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Rational expectations – a fallacious foundation for macroeconomics in a non-ergodic world

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> Strangely perhaps, the most obvious element in the inference gap for models ... lies in the validity of any inference between two such different media – forward from the real world to the artificial world of the mathematical model and back again from the model experiment to the real material of the economic world. The model is at most a parallel world. The parallel quality does not seem to bother economists. But materials do matter: it matters that economic models are only representations of things in the economy, not the things themselves.
>
> Mary Morgan: *The World in the Model*

Introduction

In the wake of the latest financial crisis many people have come to wonder why economists never have been able to predict these manias, panics and crashes that intermittently haunt our economies. In responding to these warranted wonderings, some economists have maintained that it is a fundamental principle that there cannot be any reliable way of predicting a crisis.

This is a totally inadequate answer, and more or less trying to make an honour out of the inability of one's own science to give answers to just questions, is indeed proof of a rather arrogant attitude.

The main reason given for this view is what one of its staunchest defenders, David K. Levine (2012), calls "the uncertainty principle in economics" and the "theory of rational expectations":

> In simple language what rational expectations means is 'if people believe this forecast it will be true.' By contrast if a theory is not one of rational expectations it means 'if people believe this forecast it will not be true.' Obviously such a theory has limited usefulness. Or put differently: if there is a correct theory, eventually most people will believe it, so it must necessarily be rational expectations. Any other theory has the property that people must forever disbelieve the theory regardless of overwhelming evidence – for as soon as the theory is believed it is wrong.
> So does the crisis prove that rational expectations and rational behavior are bad assumptions for formulating economic policy? Perhaps we should turn to behavioral models of irrationality in understanding how to deal with the housing market crash or the Greek economic crisis? Such an alternative would have us build on foundations of sand. It would have us create economic policies and institutions with the property that as soon as they were properly understood they would cease to function.

These are rather unsubstantiated allegations. To my knowledge, there are exceptionally few (if any) economists that really advocates constructing models based on irrational expectations. And very few of us are unaware of the effects that economic theory can have on the behaviour of economic actors.

So – to put it bluntly – Levine fails to give a fair view of the state of play among contemporary economists on the issue of rational expectations. This essay is an attempt at substantiating that verdict.

Rational expectations – a concept with a history

The concept of rational expectations was first developed by John Muth (1961) and later applied to macroeconomics by Robert Lucas (1972). In this way the concept of *uncertainty* as developed by Keynes (1921) and Knight (1921) was turned into a concept of quantifiable *risk* in the hands of neoclassical economics.

Muth (1961:316) framed his rational expectations hypothesis (REH) in terms of probability distributions:

> Expectations of firms (or, more generally, the subjective probability distribution of outcomes) tend to be distributed, for the same information set, about the prediction of the theory (or the "objective" probability distributions of outcomes).

But Muth (1961:317) was also very open with the non-descriptive character of his concept:

> The hypothesis of rational expectations *does not* assert that the scratch work of entrepreneurs resembles the system of equations in any way; nor does it state that predictions of entrepreneurs are perfect or that their expectations are all the same.

To Muth its main usefulness was its generality and ability to be applicable to all sorts of situations irrespective of the concrete and contingent circumstances at hand. And while the concept was later picked up by New Classical Macroeconomics in the hands of people like Robert Lucas and Eugene Fama, most of us thought it was such a patently ridiculous idea, that we had problems with really taking it seriously.

It is noteworthy that Lucas (1972) did not give any further justifications for REH, but simply applied it to macroeconomics. In the hands of Lucas and Sargent it was used to argue that government could not really influence the behavior of economic agents in any systematic way. In the 1980s it became a dominant model assumption in New Classical Macroeconomics and has continued to be a standard assumption made in many neoclassical (macro)economic models – most notably in the fields of (real) business cycles and finance (being a cornerstone in the "efficient market hypothesis").

Keynes, genuine uncertainty and ergodicity

REH basically says that people on the 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.

This view is in obvious ways very different to the one we connect with John Maynard Keynes. According to Keynes (1937:113) we live in a world permeated by unmeasurable uncertainty – not quantifiable stochastic risk – which often force us to make decisions based on anything but rational expectations. Sometimes we "simply do not know."

Keynes would not have accepted Muth's view that expectations "tend to be distributed, for the same information set, about the prediction of the theory." Keynes, rather, thinks that we base our expectations on the confidence or "weight" we put on different events and alternatives. To Keynes expectations are a question of weighing probabilities by "degrees of belief," beliefs that have preciously little to do with the kind of stochastic probabilistic calculations made by the rational expectations agents modeled by Lucas *et consortes*.

