperfect information, and the costly quest for information and knowledge (innovation and invention).

Out of that work has developed a picture of the capitalist economy in which market failures are endemic: Arrow and Debreu seem to have found the singular case (perfect information, complete markets) in which the economy is Pareto efficient. There exist, in general, interventions in the market (taking into account the costliness of information and of markets) which could make everyone better off. (See Greenwald and Stiglitz (1986, 1988e).) But while this work may have decreased our confidence in the efficiency of the market economy, other studies focusing on the workings of government have simultaneously decreased our confidence in the ability of the government to remedy these deficiencies. Unfortunately, theory can take us

this far, but not much further: theorems which say that the economy cannot be improved upon absolve us from the necessity of much enquiry into the nature of government of the magnitudes of the alleged market inefficiencies; but theorems which say that the economy can be improved upon are subject to the natural question: but how important are these inefficiencies? The true colors of the ideologues are quickly revealed: for they are willing to assert the unimportance of these inefficiencies without doing the necessary empirical investigations, though I have great confidence in their ability to "show" the unimportance, given enough time to do the appropriate data mining. Still, it is curious that the same "scientific" skepticism is not raised at those papers which purport to show the efficiency of the market, inevitably assuming no technical change and, at most, limited forms of imperfect information: should they not have similarly asked, "how do we know that these results are robust to such considerations?"

But there is one arena in which I would have thought the "facts" would speak for themselves: surely the Great Depression, as well as the periodic episodes of unemployment experienced by the United States and Western Europe over the past two centuries are evidence of some kind of market failure? How could a theory which predicts that there should be full employment (except when wages are zero) be taken seriously?⁴

⁴The standard answer, that the "perfect market" models are only intended to explain the "normal" economic fluctuations, not the Great Depression, is unconvincing. As I comment later, the big experiments--like the Great Depression--provide the best tests of alternative theories: under

II. Methodological Remarks on Macroeconomics

Over the past few years, I, along with several of my co-authors, have been attempting to construct macroeconomic models with consistent microeconomic foundations. This is, of course, not the only such attempt that has been made in recent years, nor even the only one in which informational imperfections have played a critical role. Indeed, it seems by now almost obvious that, in the absence of transactions/information costs,⁵ market economies must be Pareto efficient, and, in particular, the kinds of massive under-utilization of resources associated with recessions and depressions simply could not occur.

One approach has emphasized the signal-extraction problem, the difficulties that, say, General Motors had, in 1980, ascertaining whether the decline in the demand for its automobiles was due to a monetary or a real shock. (Evidently GM executives of the company were unable to read the newspaper, to find out what was happening either to monetary policy, or, for that matter, to Japanese imports; and their economists and marketing people were evidencing the same kind of incompetence that we have come to expect of government officials. Of course, part of the problem may lie in normal circumstances, it is difficult to distinguish among competing hypotheses.

⁵With some further assumptions, such as the absence of any monopoly power on the part of any agent.

the fact that the monetary statistics are of limited relevance for making inferences--which, if true, should have direct implications for the econometric work using such data.) The work in this tradition has, however, assumed that while the signal extraction problem greatly complicates the analytics of the problems facing firms and individuals, the institutional structure of the economy is essentially unaffected.

By contrast, I have been focusing on a set of information problems which have pervasive effects on how labor, capital, and product markets function. They affect the mechanisms by which risks get shared, by which information gets conveyed, and by which individuals and firms are provided incentives.

Before setting out the details of these theories as they relate to the output and employment market, I want to spend some time discussing a range of methodological issues which arise in macroeconomics.

II.A. Relationship between Micro- and Macro- Theory

There is now a general consensus that good macroeconomics must be built upon solid micro-foundations. But this perspective is often confused with an argument that there is a particular set of micro-foundations upon which it should be built: namely, competitive environments with rational agents operating with rational expectations in environments in which adverse selection and incentive (moral hazard) problems are largely absent. That is one set of hypotheses that may deserve exploration, though I am not very sanguine that such an approach will yield important insights into the questions at issue. But economics is, or at least should be, a behavioral

social science. We know, for instance, from the work of Tversky and others that there are systematic errors in individuals' perceptions. The work of Akerlof and Yellen has shown that these misperceptions can systematically get reflected in economic behavior. Exploring the causes and consequences of these seems to me an item which should have a high order on the research agenda.

A. 1. Some Qualifications on the Role of Micro-Foundations

Trade-Offs in Modeling. But like all doctrinal positions, the contention that all macroeconomic work should begin with a specification of the microeconomic foundations can be carried too far. No one should be more aware of the existence of trade-offs than economists; solving signal-extraction problems is no mean task; it is perhaps regrettable that explicit solutions can only be obtained with parameterizations whose empirical implications can be quickly rejected. But my concern here is that focusing on that problem diverts attention from other problems. We can, perhaps, all agree on our eventual goal: the construction of models which incorporate all of our concerns. But I hardly need to remind this audience that the purpose of model construction is not the replication of reality; rather it is the development of a set of lenses through which we can see the essential aspects of any phenomena. And here, as I have already indicated, is where New Keynesians and neo-classical economists differ: I see the center piece of such a construction lying in a more thorough understanding of how informational considerations affect the functioning of labor, capital, and product markets. And one cannot develop an understanding of how those

markets function from models based on representative individuals, no matter how complicated the dynamic programs which they might be solving.

First Principles? Moreover, hopefully our discipline is a cumulative science. Not every piece of research has to begin at the beginning. We know that there are good reasons, based on problems of adverse selection and moral hazard, that equity markets may not function well.⁶ We also have ample empirical evidence that firms make limited use of equity markets, and event studies confirm that when they do raise additional capital through the issue of equities, stock prices are lowered significantly. It thus seems perfectly appropriate for macroeconomic studies to begin with the hypothesis that equity markets do not function efficiently. For some purposes, it may not matter what the precise source of this market failure is; in other cases it will; and a good macroeconomic theory will try to distinguish among these cases.

Is Microeconomic Analysis Helpful at All? There is a view which is even more skeptical of the role of "simple" representative agent microeconomics in macroeconomic analysis. It is based on the results of Debreu (1974), Mantel (1974), and Sonnenschein (1972, 1973), which show that any set of market excess demand functions satisfying Walras' Law can be derived from utility maximizing individuals. Essentially, the rationality hypothesis puts no restrictions on observed behavior.

⁶See, e.g. Meyers and Majluf (1984), Greenwald, Stiglitz, and Weiss (1984), Stiglitz (1982) and Gale and Stiglitz (1989).

Of course, when economists say that a particular parameterization of behavior should be "derived" from microeconomic principles, what they mean is something different: they mean that a particular parameterization can be derived as the behavior of a single individual. But why should the economy behave as if there were a single individual? The Debreu-Mantel-Sonnenschein theorem simply reminds us that there is no reason that this should be so. Casual empiricism suggests many reasons why this should not be so, some of which I shall come to shortly.

Limited Usefulness of the Representative Agent Model for Macroeconomic Analysis. More generally, the representative agent models, as useful as they may be for teaching, have serious drawbacks when applied to macroeconomic analysis (or more serious microeconomic analysis, for that matter.)

First, they are of limited use in investigating problems arising from information asymmetries and coordination failures. Presumably, asymmetric information could only be reconciled with a representative agent model by assuming a particular kind of schizophrenia on the part of the representative agent.

Secondly, if one believes that some kinds of "market failures" are at the root of macroeconomic phenomena, one can hardly study those issues by using representative agent models: for when all individuals are identical, there is no need for trades, and hence there are no consequences of the absence of markets. For instance, risk markets entail the transfer of risk from one individual to another; but if all individuals are identical, the

absence of risk markets has no consequences: there would be no trade on those markets, even if they existed.

Indeed, even the standard method of "solving" for market equilibrium within representative agent models cannot easily be extended: conventionally, the equilibrium is solved by finding the allocation which maximizes the welfare of the representative agent, and using the Fundamental Theorem of Welfare Economics to interpret the resulting optimum as a market solution. But the Greenwald-Stiglitz theorem (1986) shows that in the presence of imperfect information and incomplete markets, market equilibrium is not even constrained optimum.⁷

What is at issue is more than theoretical niceties: credit constraints arise if one individual would like to borrow from another but cannot; but if all individuals are identical, there is no scope for intertemporal trades. Even if prices are wrong, so that all individuals believe that they would like to borrow, the real resource allocations may be unaffected: there is no one from whom they could borrow, even if prices were right.

⁷One may be able, of course, to create an "artificial" maximization problem, such that the market equilibrium corresponds to the solution to this maximization problem. Grossman's Social Nash Optimality concept was intended to define the sense in which economies with an incomplete set of risk markets are optimal. The analysis of Newbery-Stiglitz (1981), Stiglitz (1982) and Greenwald-Stiglitz (1986) makes clear exactly how artificial that maximization problem was. Nonetheless, the techniques of analysis can be used to "solve" for the market equilibrium.

II.B. Generality and Specificity in Macroeconomic Models

Macroeconomics faces a problem that much of microeconomics does not: while much of microeconomics is concerned with abstract questions such as the existence and efficiency of equilibrium, macroeconomics would like to "explain" certain phenomena.

One of the major lessons to emerge during the past quarter century-- both from the capital theoretic controversies of the 1960s and the Debreu-Sonnenschein-Mantel theorem to which I referred earlier--is that restrictions on "rationality," even combined with restrictions on technology (such as convexity) do not suffice to establish many qualitative results of interest. Some further restrictions are required. The question is, which? And this is a matter of judgment. As I have suggested, assuming all individuals or firms are identical is imposing too much structure on the economy, at least when it comes to asking questions about the short run movements in the economy. For other questions, it may not be inappropriate: for some purposes, assuming an aggregate production function may yield insights of value.

"Solubility." Tractability has been one long honored criterion for the selection of assumptions. One criterion for choosing assumptions in much recent work has been "solubility"--whether, with the given parameterizations, solutions can actually be calculated. This represents a change in perspective. Economists' lack of confidence in precise parameterizations (and in the values of the parameters in those parameterizations) had previously led most economists to attempt to derive

qualitative properties, based on qualitative restrictions (e.g. they asked, what can we say simply assuming convexity or concavity). Obviously, by imposing additional restrictions, one can make stronger statements; one can derive a more complete characterization of the economy. The problem is again one of trade-offs: the parameterizations which are soluble have properties which--on theoretical grounds alone, without engaging in much fancy econometric work--can be rejected. For instance, much work has employed the constant absolute risk aversion utility function ($U = -\exp(-\alpha Y)$). Such a utility function is simply unacceptable as description of individual behavior: it implies, for instance, that the wealth elasticity of demand for risky assets is zero, and that all individuals hold exactly the same portfolio of risky assets. Of what interest is it that a few of the properties generated by that model (say the time series of output) are consistent with the data, when there are other predictions of the model which can be easily rejected?

