Micro vs. macro

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> The most important recent development in macroeconomic theory seems to me describable as the reincorporation of aggregative problems such as inflation and the business cycle within the general framework of 'microeconomic' theory. If these developments succeed, the term 'macroeconomic' will simply disappear from use and the modifier 'micro' will become superfluous. We will simply speak, as did Smith, Ricardo, Marshall and Walras of *economic* theory.
>
> Robert Lucas: *Models of Business Cycles*

Introduction

Most New Classical and "New Keynesian" macroeconomists today seem to subscribe to a methodological individualist view, according to which the only "rigorous," "acceptable," "well-grounded" or "secure" way to do macroeconomics, is to somehow reduce it to microeconomic analysis. Implementing a microfoundationalist programme, these economists believe that macroeconomics is both dispensable and/or basically reducible to microeconomics. Adhering – consciously or not – to a methodological individualist stance, macroeconomic facts are to be explained only in terms of facts about individual agents. Only when we have arrived at explaining macroeconomic phenomena by deriving them from explanatory primary microeconomic "deep parameters" like preferences, tastes, aspirations and beliefs of individuals, have we got adequate explanations.

But as economists, philosophers, historians and methodologists – such as e. g. John King (2012), Alan Nelson (1984), Roy Bhaskar (1989), John Searle (1996), Tony Lawson (1997), Wim Meeusen (2011), James Hartley (1997) and Kevin Hoover (2001, 2009, 2010a, 2010b) – have forcefully argued, there exist overwhelmingly strong reasons for being critical and doubtful *re* methodological individualism and reductionism and the urge for microfoundations of macroeconomics. In this essay I want to elaborate on a couple of them.

Microfoundations today – on the history, significance and interpretation of earlier microfoundationalist programmes, cf. Weintraub (1979), Janssen (2006), Pålsson Syll (2011), King (2012) and Hoover (2010b, 2013) – means more than anything else trying to *reduce* macroeconomics to microeconomics by building macroeconomic models assuming "rational expectations" and hyper-rational "representative agents" optimizing over time. Both are highly questionable assumptions. That a specific theory/method/approach has been established as *the* way of performing economic analysis in the economics community, is not a proof of its validity, as we will see.

The concept of rational expectations was first developed by John Muth (1961) and later applied to macroeconomics by Robert Lucas (1972). Those macroeconomic models building on rational expectations microfoundations that are used today among both New Classical and "New Keynesian" macroconomists, basically assume that people on average hold expectations that will be fulfilled. This makes the economist's analysis enormously simplistic, since it means that the model used by the economist is the same as the one people use to make decisions and forecasts of the future.

Rather than assuming that people on average have the same expectations, someone like Keynes for example, would argue that people often have different expectations and information, and that this constitutes the basic rational behind macroeconomic needs of coordination – something that is rather swept under the rug by the extremely simple-mindedness of assuming rational expectations in representative agents models. But if all actors are alike, why do they transact? Who do they transact with? The very reason for markets and exchange seems to slip away with the sister assumptions of representative agents and rational expectations.

Microfoundations – when microeconomic modeling becomes the message

Macroeconomic models building on rational expectations microfoundations impute beliefs to the agents that is not based on any real informational considerations, but simply stipulated to make the models mathematically-statistically tractable. Of course you can make assumptions based on tractability, but then you do also have to take into account the necessary trade-off in terms of the ability to make relevant and valid statements on the intended target system. Mathematical tractability cannot be the ultimate arbiter in science when it comes to modeling real world target systems. One could perhaps accept macroeconomic models building on rational expectations microfoundations if they had produced lots of verified predictions and good explanations. But they have done nothing of the kind. Therefore the burden of proof is on those macroeconomists who still want to use models built on these particular unreal assumptions.

Using models in science usually implies that simplifications have to be made. But it comes at a price. There is always a trade-off between rigour and analytical tractability on the one hand, and relevance and realism on the other. Modern Walrasian macroeconomic models err on the side of rigour and analytical tractability. They fail to meet Einstein's 'Not More So' criterion — thereby making macroeconomics less useful and more simplistic than necessary. Models should be as simple as possible — but 'Not More So.'

If you want the model to fit reality this ought to be rather self-evident. However, when confronting modern Walrasian macroeconomic model builders with this kind of critique, a common strategy used is to actually deny that there ever was any intention of being realistic — the sole purpose of the models are to function as *bench-marks* against which to judge the real world we happen to live in. For someone devoted to the

study of economic methodology it is difficult not to express surprise at this unargued and nonsensical view. This is nothing but a new kind of *Nirvana fallacy* –and why on earth should we consider it worthwhile and interesting to make evaluations of real economies based on abstract imaginary fantasy worlds? It's absolutely unwarranted from a scientific point of view. It's like telling physiologists to evaluate the human body from the perspective of unicorns — they wouldn't take you seriously. And it is difficult from a critical realist point of view to come up with any reason whatsoever why we should judge these macroeconomic model builders differently.

In macroeconomic models building on rational expectations microfoundations – where agents are assumed to have complete knowledge of all of the relevant probability distribution functions – nothing really new happens, since they take for granted that people's decisions can be portrayed as based on an existing probability distribution, which by definition implies the knowledge of every possible event (otherwise it is in a strict mathematical-statistically sense not really a probability distribution at all) that can be thought of taking place.