REH only applies to ergodic – stable and stationary stochastic – processes. Economies in the real world are nothing of the kind. In the real world, set in non-ergodic historical time, the future is to a large extent unknowable and uncertain. If the world was ruled by ergodic processes – a possibility utterly incompatible with the views of Keynes – people could perhaps have rational expectations, but no convincing arguments have ever been put forward, however, for this assumption being realistic.

REH holds the view that people, on average, have the same expectations. Keynes, on the other hand, argued convincingly that people often have *different* expectations and information, and that this constitutes the basic rational behind macroeconomic needs of coordination. This is something that is rather swept under the rug by the extreme simple-mindedness of assuming rational expectations in representative actors models, which is so in vogue in New Classical Economics. Indeed 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 actors and rational expectations.

Mathematical tractability is not enough

It is hard to escape the conclusion that it is an enormous waste of intellectual power to build these kinds of models based on next to useless theories. Their marginal utility have long since passed over into the negative. That people are still more or less mindlessly doing this is a sign of some kind of not so little intellectual hubris.

It would be far better to admit that we "simply do not know" about lots of different things, and that we should try to do as good as possible given this, rather than looking the other way and pretend that we are all-knowing rational calculators.

Models based on REH impute beliefs to the agents that are 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. Of course, one could perhaps accept REH if it had produced lots of verified predictions and good explanations. But it has done nothing of the kind. Therefore the burden of proof is on those who still want to use models built on ridiculously unreal assumptions – models devoid of obvious empirical interest.

In reality REH is a rather harmful modeling assumption, since it contributes to perpetuating the ongoing transformation of economics into a kind of science-fiction-economics. If economics is to guide us, help us make forecasts, explain or better understand real world phenomena, it is in fact next to worthless.

Learning and information

REH presupposes – basically for reasons of consistency – that agents have complete knowledge of *all* of the relevant probability distribution functions. And when trying to incorporate learning in these models – to take the heat off some of the criticism launched against it up to date – it is always a very restricted kind of learning that is considered (cf. Evans & Honkapohja (2001)). A learning where truly unanticipated, surprising, new things never take place, but only a rather mechanical updating – increasing the precision of already existing information sets – of existing probability functions.

Nothing really new happens in these ergodic models, where the statistical representation of learning and information is nothing more than a caricature of what takes place in the real world target system. This follows from taking 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-statistical sense, not really a probability distribution – that can be thought of as taking place.

But in the real world it is – as shown again and again by behavioural and experimental economics – common to mistake a conditional distribution for a probability distribution. These are mistakes that are *impossible* to make in the kinds of economic analysis that are built on REH. On average REH agents are always correct. But truly new information will not only reduce the estimation error but actually change the entire estimation and hence possibly the decisions made. To be truly new, information has to be unexpected. If not, it would simply be inferred from the already existing information set.

In REH models new information is typically presented as something only reducing the variance of the parameter estimated. But if new information means truly new information it actually could increase our uncertainty and variance (information set (A, B) => (A, B, C)). Truly new information gives birth to new probabilities, revised plans and decisions – something the REH cannot account for with its finite sampling representation of incomplete information.

In the world of REH, learning is like being better and better at reciting the complete works of Shakespeare by heart – or at hitting bull's eye when playing darts. It presupposes that we have a complete list of the possible states of the world and that by definition mistakes are non-systematic (which, strictly seen, follows from the assumption of "subjective" probability distributions being equal to the "objective" probability distribution). This is a rather uninteresting and trivial kind of learning. It is a closed world learning, synonymous to improving one's adaptation to a world which is fundamentally unchanging. But in real, open world situations, learning is more often about adapting and trying to cope with genuinely new phenomena.

REH presumes consistent behaviour, where expectations do not display any persistent errors. In the world of REH we are always, on average, hitting the bull's eye. In the more realistic, open systems view, there is always the possibility (danger) of making mistakes that may turn out to be systematic. It is presumably one of the main reasons why we put so much emphasis on learning in our modern knowledge societies.

On risk, uncertainty and probability distributions

REH assumes that the expectations based on “objective” probabilities are the same as the “subjective” probabilities that agents themselves form on uncertain events. It treats risk and uncertainty as equivalent entities.

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 “haven’t got a clue,” when genuine uncertainty prevails – REH simply will not do. In those circumstances it is not a useful assumption. The reason is 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.

There simply is no guarantee that probabilities at time x are the same as those at time x+i. So when REH assumes that the parameter values on average are the same for the future and the past, one is – as Roman Frydman and Michael Goldberg (2007) forcefully argue – not really talking about uncertainty, but rather knowledge. But this implies that what we observe are realizations of pure stochastic processes, something – if we really want to maintain this view – we have to *argue* for.