II.C. Plausible Magnitudes and Judging the "Reasonableness" of an Explanation

I have argued that to derive results in macroeconomics, one has to impose assumptions beyond the minimal assumptions associated with rationality and convexity of technology. The problem remains, what are "reasonable" assumptions. What makes this problem so difficult is that to answer that question, we must make quantitative judgments about qualitative effects: in general, there are many "effects"--economic behavior might plausibly be affected by a myriad of variables. But some of these effects

are likely to be small, too small to account for the magnitude of changes in output or employment that we observe.

Three examples will illustrate what I have in mind.

Real Balance Effects. In much of the macroeconomic literature following World War II, real balance effects played a central role. They figured prominently in Patinkin's classic book. Even today, in a sometimes hidden form, they play a prominent role in the fixed price models (in those models, a fall in wages and prices normally will lead to an increase in output and employment, precisely because of the real balance effect.) Recent years have seen the real balance effect questioned on many grounds. It is only outside "money" and government debt that matters for the real balance effect; since it is money that we "owe" ourselves--government debt, if internally held, is just money that we owe to ourselves--what we gain on one side (as holders of the debt) we lose on the other (as taxpayers, liable for repaying the debt.)⁸ ⁹ But whether one takes these "sophisticated" views

⁸There are further issues concerning the dynamics of price declines. Falling prices gives rise to intertemporal substitution effects. If price declines are expected to continue, or to increase, these intertemporal substitution effects may lead to less consumption and investment. See below, Grandmont [1983] and Neary and Stiglitz [1982].

⁹To the extent that parents do not take fully into account their children's welfare in determining the magnitude of bequests, there may be intergenerational effects, leading to increased consumption by the current generation and reduced consumption in future generations--but this may lead

or not, it is hard to see, even under the most optimistic view, the quantitative significance of the real balance effect for short run macroeconomic analysis. Assume prices were to fall by as much as 10% in a year (this has happened only a few times in the last century); if the (outside) money supply were to remain unchanged (and if we believe that an increase in real balances is treated by households as an increase in their real wealth), and if real balances represent roughly 25% of total physical assets, and total physical assets represent 25% of total assets (including human capital), and if the elasticity of consumption with respect to real wealth were 10% (it is probably more like 6%), then the percentage change in consumption from the real balance effect would be

$$10 \times .25 \times .25 \times .1 = .06\%$$

Hence, to increase consumption by 25% would take roughly 400 years. The point is that the time scale for the real balance effect to be significant is not commensurate with the appropriate period of analysis for macroeconomic models. As a first, or even second, approximation it is far better to ignore it.

Cost of Adjusting Prices. In recent years, there has developed a huge literature attempting to explain price rigidities on the basis of adjustment costs. The title of this literature, "menu costs," appropriately signals the unimportance of these adjustment costs (at least as conventionally modelled.) (It seems as if the advocates of this approach thought by being their own first critics, they would disarm the skeptical, and thus establish the validity of their approach.) The recent spread of computers to the

to increases in consumption at the current time.

restaurant industry, allowing hourly printing of relatively fancy menus at almost zero cost should--if the theory were correct--have eliminated price rigidities in the restaurant industry. In industries in which there are no published price lists, presumably there would be no price rigidities. On the face of it, it would seem that the costs of adjusting prices are simply too small to account for significant price stickiness--of a magnitude that could cause large unemployment.¹⁰

Worse still, for the theory, is the fact that what we should be concerned with is explaining the relative rigidity of prices and the relative variability of output and employment: but surely, costs of adjusting output and employment are greater than the menu costs of adjusting prices.¹¹

¹⁰To be sure, the literature has attempted to show how small costs of adjustment could lead to large macro-economic effects. Much of this literature is based on a more basic result: whenever the economy is not (constrained) optimum, a deviation of an individual or firm from its optimum (which has a second order effect on the individual) has a first order effect on the economy (since the economy is not at its global maximum). Thus, small adjustment costs on output and employment could also have large effects on aggregate welfare.

¹¹There are other objections to the theory, the fact, for instance, that there appears to be more inertia in the rate of inflation than in the level of prices.

A rather different explanation of price rigidities is provided in Greenwald and Stiglitz [1989b] who emphasize the difference in the costs and

The Mechanisms by which Monetary and Debt Policy Exerts its Influence.

Recent years have seen several general results asserting that government financial policy has no real effects on the economy. (See Barro [1974] or Stiglitz [1988]). In establishing those results, several assumptions are made; dropping any of the assumptions will result in government financial policies having real effects. But many of these effects would seem to be too inconsequential to account for the magnitude of the effects of public financial policy. For instance, government financial policy could operate through redistribution effects across generations or within a generation, but is this a plausible mechanism? Similarly, changes in government tax rates will have dead weight loss effects. If a tax is increased in one year and decreased in another; and if the tax rates in different years differ, then the increase in deadweight loss in one year will not just offset the decrease in another. There will be real consequences of the change in total deadweight loss. But is this significant? Is this the mechanism by which government financial policy affects the economy? I think not.

II.D. Ad Hocery in Macroeconomic Analysis

It is currently fashionable to "derive" all the functions entering into a macroeconomic analysis from first principles, within the model. I have already given two objections to that approach: there are general theorems saying that any market excess demand function could be derived from rational risks of decisions concerning changes in the level of prices versus changes in quantities. (In some sense, of course, these decision costs can be simply viewed as a particular form of menu costs.)

individuals, and it seems hardly necessary to begin every paper from first principles. Macroeconomists should be able to borrow qualitative insights obtained in earlier papers.

Still, there is something very healthy about economists striving to understand better each of the assumptions which underlies their analyses. For instance, many older models simply assumed wage and price rigidities. Now we seek to explain them.

But several further caveats are in order, lest we become too unbounded in our enthusiasm for this more "rational" approach.

How primitive the assumptions? First, frequently, we only push back the frontier slightly--though that by itself is valuable. For instance, sometimes price and wage rigidities are related to the market failure of the absence of certain insurance markets; but the absence of the insurance markets needs itself to be explained.

Pseudo explanations. The fact that we can find some model in which the particular assumption can be "explained" should give us little solace, if our objective is explaining our economy. For instance, some economists have put a considerable amount of effort into explaining the cash-in-advance constraint--a constraint which is obviously not binding for most transactions (which can be conducted with credit rather than cash.) In understanding that constraint, one presumably would want to understand better why certain activities normally require cash in advance, while most activities do not. (Of course, since most activities do not require cash in

advance, understanding why those that do may make only a limited contribution to understanding macroeconomic fluctuations.)

It is easy to construct a model which explains the cash in advance constraint: assume, as some recent models have done, that individuals meet only once. If you borrow from someone, you will never see him again to repay him. Credit is infeasible. Cash is required. But so what? In our economy, we have a myriad of financial institutions which enable parties who never meet to borrow and lend to each other. What faith do we have that any propositions derived in the artificial economy in which individuals meet at most only once and there are no intervening financial institutions have any validity for our economy? These are, of course, matters of judgment: I suspect you can tell what my beliefs are.

Inexplicable Phenomena? There are some phenomena which are difficult to explain, at least within a model presuming rational individuals. Let me give two examples. My local bank once offered a new checking account, which was identical to the old, except that it offered a higher interest rate. You had to sign a form to switch your accounts. Many individuals did not switch their accounts. Seemingly, they kept their wealth in an asset which was dominated by another asset. Of course, one could, tautologically, say that the transactions cost (psychic or shoe-leather or postage) exceeded the gains.

This is only a less subtle form of a more widespread phenomena of individuals holding dominated assets. CMA (cash management accounts) provide, at almost zero cost, a way of holding government bonds that are perfectly liquid; the bonds can, in effect, instantaneously and costlessly

be converted into "money" (checking accounts), and yield a higher return than checking accounts at banks. It is hard for any economic theory to explain why such accounts did not exist twenty five years ago, and it is hard for any economic theory to explain why they are not even more widespread today. Nor can any economic theory explain well why home equity loans (allowing individuals to write checks against the equity of their house) developed only recently, or even why the typical forms that mortgages take today in the United States did not exist a quarter century ago, or why they differ from those found in many European countries.

There are other phenomena that are difficult to explain: we do not have good explanations for why contracts are not made contingent on many of the seemingly relevant and easily observable variables.

Economists have had two responses to such inexplicable phenomena. One is to suggest that because we cannot explain them, they do not exist. It is as if a biologist, finding it difficult to explain how blood can be pumped to the head of a giraffe, were to assume that it therefore must have a short neck. The fact of the matter is that contracts are not indexed, individuals hold assets that are dominated, etc. In the absence of an explanation, it is better to use descriptively accurate assumptions (contracts are not fully indexed, equity markets are limited) than to ignore the phenomena.

Two Cautionary Notes. Two caveats to this approach need, however, to be noted. First, we must be cognizant of the possibility that a change in the economic environment may lead to unpredicted changes. Take, for instance, the assumption of rigid wages, which has played such a central role in macroeconomics, and recall the days before we had good explanations of that

rigidity. For most purposes, it would have been better, I argue, to assume that wages are rigid, even though we could not explain that rigidity, than to assume that they were perfectly flexible. But in some circumstances, the assumption of perfect wage rigidity can lead to misleading results--a cautionary note on the use of fixed price models. For example, an increase in unemployment compensation will, in many of the efficiency wage models, lead to a rise in the real wage and an increase in the unemployment rate, a result that would not have been anticipated by the fixed wage models.

Secondly, sometimes things are not as they seem. Economists are used to looking behind the scenes to see what is "really" going on. For example, the absence of explicit provisions for indexing contracts does not mean that there is no indexing. There could be implicit indexing. Whether there is or not is often difficult to ascertain. (Often, I think, economists play this game too ardently: what is going on behind the scenes is as difficult to explain as what is explicitly contracted for.)