But in the real world, it is not possible to just assume that probability distributions are the right way to characterize, understand or explain acts and decisions made under uncertainty. When we simply do not know, when we have not got a clue, when genuine uncertainty prevails, macroeconomic models building on rational expectations microfoundations simply will not do. In those circumstances it is not a useful assumption. The main reason being that under those circumstances the future is not like the past, and henceforth, we cannot use the same probability distribution – if it at all exists – to describe both the past and future.

The future is not reducible to a known set of prospects. It is not like sitting at the roulette table and calculating what the future outcomes of spinning the wheel will be. We have to surpass macroeconomic models building on rational expectations microfoundations and instead try to build economics on a more realistic foundation – a foundation that encompasses both risk and genuine uncertainty.

Macroeconomic models building on rational expectations microfoundations emanates from the belief that to be scientific, economics has to be able to model individuals and markets in a stochastic-deterministic way. It's like treating individuals and markets as the celestial bodies studied by astronomers with the help of gravitational laws. Unfortunately, individuals, markets and entire economies are not planets moving in predetermined orbits in the sky.

To deliver macroeconomic models building on rational expectations microfoundations the economists have to constrain expectations on the individual and the aggregate level to be the same. If revisions of expectations take place, they typically have to take place in a known and pre-specified precise way. This squares badly with what we know to be true in real world, where fully specified trajectories of future expectations revisions are non-existent.

Further, most macroeconomic models building on rational expectations microfoundations are time-invariant and *a fortiori* give no room for any changes in expectations and their revisions. The only imperfection of knowledge they admit of is included in the error terms, error terms that are standardly assumed to be linearly additive and to have a given and known frequency distribution, so that the models can still fully pre-specify the future even when incorporating stochastic variables into the models.

In the real world there are many different expectations and these cannot be aggregated in macroeconomic models building on rational expectations microfoundations without giving rise to inconsistency. This is one of the main reasons for these models being modeled as representative agents models. But this is far from being a harmless approximation to reality. Even the smallest differences of expectations between agents would make these models inconsistent, so when they still show up they have to be considered "irrational".

It is not possible to adequately represent individuals and markets as having one single overarching probability distribution. Accepting that, does not imply that we have to end all theoretical endeavours and assume that all agents always act totally irrationally and only are analyzable within behavioural economics. Far from it. It means we acknowledge diversity and imperfection, and that macroeconomics has to be able to incorporate these empirical facts in its models.

Most models in science are representations of something else. Models "stand for" or "depict" specific parts of a "target system" (usually the real world). A model that has neither surface nor deep resemblance to important characteristics of real economies ought to be treated with *prima facie* suspicion. How could we possibly learn about the real world if there are no parts or aspects of the model that have relevant and important counterparts in the real world target system? The burden of proof lays on the macroeconomists thinking they have contributed anything of scientific relevance without even hinting at any bridge enabling us to traverse from model to reality. All theories and models have to use sign vehicles to convey some kind of content that may be used for saying something of the target system. But purpose-built assumptions made solely to secure a way of reaching deductively validated results in mathematical models, are of little value if they cannot be validated outside of the model. Assuming away problems – rather than solving them – is not a scientific approach. As Kevin Hoover (2010a:346) writes:

> The idea that macroeconomics not only needs microfoundations, but that microeconomics can replace macroeconomics completely is the dominant position in modern economics. No one, however, knows how to derive empirically relevant explanations of observable aggregate relations from the precise individual behaviors that generate them. Instead, the claims to have produced microfoundations are typically fleshed out with representative agent models in which a single agent treats the aggregates as objects of direct choice, playing by rules that appear to follow the logic and mathematics of microeconomics ...
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> I accept idealization as a strategy of model building. But legitimate idealization requires that the idealized model capture the essence of the causal structure or underlying mechanisms at work. It is only on that basis that we can trust the model to analyze situations other than the

> data to hand ... Yet, the trick of using models appropriately is that we should either be able to set aside these particularities in reasoning or show that the results of interest are robust to the range of particular forms that we might reasonably assume ...
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> The essence of the criticism of the common strategies of reducing microeconomics to macroeconomics is that it is based in model building that mixes legitimate idealizations with non-ideal, particular modeling assumptions and then relies on those assumptions at critical junctures in providing the derivation of the macroeconomic relationships from microeconomic behaviors.

All empirical sciences use simplifying or unrealistic assumptions in their modeling activities. That is no longer the issue – as long as the assumptions made are not unrealistic in the wrong way or for the wrong reasons.

Theories are difficult to directly confront with reality. Economists therefore build models of their theories. Those models are *representations* that are *directly* examined and manipulated to *indirectly* say something about the target systems. But being able to model a world that somehow could be considered real or *similar* to the real world is not the same as investigating the real world. Even though all theories are false, since they simplify, they may still possibly serve our pursuit of truth. But then they cannot be unrealistic or false in *any* way. The falsehood or unrealisticness has to be qualified.

