

# *CES Working Paper Series*

HOW MUCH DO MICRO-  
FOUNDATIONS MATTER?

Thomas Mayer

Working Paper No. 32

*Center for Economic Studies  
University of Munich  
Schackstr. 4  
8000 Munich 22  
Germany  
Telephone: 089-2180-2747  
Telefax: 089-397303*

*CES Working Paper No. 32  
April 1993*

## HOW MUCH DO MICRO-FOUNDATIONS MATTER?

### Abstract

New classical economists criticized Keynesians for not providing adequate micro-foundations. Instead of responding by developing such micro-foundations, new Keynesian theory, Keynesians could have responded that complete micro-foundations, while desirable, are not necessary. This paper presents such a response. It does so by distinguishing between formalist economics that does require fully coherent micro-foundations, and empirical science economics that does not. In the latter context Keynes' explanation of wage stickiness should not be dismissed as "sociological".

JEL classification: B40, E10

*Thomas Mayer  
University of California at Davis  
Department of Economics  
Davis, California 95616-8578  
Fax: 916-752-9382*

## HOW MUCH DO MICRO-FOUNDATIONS MATTER?

Thomas Mayer

When new classical economists announced with great vehemence that Keynesian theory is invalid because its supply function is inconsistent with any acceptable micro-foundations (a charge that could also be levied at monetarism), Keynesians responded by developing their own microeconomics, "new Keynesian theory". That Keynesians chose to fight the battle on substantive rather than on methodological grounds may be due to the low tolerance that economists have for methodological argument. Perhaps it was fortunate because the search for Keynesian micro-foundations generated important new insights. But many of these insights are problematic (see Romer, 1993), and they were acquired at the cost of much effort that might perhaps have been more productively employed elsewhere.

All the same, it is surprising that Keynesians so readily conceded the methodological issue with virtually no argument.<sup>1</sup> New classical economists acted as though the case for reductionism is self-evident, without referring to the ongoing debate about reductionism in philosophy and in many sciences, and the doubts that can be raised about reductionism in economics specifically (Woo, 1990). It is not even clear that either side was aware that reductionism is not unproblematic. Yet, given the popularity of Friedman's (1953) essay on methodology with its strong instrumentalist bent, Keynesians might well have questioned the new classical's insistence on reductionism.<sup>2</sup> Indeed one might argue that the insistence on reducing macroeconomics to microeconomics is inappropriate because microeconomics itself lacks foundations. Two basic postulates of microeconomics, that agents are fully rational and entirely self-interested, surely cannot be derived from the more basic science of psychology, but are instrumentalist (Cf. Woo, 1990.) With microeconomics thus not being reducible to the more basic science of psychology, why should macroeconomics be reducible to microeconomics? To respond that reductionism should rule within each particular science,

but not necessarily between sciences, is hardly convincing because the boundaries of various sciences are essentially arbitrary.

This paper therefore discusses whether Keynesians could legitimately have responded to new classical economists by arguing that they do not need additional micro-foundations. I will deal first with the aggregate supply function and then with the related Lucas critique.

### **Formalist Economics and Empirical Science Economics**

To decide whether macroeconomics needs micro-foundations one first has to ask what macroeconomics is trying to accomplish. One possible answer, the answer of those whom I will call "formalists", is that economic theory is a formal science that models itself on mathematics and logic. In its pure form it is an analytic discipline that deduces the implications of three postulates, rational profit maximization, decreasing marginal utility and decreasing returns (Hausman, 1992). While these postulates are intended to reflect reality as determined by introspection or casual empiricism, they are chosen, so to speak, in the ante-chamber of economics, and economics proper then deduces their implications. These implications may be "tested" by confronting them with empirical data, but such tests are not all that critical. Non-Euclidian geometry is valid even in a Euclidian world. Instead of goodness of fit to generally noisy data formalists stress the mathematical criteria of rigor, generality, parsimony and elegance. They consider a theory that does well on these four criteria preferable to one that gives a better fit to the data, but is less closely related to the basic postulates of economics. In particular, ad hocery is to be avoided, not for the reasons that concerned Popper, but more for its sin against parsimony. Problems for research are chosen largely for internal reasons, such as the technical "sweetness" of the feasible solutions, rather than for their policy relevance or for their salience in explaining observed behavior. A good example of this approach is the the new classical response to the argument that long-