II.E. Validation of a Theory

This brings me to the final methodological remark: there is a popular view that the empirical verification of a theory requires a statistical test of the goodness of fit of the model, to see whether the predictions of the theory conform with the facts. Broadly stated, this position seems unobjectionable. But operationally, such a view is often translated in macroeconomics into testing the conformity of some time series predictions of the theory. Unfortunately, there appear to be a plethora of theories which do reasonably well on this criteria: careful selection of statistical techniques, data sources, and years has enabled a succession of

economists not only to show that their theory does well, but that it is superior to at least certain specifications of competing hypotheses.

But the validity of a theory does not rest on the conformity of one or two of its predictions to reality, but on all of its predictions. Our micro-based theories have micro-predictions as well as macro-predictions. We all recognize that a theory can be a useful theory even if some of its underlying assumptions are not verified. We do not need to see atoms to believe in the atomic theory. But if there were some predictions of the theory which are falsified (to use Popper's term)--including its predictions about the behavior of micro-units--the theory must be rejected, or at least patched up. (The patching up process provides a forum for the demonstration of cleverness; and in the end, a judgment must be made whether these have produced a more refined theory, or merely a Ptolemaic exercise.)

Thus, the implicit contract theories which were so popular in earlier days, while predicting wage rigidities, yielded predictions of full employment. Attempts to extend these theories by including considerations of asymmetric information yielded, alternatively over-employment, or, under-specifications where the worker provided insurance to the firm, under-employment in all states of nature except one. The theory predicted that laid off workers were better off than retained workers; that contracts carefully specified how the firm could dispose of its capital, and that they were indexed to a host of variables, industry and aggregate measures providing information about market conditions. The inconsistency of these

predictions¹² with the facts provides a basis for a rejection of this theory, no less so than would an econometric test. Of course, the fact that these theories (at least in these forms) provided an inadequate basis for understanding unemployment does not mean that some of the insights provided by these theories have not proven, or will not prove, useful in other contexts.

Later on I shall describe some of the "facts" that a good theory needs to explain. For now, let me summarize the basic argument of this section:

(a) There should no more be a separate macro-econometrics from a microeconometrics than there should be a separate macro-theory from a micro-theory.

(b) A model is persuasive only to the extent that all of its predictions are consistent with observations. At the very least, one needs to ask of a model which has implications which are inconsistent with the facts, "Are there models which are consistent with more of the facts than the given model?"

(c)¹ Goodness of fit may not be the most persuasive test of a model. Whether a model can explain certain "critical facts" may be far more important. Thus, in the test of relativity theory versus Newtonian mechanisms, on an R^2 test, based on ordinary experience, it would have been difficult to distinguish the two. The convincing evidence for Relativity

¹²To the extent that the contracts were implicit, it is hard to verify whether or not these conditions were satisfied. A theory, most of whose predictions concern unobservables, has, in this sense, a certain advantage over theories which generate predictions about observables! For further discussion of these issues, see Newbery and Stiglitz (1987).

were the crucial tests where the theories had markedly different implications.

III. Some More Detailed Issues in the Construction of Macroeconomic Models

In the preceding section, I outlined a set of methodological issues, which, while relevant to economic theory in general, are, I think particularly important within macroeconomics. In this section, I want to raise six more detailed issues that arise in the construction of macroeconomic models. They are modelling problems. We noted earlier the difficult trade-offs that macroeconomists face: they want a model that is specific enough to derive results, but general enough to capture what is going on. These are subtle matters. Reasonable people may disagree about the best research strategies.

III.A. Aggregation of Individuals and Firms

I have already inveighed strongly against the excessive aggregation of individuals, the use of representative agents. It is well known that the conditions under which one can legitimately form aggregates, e.g. an aggregate consumption function, ignoring distribution, are very restrictive. Essentially, what is required is that all consumption functions be linear in income (with the same slopes). Clearly, this is an assumption which is not likely to be satisfied in the economy. For macroeconomic analysis, of course, the relevant issue is not whether this is a "correct" or "incorrect" assumption. It is obviously incorrect. Doing rigorous, sophisticated

analysis with an obviously incorrect assumption does not make it any more correct. But that is not my objection. The question is, does it lead to misleading results, or at least, does it preclude considering some important aspects of the economy? I think it does. Liquidity constraints introduce an important source of non-linearity, which needs to be taken into account. To put it another way, if the mechanism by which monetary policy operates is to reduce consumption of those who are facing liquidity constraints, the distribution of assets in the economy is crucial in determining the effect of a change in present income on present consumption, and a representative agent model--by assumption--precludes considering this possible channel for monetary policy.

Aggregation of firms. Assuming all firms are identical may similarly lead to difficulties. If all firms are assumed to be identical, there is, for any job, either an excess number of applicants or an insufficient number of applicants. Thus, there is no ambiguity about what is meant by involuntary unemployment: if some (all) firms have vacancies, anyone who does not work is voluntarily unemployed. He has chosen not to work. But, conversely, if some (all) firms have excessive applicants, anyone who applies for a job and does not get it is involuntarily unemployed. But in real economies, there simultaneously exist vacancies and excessive applicants for some jobs. To analyze this, we cannot assume all firms are identical. In such a world an individual could, at the same time, be both voluntarily and involuntarily unemployed; i.e., he could have applied for a number of jobs for which he was perfectly well qualified (i.e., as qualified as the individuals who obtain the jobs) and not obtained them, and in this sense, his unemployment is due not to his choice but to the random selection of the market. But at

the same time he has chosen not to apply for a job which he might have obtained, and in this sense he is voluntarily unemployed. In the end, I suspect that appending the term voluntary or involuntary to unemployment in such situations is not very helpful (although we are more likely to be sympathetic to "involuntary" unemployment than to "voluntary" unemployment). The important point is that the structure of the economy is such that individuals who in other situations, would have been gainfully employed, are not working. The equilibrium of the economy is inefficient and there are government policies which can make improvements. (We shall return to these questions later.)

Much of the New Keynesian economics is concerned with problems of imperfect information, of situations where there are important differences among individuals and firms, but these differences cannot be easily observed (directly). Again, assuming all individuals or all firms are identical makes it impossible to analyze the central issues..

But if the use of the representative individual or firm seems too limiting, the alternative, employed in conventional general equilibrium analysis, of simply denoting different firms and individuals by different superscripts and subscripts, and forming aggregate demands and supplies by summing up, seems too general.

Good macroeconomic theory requires the judicious choice of the appropriate level of aggregation, of introducing just enough complexity to be able to explain the phenomena at hand.

III.B. Aggregation of commodities and assets

Keynes, and most subsequent writers, focused on two financial assets, usually "money" and "bonds" (usually long-term bonds or perpetuities). Equities were aggregated, presumably, with long-term bonds and short-term bonds with money. Clearly one wants to aggregate assets which are close substitutes to each other, but long-term bonds tend to increase in value in recessions, while equities tend to decrease in value--they are negatively correlated. Hence these two assets are complements rather than substitutes (one serves like an insurance policy for the other). This aggregation error has important implications for the mechanisms by which monetary policy affects investment.

Indeed, another central theme of New Keynesian economics is that (i) bank loans may differ from government bonds--firms may be credit rationed, so that the market interest rate on bonds may not be relevant for determining the level of investment; and (ii) equity and debt (as well as the form that debt takes) have fundamentally different implications for firm behavior. Equity entails risk sharing, while debt imposes a risk of bankruptcy on the firm.

III.C. Adjustment Speeds

The central role of dynamics in macroeconomic analysis is now generally recognized. Good macroeconomic theory must not only specify clearly what assumptions it is making about relative speeds of adjustment, but it must also provide an explanation of why certain variables seem to

adjust so slowly. In the Great Depression, wages fell. Why did they not fall faster?

Explaining dynamics. Traditional economic theory assumed rational maximizing equilibrium behavior, but imposed ad hoc adjustments. (This is clearly apparent in Samuelson's classic Foundations of Economic Analysis.) If there are adjustment costs, they should be part of the maximization problem.

Recent theories of investment (adjustment of capital) and pricing have done precisely that (See, e.g. Abel [1982] and the menu cost literature to which I referred earlier.) But the heart of the matter is not the existence of adjustment costs, but the relative speeds of adjustment. Formal models, analyzing one decision at a time, cannot address that issue. That is why in recent work Greenwald and I (1989b) have attempted to develop a portfolio theory of firm behavior, in which all of the actions of the firm are considered simultaneously.

Intuitively, as we suggested earlier, it would seem that costs of adjustment for employment and output are much greater than for wages and prices, and hence simple "menu" adjustment cost theories cannot provide a convincing explanation of the relative speeds of adjustment.

Exploring the consequences of relative speeds of adjustments: As we have noted earlier, macroeconomics often proceeds simultaneously on several different planes. Even if we do not have a completely convincing explanation for why wages and prices adjust more slowly in the short run

than output and employment, the fact is they do, and a central objective of macroeconomics is to explore the consequences of this.

The traditional Keynesian model generated what is usually referred to as a short-run equilibrium, that is, given the values of the exogenous variables there was a (usually unique) solution for the current endogenous (usually flow) variables. It was assumed that these were attained in a relatively short time: a change in some exogenous variable would imply that there would be a new set of equilibrium values for the endogenous variables and it was believed that the economy quickly adjusted to this change. At the same time it was recognized that there were economic forces leading the exogenous variables to change.

The most obvious example, perhaps, was the capital stock: in the Keynesian model there was investment but the capital stock remain unchanged. Whether that was a good assumption or not depended on (a) how fast the capital stock changed, and (b) how important (in its effect on the equilibrium values of the endogenous variable) a change in the capital stock was. If the average life of capital was 50 years, then the change in capital stock occurring over one, or even five, years might be quite small; but if the average age is ten years then the change occurring in five years would be quite large. The effect of a change in the capital stock is mainly through the employment-output relationship, and the nature of the complementarity between labor and capital, as well as the extent of embodiment of technical change,¹³ are both important in determining the

¹³Technical change is "embodied" in capital if the introduction of new techniques requires a change in capital (machines); see Solow (1957).

effect of a change in the capital stock on the employment-output relationship.

Thus, any short-run macroeconomic model can be viewed as "cutting into a dynamic process," of saying that some variables adjust more rapidly than others. More particularly, it is assumed that the present value of certain variables adjusts fully to their "equilibrium" value -- equilibrium given the value of certain other variables. Other variables are assumed to adjust, but too slowly to worry about for short-run analysis.