Microfounded macromodels should enable us to posit counterfactual questions about what would happen if some variable was to change in a specific way (hence the assumption of structural invariance, that purportedly enables the theoretical economist to do just that). But do they? Applying a "Lucas critique" on most microfounded macromodels, it is obvious that they fail. Changing "policy rules" cannot just be presumed not to influence investment and consumption behaviour and *a fortiori* technology, thereby contradicting the invariance assumption. Technology and tastes cannot live up to the status of an economy's deep and structurally stable Holy Grail. They too are part and parcel of an ever-changing and open economy.

Without export certificates, models and theories should be considered unsold. Unfortunately this understanding has not informed modern neoclassical economics, as can be seen by the profuse use of representative agent models. For quite some time now, it has been a common feature of modern neoclassical macroeconomics to use simple dynamic stochastic general equilibrium –DSGE – models where representative agents are supposed to act in a world characterized by complete knowledge, zero transaction costs and complete markets.

In these models, the actors are all identical. This has, of course, far-reaching analytical implications. Situations characterized by asymmetrical information – situations most of us consider to be innumerable – cannot arise in such models. If the aim is to build a macro-analysis from micro-foundations in this manner, the relevance of the procedure is highly questionable – Robert Solow (2010) even considered the claims made by protagonists of representative agent models "generally phony."

One obvious critique – cf. Pålsson Syll (2001) – is that representative agent models do not incorporate distributional effects – effects that often play a decisive role in macroeconomic contexts. Investigations into the operations of markets and institutions usually find that there are overwhelming problems of coordination. These are difficult, not to say impossible, to analyze with the kind of Robinson Crusoe models that, e. g., real business cycle theorists employ and which exclude precisely those differences between groups of actors that are the driving force in many non-neoclassical analyses.

The choices of different individuals have to be shown to be coordinated and consistent. This is obviously difficult if the macroeconomic models don't give room for heterogeneous individuals (this lack of understanding the importance of heterogeneity is perhaps especially problematic for the modeling of real business cycles in dynamic stochastic general equilibrium models). Assuming away the heterogeneity that exists at an individual level by using representative agent models, are certainly more manageable, however, from a realist point of view, these models are also less relevant and have a lower explanatory potential. As Kevin Hoover (2009:405) writes:

> The irony of the program of microfoundations is that, in the name of preserving the importance of individual intentional states and preserving the individual economic agent as the foundation of economics, it fails to provide any intelligible connection between the individual and the aggregate. Instead, it embraces the representative agent, which is as close to an untethered Hegelian World (or Macroeconomic) Spirit as one might fear in the microfoundationist's worst nightmare.

Or as Robert Gordon (2009:25-26) has it:

> In the end, the problem with modern macro is that it contains too much micro and not enough macro. Individual representative agents assume complete and efficient markets and market clearing, while the models ignore the basic macro interactions implied by price stickiness, including macro externalities and coordination failures. In an economy-wide recession, most agents are not maximizing unconditional utility functions as in DSGE models but are maximizing, i.e., trying to make the best out of a bad situation, under biting income and liquidity constraints. Perceptive comments by others as cited above reject the relevance of modern macro to the current cycle of excess leveraging and subsequent deleveraging, because complete and efficient markets are assumed, and there is no room for default, bankruptcy, insolvency, and illiquidity.

Both the "Lucas critique" and Keynes' critique of econometrics argued that it was inadmissible to project history on the future. Consequently an economic policy cannot presuppose that what has worked before, will continue to do so in the future. That macroeconomic models could get hold of correlations between different "variables" was not enough. If they could not get at the causal structure that generated the data, they were not really "identified". Lucas himself drew the conclusion that the problem with unstable relations was to construct models with clear microfoundations, where forward-looking optimizing individuals and robust, deep, behavioural parameters are seen to be stable even to changes in economic policies.

The purported strength of New Classical and "New Keynesian" macroeconomics is that they have firm anchorage in preference based microeconomics, and especially the decisions taken by intertemporal utility maximizing "forward looking" individuals. To some of us, however, this has come at too high a price. The almost quasi-religious insistence that macroeconomics has to have microfoundations – without ever presenting neither ontological nor epistemological justifications for this claim – has put a blind eye to the weakness of the whole enterprise of trying to depict a complex economy based on an all-embracing representative agent equipped with superhuman knowledge, forecasting abilities and forward-looking rational expectations. It is as if – after having swallowed the sour grapes of the Sonnenschein-Mantel-Debreu-theorem – these economists want to resurrect the omniscient Walrasian auctioneer in the form of all-knowing representative agents equipped with rational expectations and assumed to somehow know the true structure of our model of the world. How that could even be conceivable is beyond imagination, given that the ongoing debate on microfoundations, if anything, shows that not even we, the economists, can come to agreement on a common model.

Microfoundations – Walrasian "Santa Claus" economics trying to get around Sonnenschein-Mantel-Debreu

Almost a century and a half after Léon Walras founded neoclassical general equilibrium theory, economists still have not been able to show that markets *move* economies *to* equilibria. What we do know is that unique Pareto-efficient equilibria do *exist*.