term wage contracts inhibit wage and price flexibility. They argue that if long-term contracts would interfere with the appropriate adjustment of wages these contracts would be indexed. They are not concerned with the fact that very few contracts are anywhere near fully indexed, and with the fact that it is hard to see how unindexed or only partially indexed contracts would not interfere with wage flexibility.<sup>3</sup> The world of formalist economics need not correspond to the observable world.

The alternative answer to the question of what economics is about takes as its model, not mathematics and logic, but the empirical sciences, fields that may employ heavy analytic machinery, but which evaluate hypotheses primarily by their fit to observations. They, too, favor statements that are rigorous, general, elegant and parsimonious, but these criteria do not dominate. I will call this approach "empirical science economics". For it the postulate of rational utility maximization is a highly useful working hypothesis, but not an axiom. If necessary it can be relaxed.<sup>4</sup> It treats economic theory not as an imposing structures hewn from the white marble of indubitable truth, but as a convenient tool. Unlike the formalists, empirical science economists are willing to fudge the logic if that is necessary to get the prediction to agree with the data.<sup>5</sup>

The distinction between formalist and empirical science economics is not the distinction between theory and empirical work. Empirical science economics also contains a heavy element of theory. The difference is in the main goals set for the theory; on the one hand to show the implications of certain axioms, and on the one to explain a set of observations. To be sure, formalists also want their theories to fit the data, and empirical science economists do want their theories to be rigorous, general, parsimonious and elegant, but they differ in the relative importance they accord to these goals.

Although for ease of exposition I have drawn a sharp distinction between the two types of economics, there is a continuum rather than a clearly defined dichotomy. Some economists might give equal weight to empirical verification and to formalist criteria. But even though

such a middle ground does exist, a considerable part of the disagreement among economists can be understood as a debate between those using formalist criteria and those using empirical science criteria (Mayer, 1993b, Chs. 7 and 8). In large part that is so also for the debate about the need for micro-foundations.

### **Formalist Economics and Micro-foundations**

The formalist research program is to deduce as much as possible from the above-mentioned basic postulates. It is not clear what role this leaves for macroeconomics. To the extent that this program is successful macroeconomics can be eliminated, since all of its valid propositions can be deduced from microeconomics. If macroeconomics is kept at all it serves merely as a kind of shorthand, a quick way of expressing certain results deduced by microeconomics. For that role it clearly needs micro-foundations. Any macroeconomic statement not translatable into microeconomics, has no more validity than an alleged theorem that lacks a proof. No wonder that formalists may find it hard to see how anyone can deny the need for micro-foundations.

Can one respond that, as discussed above, microeconomics, just as much as traditional Keynesian theory, itself lacks foundations? No, not within the framework of formalist economics. That is an intellectual discipline within its own right, and can start from any axiom set it pleases.

Looking at the demand for micro-foundations as a manifestation of formalism helps to explain a peculiarity of this demand. Not just any grounding in some theory or other of the household and the firm will do. Only a grounding in the theory of competitive markets unhindered by any difficulties in trading is acceptable. As Willem Buiter (1989, pp. 10-11) has remarked:

It is ironic that at the very time that macroeconomic theory was abuzz with exciting new developments (the asymmetric information paradigm, principal

agent theories, monopolistic competition, oligopoly and game theoretic approaches to rivalry between firms, etc.) the ...[new classical] should have opted for a recycling of the conventional pre-Seventies competitive paradigm. ... Macroeconomic modelling should start from the self-evident and crucial facts of (1) incompleteness of markets, (2) noncompetitive behavior in most of the markets that do exist and (3) essential heterogeneity among economic agents.