Any meaningful macroeconomic analysis will likely entail some division of economic variables according to the speed of adjustment, but considerable care needs to be exercised in the decision of which variables are to be treated as "fast" adjusting and which as "slow."

Formally we have suggested that any macroeconomic system can be described by a set of dynamic relations:

$$(1) \quad \dot{\underline{x}} = f(\underline{x}, \underline{y})$$

$$(2) \quad \underline{y} = g(\underline{x})$$

where \underline{x} is the vector of variables which adjust slowly, \underline{y} is the vector of variables which adjust instantaneously to the values of the remaining variables. The short-run macroeconomic equilibrium is concerned with equation (2).

It is, of course, not obvious that having two categories is an adequate simplification; one might want at least to consider three categories, in which case one would discuss "short-short run equilibrium," "short run equilibrium" and "long run equilibrium."

For instance, consider the case where (1) takes on the form

$$(1a') \quad \dot{x}_1 = a(x_1 - x_1^*) + b(x_2 - x_2^*)$$

$$(1b') \quad \dot{x}_2 = c(x_2 - \alpha - \beta x_1).$$

If c is very large and a and b are small, we can "approximate" the dynamics by letting

$$\dot{x}_1 \approx a(x_1 - x_1^*) + b(\alpha + \beta x_1 - x_2^*) = (a + b\beta)(x_1 - x_1^*).$$

In Figure 1 we present the phase diagram depicting the dynamics of equation (1a') and (1b') for different values of c . As c increases, the paths approach the $\dot{x}_2 = 0$ line quicker and quicker. In the limit, the dynamics can be described as first going, for the given value of x_1 , to the equilibrium value of x_2 , and then moving along the $\dot{x}_2 = 0$ curve until the stationary point is attained.

The relative speeds of adjustment of different variables is important, not only for understanding the dynamics of the economy, but also for understanding the short-run consequences of any policy change. In many instances, one can show that the short-run and long-run effects on some important economic variable of certain policy measures are just the opposite. Differences in policy prescriptions may, in some cases, be attributed to differences in judgments concerning the dynamics of adjustment.

What is clear, however, is that much of the macroeconomic theory of the past fifty years has made a set of particularly unpersuasive implicit assumptions concerning dynamics. In much of the earlier literature, real balance effects played a crucial role. As I argued earlier, in the time scale which would be required for real balance effects to play any significant role, it is inappropriate to assume the capital stock, expectations, money supply, etc. are constant.

Let me further illustrate the importance of assumptions concerning relative speeds of adjustment with three additional examples. One important class of macroeconomic models takes wages and prices as the "state variables," the exogenous variables to which everything else adjusts. Assume that all markets except the labor market are in equilibrium, i.e., the goods market is in equilibrium in the sense that supply equals effective demand (it is not, however, in the Walrasian equilibrium). Consider, then, the effect of a 10% reduction in wages and prices in an economy which is at an unemployment equilibrium. This will leave real wages unchanged and, hence, employment unchanged. Effective demand at any given interest rate will have increased because of the real balance effect, so that for equilibrium in the goods market to be attained the rate of interest must rise. The implicit dynamic sequence is that the fall in prices and wages leads to increased consumption (in real terms), leading to reduced real savings since, at the given real wage, output remains unchanged. The lower real savings means that investment demand exceeds savings and, hence, the interest rate rises.

This is obviously markedly different from the effects in the short run in the more standard model where prices are assumed to be flexible and there is a real balance effect. Then a fall in wages and prices would initially increase consumption, leading to a rise in prices relative to wages, i.e., a decline in real wages and an increase in employment.

The second example is concerned with expectations. Keynes (and most post-Keynesians) could never make up his mind to what extent expectations ought to be treated as a state variable. For the most part they were treated as exogenous. Expected quasi-rents did not depend (evidently) on

current wages or prices or output. If they did, the IS curve would have had a quite different shape than that usually depicted. See Akerlof and Stiglitz (1966).

Similarly, interest rates were expected to return to their normal level suggesting that long run interest rate expectations were exogenous. On the other hand, the argument for why the price of a long term bond was inversely proportional to the (short term) interest rate usually makes use of the hypothesis that the short term interest rate is expected to prevail in the future. Clearly, subsequent developments, particularly the accelerator model, have made extensive use of an endogenous expectations theory, i.e., that current output is crucial in determining expected output in the future and, hence, desired investment.

The third example is one which was at the center of discussion of "money and growth" dynamics some years ago (but for no good reason, seems of little interest recently). Some money and growth models have taken the price level as an exogenous variable, and some have taken it to be an endogenous variable. In the former it is prices next period (or implicitly prices in the future markets) which adjust to clear the market today. In the latter, these expectations are usually taken as given and current prices adjust. The differences in stability properties of the various models are at least partially due to these differences in specification.

The point of these examples is to emphasize that the choice of what variables are to be taken as state variables (treated as exogenous at any moment) is crucial in determining the behavior of the system and in predicting responses to various kinds of changes.

A good macroeconomic theory must not only specify clearly what assumptions it is making about relative speeds of adjustments, but it must also provide an explanation of why certain variables seem to adjust so slowly. Keynes attempted to provide an institutional explanation for the rigidity of money wages, but this explanation, although clearly part of the story, is of only limited use: it does not explain why, in the Great Depression, wages fell by as much as they did. Nor does it explain the difference in wage rigidities between different periods. Clearly, whatever the institutions are that make wages rigid, they do not make them perfectly rigid. But what puts a limit on the degree of flexibility? Recent "implicit" contract theories have attempted to provide an explanation which is better grounded in economic theory but, as we shall comment below, this theory is also not very persuasive. Elsewhere we provide an explanation based on information-theoretic concepts (Greenwald-Stiglitz [1989b]).

It is important to understand the determinants of the speeds of adjustment, not only to confirm that one has made a plausible story about relative speeds of adjustment but also because it is conceivable, or even likely, that many economic policies affect speeds of adjustment; i.e., some variable which normally adjusts slowly without government intervention, could respond very rapidly to certain types of perturbations, or conversely.

In the view that is taken here the central issue of macroeconomics is not whether there exists an unemployment equilibrium, i.e., a configuration of wages, prices, etc., such that there is no mechanism by which the economy returns eventually to full employment, although elsewhere we have, in fact, shown that to be the case. Rather, the central issue is, are there reasons to believe that adjustment speeds in the response to, say, unemployment are

sufficiently slow that the restoration of full employment is a slow and lengthy process? In this sense, the reconciliation of traditional microeconomic general equilibrium analysis and macroeconomic analysis becomes an easier task; for the traditional general equilibrium analysis either does not concern itself with the question of how equilibrium is to be attained, or if it does, postulates an adjustment process which is no less ad hoc and no more convincing than those underlying traditional Keynesian analysis.

III.D. Equilibrium and Market Clearing

Keynes argued that the economy could be in equilibrium and one of the markets -- that for labor -- not clear (there was an excess supply of labor). Those who believed in the classical model argued that although this might be a characteristic of a short-run equilibrium, it could not be part of a long-run equilibrium.

Analytically, the distinction is seen clearly in our simple model of equations (1) and (2). A short-run equilibrium is characterized by a solution \underline{y} for a given set of state variables \underline{x} . If one of the current variables is the unemployment rate, then the question of existence of a short-run unemployment equilibrium is simply whether for some values of the state variables, the solution to (2) entails a positive unemployment rate.

The long-run equilibrium of the economy is the situation where nothing changes, i.e.,

$$\dot{\underline{x}} = \underline{f}(\underline{x}^*, \underline{g}(\underline{x}^*)) = 0 \quad \text{and} \quad \underline{y}^* = \underline{g}(\underline{x}^*).$$

The existence of unemployment in the long run then is concerned with whether (again interpreting one of the variables y , as unemployment) it is possible that when the state variables are at their long-run equilibrium value, there can be positive unemployment.

If one takes this view, and then postulates that whenever there is excess supply of labor, wages fall, then in the long-run equilibrium by definition unemployment cannot exist. Similarly, if one postulates that wages do not fall even when there is excess supply of labor, it is not hard to see that, even in the long run, there may be unemployment. This analysis seems to suggest that the question of whether there can be unemployment in the long run is almost a definitional matter: it seems to follow almost immediately from the dynamic assumptions one makes. Matters are, fortunately, somewhat more interesting than this would suggest.

First, I would argue that the existence of a long-run equilibrium with unemployment is not necessary for there to be an "interesting" Keynesian problem: does it make much difference whether the response to unemployment is a fall in the wage at an infinitesimally slow rate or if it does not occur at all? Does one get much insight from remarking that, in the former case, the unemployment is only present in the short-run equilibrium, not in the long run?

Secondly, we can construct models in which the economy converges to a "quasi-steady" state -- in which real variables may be constant but monetary variables are always changing. In such an economy, wages and prices could be falling or rising but real wages, employment, etc. could be constant. In such a model, the question of whether there exists unemployment in the long run is an analytical question, and does not follow (at least quite so)

immediately from the assumptions. Elsewhere Robert Solow and I have constructed such a model (Solow-Stiglitz [1968]). In these models unemployment may be a function of real wages, and real wages, if they adjust, adjust slowly.

Thirdly, and most importantly, there is a sense in which the economy is always in the short run. The economy never settles down to the mythical steady state. Analytically, to capture this we need to formulate a stochastic model:

$$(3) \quad \dot{\underline{x}} = f(\underline{x}, \underline{y}) + \underline{z}_1$$

$$(4) \quad \dot{\underline{y}} = g(\underline{x}) + \underline{z}_2$$

where \underline{z} represents a set of stochastic terms (the distributions of which may in fact be a function of the state variables). In this model the economy is never at rest; it is always subjected to changes. There is a steady state (if the disturbances are stationary over time). In the long run we can characterize the distribution of \underline{x} and \underline{y} ; we can ask how these distributions will change with changes in the distribution of the stochastic terms or of the "structure" of the economy (as represented by the functions f and g).