In physics it may possibly not be straining credulity too much to model processes as ergodic – where time and history do not really matter – but in social and historical sciences it is obviously ridiculous. If societies and economies were ergodic worlds, why do econometricians fervently discuss things such as structural breaks and regime shifts? That they do is an indication of the unrealisticness of treating open systems as analyzable with ergodic concepts.

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. A more realistic foundation for economics has to encompass both ergodic and non-ergodic processes, both risk and genuine uncertainty. Reading advocates of REH one comes to think of Robert Clower’s (1989:23) apt remark that

> much economics is so far removed from anything that remotely resembles the real world that it’s often difficult for economists to take their own subject seriously.

Where do probabilities come from in REH?

In REH models, events and observations are as a rule interpreted as random variables, as if generated by an underlying probability density function, and *a fortiori* – since probability density functions are only definable in a probability context – consistent with a probability.
When attempting to convince us of the necessity of founding empirical economic analysis on probability models, advocates of REH actually force us to (implicitly) interpret events as random variables generated by an underlying probability density function. This is at odds with reality. Randomness obviously is a fact of the real world. Probability, on the other hand, attaches to the world via intellectually constructed models, and is only a fact of a probability generating machine or a well constructed experimental arrangement or “chance set-up”. Just as there is no such thing as a “free lunch,” there is no such thing as a “free probability.” To be able at all to talk about probabilities, you have to specify a model. If there is no chance set-up or model that generates the probabilistic outcomes or events – in statistics one refers to any

process where you observe or measure as an *experiment* (rolling a die) and the results obtained as the *outcomes* or *events* (number of points rolled with the die, being e. g. 3 or 5) of the experiment –there strictly seen is no event at all.

Probability is a relational element. It always must come with a specification of the model from which it is calculated. And then to be of any empirical scientific value it has to be *shown* to coincide with (or at least converge to) real data generating processes or structures – something seldom or never done!

And this is the basic problem with economic data. If you have a fair roulette-wheel, you can arguably specify probabilities and probability density distributions. But how do you conceive of the analogous – to speak with science philosopher Nancy Cartwright (1999) – "nomological machines" for prices, gross domestic product, income distribution, etc.? Only by a leap of faith. And that does not suffice. You have to come up with some really good arguments if you want to persuade people into believing in the existence of socio-economic structures that generate data with characteristics conceivable as stochastic events portrayed by probabilistic density distributions.

From a realistic point of view we have to admit that the socio-economic states of nature that we talk of in most social sciences – and certainly in economics – are not amenable to analysis as probabilities, simply because in the real world open systems that social sciences (including economics) analyze, there are, strictly seen, no probabilities to be had!

The processes that generate socio-economic data in the real world cannot *simpliciter* be assumed to always be adequately captured by a probability measure. And, so, it cannot convincingly be maintained, as in REH, that it should be mandatory to treat observations and data – whether cross-section, time series or panel data – as events generated by some probability model. The important activities of most economic agents do not usually include throwing dice or spinning roulette-wheels. Data generating processes – at least outside of nomological machines like dice and roulette-wheels – are not self-evidently best modeled with probability measures.

If we agree on this, we also have to admit that theories like REH, lacks a sound justification. I would even go further and argue that there is no justifiable rationale at all for this belief that all economically relevant data can be adequately captured by a probability measure. In most real world contexts one has to *argue* one's case. And that is obviously something almost never done by practitioners of REH and its probabilistically based econometric analyses.

The conception of randomness in REH

Deep down there is also a problem with the conception of randomness in REH models. In REH models probability is often (implicitly) defined with the help of independent trials – two events are said to be *independent* if the occurrence or nonoccurrence of either one has no effect on the probability of the occurrence of the other – as drawing cards from a deck, picking balls from an urn, spinning a roulette wheel or tossing coins – trials which are only definable if somehow set in a probabilistic context.

But if we pick a sequence of prices – say 2, 4, 3, 8, 5, 6 – that we want to use in an econometric regression analysis, how do we know the sequence of prices is random and *a* *fortiori* being able to treat it as generated by an underlying probability density function? How can we argue that the sequence is a sequence of probabilistically independent random prices? And are they really random in the sense that is most often applied in REH models (where X is called a *random variable* only if there is a sample space S with a probability measure and X is a real-valued function over the elements of S)?