The reason why so many formalists insist on basing macroeconomics on a simplistic version of microeconomics is surely not a desire for a theory that is easy to learn. Instead, it is due in part to the need for tractability, and in part it represents an insistence that all mutually profitable trades are carried out, that there are no \$5 bills lying on the pavement.<sup>6</sup> That sort of microeconomics has the simplest and most direct links to the cherished postulate of rational utility maximizing behavior. But the proposition that all feasible trades are carried out is problematic. Is it possible for firms to collude? If so, then competitive theory must be jettisoned. But if collusion is not considered feasible, what else is not feasible? Unless this question is answered, the proposition that all feasible trades are carried out is vacuous and consistent with wage and price stickiness. Thus, while it is understandable that formalists wish to reduce macroeconomics to a "clean" microeconomics in which all mutually profitable trades take place, macroeconomists do not have to satisfy that wish, even within the context of formalist economics.

### **The Need for Micro-foundations in Empirical Science Economics**

For empirical science economics it is not absolutely necessary that macroeconomics have a solid microeconomic foundation. If it explains and predicts well, it may be useful even without solid foundations in micro theory, just as empirical-science microeconomics is useful, despite its lack of foundations in psychology. All the same, microeconomic foundations are desirable. Even empirical science economics assigns some weight to elegance and the other formalist criteria. An additional and much more important reason is that by

developing micro-foundations one guards against error. The theory of the consumption function provides a persuasive example. Keynes (1936, pp. 96) did not provide an adequate microeconomic foundation for his consumption function, though the required micro theory was available (Fisher, 1930). He relied on nothing more than the following:

The fundamental psychological law upon which we are entitled to depend with great confidence both a priori from our knowledge of human nature and from the detailed facts of experience, is that men are disposed, as a rule and on the average, to increase their consumption as their income increases, but not by as much as the increase in their income.

Had Keynes used instead of this casual empiricism Irving Fisher's (1930) microeconomic theory of the consumption function, forecasts of postwar unemployment in the United States would not have been as far off the mark as they actually were. It was not until the 1950s that Modigliani and Brumberg (1952) and Friedman (1957) published a widely accepted consumption function with solid micro foundation, in Friedman's case by explicitly going back to Fisher. Similarly, had economists shown more concern about its loose microeconomic basis, the original Phillips curve, with its flawed assumption that money wages and not real wages matter, would not have been allowed to persist until 1968.<sup>7</sup>

In particular, it is useful to see if a macroeconomic proposition has either (1) a sound micro-foundation, (2) cannot be related to any microeconomic proposition, or (3) is inconsistent with received microtheory. In the second case (unrelatedness) we might accept the macroeconomic proposition simply as an observed regularity. This entails some risk because previously observed regularities that have no theoretical basis may suddenly break down, particularly when they are used as policy guides.<sup>8</sup> The behavior of M-1 velocity in the 1980s is a good example. All the same, observed but unexplained regularities do not lack scientific status. The constant of gravitation provides one example.

How about the third case where an observed macroeconomic relation does contradict microeconomic theory? For example, a macroeconomic model might require the assumption of rapid wage and price flexibility, despite microeconomic evidence that wages and prices



are sticky. The principle of methodological individualism may seem to imply that such a model must be abandoned. That is the appropriate response in many cases, but not in all. First, aggregates are not always just summations of their individual components.<sup>9</sup> The supply curve of an industry is not derived by summing the supply curves of individual firms, and the same is true of demand curves. The paradox of thrift tells us that saving by an individual agent does not necessarily increase national saving. Assumptions, such as independence of tastes, which are useful at one level of analysis may be inappropriate at another level.

There is nothing inherently "unscientific" about using conflicting theories at different levels of analysis. Physicists use different theories when dealing with subatomic particles and with larger objects. The extent to which it is appropriate to use contradictory theories at different levels is related to the debate about realism vs. instrumentalism. If theories are nothing but "inference tickets", then it is perfectly legitimate to have contradictory theories. And even a realist might allow the use of different theories at different levels as a temporary subterfuge. All the same, a contradiction between macroeconomics and microeconomics sends a danger signal, and should at the least make the macroeconomist careful.