In this model unemployment can occur periodically even in the long run. The question of whether unemployment can persist becomes simply a question-- is unemployment an "absorbing state," i.e., a state which, once one enters it, one cannot leave? Most reasonable hypotheses would clearly make it not so. The question of whether in the long run unemployment is possible is simply the converse; i.e., if one is in a full employment state can one ever leave it? And again, most reasonable hypotheses would make full employment not an absorbing state. The question then becomes: what can one say about the kinds of disturbances which lead the economy from full employment to

unemployment and what kinds of processes (and disturbances) then lead to a return of full employment? How frequently can we expect unemployment, and with what duration?

Equilibrium and disequilibrium: Much of the confusion over the question of whether there can exist a short or long run equilibrium with unemployment arises from confusions about the meaning of equilibrium.

Quite often the term "disequilibrium" is used to suggest that if some feature of the world were changed, but others were not, the given values of certain economic variables would not occur; for instance, the sentence, "The capital stock is not in equilibrium" quite often means, "If there were no costs associated with adjusting the capital stock then the capital stock we would have is not the capital stock we presently have," or "If two years ago we had had perfect information concerning the level of demand today, we would have installed more capital." I am not sure identifying such situations and labelling them as "disequilibrium" adds much insight. (In a rather different context Grossman and Stiglitz (1976, 1980) have considered the behavior of a stock market with costly information. They show that prices in the market will reflect the information of the informed but prices will not be perfectly arbitrated; for if they were, speculators (arbitrageurs) would be unable to procure a return on their expenditures to obtain information; there is "an equilibrium degree of disequilibrium." That is, prices differ systematically from what they would have been had information been free; but information is not free and the economy is in equilibrium in the sense that no one has an incentive to change his behavior.)

Since by their very nature the stochastic elements represent contingencies which could not be foreseen (i.e., which were not perfectly dependent on the state variables), individuals will persistently find themselves in situations to which they have to make adjustments; they are never in "equilibrium." But at the same time, if at each moment they take the best actions they can, subject to the information which they have available, is it not equally meaningful to describe their behavior as always in "equilibrium"?

I have written equations (1) - (4) without time appearing explicitly in the right hand side. In particular, I have characterized the disturbances to the economy by stationary distributions. This allows me to solve for steady state distributions; these steady state distributions describe the long-run equilibrium of the economy. This is the sense in which I have engaged in "equilibrium analysis." I have not, in other words, analyzed the effects of particular disturbances, e.g., the Watergate affair, except as they can be subsumed as particular outcomes of the random processes. This, it seems to me, is more properly the affair of economic history than of economic theory.

Of course, once one admits the possibility that one market is not in equilibrium (in the sense that supply does not equal demand in that market)¹⁴ -- in the short run or the long run-- one must contemplate the

¹⁴I hesitate even to use the vocabulary of "market equilibrium" with "demand equalling supply." In any full account, demand and supply are both dynamic concepts. The observed level of, say, employment may differ from what a simple static demand and supply analysis might suggest; yet each

possibility that more than one market is not in equilibrium.¹⁵ Indeed, we argued earlier that there were convincing reasons to believe that the goods market was not in equilibrium.¹⁶ Not only would assuming that the goods market was in equilibrium imply that real wages should have risen far more in the Great Depression than, in fact, they did (if they rose at all), but

individual or firm, at that moment, is doing what it wishes to do, given the economic environment. It is in a "momentary" equilibrium. This state of affairs is, of course, perfectly consistent with changing behavior over time (both in response to changes in the external environment, and as a consequence of previous actions taken by the individual or firm). (Whether it is appropriate to describe any shortfall in employment as "involuntary unemployment" is a question we take up below.)

¹⁵Standard Walrasian analysis provides another example of the ambiguities in the concept of equilibrium. In standard expositions, we often say that Walras' law implies that if one market is out of equilibrium, at least one other market must also be. If the labor market is out of equilibrium, either the goods market or the bond market is out of equilibrium. But, of course, standard Keynesian effective demand analysis says that the goods market may be in equilibrium, given the quantities of goods individuals and firms actually demand. It seems quite reasonable that participants in the goods market might respond more effectively to demands they actually see, than to the kinds of "conjectural" demands associated with Walrasian analysis, that is the demands they would perceive if, say, the labor market cleared.

¹⁶In the "effective demand" sense.

it also would imply that were one to ask a businessman in the middle of the Great Depression if he would be willing to sell more of whatever he produces at current wages and prices, he would have said "no."¹⁷ Casual evidence suggests the contrary. The analysis of economies in which markets other than the labor market are not in equilibrium is one of the main subjects of my paper with Solow (1968) and the subsequent fixed price literature (Barro-Grossman (1971), Benassy (1975, 1982), Grandmont (1983, 1985), and Malinvaud (1977)).

Just as alternative assumptions concerning relative speeds of adjustment have important implications for the effects of different policies, so do different assumptions concerning which markets are and are not "clearing." For instance, let us return to the first example discussed above, where wages and prices were assumed rigid, and the goods market was assumed to clear.

As a contrast, consider a model where employment, wages and prices are taken as state variables in the short-short run and the goods market is assumed to be out of equilibrium in the sense that the level of effective demand is less than the amount which firms are willing to supply at the given real wage. Then the immediate effect of a lowering of wages and prices is an increase in output since effective demand is increased (as a result of the real balance effect).

¹⁷An alternative explanation to assuming that the goods market was not in equilibrium is to assume that all firms possess some monopoly power. But there is little basis to suggest that market power measures significantly in recessions.

And as in the earlier discussion of the relative speeds of adjustment, it is important to know why markets are not clearing, if it is assumed that they are not. Is it, for instance, simply because wages and prices are rigid? The failure of markets to clear follows, then, as a corollary of the analysis of adjustment speeds. Or can there be a sense of equilibrium in which markets do not clear, i.e., wages and prices do not fall because the market is in equilibrium (even though demand does not equal supply)?

In Greenwald-Stiglitz (1989b) we argue that, at least from the perspective of firms (who, after all, are the primary agents responsible for adjusting prices and quantities), the decisions of how much to adjust each of the variables can be viewed as part of a general dynamic "portfolio" problem. There are risks and returns associated with each adjustment. There are short and long run consequences. And the firm must take into account all of these, as well as the interrelations among them (as reflected, for instance, by the variance covariance matrix.)

III.E. Information and Controlability

The next question, about which I shall have only a limited amount to say, concerns the controlability of the economy. To understand the issues, we need to expand our basic model (as represented by (1) and (2) in the non-stochastic case and (3) and (4) in the stochastic case) by introducing a set of control variables, say c . For the non-stochastic case, we write

$$(5) \quad \dot{\underline{x}} = f(\underline{x}, \underline{y}, \underline{c})$$

$$(6) \quad \underline{y} = g(\underline{x}, \underline{c}),$$

c being the vector of variables under the control of the government (we shall ignore the questions of the free will/predestination variety, and assume that the government can, in fact, choose c at will.) In the non-stochastic environment described by (3) and (4), the question of controlability is a simple one. In general, a change in c will affect various private decisions (various y and \dot{x}). The question is, can the private sector completely offset the actions of the government so that all the variables of fundamental interest, e.g., output and unemployment, remain unchanged? For instance, if commercial paper was a perfect substitute for bank loans for some large corporations, then a policy which restricted the availability of credit through the banking system might well shift large corporations into the commercial paper market, but leave all real aggregates unaffected. In this example, the government lacks controlability. If, however, the government has control of some commodity ("bank loans") for which there exists no close substitutes, then controlling the supply of that commodity can affect the equilibrium in the same way that the behavior of any large producer who restricted the supply of the commodity that he produced could and indeed would affect the equilibrium by the actions he took.

What I want to argue now is that in a stochastic environment, the argument that the government cannot significantly affect the equilibrium of the economy is even weaker. We then formulate the government's behavior as a policy, a rule, which describes the action to be taken in response to the observation of certain variables. The argument has been put that eventually the individuals will discover the fundamental relations describing the economy (including the policy of the government), and having discovered

these relations, will take offsetting actions to negate any governmental policy. That is, in a "rational expectations equilibrium," government policy is ineffective. Several questions may be raised concerning this view.

First, we would argue that the assumptions that the economy is in a rational expectations equilibrium, though analytically useful, is not always empirically convincing. There are numerous instances of individuals basing behavior on beliefs that are either not supported by the facts or, indeed, incongruent with them. There are numerous instances (e.g., corn hog cycle) in which individuals' expectations appear to be far from "rational." Secondly, it takes time to discover certain relationships so that the attainment of a "rational expectations equilibrium" following a change in certain structural variables may take a long time. Indeed, one might argue that one of the objectives of economics research is to discover relationships which have not been previously discovered.

If we are concerned with describing the behavior of the economy, then it may be best to model the economy as if it were not in rational expectations equilibrium. Moreover, once one recognizes that obtaining, storing and analyzing information is costly, it is not clear that the distinction between rational and non-rational expectations is either meaningful or insightful. For instance, a "myopic" equilibrium in which individuals only look at current values of a variable is "rational" if observing other potentially relevant variables is very costly and if storing past data is also very costly.

More generally, for the private sector to be able to offset completely the actions of the government requires that the lags between the

observation of a variable and the implementation of a policy-dependent variable are no greater (for any variable or policy) for the private sector than for the public sector; that the lag between the event and the observation of the event is not greater in the private sector than in the public; and that the cost of observation and implementation of the offsetting actions are precisely zero.

Schematically, we can represent the process of the government action and the private sector's offsetting action below:

Event is	
observed by	<u>action c</u>
public sector	
Event	Net outcome: 0
Event is	
observed by	<u>action-c</u>
private sector	

Clearly, if the private sector's lags exceed those of the government sector, then the government policy can have an effect. Note that what is crucial is the observation plus action lag; thus, it is conceivable that the private sector does not observe the event at all, but only observes the government action, but if it could instantaneously respond with countervailing action -- "-c" -- the government's effects would be only fleeting.

We need to emphasize also the importance of the assumption of costless information and costless implementation of the offsetting action. The argument is the familiar one: if it is costly to monitor the government's action, then those who monitor it must receive compensation for doing so. But if the effects of the government action were perfectly arbitrated out, those who monitor government action would receive no compensation; hence they cannot be completely arbitrated out.