Bypassing the scientific challenge of going from describable randomness to calculable probability by simply assuming it, is of course not an acceptable procedure. Since a probability density function is a "Gedanken" object that does not exist in a natural sense, it has to come with an export license to our real target system if it is to be considered usable. Among those who at least honestly try to face the problem – the usual procedure is to refer to some artificial mechanism operating in some "games of chance" of the kind mentioned above and which generates the sequence. But then we still have to show that the real sequence somehow coincides with the ideal sequence that defines independence and randomness within our nomological machine, our probabilistic model.

So why should we define randomness with probability? If we do, we have to accept that to speak of randomness we also have to presuppose the existence of nomological probability machines, since probabilities cannot be spoken of – and actually, to be strict, do not at all exist - without specifying such system-contexts (how many sides do the dice have, are the cards unmarked, etc.)

If we do adhere to the REH paradigm we also have to assume that all noise in our data is probabilistic and that errors are well-behaving, something that is hard to justifiably argue for as a real phenomena, and not just an operationally and pragmatically tractable assumption. Accepting the usual REH domain of probability theory and sample space of infinite populations – just as Fisher's (1922:311) "hypothetical infinite population, of which the actual data are regarded as constituting a random sample", von Mises' "collective" or Gibbs' "ensemble" – also implies that judgments are made on the basis of observations that are actually never made!

Infinitely repeated trials or samplings never take place in the real world. So that cannot be a sound inductive basis for a science with aspirations of explaining real world socio-economic processes, structures or events. It's not tenable. As David Salsburg (2001:146) notes on probability theory:

> [W]e assume there is an abstract space of elementary things called 'events' … If a measure on the abstract space of events fulfills certain axioms, then it is a probability. To use probability in real life, we have to identify this space of events and do so with sufficient specificity to allow us to actually calculate probability measurements on that space … Unless we can identify [this] abstract space, the probability statements that emerge from statistical analyses will have many different and sometimes contrary meanings.

Just as e. g. Keynes (1921) and Georgescu-Roegen (1971), Salsburg (2001:301f) is very critical of the way social scientists – including economists and econometricians – uncritically and *without arguments* have come to simply assume that one can apply probability distributions from statistical theory on their own area of research:

> Probability is a measure of sets in an abstract space of events. All the mathematical properties of probability can be derived from this definition. When we wish to apply probability to real life, we need to identify that abstract space of events for the

> particular problem at hand … It is not well established when statistical methods are used for observational studies … If we cannot identify the space of events that generate the probabilities being calculated, then one model is no more valid than another … As statistical models are used more and more for observational studies to assist in social decisions by government and advocacy groups, this fundamental failure to be able to derive probabilities without ambiguity will cast doubt on the usefulness of these methods.

This importantly also means that if advocates of REH cannot show that data satisfies *all* the conditions of the probabilistic nomological machine – including e. g. the distribution of the deviations corresponding to a normal curve – then the statistical inferences used lack sound foundations!

Of course one could treat our observational or experimental data as random samples from real populations. I have no problem with that. But probabilistic econometrics does not content itself with that kind of populations. Instead it creates imaginary populations of "parallel universe" and assumes that our data are random samples from that kind of populations. But this is actually nothing but hand-waving! And it is inadequate for real science. As eminent mathematical statistician David Freedman(2009:27) writes:

> With this approach, the investigator does not explicitly define a population that could in principle be studied, with unlimited resources of time and money. The investigator merely *assumes* that such a population exists in some ill-defined sense. And there is a further assumption, that the data set being analyzed can be treated *as if* it were based on a random sample from the assumed population. These are convenient fictions ... Nevertheless, reliance on imaginary populations is widespread. Indeed regression models are commonly used to analyze convenience samples ... The rhetoric of imaginary populations is seductive because it seems to free the investigator from the necessity of understanding how data were generated.

REH and the applicability of econometrics

A rigorous application of econometric methods in REH models presupposes that the phenomena of our real world economies are ruled by stable causal relations between variables. A perusal of the leading econom(etr)ic journals shows that most econometricians still concentrate on fixed parameter models and that parameter values estimated in specific spatio-temporal contexts are *presupposed* to be more or less exportable to totally different contexts. To warrant this assumption one, however, has to convincingly establish that the targeted acting causes are stable and invariant so that they maintain their parametric status after the bridging. The endemic lack of predictive success of the econometric project indicates that this hope of finding fixed parameters is a hope for which there is no other ground than hope itself.

Science should help us penetrate to "the true process of causation lying behind current events" and disclose "the causal forces behind the apparent facts" [Keynes 1971-89 vol. XVII:427]. We should look out for causal relations. But models can never be more than a starting point in that endeavour. There is always the possibility that there are other variables – of vital importance and although perhaps unobservable and non-additive not necessarily epistemologically inaccessible – that were not considered for the model.