But what good does that do? As long as we cannot show, except under exceedingly unrealistic assumptions, that there are convincing reasons to suppose there are forces which lead economies to equilibria - the value of general equilibrium theory is next to nil. As long as we cannot really demonstrate that there are forces operating – under reasonable, relevant and at least mildly realistic conditions – at moving markets to equilibria, there cannot really be any sustainable reason for anyone to pay any interest or attention to this theory. A stability that can only be proved by assuming "Santa Claus" conditions is of no avail. Most people do not believe in Santa Claus anymore. And for good reasons.

Simply assuming the problem away or continuing to model a world full of agents behaving as economists – "often wrong, but never uncertain" – and still not being able to show that the system under reasonable assumptions converges to equilibrium, is a gross misallocation of intellectual resources and time.

Here's what a leading microeconomist – Alan Kirman (1989:129) – writes on the issue:

> Starting from 'badly behaved' individuals, we arrive at a situation in which not only is aggregate demand a nice function but, by a result of Debreu, equilibrium will be 'locally unique. Whilst this means that at least there is some hope for local stability, the real question is, can we hope to proceed and obtain global uniqueness and stability?
>
> The unfortunate answer is a categorical no! [The results of Sonnenchein (1972), Debreu (1974), Mantel (1976) and Mas Collel (1985)] shows clearly why any hope for uniqueness or stability must be unfounded ... There is no hope that making the distribution of preferences or income 'not to dispersed' or 'single peaked' will help us to avoid the fundamental problem.
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> The idea that we should start at the level of the isolated individual is one which we may well have to abandon ... we should be honest from the outset and assert simply that *by assumption* we postulate that each sector of the economy behaves as one individual and not claim any spurious microjustification ...
>
> Economists therefore should not continue to make strong assertions about this behaviour based on so-called general equilibrium models which are, in reality, no more than special examples with no basis in economic theory as it stands.

Kenneth Arrow (1968) argues in a similar vein against the kind of reductionism implied in the microfoundationalist attempts at redirecting economics:

> The economy is irreducible ... in the sense that no matter how the households are divided into two groups, an increase in the initial assets held by the members of one group can be used to make feasible an allocation which will make no one worse off and at least one individual in the second group better off.
>
> It is perhaps interesting to observe that “atomistic” assumptions concerning individual households and firms are not sufficient to establish the existence of equilibrium; “global” assumptions ... are also needed (though they are surely unexceptionable). Thus, a limit is set to the tendency implicit in price theory, particularly in its mathematical versions, to deduce all properties of aggregate behavior from assumptions about individual economic agents.

Getting around Sonnenschein-Mantel-Debreu using representative agents may be – as noted by Meeusen (2011) – very expedient from a purely formalistic point of view. But from a scientific point of view it is hardly relevant or realistic. As Rizvi (1994:363) maintains:

> The impact of SMD theory is quite general ... Its chief implication, in the authors view, is that the hypothesis of individual rationality, and the other assumptions made at the micro level, gives no guidance to an analysis of macro-level phenomena: the assumption of rationality or utility maximisation is not enough to talk about social regularities. This is a significant conclusion and brings the microfoundations project in GET [General Equilibrium Theory] to an end ... A theory based on micro principles or on appeals to them and which purports to analyse micro-level regularities *must* deal with aggregation; not doing so is not an option.

In microeconomics we know that (ideal) aggregation really presupposes homothetic an identical preferences, something that almost never exist in real economies – if they do, it means that you and multi-billionaire Richard Branson have the same preferences and that we after having had, e. g. a 99 % “haircut,”

still spend the same proportion of our incomes on, e. g. bread and butter, as before the massive income reduction.

To illustrate – following Nelson (1984) and Hoover (2001) – assume we have a very simple economy consisting of two consumers (i) trying to optimally choose consuming two commodities (c1 and c2) in two time periods by maximizing a logarithmic Cobb-Douglas utility function of the form $u^i = c^i1 + a^ic^i2$, given the (always satisfied) budget constraint $y = c^i1 + pc^i2$ (where y is income and p the price of commodity 2 in terms of the numéraire, commodity 1). Demand for commodity 1 is

(1) $c^i1 = y^i/(1 + a^i)$.

Aggregating (indicated by upper-case letters) the demand for commodity 1 we get

(2) $C1 = Y/(1 + a) = c^i1 + c^i2 = y^1/(1 + a^1) + y^2/(1 + a^2) = [y^1(1 + a^1) + y^2(1 + a^2)]/[(1 + a^1)(1 + a^2)]$

$= [Y + a^1y^1 + a^2y^2]/[(1 + a^1)(1 + a^2)]$,

where the last equality follows from $Y = y^1 + y^2$. As can easily be seen, (1) and (2) are only of an identical form if all consumers have identical preferences – that is, $a^1 = a^2 = a$ – and homothetic utility functions yielding linear Engel curves, as e. g. the Cobb-Douglas utility function.

If these requirements are fulfilled, ideal aggregation from micro to macro can take place. Why? As Hoover (2001:79) puts it:

> In such circumstances, for a fixed aggregate income, redistributing that income among the individual consumers will not affect demands for individual goods and, therefore, will not affect relative prices ... and we can add up individual quantities to form economy-wide aggregates without loss of information.