These arguments are all strengthened if the government pursues a stochastic policy. Assume that what the government in effect announces is a stochastic policy that, a particular fraction of the time, when the state variables are "such and such," reserve requirements will be changed. If it requires time to develop the alternative financial instruments, then the change in the reserve requirement may have a substantial impact on the short-run equilibrium. This may be true even if the policy of using reserve requirements is known in advance (but obviously the stochastic disturbance leading to the state in which it is employed is not); for the states in which the alternative financial instruments would be used occur sufficiently infrequently that it may not pay to develop them (or develop them completely) in anticipation of this stochastic event.

At a conceptual level, then, there seems to be no argument but that the government can, in principle, have an effect on the macroeconomic equilibrium of the economy. This, of course, does not answer either the question of whether, in fact, policies in the past have had a significant effect (although the evidence seems fairly clear that they have had a significant effect, although not always in the right direction), or even if

government policies have had limited efficacy in the past, whether there exist policies which are more effective.

III.F. Rational Expectations

During the 1970s and early 1980s, there was much discussion of a rational expectations revolution in macroeconomics. Like any important set of ideas, it had many variants, but among the widely cited tenets were

(a) Government intervention was not necessary, because markets were efficient and cleared; and

(b) In any case, government intervention was ineffective, because private actors perfectly offset the actions of the government. The inefficacy is unimportant, because without government intervention, markets are already efficient.¹⁸

¹⁸While the "rational expectations" approach is closely associated with the "Chicago School," not all "Chicago" economists agree with the second tenet. Friedman (1968), for instance, has argued that monetary policy is responsible for much of the economy's fluctuations. In this sense, Friedman is, in many ways, closer to the Keynesians than to the real business cycle theorists. He believes, for instance, that there are short run rigidities (e.g. wage and price rigidities) such that any action by the monetary authority cannot immediately and costlessly be offset by changes in the price level. He differs from most Keynesians in his judgment about the ability of the government to improve the economy, and, in certain circumstances, on the relative magnitudes of certain key parameters.

At one level, rational expectations model represented the natural extension to a stochastic environment of the perfect foresight models that had been extensively employed (and criticized) in the preceding decade in which growth was a central object of study by economists. The perfect foresight-rational expectations assumptions can be viewed as a convenient benchmark. They enable us to engage in a thought experiment: to what extent do any results derived depend on "irrational" expectations, e.g. myopia, and to what extent do they depend on other assumptions, e.g. concerning how markets function?

The use of the rational expectations model as a description of the economy is quite another matter. One might view it as an empirical hypothesis to be tested--and there has been considerable work over the past decade and a half doing precisely that. In some cases, the data seem consistent with rational expectations; in many others, it does not. Most observers think that the fall in the stock market price by 20% in October 1987 could not possibly be interpreted as a "rational" response to a change in fundamentals occurring within the span of a day or two. But even more fundamentally, the special nature of the economic circumstances that the economy finds itself in, year after year, casts doubt on the usefulness of the rational expectations hypothesis as a description of important aspect of many important decisions, including the investment decisions of firms. Consider the stock market crash of October 1987. There had been only one similar stock market crash in this century. A sample of one is clearly too small to have much confidence for purposes of statistical inference. And the economic environment of 1987 was much different from 1929--the legal and

regulatory environment, for example, had changed markedly. Accordingly, there was no relevant historical sample.

Similar issues arise in other contexts. How will rivals respond to a change in my prices? The international environment today is different from what it was a decade ago. Do Japanese firms respond the same way that American firms?

What are the consequences of the Tax Reform Act of 1986? We had never had a tax change of that magnitude. Not even the experts could agree on the consequences of some parts, e.g. the incidence of the corporate income tax. How, then are we to believe that the behavior of the economy should be described as if the responses of the economy were common knowledge, that is, all individuals knew the responses, and all knew that everyone else knew those responses, et cetera.

The issue is not whether there is some experience which might be relevant. After all, firms must make predictions, and those predictions must be based on some inferences from previous behavior. But there is no simple formula for distilling out of the past what are the relevant experiences. Deciding on what are the salient characteristics of the environment cannot be--or at least so far has not been--reduced to a straightforward statistical procedure. It requires business judgment. From this perspective, the issue may not be so much whether the expectations of businessmen are "rational" but whether they--or their aggregate consequences-- can be simply described by a simple statistical model of expectations formation.

Unfortunately, we almost never test a model of expectations formation in isolation (we could, by looking at survey results on expectations, but

such surveys tend not to lend much support to the rational expectations hypothesis, and accordingly get dismissed) but rather in conjunction with some model of a particular market. When the resulting test of the rational expectations model fails, we thus can only reject the joint hypothesis.

But my concern here is not so much with the hypothesis of rational expectations, and in particular with the testing of that hypothesis, but rather with the "joint" hypotheses characterizing much of the rational expectations literature.

That literature has raised some fundamental questions, which we need to address, at least briefly:

- (a) To what extent do traditional Keynesian models depend on "irrational" expectations?
- (b) To what extent do the conclusions of the rational expectations literature concerning the efficiency of markets and the impotency of government depend on the "expectations" assumptions, rather than on other structural assumptions that they have employed?
- (c) To what extent can those particular joint hypotheses be supported?

Clearly, as we have already commented, Keynes employed a peculiar set of expectational hypotheses. Sometimes he assumed static expectations, sometimes a regression towards "normality." But the New Keynesian economics (see, e.g. papers by Greenwald and Stiglitz (1986) and Neary and Stiglitz (1982)) have shown clearly that

(i) Even with rational expectations, market equilibrium may not be efficient (it is not even constrained Pareto efficient). There are welfare enhancing government interventions, which take account both the costs

associated with establishing and running markets and the imperfections and costs of information.

(ii) With rational expectations, there may be unemployment.

(iii) With rational expectations, small disturbances may give rise to large consequences; and

(iv) In particular, there may be significant multipliers associated with government actions, both monetary and fiscal policy: government actions may not only have real effects, but those effects may be larger than they would have been in the absence of rational expectations.

The intuition behind the latter result is simple: a major limit on the magnitude of the effect an increase in expenditure today are leakages associated with savings. But to the extent that increased savings leads to increased consumption in some future, demand constrained period, it leads to increased output in that period, and that increased output and employment has, if rationally anticipated, effects in earlier periods. It has a spill over back to the current period.

Thus, the results on the efficiency of the market depend on the special representative agent with perfect information assumption--an economic environment in which the absence of markets has few if any consequences. New classical economics obtains results similar to old classical economics not because it has added a new set of insights, derived from rational expectations, but because it has retained an old set of assumptions, concerning perfect markets and market clearing.

When the joint hypotheses underlying much of the analysis are subjected to rigorous testing, they do not fare well. Recent econometric work suggests, for instance, that the permanent income hypothesis does not do well; there appears to be excessive sensitivity to current income, of a kind which is consistent with credit rationing on the part of some individuals. Micro-data is consistent with this perspective. Similarly, investment studies show the importance of financial constraints; for firms accounting for much of the variability of investment, the neoclassical investment model does not fare well.

IV. The Questions of Macroeconomics

Macroeconomics, as I have said, is concerned with interpreting, and predicting, the movements in the aggregates that describe the economy-- output, employment, price levels, et cetera. In this lecture, I am particularly concerned with the short run movements in the real variables which describe the economy. (As I suggested earlier, it may not be possible to separate completely these short run movements from longer run changes; still, it is necessary to cut somehow into any complicated interrelated system.)

There are a number of ways of dividing the issues which any good (complete) macroeconomic theory must address.

A. The Short Run Dynamics of the Economy. One formulation stresses a study of the changes in the economy. The three questions which it stresses are

1. What are the sources of the shocks to the economic system?¹⁹
2. What is the mechanism by which shocks get transmitted from one firm to another, from one sector to another, and perpetuated?
3. Why is it that shocks get amplified, rather than dampened (as they normally would through the price mechanism)? That is, as Figure 2 illustrates, in the conventional demand and supply diagram, a disturbance--say an exogenous increase in demand--leads to an increase in the price, so that the equilibrium increase in output is smaller than the initial disturbance.

Keynes did not have a clearly articulated theory of the sources of disturbance, and much of the later business cycle literature suggested that in fact that were no external sources of disturbance. The internal structure of the economy itself led to fluctuations. The more recent Real Business Cycle literature has emphasized the role of technology shocks; but as standard criticisms of that literature point out, it is hard to see (given the postulated structure of the economy) how the kinds of observed technology shocks translate into the magnitudes of observed cyclical movements, and in particular, it is hard to find (except in a tautological sense) the negative technology shocks associated with the Great Depression and the other major downturns of the economy.

¹⁹It should be clear, however, that I am not precluding here the possibility that downturns arise not from exogenous shocks, but from the internal dynamics of the economy, as emphasized in traditional business cycle theory.

There is an interesting conflict between the two major neoclassical schools in this respect: while the new classical economists and monetarists see monetary policy as the major exogenous disturbance--it is hard to see any other disturbance large enough to account for the economy's fluctuations--real business cycle theorists believe monetary policy is irrelevant (only real variables matter).

The Greenwald-Stiglitz version of New Keynesian economics, on the other hand, takes a more agnostic view on the source of disturbances--there may be real or monetary disturbances. But what it emphasizes--and where it differs from the traditional neoclassical models--is the instability of the economy: how small disturbances can (because of the risk averse behavior of firms) lead to large changes in output and employment.

In this sense, their analysis is very much in the spirit of Kahn and Keynes (1936), who looked for a multiplier process. We now recognize that an essential part of the Kahn-Keynes multiplier process is the rigidity of prices and wages. The Metzler-Samuelson accelerator model (Metzler [1941], Samuelson [1939]) further emphasized the instability of the economy, but employed, in addition to the assumptions associated with the Kahn-Keynes multiplier (Kahn [1931], Keynes [1936]), an investment accelerator, based on a fixed coefficients technology of production and a particular (and unconvincing) assumption of expectation formation. Greenwald-Stiglitz show how similar multiplier-accelerator phenomena can be obtained without assuming the price and wage rigidities of Kahn and Keynes and without making the strong assumption concerning technology and expectation formation that underlay the Samuelson-Metzler-Hicks (Samuelson [1939], Metzler [1941], Hicks [1956]) analysis. Their analysis is solely predicated on the

(observed and explained) imperfections in equity markets, leading to risk averse behavior on the part of firms.

These are questions, however, which I cannot take up further in this lecture.

B. The Three Markets and the Three Quandaries

Another formulation stresses the central problems of interpreting phenomena in each of the three main markets of the economy, the labor, capital, and product markets.