This is a more fundamental and radical problem than the celebrated “Lucas critique” has suggested. This is not the question if deep parameters, absent on the macro level, exist in “tastes” and “technology” on the micro level. It goes deeper. Real world social systems are not governed by stable causal mechanisms or capacities. It is the criticism that Keynes [1951(1926): 232-33] first launched against econometrics and inferential statistics already in the 1920s:

> 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.

The kinds of laws and relations that econom(etr)ics has established, are laws and relations about entities in models that presuppose (cf. Chatfield (1995)) causal mechanisms being atomistic and additive. When causal mechanisms operate in real world social target systems they only do it in ever-changing and unstable combinations where the whole is more than a mechanical sum of parts. If economic regularities obtain they do (as a rule) only because we engineered them for that purpose. Outside man-made nomological machines they are rare, or even non-existant. Unfortunately that also makes most of the achievements of econometrics – as most of contemporary endeavours of economic theoretical modeling based on REH – rather doubtful.

Where is the evidence?

Instead of assuming REH to be right, one ought to confront the hypothesis with the available evidence. It is not enough to construct models. Anyone can construct models. To be seriously interesting, a model has to come with an aim, it has to have an intended use. If the intention of REH is to help us explain real economies, it has to be evaluated from that perspective. A model or hypothesis without a specific applicability does not really deserve our interest.

To say, as Prescott (1977:30) that

> one can only test if some theory, whether it incorporates rational expectations or, for that matter, irrational expectations, is or is not *consistent* with observations

is not enough. Without strong evidence, all kinds of absurd claims and nonsense may pretend to be science. When it comes to rationality postulates, we have to demand more of a justification than this rather watered-down version of “anything goes.” Proposing REH, one also has to *support* its underlying assumptions. None is given, which makes it rather puzzling how REH has become the standard modeling assumption made in much of modern macroeconomics. Perhaps the reason is, as Paul Krugman (2009) has it, that economists often mistake “beauty, clad in impressive looking mathematics, for truth.” But I think Prescott’s view is also the reason why REH economists are not particularly interested in empirical examinations of how real choices and decisions are made in real economies. In the hands of Lucas *et consortes*, REH has been transformed from being an – in principle – testable *hypothesis* to being an irrefutable *proposition*.

Rational expectations, the future, and the end of history

REH basically assumes that all learning has already taken place. This is extremely difficult to vision in reality, because that means that history has come to an end. When did that happen? It is indeed a remarkable assumption, since in our daily life, most of us experience a continuing learning. It may be a tractable assumption, yes. But helpful to understand real world economies? No. REH builds on Savage's (1954) "sure thing principle," according to which people never make systematic mistakes. They may "tremble" now and then, but on average, they always make the right – the rational – decision. That kind of models is not useful "as-if" representations of real world target systems.

In REH agents know all possible outcomes. In reality, many of those outcomes are yet to be originated. The future is not about known probability distributions. It is not about picking the right ball from an urn. It is about new possibilities. It is about inventing new balls and new urns to put them in. If so, even if we learn, uncertainty does not go away. As G. L. S. Shackle (1972:102) argued, the future "waits, not for its contents to be discovered, but for that content to be originated."

As shown already by Davidson (1983) REH implies – by the implicit ergodicity assumption – that relevant distributions have to be *time independent*. But this amounts to assuming that an economy is like a closed system with known stochastic probability distributions for all different events. In reality it is straining one's beliefs to try to represent economies as outcomes of stochastic processes. An existing economy is a single realization *tout court*, and hardly conceivable as one realization out of an ensemble of economy-worlds, since an economy can hardly be conceived as being completely replicated over time.

The arrow of time and the difference between time averages and ensemble averages

In REH we are never disappointed in any other way than as when we lose at the roulette wheels, since "averages of expectations are accurate" (Muth 1961:316). But real life is not an urn or a roulette wheel, so REH is a vastly misleading analogy of real world situations. It is not even useful for non-crucial and non-important decisions that are possible to replicate perfectly (a throw of dices, a spin of the roulette wheel etc.).

Time is what prevents everything from happening at once. To simply assume that economic processes are ergodic – *a fortiori* in any relevant sense timeless – and concentrate on ensemble averages is not a sensible way for dealing with the kind of genuine uncertainty that permeates open systems such as economies.

Since ergodicity and the all-important difference between time averages and ensemble averages are somewhat difficult concepts, let me just try to explain the meaning of these concepts by means of a couple of simple examples. Let's say you're offered a gamble where on a roll of a fair die you will get €10 billion if you roll a six, and pay me €1 billion if you roll any other number. Would you accept the gamble?