However, if these patently unreal assumptions are *not* fulfilled, there is no guarantee of a straightforward and constant relation between individuals (micro) and aggregates (macro). The results given by these assumptions are *a fortiori* not robust and do not capture the underlying mechanisms at work in any real economy. And as if this impossibility of ideal aggregation was not enough, there are obvious problems also with the kind of microeconomic equilibrium that one tries to reduce macroeconomics to. Decisions of consumption and production are described as choices made by a single agent. But then, who sets the prices on the market? And how do we justify the assumption of universal consistency between the choices? Models that are critically based on particular and odd assumptions – and are neither robust nor congruent to real world economies – are of questionable value.

And is it really possible to describe and analyze all the deliberations and choices made by individuals in an economy? Does not the choice of an individual presuppose knowledge and expectations about choices of

other individuals? It probably does, and this presumably helps to explain why representative agent models have become so popular in modern macroeconomic theory. They help to make the analysis more tractable.

One could justifiably argue that one might just as well accept that it is not possible to coherently reduce macro to micro, and accordingly that it is perhaps necessary to forswear microfoundations and the use of rational-agent models all together. Microeconomic reasoning has to build on macroeconomic presuppositions. Real individuals do not base their choices on operational general equilibrium models, but rather use simpler models. If macroeconomics needs microfoundations it is equally necessary that microeconomics needs macrofoundations.

On the impossibility of microfoundational reductionism

Alan Kirman (1992) maintains that the use of representative agent models is unwarranted and leads to conclusions that are usually both misleading and false. It's a fiction basically used by some macroeconomists to justify the use of equilibrium analysis and a kind of pseudo-microfoundations. Microeconomists are well aware that the conditions necessary to make aggregation to representative agents possible are not met in actual economies. As economic models become increasingly complex, their use also becomes less credible.

Already back in the 1930s, Keynes (1939) held a similar anti-reductionist view:

> I have called my theory a *general* theory. I mean by this that I am chiefly concerned with the behaviour of the economic system as a whole, – with aggregate incomes, aggregate profits, aggregate output, aggregate employment, aggregate investment, aggregate saving rather than with the incomes, profits, output, employment, investment and saving of particular industries, firms or individuals. And I argue that important mistakes have been made through extending to the system as a whole conclusions which have been correctly arrived at in respect of a part of it taken in isolation ...
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> Quite legitimately we regard an individual's income as independent of what he himself consumes and invests. But this, I have to point out, should not have led us to overlook the fact that the demand arising out of the consumption and investment of one individual is the source of the incomes of other individuals, so that incomes in general are not independent, quite the contrary, of the disposition of individuals to spend and invest; and since in turn the readiness of individuals to spend and invest depends on their incomes, a relationship is set up between aggregate savings and aggregate investment which can be very easily shown, beyond any possibility of reasonable dispute, to be one of exact and necessary equality. Rightly regarded this is a banale conclusion.

Actually, Keynes way back in 1926 [Keynes 1933(1926)] more or less buried any ideas of microfoundations:

> The atomic hypothesis which has worked so splendidly in Physics breaks down in Psychics. We are faced at every turn with the problems of Organic Unity, of Discreteness, of

> Discontinuity – the whole is not equal to the sum of the parts, comparisons of quantity fails us, small changes produce large effects, the assumptions of a uniform and homogeneous continuum are not satisfied. Thus the results of Mathematical Psychics turn out to be derivative, not fundamental, indexes, not measurements, first approximations at the best; and fallible indexes, dubious approximations at that, with much doubt added as to what, if anything, they are indexes or approximations of.

Where "New Keynesian" and New Classical economists think they can rigorously deduce the aggregate effects of the acts and decisions of consumers and firms with their reductionist microfoundational methodology, they actually have to put a blind eye on the emergent properties that characterize all open social and economic systems. The interaction between animal spirits, trust, confidence, institutions etc., cannot be deduced or reduced to a question answerable on the individual level. Macroeconomic structures and phenomena have to be analyzed on their own terms.

Contrary to the microfoundational programme of Lucas *et consortes*, Keynes didn't consider equilibrium as the self-evident axiomatic starting point for economic analysis. Actually it was the classical idea of equilibrium that had made economics blind to the obvious real fact that involuntary outcomes, such as unemployment, are a common feature of market economies – and Keynes wanted to develop a more realist alternative, breaking with the conception of economics as an equilibrium discipline.

Even if economies naturally presuppose individuals, it does not follow that we can infer or explain macroeconomic phenomena solely from knowledge of these individuals. Macroeconomics is to a large extent emergent and cannot be reduced to a simple summation of micro phenomena. Moreover, as we have already argued, even these microfoundations aren't immutable. Lucas and the new classical economists' deep parameters – "tastes" and "technology" – are not really the bedrock of constancy that they believe (pretend) them to be.

For Alfred Marshall economic theory was "an engine for the discovery of concrete truth". But where Marshall tried to describe the behaviour of a typical business with the concept "representative firm," his modern heirs don't at all try to describe how firms interplay with other firms in an economy. The economy is rather described "as if" consisting of one single giant firm/consumer/household – either by inflating the optimization problem of the individual to the scale of a whole economy, or by assuming that it's possible to aggregate different individuals' actions by a simple summation, since every type of actor is identical. But it would most probably be better if we just faced the fact that it is difficult to describe interaction and cooperation when there is essentially only one actor – instead of sweeping aggregation problems, fallacies of composition and emergence under the rag.