1. The "labor market quandary". We can state the labor market quandary in terms of the conventional demand and supply diagrams for labor. We observe large variations in employment with relatively little variation in real wages.²⁰ It is simply difficult to reconcile this with standard neoclassical markets always operating in equilibrium. Attempts to reconcile

²⁰This is not the occasion to review the vast empirical literature on this question which developed subsequent to the work of Dunlop (1938) and Tarshis (1939) questioning the implication of Keynes' theory that real wages should rise in a recession. Though whether real wages increased or decreased may depend on what price deflator is used, it is clear that, at least in many industries, real product wages (which are relevant for the analysis of the demand for labor) decreased, and the average value of real product wages certainly did not increase to the extent predicted by Keynesian theory. Because of changes in the price of agricultural goods to industrial goods, it is likely that, for many workers, real consumer wages may have increased slightly.

the facts with a market clearing model take on two forms. The first requires that the supply schedule of labor is close to horizontal, combined with shocks to the demand curve for labor (Figure 3). The hypothesis of a horizontal supply curve of labor seems inconsistent with all microeconomic studies of labor markets. Moreover, if individuals are really indifferent to working or not working, or content to work either 20 or 40 hours a week, why then the concern about unemployment? Why do the unemployed workers seem willing to wait hours in queues seeking jobs? Is it really the most enjoyable way to spend one's leisure?²¹

The second has shifts in the demand and supply curves for labor which are just of the right magnitude to offset each other, so that the equilibrium real wage does not change much. The occurrence and persistence of this coincidence seems a weak reed on which to hang a theory, and the

²¹Moreover, some of the recent attempts to come to terms with observed patterns of hours worked and employment seem particularly unconvincing. While it may well be the case that non-convexities either in production or work/leisure technologies (transportation costs) may account for the fact that as labor demand is reduced, workers do not reduce the hours worked per day or, even, per week, reasonable hypotheses concerning individual preferences lead in such situations to job rotation, rather than to prolonged unemployment, an observation which was made earlier in the context of the debate over the consequences of unemployment insurance in the implicit contract literature (see, e.g. Stiglitz [1986]).

implied shifts in the supply curve seem implausible, even when doctored up to include intertemporal substitutability in labor supply.²²

And whence the shocks to the demand curve for labor? In competitive theory, with price and wage taking firms, these shocks in the demand curve presumably represent changes in technology.²³ In the absence of wars, there are simply not the changes in technology which can account for shifts of the magnitude of those observed. Even technological revolutions as major as those associated with the transistor and computer take years, even decades, to have their effects diffuse through the economy.²⁴ We shall return to these issues in section 4.

²² Given the relatively small fluctuations in real interest rates and real wages during the period 1950-1977, it is hard to see any large role for intertemporal substitution during that period. The varying cyclical movements in other periods makes the intertemporal substitution hypothesis even more problematic. The level of substantial micro-evidence corroborates these effects casts further doubt on the approach.

²³ Elsewhere (Greenwald and Stiglitz (1987a)) present an alternative explanation, arising from certain imperfections in capital and futures markets.

²⁴ Taylor has presented other convincing arguments against the real business cycle. After observing the differences in the magnitudes of the fluctuations in Japan, Europe, and the United States, he asks why should the technological shocks hitting the U.S. be so much larger than those affecting Japan, or even Europe. See Taylor (1987).

(2) The Product Market Quandary: The magnitudes of the responses. I now come to the second quandary--how can we explain observed patterns of changes in output? We can divide the analysis of this problem into two parts. First, given that real product wages do not change very much, how can we explain the relatively large variations in output? Is it possible that the supply functions of firms change so much? Is it plausible that firms remain off their supply functions for long?

The second part of the quandary allows for the possibility of changes in wages and price. While general equilibrium theory has taught us that there are relatively few general results on the stability of economic systems, the standard stories we present in price theory suggest that price adjustments dampen shocks. A shock to one sector, say an unexpected fall in demand, gets partly absorbed by a reduction in price, so that the actual reduction in quantity produced is smaller than the shift in the demand curve; price adjustments serve to dampen shocks. General equilibrium effects serve further to dampen the aggregate effect of any particular shock. Corresponding to a reduction in the demand curve facing one industry there must be an increase in the demand facing another. Thus, the aggregate effects of a shift in demand are likely to be small.

Yet we see many instances when a seeming small disturbance to the economy has large macroeconomic effects. The oil price shock provided the most recent example. Viewed as a negative shock to the aggregate production function for an oil importing country such as the United States, the increases in the price of energy in 1973 should have had only a limited effect on the economy, much smaller than the observed effects.

Of course, traditional macro-theory provides an explanation: it focuses on multipliers. But how do we obtain general equilibrium multipliers rather than dampeners?

I will not have time in this lecture to provide the answers to either of these questions. Let me simply assert that the New Keynesian theories, based on incomplete markets and imperfect information, do provide answers to both. In conventional theory, these multipliers arise out of fixed prices; that is, once quantities become the locus of adjustment, shocks to one sector may have multiplier effects on the aggregate. Once one has constructed a theory explaining fixity of prices (wages, interest rates), as the efficiency wage theories and the credit rationing theories we have constructed do, then it is easy to show that there may be these multiplier responses to disturbances.

(3) The Capital Market Quandary: The Role of Money and Credit. The classical dichotomy argued that money should have no real effects. Recent work on perfect capital markets has extended and refined those conclusions: public financial policy should, in a world of perfect markets, be irrelevant (Stiglitz (1988), Barro (1974)). Yet there is considerable evidence that monetary policy has effects. Volker made a difference. The monetary authorities may be unable to bring us out of a recession, but they can spin the economy into one. Why is this? What is the mechanism by which monetary policy works?

Nominal price and wage rigidities provide the easy answer. But how do we explain these rigidities? Money illusion? But this seems to put the burden on irrationality, and with irrationality one can explain almost

anything. Staggered contracts? This explains momentary wage rigidities, of a month's duration; but as contracts come up for negotiation, the "free" contracts should prove freely flexible. Besides, what is "relevant" is price rigidities, and one needs to explain why contracts are not indexed in such a way as to remove most or all of the effects of monetary policy.

Some versions of New Keynesian economists have addressed these questions directly, and have several theories of nominal rigidities. (See Stiglitz [1985], Greenwald and Stiglitz [1989b]).

C. The Stylized Facts.

Beyond these broad issues, a good macroeconomic theory must address, or at least be consistent with, the important aspects of the economy's fluctuations. I cannot provide here a full range of the kinds of phenomena for which I think a good macro-theory needs to provide an interpretation, but let me just mention a few.

One of the reasons that it is critical for a good macroeconomic theory to explain key aspects of the patterns of fluctuations is that much of our concern over these fluctuations arises from the fact that their impact is borne disproportionately by certain sectors and certain groups of individuals. If credit markets were perfect, and if the impact of cyclical fluctuations were uniform, then none of the post-War recessions would have had much of an effect on life-time incomes (utility), and even the Great Depression would have reduced life-time incomes by little more than the cumulative effects of technical progress increases it.

(a) Perhaps the most important issue is the explanation of unemployment, of the failure of wages to adjust to clear the labor market.²⁵ But beyond that, a good macroeconomic theory must explain (i) why there are lay-offs rather than work sharing (including the absence of job rotation); and (ii) why, at least in the United States, unemployment is concentrated in certain groups.

In posing the problem this way, I have taken a stand on what to a few economists may seem a controversial issue: is there any unemployment, and in particular, is there any involuntary unemployment? All economists would agree that there are variations in the level of employment, and that there are variations in the demographics which describe those people in the labor force who, at any given moment, could be gainfully employed but are not. The task of isolating and understanding the reasons for such variations, I believe, will bear much more fruit than debating or redefining such concepts as "unemployment," "voluntary unemployment" and involuntary employment."²⁶

²⁵This parallels the first quandary discussed in the previous subsection.

²⁶At the analytic level, the recent research of labor and other markets with imperfect information, including the efficiency wage theories, have established that (i) market equilibrium may not be (constrained) Pareto efficient--that is, there exist government interventions which, while respecting the limitations on information and other restrictions facing markets, can make all individuals better off (see Greenwald and Stiglitz [1986, 1988a]), and (ii) individuals with no, or only slight, differences in observable characteristics may receive markedly different treatments,

(b) The theory must explain the cyclical patterns of inventory movements (inventories do not seem to provide the buffer role suggested by neo-classical doctrine)²⁷ as well as the cyclical patterns of investment in machines, buildings (commercial, residential) and R & D.

(c) The theory must explain the failure of prices to move over the cycle anywhere near the way that competitive theory (or even monopoly theory with constant elasticity demand curves) would have predicted²⁸. The fact that among the sectors exhibiting the greatest cyclical movements are some with considerable price flexibility (construction), raises doubts about the central role sometimes assigned to fixed-price assumptions; and the fact that unemployment arises in economies experiencing considerable inflation, where the constraints on downward movements in wages and prices would not

including markedly different levels of expected utility. Some individuals may be employed at high paying jobs, while observationally similar, or even identical, individuals may have to choose either between waiting for a good job, remaining unemployed in the interim, or accepting a much lower-paying job. The fact that it may be rational for an individual to remain unemployed rather than accepting a low paying job (see Ma and Weiss [1990] and Greenwald and Stiglitz [1987a])--and in this sense the unemployment is voluntary--does not make the pain of unemployment any the less, and does not mean that there are not government actions which would improve welfare.

²⁷Cf. Blinder (1986).

²⁸Cf. Hall (1988).

seem to be binding, again raises questions about the central role of normal price rigidities.²⁹

(d) The theory must also explain the "time" series properties of the aggregate series, the facts that there are marked fluctuations in output and employment, and that there is serial correlation.

V. The Labor Market Quandary

In this final section of this lecture, I want to illustrate the approach of New Keynesian economics, by examining the labor market quandary, discussed earlier. Recent years have seen the growth of several alternative explanations. We describe and comment briefly on each of these.

(i) The implicit contract theory, which argues that observed real wages have little to do with current economic activity. Elsewhere, I (and others) have provided criticisms both of the simpler versions of that

²⁹This list of the stylized facts which a good theory should explain is not meant to be exhaustive. A good theory should also come to terms with observed patterns of serial correlations and cross correlations of the variables.