If you're a neoclassical economist you probably would, because that's what you're taught to be the only thing consistent with being rational. You would arrest the arrow of time by imagining six different "parallel universes" where the independent outcomes are the numbers from one to six, and then weight them using their stochastic probability distribution.

Calculating the expected value of the gamble – the ensemble average – by averaging on all these weighted outcomes you would actually be a odd person if you didn't take the gamble (the expected value of the gamble being 5/6*€0 + 1/6*€10 billion = €1.67 billion).

If you're not a neoclassical economist you would probably trust your common sense and decline the offer, knowing that a large risk of bankrupting one's economy is not a very rosy perspective for the future. Since you can't really arrest or reverse the arrow of time, you know that once you have lost the €1 billion, it's all over. The large likelihood that you go bust weights heavier than the 17 % chance of you becoming enormously rich. By computing the time average – imagining one real universe where the six different but dependent outcomes occur consecutively – we would soon be aware of our assets disappearing, and *a fortiori* that it would be irrational to accept the gamble. [From a mathematical point of view you can somewhat non-rigorously describe the difference between ensemble averages and time averages as a difference between arithmetic averages and geometric averages. Tossing a fair coin and gaining 20 % on the stake (S) if winning (heads) and having to pay 20 % on the stake (S) if loosing (tails), the arithmetic average of the return on the stake, assuming the outcomes of the coin-toss being independent, would be [(0.5*1.2S + 0.5*0.8S) - S)/S] = 0 %. If considering the two outcomes of the toss not being independent, the relevant time average would be a geometric average return of square-root[(1.2S *0.8S)]/S - 1= -2 %.]

Why is the difference between ensemble and time averages of such importance in economics? Well, basically, because when – as in REH – assuming the processes to be ergodic, ensemble and time averages are identical. [Assume we have a market with an asset priced at €100. Then imagine the price first goes up by 50 % and then later falls by 50 %. The ensemble average for this asset would be €100 – because we here envision two parallel universes (markets) where the asset price falls in one universe (market) with 50 % to €50, and in another universe (market) it goes up with 50 % to €150, giving an average of 100€ ((150+50)/2). The time average for this asset would be 75 € – because we here envision one universe (market) where the asset price first rises by 50 % to €150, and then falls by 50 % to €75 (0.5*150).]

From the ensemble perspective nothing, on average, happens. From the time perspective lots of things, really, on average, happen. Assuming ergodicity there would have been no difference at all. When applied to the neoclassical theory of expected utility – which usually comes with REH models – one thinks in terms of "parallel universe" and ask what is the expected return of an investment, calculated as an average over the "parallel universe"? In our coin-tossing example, it is as if one supposes that various "I" is tossing a coin and that the loss of many of them will be offset by the huge profits one of these "I" does. But this ensemble average does not work for an individual, for whom a time average better reflects the experience made in the "non-parallel universe" in which we live.

Time averages gives a more realistic answer, where one thinks in terms of the only universe we actually live in, and ask what is the expected return of an investment, calculated as an average over time. Since we cannot go back in time – entropy and the arrow of time make this impossible – and the bankruptcy option is always at hand (extreme events and "black swans" are always possible) we have nothing to gain from – as in REH models – thinking in terms of ensembles.

Actual events follow a fixed pattern of time, where events are often linked in a multiplicative process (as e. g. investment returns with "compound interest") that is basically non-ergodic.

Instead of arbitrarily assuming that people have a certain type of utility function – as in the neoclassical theory – time average considerations show that we can obtain a less arbitrary and more accurate picture of real people's decisions and actions by basically assuming that time is irreversible. When our assets are gone, they are gone. The fact that in a parallel universe it could conceivably have been refilled, is of little comfort to those who live in the one and only possible world that we call the real world.

REH and modeling aspirations of Nirvana

REH comes 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. But – individuals, markets and entire economies are not planets moving in predetermined orbits in the sky.

To deliver, REH has to constrain expectations on the individual and the aggregate level to actually be the same. If revisions of expectations take place in the REH models, 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 the real world, where fully specified trajectories of future expectations revisions are non-existent.

Most REH models are time-invariant and so give no room for any changes in expectations and their revisions. The only imperfection of knowledge they admit is included in the error terms – error terms that are assumed to be additive and have a given and known frequency distribution, so that the REH models can still fully pre-specify the future even when incorporating these stochastic variables into the models.

In the real world there are many different expectations and these cannot be aggregated in REH models without giving rise to inconsistency (acknowledged by Lucas (1995:225) himself). This is one of the main reasons for REH models being modeled as representative actors models. But this is far from being a harmless approximation to reality (cf. Pålsson Syll (2010)). Even the smallest differences of expectations between agents would make REH 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 – as advocates of REH seem to think – 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 economic theory has to be able to incorporate these empirical facts in its models. Incompatibility between actual behaviour and REH behaviour is not a symptom of "irrationality". It rather shows the futility of trying to represent real world target systems with models flagrantly at odds with reality.