Those who want to build macroeconomics on microfoundations usually maintain that the only robust policies and institutions are those based on rational expectations and representative agents. But there is really no support for this conviction at all. On the contrary – if we want to have anything of interest to say on real economies, financial crisis and the decisions and choices real people make, it is high time to

redirect macroeconomics away from constructing models building on representative agents and rational expectations-microfoundations. Since representative-agent-rational-expectations (RARE) microfounded macroeconomics has nothing to say about the real world and the economic problems out there, why should we care about it? The final court of appeal for macroeconomic models is the real world, and as long as no convincing justification is put forward for how the inferential bridging *de facto* is made, macroeconomic modelbuilding is little more than hand waving that give us rather little warrant for making inductive inferences from models to real world target systems. Even though equilibrium according to Lucas (Snowdon 1998:127) is considered "a property of the way we look at things, not a property of reality," this is hardly a tenable view. Analytical tractability should not be transformed into a methodological virtue. If substantive questions about the real world are being posed, it is the formalistic-mathematical representations utilized to analyze them that have to match reality, not the other way around.

Given that, I would say that macroeconomists - especially "Keynesian" ones – ought to be even *more* critical of the microfoundations dogma than they are. If macroeconomic models – no matter of what ilk – build on microfoundational *assumptions* of representative agents, rational expectations, market clearing and equilibrium, and we *know* that real people and markets cannot be expected to obey these assumptions, the warrants for supposing that conclusions or hypotheses of causally relevant mechanisms or regularities can be bridged, are obviously non-justifiable. Incompatibility between actual behaviour and the behaviour in macroeconomic models building on RARE microfoundations shows the futility of trying to represent real-world economies with models flagrantly at odds with reality.

In the conclusion to his book *Models of Business Cycles* Robert Lucas (1987:66-108) (in)famously wrote:

> It is remarkable and, I think, instructive fact that in nearly 50 years that Keynesian tradition has produced not one useful model of the individual unemployed worker, and no rationale for unemployment insurance beyond the observation that, in common with countercyclical cash grants to corporations or to anyone else, it has the effects of increasing the total volume of spending at the right times. By dogmatically insisting that unemployment be classed as 'involuntary' this tradition simply cut itself off from serious thinking about the actual options unemployed people are faced with, and hence from learning anything about how the alternative social arrangements might improve these options …
>
> If we are honest, we will have to face the fact that at any given time there will be phenomena that are well-understood from the point of view of the economic theory we have, and other phenomena that are not. We will be tempted, I am sure, to relieve the discomfort induced by discrepancies between theory and facts by saying the ill-understood facts are the province of some other, different kind of economic theory. Keynesian 'macroeconomics' was, I think, a surrender (under great duress) to this temptation. It led to the abandonment, for a class of problems of great importance, of the use of the only 'engine for the discovery of truth' that we have in economics.

Thanks to latter-day Lucasian New-Classical-New-Keynesian-RARE-microfoundations-economists, we are supposed not to – as our "primitive" ancestors – use that archaic term 'macroeconomics' anymore (with

the possible exception of warning future economists not to give in to "discomfort.") Being intellectually heavily indebted to the man who invented macroeconomics – Keynes – I firmly decline to concur.

Microfoundations – and *a fortiori* rational expectations and representative agents – serve a particular theoretical purpose. And as the history of macroeconomics during the last thirty years has shown, the Lucasian microfoundations programme for macroeconomics is only methodologically consistent within the framework of a (deterministic or stochastic) general equilibrium analysis. In no other context has it been considered *possible* to incorporate this kind of microfoundations – with its "forward-looking optimizing individuals" – into macroeconomic models.

This is of course not by accident. General equilibrium theory is basically nothing else than an endeavour to consistently generalize the microeconomics of individuals and firms on to the macroeconomic level of aggregates. *But it obviously doesn't work*. The analogy between microeconomic behaviour and macroeconomic behaviour is misplaced. Empirically, science-theoretically and methodologically, neoclassical microfoundations for macroeconomics are defective. Tenable foundations for macroeconomics really have to be sought for elsewhere.

Microfounded DSGE models – spectacularly useless and positively harmful

Economists working within the Post Keynesian tradition, have always maintained that there is a strong risk that people may find themselves unemployed in a market economy. And, of course, unemployment is also something that can take place in microfounded DSGE models – but the mechanism in these models is of a fundamentally different kind.

In the basic DSGE models the labour market is always *cleared* – responding to a changing interest rate, expected life time incomes, or real wages, the representative agent maximizes the utility function by varying her labour supply, money holding and consumption over time. Most importantly – if the real wage somehow deviates from its "equilibrium value," the representative agent adjust her labour supply, so that when the real wage is higher than its "equilibrium value," labour supply is increased, and when the real wage is below its "equilibrium value," labour supply is decreased.

In this model world, unemployment is always an optimal choice to changes in the labour market conditions. Hence, unemployment is totally voluntary. To be unemployed is something one optimally chooses to be.

