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Book Review — Digitized Version

[Book Review of] Attfield, Clifford L. F.: Rational expectations in macroeconomics, Oxford, Blackwell, 1985

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Rezensionen – Book Reviews

Attfield, Clifford L.F., David Demery, Nigel W. Duck, Rational Expectations in Macroeconomics. An Introduction to Theory and Evidence. Oxford, New York 1985. Basil Blackwell. XII, 211 pp.

The revolution of rational expectations has affected economists in an awkward way. On the one hand, the basic idea behind this theory is usually "accepted", simply because the concept of rational expectations is one (important) extension of equilibrium economics everybody has learned. On the other hand, however, the implications and policy conclusions are so strong – though not totally new – that most economists are offended because they got used to a different kind of macroeconomics. And – after all – is reality not totally different?

This book gives an excellent impression of just these mixed feelings about the impact of the revolution. While reading it, one gets almost convinced in the beginning that the REH is a long needed change in thinking about, for example, business cycles, whereas later on one gets doubts, especially at the end when most of the empirical work of the new classicals is questioned by more recent and – supposedly – stronger or more sophisticated tests. In this sense, it is an honest book, because it leaves the questions open when they are. And in the heated debate between schools, this book is refreshingly free of ideology and dogmatism.

The book explains in a very systematic manner the main contents and implications of the REH. The logic of rational expectations is that an equilibrium model just cannot do without this hypothesis. Every other theory about expectations is, of course, possible, and nobody can prove who is right and who is wrong. But the way expectations are handled is a very critical matter for anyone who tries to model an economy. However abstract this model may be, at a certain point the economist has to decide whether he allows for ex-ante systematic errors or not. What REH implies is well demonstrated in the "flexible price macroeconomic model" (Ch. 3): Only random shocks can cause deviations of output from the natural level, and the size of the effect is negatively correlated with the variance of aggregate demand (Lucas effect).

Much of the debate on REH deals with plausibility issues, but some things only materialize when put down on paper. The econometric issues (Ch. 6) of the observational equivalence and the Lucas critique are known to everyone doing econometric work. However, some continue using the "old method" to give quantitative advice to economic policy. This neglect must imply the opinion that, for example, parameters do not change so quickly "in reality", that the mistakes one can make are only very small. That this is not so has been demonstrated on a theoretical basis and is also the subject of many empirical tests.

The book covers the main empirical contributions which were usually published at the same time the theory was done (Ch. 7). Both theory and empirical work together had a strong impact on economic thinking, and in the late 1970s many economists felt inclined to follow the revolution. The "failure of Keynesianism" became obvious when expansionary measures of monetary and fiscal policy led to a new round of inflation without substantial progress for unemployment. This was Lucas' "clear-cut

experiment" of macroeconomics. Up to this part of the book, the reader also gets the idea that the REH could claim victory.

This claim would indeed have been premature, as we are told in the following chapters. It is a bit surprising that most of the REH pioneers have somewhat resigned, thinking maybe the revolution was done and the new theory sufficiently established so that every criticism would do no more than a little scratch. Barro, Lucas and Lucas/Sargent have published their own and others' collected work and since then have not been so actively in the front of the debate as in the preceding decade. Thus there was no strong response to the critics who seemed to gain ground on a technical level in the sense that they questioned the validity or robustness of the pioneer empirical work by formulating better models and by estimating with more sophisticated methods.

Though many of the points are well taken, the authors are a little too cautious and mild with the critics (Chs. 8 – 10). The critique that there may be measurement errors in Lucas' 1973 calculations may be right, but it does not really hit the mark. Who would argue today that the Phillips curves in Argentina, Brazil and similar countries are not steeper than those in economies with low inflation? Weren't the Phillips curves in many Western industrial countries steeper in the 1970s than in the 1960s? Isn't this what the "Lucas trade off" wanted to explain?

Some of the tests quoted in Ch. 9 do not confirm the hypothesis of efficiency in financial markets. This, however, should not lead to the conclusion that expectations are not rational. If someone finds some kind of systematic error in the forecasts for exchange rates, would he seriously claim that anyone can make safe gains in this market? Rational expectations mean that at each point of time "fair bets" are taking place: Nobody knows ex ante if he is right or wrong. *After the fact,* one may find some correlation of errors, but that does not mean that *ex ante* there were unexploited profit opportunities. If there were, those economists should speculate and become rich instead of wasting time writing articles.