#### **Keynesian Wage Stickiness Revisited**

What does this suggest about the old Keynesian position that wages are sticky because workers are concerned with relative wages, and there is no mechanism that allows for a coordinated economy-wide reduction in nominal wages? As formalist economics it is unsatisfactory because it adds an ad hoc assumption about relative wages and does not allow some mutually beneficial trades to be carried out. But Keynesians are empirical science economists, not formalists. And as empirical science economics the Keynesian argument may well be sound (see Tobin, 1993, p. 56). Coordination failure in nominal wage reductions seems highly plausible, so that the burden of the proof is on anyone who seeks to deny it.

Whether there is an emulation effect for nominal wages strong enough to bear the weight that Keynesian theory puts on it is an empirical issue that cannot be resolved by methodological proclamations.

Robert Frank (1985) has shown that an emulation effect can account for much otherwise puzzling economic behavior. In particular, it plays a central role in explaining an important feature of the consumption function. Cross-section data show that the marginal propensity to consume is less than the average propensity. This implies that as income rises over time the saving/income ratio should also rise. But time-series data show that it does not. The permanent income theory and life cycle hypothesis tried to explain this contradiction as due to a confounding of permanent and transitory income, but numerous tests showed that this distinction can account for only a part of the contradiction between the time-series and the cross-section data (see Mayer 1972). However, Duesenberry's (1949) relative income theory, which relies on an emulation effect, can resolve the contradiction. With the emulation effect playing such a large role in other parts of economics, there is no a priori reason why it should not account for nominal wage stickiness.<sup>10</sup> Whether, for this reason or another, nominal wages are actually sticky is, of course, an empirical issue.<sup>11</sup>

Is putting an agent's relative wage into the utility function ad hoc, and therefore to be avoided? Not in the Popperian sense of the term since, as just discussed, there is much empirical evidence for an emulation effect. It is ad hoc only in the Lakatosian sense of ad hoc, which treats as ad hoc something that does not fit readily into the heuristic of the theory. (See Hands, 1988) While conviction on a charge of Popperian ad hocness would disqualify a theory, a guilty plea on a charge of Lakatosian ad hocness does not. Economic theory is not sufficiently well developed and well confirmed for that.

The charge that the Keynesian theory of wage stickiness is "sociological" is also not convincing. Having one's relative wage, as well as one's real wage, in one's utility function is neither irrational nor somehow unnatural (see Frank, 1985). It can just as readily be

considered economic as sociological. And even if it were sociological, that would not disqualify it. That sociology is not as well developed as economics, does not mean that every sociological hypotheses is necessarily invalid or only weakly established.<sup>12</sup> Keynesians could therefore have stayed with the Keynes' own position, and need not have developed new Keynesian theory.

### **The Lucas Critique**

The Lucas critique is a more worrisome challenge to macroeconomics than is the new classical's critique of the Keynesian supply function. Within the context of formalist economics it is devastating: Using traditional macroeconomics to predict the effects of policies involves a logical contradiction. But for empirical science economics there is a possible "out". As traditional macroeconomists usually point out, most policy changes might have only second-order effects on behavioral parameters. The empirical evidence here is mixed. Though skimpy, it suggests that the Lucas critique is less relevant to labor markets than to financial markets.<sup>13</sup>

So what should economists do when asked for macroeconomic policy advice? One possibility is to say: "I don't know." <sup>14</sup> Instead of giving policy advice economists could retreat to their studies, and not emerge until they obtain reliable deep parameters. That might be a good solution if these deep parameters could be estimated, say within five or even ten years. But it seems unlikely. Econometric studies suffer not only from noisy data, but often also lack robustness with respect to minor changes in the specification of the model or in the sample period. Moreover, giving policy advice frequently requires estimating not just one relationship, but several, and errors in any one of the estimates may be fatal. In addition, there is the problem of standard errors. The true standard error of a conclusion based on the estimation of several parameters is some combination of the standard errors of all of these

parameters, and that might produce a range that is too broad for useful policy advice.