Although this picture of unemployment as a kind of self-chosen optimality, strikes most people as utterly ridiculous, there are also, unfortunately, a lot of neoclassical economists out there who still think that price and wage rigidities are the prime movers behind unemployment. What is even worse is that some of them

even think that these rigidities are the reason John Maynard Keynes gave for the high unemployment of the Great Depression. This is of course pure nonsense. For although Keynes in *General Theory* devoted substantial attention to the subject of wage and price rigidities, he certainly *did not* hold this view. That's rather the view of microfounded DSGE modelers, explaining variations in employment (and *a fortiori* output) with assuming nominal wages being more flexible than prices – disregarding the lack of empirical evidence for this rather counterintuitive assumption.

Since unions/workers, contrary to classical assumptions, make wage-bargains in nominal terms, they will – according to Keynes – accept lower real wages caused by higher prices, but resist lower real wages caused by lower nominal wages. However, Keynes held it incorrect to attribute "cyclical" unemployment to this diversified agent behaviour. During the depression money wages fell significantly and – as Keynes noted – unemployment still grew. Thus, even when nominal wages are lowered, they do not generally lower unemployment.

In any specific labour market, lower wages could, of course, raise the demand for labour. But a general reduction in money wages would leave real wages more or less unchanged. The reasoning of the classical economists was, according to Keynes, a flagrant example of the *fallacy of composition*. Assuming that since unions/workers in a specific labour market could negotiate real wage reductions via lowering nominal wages, unions/workers in general could do the same, the classics confused micro with macro.

Lowering nominal wages could not – according to Keynes – clear the labour market. Lowering wages – and possibly prices – could, perhaps, lower interest rates and increase investment. But to Keynes it would be much easier to achieve that effect by increasing the money supply. In any case, wage reductions was not seen by Keynes as a general substitute for an expansionary monetary or fiscal policy. And even if potentially positive impacts of lowering wages exist, there are also more heavily weighing negative impacts – management-union relations deteriorating, expectations of on-going lowering of wages causing delay of investments, debt deflation et cetera.

So, what Keynes actually did argue in *General Theory*, was that the classical proposition that lowering wages would lower unemployment and ultimately take economies out of depressions, was ill-founded and basically wrong. To Keynes (1936:7-16), flexible wages would only make things worse by leading to erratic price-fluctuations. The basic explanation for unemployment is insufficient aggregate demand, and that is mostly determined *outside* the labour market:

> The classical school [maintains that] while the demand for labour at the existing money-wage may be satisfied before everyone willing to work at this wage is employed, this situation is due to an open or tacit agreement amongst workers not to work for less, and that if labour as a whole would agree to a reduction of money-wages more employment would be forthcoming. If this is the case, such unemployment, though apparently involuntary, is not strictly so, and ought to be included under the above category of 'voluntary' unemployment due to the effects of collective bargaining, etc ...

> The classical theory ... is best regarded as a theory of distribution in conditions of full employment. So long as the classical postulates hold good, unemployment, which is in the above sense involuntary, cannot occur. Apparent unemployment must, therefore, be the result either of temporary loss of work of the 'between jobs' type or of intermittent demand for highly specialised resources or of the effect of a trade union 'closed shop' on the employment of free labour. Thus writers in the classical tradition, overlooking the special assumption underlying their theory, have been driven inevitably to the conclusion, perfectly logical on their assumption, that apparent unemployment (apart from the admitted exceptions) must be due at bottom to a refusal by the unemployed factors to accept a reward which corresponds to their marginal productivity ...
>
> Obviously, however, if the classical theory is only applicable to the case of full employment, it is fallacious to apply it to the problems of involuntary unemployment – if there be such a thing (and who will deny it?). The classical theorists resemble Euclidean geometers in a non-Euclidean world who, discovering that in experience straight lines apparently parallel often meet, rebuke the lines for not keeping straight – as the only remedy for the unfortunate collisions which are occurring. Yet, in truth, there is no remedy except to throw over the axiom of parallels and to work out a non-Euclidean geometry. Something similar is required to-day in economics. We need to throw over the second postulate of the classical doctrine and to work out the behaviour of a system in which involuntary unemployment in the strict sense is possible.

People calling themselves "New Keynesians" ought to be rather embarrassed by the fact that the kind of microfounded DSGE models they use, cannot incorporate such a basic fact of reality as involuntary unemployment. Of course, working with representative agent models, this should come as no surprise. The kind of unemployment that occurs is voluntary, since it is only adjustments of the hours of work that these optimizing agents make to maximize their utility.

Kevin Hoover (2001:82-86) – who has been scrutinizing the microfoundations programme for now more than 25 years – writes:

> Given what we know about representative-agent models, there is not the slightest reason for us to think that the conditions under which they should work are fulfilled. The claim that representative-agent models provide microfundations succeeds only when we steadfastly avoid the fact that representative-agent models are just as aggregative as old-fashioned Keynesian macroeconometric models. They do not solve the problem of aggregation; rather they assume that it can be ignored. While they appear to use the mathematics of microeconomis, the subjects to which they apply that microeconomics are aggregates that do not belong to any agent. There is no agent who maximizes a utility function that represents the whole economy subject to a budget constraint that takes GDP as its limiting quantity. This is the simulacrum of microeconomics, not the genuine article ...
>
> [W]e should conclude that what happens to the microeconomy is relevant to the macroeconomy but that macroeconomics has its own modes of analysis ... [I]t is almost certain that macroeconomics cannot be euthanized or eliminated. It shall remain necessary for the serious economist to switch back and forth between microeconomics and a relatively autonomous macroeconomics depending upon the problem in hand.