There are other stylized facts about which there is less agreement. Views, for instance, concerning the direction and significant of movements in real interest rates depend on whether one calculates it by taking, say, nominal short term interest rates and subtracting reported expected rates of inflation, or subtracting the actual rate of inflation.

theory, as well as the more refined attempts to extract from that theory a theory of unemployment.³⁰

The most telling criticisms of the theory are that (a) while it may provide an explanation of wage rigidity, it does not provide an explanation of unemployment; and (b) in particular, it does not provide an explanation of either the form or pattern of unemployment.³¹

We note further that that theory suggests that one looks to new hires and spot labor markets to observe what is happening to "shadow" wages; and I suspect if one does that, the first quandary becomes more striking: real product wages in these markets undoubtedly fall more than observed average real wages.

(ii) Efficiency wage and related theories. These theories argue that equilibrium may be consistent with high and varying levels of unemployment.

Elsewhere (for a survey, see Stiglitz (1986, 1987c), Yellen (1984)) I have argued that the efficiency wage theories provide at least part of the explanation for non-market clearing.³² These theories hold that the productivity of the firm's workers depends (in part) on the wages paid.

³⁰See, for instance, Azariadis and Stiglitz (1983), Newbery and Stiglitz (1987) or Stiglitz (1986).

³¹An exception is the version of implicit contract theory combining it with search and efficiency wages (Arnott, Hosios, and Stiglitz (1988)).

³²Insider-outsider theory (Lindbeck-Snower [1988]) provides an alternative set of conditions under which equilibrium will be characterized by unemployment.

Reducing wages below some critical level actually increases labor costs. Hence, if at the efficiency wage, the wage at which labor costs are minimized, there is an excess supply of workers, firms will still not lower their wages, as conventional supply and demand analysis would have suggested; for to do so would actually lower firm's profits. Several alternative explanations for why productivity might depend on wages have been explored--wages affect the mix of applicants (the adverse selection effect); the incentive of workers to work hard; and labor turnover. Efficiency may also be affected by morale concerns or by nutritional considerations. The different theories have somewhat different implications; Still, the major consequence is the same for all the variants: the possible existence of equilibrium unemployment.³³

(iii) Disequilibrium theories, in which wages do fall in the presence of unemployment, but in which unemployment can persist nonetheless because prices fall simultaneously. Thus, it is the decentralized adjustment mechanism which is to blame for the persistence of unemployment. Some twenty years ago, Solow and I (Solow and Stiglitz, 1967) explored this possibility. An important difference between our work and much of the subsequent fixed-price literature (besides the explicit formulation of the dynamics of adjustment) arose from our conviction that real balance effects simply were quantitatively of insufficient importance to play a first order

³³ Elsewhere, I have also argued that these theories provide a much more convincing explanation of the other labor market phenomena to be explained, say, than the implicit contract theories (Stiglitz, 1986, 1987), though models combining elements of both may eventually prove fruitful (Arnott, Hosios, Stiglitz, 1988).

effect in the analysis of short run equilibrium. That is why we postulated that proportional reductions in wages and prices would leave the aggregate demand curve (relatively) unaffected.³⁴

It is, in fact, quite likely that a theory combining efficiency wage considerations and disequilibrium dynamics can account for the observed patterns in the labor market. Efficiency wage theory argues that wages are chosen to maximize (expected utility of) profits. In the absence of uncertainty, accordingly, a small change in the economic environment would normally necessitate a small change in wages; and the loss of expected profits from failing to respond would be small. (Akerlof and Yellen (1985)). The firm knows (or has views about) the consequences of it maintaining its current wage policy (say wage rate); there is greater (subjective) uncertainty about the consequences of any change in wage policy; it must judge not only how its workers, and potential workers, might respond, but it also must judge how its competitors (both in the labor and product market) will respond. As a result, the optimal policy, the one which maximizes expected utility of profits (given the risk aversion of firms, or the firm's managers) may entail maintaining the firm's wage policy. This explains why firms may adjust wages slowly, or not at all, to changes in economic environment; the interdependence among wages, to which

³⁴Actually, it seemed equally plausible that a detailed examination of the short run effect of price adjustments would show that these exacerbated the problem rather than alleviating it; that is, if individuals or firms extrapolated a fall in price, they might be lead to reduce their consumption and investment. Neary and I (1983) and Grandmont (1983) showed that this was in fact the case.

efficiency wage theory gives rise, argues further that given that other firms have not changed their wages, or have changed their wages only very little, the wage that maximizes expected profits will not change, or will not change very much. Thus, while the earlier Solow-Stiglitz model postulated that wage adjustments might be slow, efficiency wage theory, combined with firm risk aversion, provides an explanation for why those adjustments are likely in fact to be slow (or non-existent.)³⁵ (Elsewhere, we have provided an explanation for why firms should, in fact, behave in a risk averse manner.³⁶)

³⁵Staggered contract theory can be thought of as a particular formulation of, or explanation of, slow wage adjustments. But note that the fact that contracts are long lived does not by itself provide much of an explanation for unemployment, unless accompanied by, say, efficiency wage considerations. For as contracts come up for negotiation, wages on the flexible margin should adjust, to absorb all workers willing to work.

³⁶The ideas summarized in this last paragraph have been developed more extensively and formally by Greenwald and Stiglitz (1989b). I have deliberately been somewhat vague about how a wage policy is defined, e.g. whether it represents a constant nominal wage, a constant real wage, or a constant relative wage. In fact, in some versions of efficiency wage theory (Stiglitz, 1985) there exist Nash equilibria in which nominal wages are rigid--all firms believing other firms are keeping their nominal wages fixed, find it optimal to keep their wages fixed--and other equilibria in which real wages are fixed. In Greenwald and Stiglitz (1989b) we present a general theory of adjustment, in which what is entailed by "maintaining the

(v) Imperfect Competition (unions).

The presence of imperfect competition in the labor market provides an explanation of why workers do not sell all the labor that they would like to at the going wage. But in the U.S., less than a fifth of the labor force in the private sector are in unions. At most, unions can explain why some workers get paid more than other workers--alone they cannot explain unemployment. (Indeed, such theories cannot explain the variability of employment at a fixed real consumption wage unless accompanied either simultaneously by shocks to preferences, or unless accompanied by one of the other considerations which we have raised here.)

(v) Insider outsider theory. Much of the specific knowledge of a firm lies in its current workers. New workers have to be trained, and the training is largely done by old workers. Thus, in most firms, new workers cannot simply replace old workers, particularly without the "consent" of the old workers; and the older workers are unlikely to give that consent. The fact that insiders and outsiders are not perfect substitutes gives insiders considerable bargaining power within the firm; alternatively, we can view the problem of management as paying a high enough wage to insiders--and having a form of commitment that they will continue to pay high wage-- to

current policy" is explicitly defined within the context of the firm's problem of maximizing its expected utility of profits. We argue there that the uncertainties associated with changing wage policies are such as to make wage rigidities (as opposed to rigidities in other decision variables) particularly likely.

motivate them to train the new hires well. Thus insider-outsider theory can be viewed as combining elements of imperfect competition in the labor market with efficiency wage concerns. Note, too, what is relevant for a hiring decision (as in the next theory to be described) is the lifetime wage of the worker; his willingness to accept to a low wage today does not necessarily mean that, once he becomes an insider, he will not demand a higher wage. Thus, workers find it difficult to commit themselves to a low lifetime wage; in effect, workers cannot lower their wages.

(vi) Employment as an investment decision. To the extent that there are fixed costs associated with hiring, training, and firing workers, the decision to hire workers is an investment decision. Workers and firms can be thought of as having to make a decision on when to make this critical decision. In making this timing decision, firms must compare the stream of returns they obtain as a result of making the decision at different times, evaluating the cost of capital at the appropriate shadow price for the period in which the investment is made. If, as we have argued elsewhere, there is capital rationing, the life-time wage which workers would have to be willing to accept to make firms willing to make the investment during recessionary periods may be much lower than that if the investment decision is postponed to better times.^{37 38}

³⁷This argument is developed in greater detail in Greenwald and Stiglitz (1987a). Note that if firms can discriminate between old and new workers, the workers who choose to remain unemployed because of the low wage offer are, in a sense, voluntarily unemployed. On the other hand, if firms

VI. Summary

New Keynesian economics can be viewed as a dynamic research programme. It seeks to understand the causes of imperfections in the labor, capital and product markets, their macroeconomic consequences, and on the basis of this, to derive policy prescriptions. Various variants of the school emphasize various imperfections. But the differences among the various strands are less important than the similarity of their underlying view of the economy. In most cases, the explanations are not mutually exclusive. The debate is over the relative importance of various explanations.

The conformity of a theory to the basic qualitative facts of the economy seems to be the first hurdle to which any theory should be subjected. If it fails to meet that test, there is little to be gained from the sophisticated testing of one or two of its implications, for in the end,

cannot discriminate between old and new workers (a hypothesis for which insider-outsider theory provides some justification, as does standard efficiency wage theory, focusing on morale considerations) then the firm will not be willing to hire workers at the going wage, and workers are, in this sense, involuntarily unemployed.

³⁶One might ask, couldn't workers absorb some of the risk of firms, by making their pay contingent on the performance of the firm. This makes workers in effect equity holders, and the standard Greenwald-Stiglitz arguments for why equity markets are imperfect apply.

as I have emphasized, a theory must be judged by the consistency of all of its implications with the facts. The New Keynesian theories pass, or perhaps I should say more modestly, show the promises of passing, that hurdle.

To conclude: the theory is still at a rudimentary stage. There are more qualitative implications to be drawn from the theory, to be judged against the empirical evidence. There are refinements to the theory: among these are developing a more formal dynamics, and integrating the important lessons to be drawn from other lines of research in macroeconomics of the past quarter century.³⁹ At the same time, we are hopeful that quantitative testing both of the various pieces of the model, and of the model as a whole, will not only show that it provides a superior model to alternatives, but will provide further insights leading to further refinements of the theory. It is an exciting research programme, one which I hope I have persuaded some of you to join.

³⁹I have referred to some of these, such as implicit contract and insider-outsider theory, earlier in this talk. My contention at that point was that these theories, by themselves, provided an inadequate explanation of the central questions at issue. The insights provided by these theories, combined with the theory we have presented here, will, I think, provide a more complete description of the central macro-economic phenomenon.

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