One approach that stresses this important distinction of ex-ante and ex-post errors has been formulated by Brunner, Cukierman, Meltzer (a contribution which, unfortunately, is not mentioned in the book). Here, business cycles are due to the inability of agents to distinguish between permanent and transitory changes of, for example, monetary policy. At each point of time, they make the best possible forecast using all relevant information (= rational expectations), and markets are always allowed to clear. The persistence of deviations from the natural rate are created by errors which are systematic in an ex-post sense. The authors are also unduly defensive when it comes to current policy issues, especially the U.S. experience and the Thatcher policy. In spite of common myths in the media (and also among economists), it is hard to see that the policies of Reagan and Thatcher during the last years have much to do with the recommendations of the new classical theory. It is more than a misunderstanding that we have had a monetarist experiment or a clear-cut change of the policy regime in either country: Monetary policy has been highly unstable and rarely hit the targets, and fiscal policy – though in part doing more for the long-run targets than before through, for example, tax cuts – has not followed the recommendations of steadiness and predictability. And besides, the new classicals never said the natural rate was constant. There are many factors on the "supply side" of the economy which lead us to believe that the natural rate of unemployment has increased during the last years. It is far too easy – and from all we know probably wrong – to say that the increase of unemployment was due to excessively tight monetary and fiscal policy. The authors fail to stress

that the Thatcher policy is not a test of the recommendations of the new classical theory.

The conclusions of the book are therefore a little disappointing. What was it all for? "it (= REH) has not established itself as an empirically sound hypothesis" (p. 198). – Which of the presently held hypotheses on business cycles is "empirically sound" or at least significantly superior to REH? Have the Keynesian models been unduly criticized; should they now be considered "established" again? Can REH models not explain some features of business cycles better than, for example, "older" monetarist theories? In some way, proponents of REH would not feel too sorry about not having superior empirical evidence on their side, because they are also very cautious about what econometrics can show. The main thrust of the revolution was rather that we should have a better understanding of business cycles and other phenomena. The contribution was mainly to stress that we should try to understand reality as the result of behavior on the side of agents. What is reality? Nobody knows. If we knew we would not need the theory. The only facts we have are time series, which are mirrors of reality. The REH message is that nobody should believe to have superior knowledge. Time series are an outcome of behavior, and no model builder should assume to have more knowledge about behavior than the agents "who do the behaving". This he would have to assume if he favored active stabilization policies. Therefore one should always be careful in saying that reality is different than the theory suggests and we need more "realistic" assumptions. For example, the inertia of prices and wages is not a fact in this sense, and even if it was it can be explained in several ways – also by equilibrium theory. Thus "more realism" cannot be an argument in favor of the sticky-price-model of Fischer-Phelps-Taylor (Ch. 4). Taking this assumption of inertia for granted, stabilization policy is possible. However, one must also drop the postulate of optimizing behavior, which most new classicals would refuse to do. Which model is more realistic cannot be judged a priori. Apart from this, would it really be desirable to make policy recommendations and decisions on the basis of some observable or predictable error on the part of agents? Should policy *want* to fool the people?

All in all, the book is worth reading and is surely an excellent introduction into the subject, in spite of the sometimes weak defense of REH. Possibly also because it leaves it very much to the reader to judge on the validity of criticisms, it has its value in the fact that it is free of many ideological phrases and quarrels we read so much in other books. It is designed to inform students – and one should add: any economist who wants to learn about an important new way of thinking – about what has happened in an important area of economics during the last 15 years. It is more valuable than many of the existing textbooks of macroeconomics where a chapter is added later on to make it up to date. The book is also less formal than e.g. Begg's on this subject; therefore it is, I suppose, more attractive to many economists.

Joachim Scheide

Brems, Hans, Pioneering Economic Theory, 1630– 1980. A Mathematical Restatement. Baltimore, London 1986. The Johns Hopkins University Press. XV, 411 S.

Hans Brems hat eine Art dogmengeschichtliches Werk produziert, das gleichermaßen interessant, anregend und anfechtbar ist. Brems geht von der sicherlich weithin