Methodological implications of the critique

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 theoretical economists 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 – like homogeneity, invariance, additivity, etc. – 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.

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.

To some theoretical economists it is deemed quite enough to consider economics as a mere "conceptual activity" where the model is not so much seen as an abstraction from reality, but rather a kind of "parallel reality." By considering models as such *constructions*, the economist distances the model from the intended target, only demanding the models to be *credible*, thereby enabling him to make inductive inferences to the target systems.

But what gives license to this leap of faith, this "inductive inference"? Within-model inferences in formal-axiomatic models are usually deductive, but that does not come with a warrant of reliability for inferring conclusions about specific target systems. Since all models in a strict sense are false (necessarily building in part on false assumptions) deductive validity cannot guarantee epistemic truth about the target system. To argue otherwise would surely be an untenable overestimation of the epistemic reach of "credible" models".

Models do not only face theory. They also have to look to the world. Being able to model a credible world, 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 in one sense 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 in terms of resemblance, relevance, etc.

Robust theorems are exceedingly rare or non-existent in economics. Explanation, understanding and prediction of real world phenomena, relations and mechanisms therefore cannot be grounded (solely) on robustness analysis. Some of the standard assumptions made in neoclassical economic theory – on rationality, information-handling and types of uncertainty – are not possible to make more realistic by "de-idealization" or "successive approximations" without altering the theory and its models fundamentally.

If we cannot show that the mechanisms or causes we isolate and handle in our models are stable, in the sense that when we export them from are models to our target systems they do not change from one situation to another, then they only hold under *ceteris paribus* conditions and *a fortiori* are of limited value for our understanding, explanation and prediction of our real world target system.

The obvious ontological shortcoming of the epistemic approach that REH so well represents, is that “similarity” or “resemblance” *tout court* do not guarantee that the correspondence between model and target is interesting, relevant, revealing or somehow adequate in terms of mechanisms, causal powers, or tendencies. No matter how many convoluted refinements of concepts made in the model, if the model is not similar in the appropriate respects – such as structure, isomorphism, etc. – it does not bridge to the world, but rather misses its target.

To give up the quest for truth and to merely study the internal logic of “credible” worlds is not compatible with scientific realism. Constructing “credible” models somehow “approximating” reality, are rather unimpressive attempts at legitimizing using fictitious idealizations for reasons more to do with model tractability than with a genuine interest of understanding and explaining features of real economies. Many of the model-assumptions standardly made in REH models are *restrictive* rather than *harmless* and could therefore not in any sensible meaning be considered approximations at all.

The modeling tradition of economics – and certainly REH models – may be characterized as one concerned with “thin men acting in small worlds.” But, as May Brodbeck (1968[1959]) had it: “Model ships appear frequently in bottles; model boys in heaven only.”

Why should we be concerned with economic models that are purely hypothetical constructions? Even if a constructionist approach should be able to accommodate the way we learn from models, it is of little avail to treat models as some kind “artefacts” or “heuristic devices” that produce claims, if they do not also connect to real world target systems.

The final court of appeal for economic models is the real world, and as long as no convincing justification is put forward for how the inferential bridging *de facto* is made, “credible” counterfactual worlds is little more than “hand waving” that give us rather little warrant for making inductive inferences from models to real world target systems. Inspection of the models shows that they have features that strongly influence the results obtained in them and that will not be shared by the real world target systems. Building on assumptions such as REH, economics becomes exact, but exceedingly narrow, and in a realist perspective, rather irrelevant. 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.

The theories and models that economists construct describe imaginary worlds using a combination of formal sign systems such as mathematics and ordinary language. The descriptions made are extremely thin and to a large degree disconnected to the specific contexts of the targeted system that one (usually) wants to (partially) represent. This is not by chance. These closed formalistic-mathematical theories and models are constructed for the purpose of being able to deliver purportedly rigorous deductions that may somehow be exportable to the target system. By analyzing a few causal factors in their “laboratories” neoclassical economists hope they can perform “thought experiments” and observe how these factors operate on their own and without impediments or confounders.