A more practical alternative is to invest only some of our resources in digging for deep parameters, while most economists continue to give policy advice. They should take account of the Lucas critique, both by warning policy-makers about the fallibility of their advice, and also by making some rough, common-sense allowance for policy-induced changes in parameters. Consider for instance a paradigmatic case for the Lucas critique, estimating the effect of a temporary tax cut on consumption in an economy which has never experienced one. One might make a rough estimates of capital rationing and of irrational behavior, as well as of the proportion of the population that will not believe the government, and will think that the tax cut is permanent. The result obtained would be only a rough estimate. But it would not be all that much better if we somehow had estimates of deep parameters of the utility and production functions. We would still have to guess the extent to which the public believes the government, and that means that our answer to the policy-maker's question would be only a crude approximation. Adding a number that lies, say somewhere between 2 and 3, to another number estimated to five places of decimals does not yield an answer that is reliable to five places. But when giving policy advice, precision, though desirable, is often far from necessary. The useful things we have to say are generally broad and vague. Hence as empirical science economics macroeconomics can provide useful information even without knowing deep parameters.

### **Conclusion**

If treated as a contribution to formalist economics Keynesian economics is indeed invalid because of its ad hoc assumption of wage and price stickiness. But Keynesian economics is not intended to be formalist economics. It is empirical science economics. For that what matters is whether the empirical evidence supports the Keynesian wage stickiness

hypothesis, and not whether it can be deduced from the narrow set of postulates used in formalist economics, a set that does not allow relative wages to be in the utility function. Similarly, the Lucas critique, while devastating in the context of formalist economics is, in the context of empirical science economics, just one of the many sources of error in our estimates. (See Sims, 1982).

Keynesians could therefore have replied to the new classicals' charge of ad hocness and failure to conform to the heuristics of economics with a methodological defence. They need not have taken the trouble of developing new Keynesian theory. It would therefore be interesting to analyze the rhetoric of this debate. To what extent did the extreme self-confidence and the strong language of the new classicals discomfort the Keynesians so much that they made an unnecessary concession? To what extent was their decision to reply on the substantive rather than the methodological level due to the poor reputation of methodological arguments in economics?

And to what extent was it due to the inherent advantages of doing more work on the micro-foundation of the aggregate supply curve, regardless of new classical criticisms.

## ENDNOTES

1. But Alan Blinder (198) and Robert Solow (1986) did point out that other sciences get along without the reductionism that the new classicals insist on. Moreover, in countering the Lucas critique traditional macroeconomists have taken the methodological position employed in this paper, but they have done so without setting it into its proper context.
2. Instrumentalism is a philosophy of science that treats theories as merely convenient tools for analysis, as ways of making inferences, that need not be true in and of themselves.
3. This cannot be justified by an appeal to Friedman's position that the realism of assumptions is irrelevant because indexing can be treated as an assumption of the theory. (See Mayer, 1993a).
4. This is so not just in papers that appear in institutionalist journals, but can occur even in the American Economic Review (see Asubel, 1991) .
5. For a further discussion of the difference between formalist and empirical science economics see Mayer (1993b).
6. An additional (or alternative ?) explanation is that the assumption that all profitable trades are carried out is useful in establishing the policy invariance proposition. But that proposition is not central to new classical theory (see Mayer, 1993b, pp. 80-81).
7. Phillips himself showed some awareness of the money-wage, real-wage problem, but the subsequent literature ignored it. (I am indebted for this observation to Nancy Wulwick.)
8. What is known as Goodhart's law (after Charles Goodhart) states that as soon as any observed regularity is used as a basis for policy, it ceases to hold.
9. This is consistent with the set of lower-level components being "all there is". Theories are tools that approximate reality, not photographs of reality. Hence, the lower-level theories may omit certain items that are highly relevant at a higher level. Reductionism, even if possible is not always appropriate. (See Alan Garfinkel, 1981, Ch. 2).
10. For a discussion of the empirical evidence on concern about relative wages as an explanation of nominal wage stickiness see James Haley (1990).
11. For data showing that nominal wages are not as inflexible as Keynesians seem to assume see Kenneth McLaughlin (1990).
12. In his recent appraisal of economics, Hausman (1992, p. 274) pointed out the strong commitment that economists have to keep economics independent from other social sciences. He concluded that: "it stands in the way of progress."  
A more serious problem than the introduction of sociological factors is to explain why unemployed workers do not bid down wages and replace those workers who are unwilling to take nominal wage cuts. James Haley (1990, pp. 143-44) suggests that workers implicitly agree not underbid each other, or that firms may be concerned about their reputation in the labor market if they use the existence of unemployed workers to force down wages. Insider-outsider theory suggests that workers may be unwilling to help train new, lower-paid recruits. Still another reason may be that the relative wage that concerns workers is not just their wage relative to other workers, but also their current wage relative to their previous