Alternatives to microfoundations

Defenders of microfoundations – and its concomitant rational expectations equipped representative agent's intertemporal optimization – often argue as if sticking with simple representative agent macroeconomic models doesn't impart a bias to the analysis. It's difficult not to reject such an unsubstantiated view.

Economists defending the microfoundationalist programme often also maintain that there are no methodologically coherent alternatives to microfoundations modeling – economic models based on the choices and acts of individuals is the only scientific game in town. That allegation is of course difficult to evaluate, but as argued in this essay, the kind of miocrofoundationalist macroeconomics that New Classical economists and "New Keynesian" economists are pursuing, is certainly *not* methodologically coherent. And that ought to be rather embarrassing for those ilks of macroeconomists to whom axiomatics and deductivity is the hallmark of science *tout court*.

The fact that Lucas introduced rational expectations as a consistency axiom is not really an argument for why we should accept it as an acceptable assumption in a theory or model purporting to explain real macroeconomic processes. And although virtually any macroeconomic empirical claim is contestable, the same goes for microeconomics.

Of course there are alternatives to neoclassical general equilibrium microfoundations – behavioural economics and Frydman & Goldberg's (2007) "imperfect knowledge" economics being two noteworthy examples that easily come to mind. And for those who have not forgotten the history of our discipline – and who have not bought the sweet-water nursery tale of Lucas *et consortes* that Keynes was not "serious thinking" – it can easily be seen that there exists a macroeconomic tradition inspired by Keynes that has preciously little to do with any New Synthesis or "New Keynesianism."

Its ultimate building-block is the perception of genuine uncertainty and that people often "simply do not know." Real actors can't know everything and their acts and decisions are not simply possible to sum or aggregate without the economist risking to succumb to the fallacy of composition. Instead of basing macroeconomics on unreal and unwarranted generalizations of microeconomic behaviour and relations, it is far better to accept the ontological fact that the future to a large extent is uncertain, and rather conduct macroeconomics on this fact of reality.

Conclusion

Henry Louis Mencken once wrote that "there is always an easy solution to every human problem – neat, plausible and wrong." Assuming instant and unmodeled market clearing and/or approximating aggregate behaviour with unrealistically heroic assumptions of intertemporally optimizing rational-expectations-

representative-agents, just will not do. The assumptions made, surreptitiously eliminate the very phenomena we want to study: uncertainty, disequilibrium, structural instability and problems of aggregation and coordination between different individuals and groups. Reducing macroeconomics to microeconomics, and microeconomics to refinements of hyper-rational Bayesian deductivist models, is not a viable way forward. It will only sentence to irrelevance the most interesting real world economic problems. Murder is probably the only way of reducing biology to chemistry – and disregarding Sonnenschein-Mantel-Debreu and trying to reduce macroeconomics to Walrasian general equilibrium microeconomics – basically means committing the same crime.

Commenting on the state of standard modern macroeconomics, Willem Buiter (2009) argues that neither New Classical nor "New Keynesian" microfounded DSGE macro models has helped us foresee, understand or craft solutions to the problems of today's economies:

> Most mainstream macroeconomic theoretical innovations since the 1970s ... have turned out to be self-referential, inward-looking distractions at best. Research tended to be motivated by the internal logic, intellectual sunk capital and aesthetic puzzles of established research programmes rather than by a powerful desire to understand how the economy works ...
>
> Both the New Classical and New Keynesian complete markets macroeconomic theories not only did not allow questions about insolvency and illiquidity to be answered. They did not allow such questions to be asked ...
>
> Charles Goodhart, who was fortunate enough not to encounter complete markets macroeconomics and monetary economics during his impressionable, formative years, but only after he had acquired some intellectual immunity, once said of the Dynamic Stochastic General Equilibrium approach which for a while was the staple of central banks' internal modelling: "It excludes everything I am interested in". He was right. It excludes everything relevant to the pursuit of financial stability.

Buiter's verdict is a worrying confirmation of neoclassical mainstream macroeconomics becoming more and more a "waste of time." Why do these economists waste their time and efforts on it? Besides aspirations of being published, Frank Hahn (2005) probably gave the truest answer, when interviewed on the occasion of his 80th birthday, he confessed that some economic assumptions didn't really say anything about "what happens in the world," but still had to be considered very good "because it allows us to get on this job."

The real macroeconomic challenge is to accept uncertainty and still try to explain why economic transactions take place – instead of simply conjuring the problem away by assuming uncertainty to be reducible to stochastic risk and disregarding the obvious ontological and methodological problems inherent in the individualist-reductionist microfoundations programme. That is scientific cheating. And it has been going on for too long now.

The Keynes-inspired building-blocks are there. But it is admittedly a long way to go before the whole construction is in place. But the sooner we are intellectually honest and ready to admit that modern neoclassical macroeconomics and its microfoundationalist programme has come to way's end – the sooner we can redirect our aspirations to more fruitful endeavours.

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