Unfortunately, this is not so. And the reason is simple: economic causes never act in a vacuum. Causes have to be set in a contextual structure to be able to operate. This structure has to take some form or other, but instead of incorporating structures that are true to the target system, the settings made in economic models are rather based on formalistic mathematical tractability. In the models – such as those building on REH – they appear as unrealistic assumptions, usually playing a decisive role in getting the deductive machinery deliver “precise” and “rigorous” results. This, of course, makes exporting to real world target systems problematic, since these models – as part of a deductivist covering-law tradition in economics – are thought to deliver general and far-reaching conclusions that are externally valid. But how can we be sure the lessons learned in these theories and models have external validity, when based on highly specific unrealistic assumptions? As a rule, the more specific and concrete the structures, the less generalizable the results. Admitting that we *in principle* can move from (partial) falsehoods in theories and models to truth in real world target systems does not take us very far, unless a thorough explication of the relation between theory, model and the real world target system is made. If models *assume* representative actors, 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 hypothesis of causally relevant mechanisms or regularities can be bridged, are obviously non-justifiable. To have a deductive warrant for things happening in a closed model is no guarantee for them being preserved when applied to an open real world target system.

Conclusion

The financial crisis of 2007-08 hit most laymen and economists with surprise. What was it that went wrong with mainstream neoclassical macroeconomic models, since they obviously did not foresee the collapse or even make it conceivable?

As I have tried to show in this essay, one important reason ultimately goes back to how these models handle data. In REH-based modern neoclassical macroeconomics – Dynamic Stochastic General Equilibrium (DSGE), New Synthesis, New Classical, “New Keynesian” – variables are treated as if drawn from a known “data-generating process” that unfolds over time and on which one therefore have access to heaps of historical time-series. If one does not assume the “data-generating process” to be known – if there is no “true” model – the whole edifice collapses.

Building on REH, modern macroeconomics obviously did not anticipate the enormity of the problems that unregulated “efficient” financial markets created. Why? Because it builds on the myth of us knowing the “data-generating process” and that we can describe the variables of our evolving economies as drawn from an urn containing stochastic probability functions with known means and variances.

This is like saying that you are going on a holiday-trip and that you know that the chance the weather being sunny is at least 30%, and that this is enough for you to decide on bringing along your sunglasses or not. You are supposed to be able to calculate the expected utility based on the given probability of sunny weather and make a simple decision of either-or. Uncertainty is reduced to risk. But this is not always possible. Often we “simply do not know.” According to one model the chance of sunny weather is perhaps somewhere around 10 % and according to another – equally good – model the chance is perhaps somewhere around 40 %. We cannot put exact numbers on these assessments. We cannot calculate means and variances. There are no given probability distributions that we can appeal to.

In the end this is what it all boils down to. We all know that many activities, relations, processes and events are of the Keynesian uncertainty type. The data do not – as REH models assume – unequivocally single out one decision as the only “rational” one. Neither the economist, nor the deciding individual, can fully pre-specify how people will decide when facing uncertainties and ambiguities that are ontological facts of the way the world works.

Some macroeconomists, however, still want to be able to use their hammer. So they decide to pretend that the world looks like a nail, and pretend that uncertainty can be reduced to risk. So they construct their mathematical models on that assumption. The result: financial crises and economic havoc.

How much better – how much bigger chance that we do not lull us into the comforting thought that we know everything and that everything is measurable and we have everything under control – if instead we would just admit that we often “simply do not know,” and that we have to live with that uncertainty as well as it goes. Fooling people into believing that one can cope with an unknown economic future in a way similar to playing at the roulette wheels, is a sure recipe for only one thing – economic catastrophy. The *unknown knowns* – the things we fool ourselves to believe we know – often have more dangerous repercussions than the "Black Swans" of Knightian unknown unknowns, something quantitative risk management – based on the hypotheses of market efficiency and rational expectations – has given ample evidence of during the latest financial crisis.

Defenders of REH, like David K. Levine (2012), maintains that “the only robust policies and institutions – ones that we may hope to withstand the test of time – are those based on rational expectations – those that once understood will continue to function.” As argued in this essay, 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 place the rational expectations hypothesis where it belongs – in the dustbin of history.

Interestingly enough, the main developer of REH himself, Robert Lucas – in an interview with Kevin Hoover (2011) – has himself had some second-thoughts on the validity of REH:

> Kevin Hoover: The Great Recession and the recent financial crisis have been widely viewed in both popular and professional commentary as a challenge to rational expectations and to efficient markets … I’m asking you whether you accept any of the blame … there’s been a lot of talk about whether rational expectations and the efficient-markets hypotheses is where we should locate the analytical problems that made us blind.
>
> Robert Lucas: You know, people had no trouble having financial meltdowns in their economies before all this stuff we’ve been talking about came on board. We didn’t help, though; there’s no question about that. We may have focused attention on the wrong things, I don’t know.

We’re looking forward to see some more future second-thoughts on the subject from other advocates of REH as well. Better late than never.

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