wages. These explanations may, or may not sound very plausible, but facts do not disappear merely because we cannot explain them. The fact that unemployed workers do not bid down wages is a problem for all macro and labor market theories. Real business cycle theory does not provide a plausible solution, because it is hard to believe that the long-term unemployed are just enjoying leisure. Lucas's paradigmatic case of teachers who take long summer vacations and work hard during the school year does not sound applicable to those who have been unemployed for more than a year. It is better to admit that we do not know why the unemployed do not bid down wages faster than they actually do.

13. For a survey of the evidence see Mayer (1993b, pp.97-102).

14. When Lucas was asked what he would do if he were chairman of the Council of Economic Advisers he replied: "I would resign." Klammer (1984, p. 54).

## References

- Asubel, Lawrence (1991) "The Failure of Competition in the Credit Card Market," American Economic Review, vol. 8, March, pp. 50-81
- Buiter, Willem (1989) Macroeconomic Theory and Stabilization Policy, Manchester, Manchester University Press.
- Duesenberry, James (1949) Income, Saving and the Theory of Consumer Behavior, Cambridge, Mass., Harvard University Press.
- Fisher, Irving (1930) The Theory of Interest
- Frank, Robert (1985) Choosing the Right Pond, Oxford, Oxford University Press.
- Friedman, Milton (1953) The Methodology of Positive Economics, Chicago, University of Chicago Press.
- Friedman, Milton (1957) A Theory of the Consumption Function, Chicago, University of Chicago Press.
- Garfinkel, Alan (1981) Forms of Explanation, New Haven, Yale University Press.
- Haley, James (1990) "Theoretical Foundations for Sticky Wages," Journal of Economic Surveys, vol 4, number 2, pp. 116-55.
- Hands, D. Wade (1988) "Ad Hocness in Economics and the Popperian Tradition," in Neil de Marchi (ed.) The Popperian Legacy in Economics, Cambridge, Cambridge University Press.
- Hausman, Daniel (1992) The Inexact and Separate Science of Economics, New York, Cambridge University Press.
- Keynes, J. M. (1936) The General Theory of Employment, Interest and Money, New York, Macmillan.
- Klamer, Arjo (1984) Conversation with Economists, Totowa, N.J., Rowman and Allanheld
- McLaughlin, Kenneth (1990) "Rigid Wages", University of Rochester, Center for Economic Research, working-paper, No. 29
- Mayer, Thomas (1972) Permanent Income, Wealth and Consumption, Berkeley, Cal., University of California Press.
- Mayer, Thomas (1993a) "Milton Friedman's Methodology of Economics: A Soft Reading," Economic Inquiry, forthcoming.
- Mayer, Thomas (1993b) Truth versus Precision in Economics, Aldershot, England, Edward Elgar.
- Modigliani, Franco and Brumberg, Richard (1955) "Utility Analysis and the Consumption Function," in K. Kurihira, Post-Keynesian Economics, London, Allen and Unwin



Romer, David (1993) "The New Keynesian Synthesis," Journal of Economic Perspectives, vol. 7, Winter, pp. 5-22.

Sims, Christopher (1982) "Policy Analysis with Econometric Models," Brookings Papers on Economic Activity, no. 1, pp. 107-52

Tobin, James (1993) "Price Flexibility and Output Stability: An Old Keynesian View," Journal of Economic Perspectives, vol. 7, Winter, pp. 45-66.

Woo, Henry (1990) "Scientific Reduction, Reductionism and Metaphysical Reduction - A Broad View of Economic Methodology," Methodus, vol. 2, number 2, pp. 61-68.

## *CES Working Paper Series*

---

- 01 Richard A. Musgrave, Social Contract, Taxation and the Standing of Deadweight Loss, May 1991
- 02 David E. Wildasin, Income Redistribution and Migration, June 1991
- 03 Henning Bohn, On Testing the Sustainability of Government Deficits in a Stochastic Environment, June 1991
- 04 Mark Armstrong, Ray Rees and John Vickers, Optimal Regulatory Lag under Price Cap Regulation, June 1991
- 05 Dominique Demougin and Aloysius Siow, Careers in Ongoing Hierarchies, June 1991
- 06 Peter Birch Sørensen, Human Capital Investment, Government and Endogenous Growth, July 1991
- 07 Syed Ahsan, Tax Policy in a Model of Leisure, Savings, and Asset Behaviour, August 1991
- 08 Hans-Werner Sinn, Privatization in East Germany, August 1991
- 09 Dominique Demougin and Gerhard Illing, Regulation of Environmental Quality under Asymmetric Information, November 1991
- 10 Jürg Niehans, Relinking German Economics to the Main Stream: Heinrich von Stackelberg, December 1991
- 11 Charles H. Berry, David F. Bradford and James R. Hines, Jr., Arm's Length Pricing: Some Economic Perspectives, December 1991
- 12 Marc Nerlove, Assaf Razin, Efraim Sadka and Robert K. von Weizsäcker, Comprehensive Income Taxation, Investments in Human and Physical Capital, and Productivity, January 1992
- 13 Tapan Biswas, Efficiency and Consistency in Group Decisions, March 1992
- 14 Kai A. Konrad and Kjell Erik Lommerud, Relative Standing Comparisons, Risk Taking and Safety Regulations, June 1992
- 15 Michael Burda and Michael Funke, Trade Unions, Wages and Structural Adjustment in the New German States, June 1992
- 16 Dominique Demougin and Hans-Werner Sinn, Privatization, Risk-Taking and the Communist Firm, June 1992
- 17 John Piggott and John Whalley, Economic Impacts of Carbon Reduction Schemes: Some General Equilibrium Estimates from a Simple Global Model, June 1992
- 18 Yaffa Machnes and Adi Schnytzer, Why hasn't the Collective Farm Disappeared?, August 1992
- 19 Harris Schlesinger, Changes in Background Risk and Risk Taking Behavior, August 1992

- 20 Roger H. Gordon, Do Publicly Traded Corporations Act in the Public Interest?, August 1992
- 21 Roger H. Gordon, Privatization: Notes on the Macroeconomic Consequences, August 1992
- 22 Neil A. Doherty and Harris Schlesinger, Insurance Markets with Noisy Loss Distributions, August 1992
- 23 Roger H. Gordon, Fiscal Policy during the Transition in Eastern Europe, September 1992
- 24 Giancarlo Gandolfo and Pier Carlo Padoan, The Dynamics of Capital Liberalization: A Macroeconometric Analysis, September 1992
- 25 Roger H. Gordon and Joosung Jun, Taxes and the Form of Ownership of Foreign Corporate Equity, October 1992
- 26 Gaute Torsvik and Trond E. Olsen, Irreversible Investments, Uncertainty, and the Ramsey Policy, October 1992
- 27 Robert S. Chirinko, Business Fixed Investment Spending: A Critical Survey of Modeling Strategies, Empirical Results, and Policy Implications, November 1992
- 28 Kai A. Konrad and Kjell Erik Lommerud, Non-Cooperative Families, November 1992
- 29 Michael Funke and Dirk Willenbockel, Die Auswirkungen des "Standortsicherungsgesetzes" auf die Kapitalakkumulation – Wirtschaftstheoretische Anmerkungen zu einer wirtschaftspolitischen Diskussion, January 1993
- 30 Michelle White, Corporate Bankruptcy as a Filtering Device, February 1993
- 31 Thomas Mayer, In Defence of Serious Economics: A Review of Terence Hutchison; Changing Aims in Economics, April 1993
- 32 Thomas Mayer, How Much do Micro-Foundations Matter?, April 1993