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"i.i.d."? or: On So-called Precise Statements in Economics[1]

1. Confusing expectations and non-homogeneity of actors

1.1 Economics is a quantitative science in two dimensions: money and time. Only insofar as it is a quantitative science may it claim objectivity and some general validity. In other words, only as long as the economist makes basically quantitative statements in terms of money valuations and basically over time may s/he claim to be an expert; as soon as political or other value judgements enter s/he speaks only as one among millions of political decision takers *aka* voters. The analysis of the manifold consequences of political decisions, however, can indeed be part of economic science.

Economics is close to history. But while history typically analyses unique (though possibly recurring) events, economics tends to group events into classes of similarities, so that history becomes a borderline case for economic analysis, and only for that of monetary phenomena. We are now in the midst of the eighth great financial breakdown since 1720, each one historically unique but also – as viewed by economic analysis – sufficiently similar to those that went before.

Economics tends to think of itself as close to the natural sciences and as such widely uses mathematical and statistical methods. But in contrast to the natural sciences proper, economics studies group behaviour, the behaviour of groups of human beings, often of very large groups of humans, as in the case of international financial markets (my main field).[2]

Nonetheless, as a quantitative science of only money valuations over time economics has little in common with sociology.

1.2 On the face of it, economics with its only two dimensions should be much simpler than the four-dimensional physical sciences. But because of the behaviour of a multitude of human beings it is actually rather more complex. The relevant set of human beings may have sharply divergent ideas about money valuations, and the changeability of their ideas can make economic "time" flow at quite different speeds through objective time – differently in fact for every single human being. In other words, as long as we are not dealing with momentary, hand-to-mouth transactions, human expectations over time are the be-all and end-all of economic analysis; and those different expectations are held with varying, often high uncertainty on the part of all the different actors, to boot. And in the monetary field since 1973 we lack any currency which can serve as a notionally unique standard of value.

[1] I owe a large debt to the invaluable critical comments by G. Tichy on the original version of this paper. The same holds for his comments on the essays on the Financial Market Crisis and on Financial Consolidation.

[2] Non-homogeneous group behaviour makes for the intricacy and complexity of economic problems and by necessity entails much more tentative conclusions than those available in the natural sciences proper. As the Nobel Laureate and former member of the Austrian Academy of Sciences Friedrich A. Hayek put it: "Physics is everything which is easily explainable"!

Once put in this way, it is practically self-evident that different individual money valuations, diverse concepts of the time dimension and diverse and uncertain expectations about the (unknown) future have to be taken into account by the scientist as best s/he can. Expectations, often idiosyncratic expectations, become vitally important in a developed world of very complex and long-term financial transactions and with legally guaranteed free international capital movements.

1.3 This problem of economics can be both generalised and discussed on a more theoretical level: While in the natural sciences proper we deal with humanly controlled experiments or, at worst, with natural experiments due to one clear natural change[3], in economics we deal with uncontrolled quasi-experiments, which result from the analysis of many non-understood changes. To deal with that, highly complex econometric methods and econometric reasoning have been developed. Thus, as Leamer pointed out, empirical analysis has merely "transferred findings from one historical experience to a domain in which they no longer applied". Usually lacking is "the admission that the historical data are compatible with countless alternative data-generating models". Economists mix together "thought experiments ... data from non experiments, accidental experiments and designed experiments"[4]. Attempts at too precise predictions are apt to founder on layer after layer of complex preconditions. Therefore, as Hayek put it, economists have to content themselves with mere pattern prediction.[5] Or, as I would put it, economists have to content themselves with statements which are likely to be more or less correct under not very precisely defined "normal" circumstances. And, as Alan Greenspan put it,[6] in his forecasts for policy purposes he constantly had to be hovering somewhere between Knightian risk, where probabilities of correctness can be given, and Knightian uncertainty, where probabilities remain unknown. No clear falsification in the mood of Popper is possible in economics: The realm of uncertainty is huge.[7] Even the availability of an ever increasing volume of data and the ever increasing sophistication of their analysis have not necessarily been helpful: Analysts feel overpowered and uncertainty once more increases.

1.4 And that makes for the topic of this paper: The attempt to derive ever more precise statements about a very complex environment, the attempt at over-generalisation (occasionally attempted in the quest for a Nobel Memorial Prize). This results in conflicting theories, undetected serious mathematical mistakes, assumptions far off any empirical reality and undefined fields of applicability. But in contrast to the natural sciences proper, tests for economic theories cannot be constructed; one has to wait, sometimes for many years, for the (at least more or less) appropriate economic conditions to come about. Thus, economics brims with remarkable conclusions whose invalidity is discovered only much later.

In monetary and financial economics, and especially in the case of unhindered international capital movements, expectations about future developments figure large and there is no way to coordinate divergent expectations about future financial developments. He who sells a given financial asset typically believes that it will fall in value, and he who buys it typically assumes the contrary, viz. that it

[3] As was the case in an example noted by Ch. Sturmbauer, where Lake Victoria received a huge inflow of water from a river system, which changed the fish population completely. See Ch. Sturmbauer, "Die Seen Ostafrikas und ihre Buntbarsche", Biologie in unserer Zeit, vol. 30 (6), 2000, pp. 353–364. Sturmbauer notes in his "study of adaptive radiation": "Speciation rates in cichlid radiations are stunningly rapid, possibly amounting to one new species every 50 years." (p. 362).

[4] Edward E. Leamer, "Tantalus on the Road to Asymptopia", Journal of Economic Perspectives 24/2, Spring 2010, pp. 31–46, here in succession p. 36, p. 38 and p. 44.

[5] F. A. Hayek, "The Theory of Complex Phenomena"; M. Bunge (ed.), *The Critical Approach to Science and Philosophy.* In Honor of Karl R. Popper, Glencoe Ill., pp 332–349.

[6] A. Greenspan, "Risk and Uncertainty in Monetary Policy", American Economic Review, Papers and Proceedings, 94/2, May 2004, pp. 33–40, here p. 36f.

[7] F. H. Knight, *Risk, Uncertainty and Profit,* New York, 1921.

will rise. In fact, the market for futures will function only if there is such a divergence of expectations. Keynes rightly stressed its importance: "It is interesting that the stability of the system and its sensitiveness to changes in the quantity of money should be so dependent on the existence of a variety of opinion about what is uncertain"[8]. Such a variety of expectations about the future is typical of a state of relatively high economic development. But it is difficult to model and results in conflicting theories.

1.5 The centrality of expectations distinguishes economics from all other life sciences. But even more: the analysis of human action may influence those actions. In certain branches of economics practical actors learn, often quickly, what theory has to say, and act accordingly. The early Nobel Laureate Milton Friedman was the first economist whose ability to change economic behaviour by his teaching was noted. In fact, R. Klump has pointed out[9] an instance where Friedman's analysis of a certain type of market behaviour had got the facts wrong, but changed behaviour in such a way that his analysis became correct: He explained interest rates above all in nominal terms by expectations of inflation, and that changed market behaviour for decades.

So far such learning processes activated by economic analysis have been noted only in financial markets and in monetary matters, which in a financially global world with free capital movements become rather intricate[10]. And it was the most fundamental consequence of the Crash of 2008 that behaviour in financial markets, which had been learned from a certain type of economic analysis, was suddenly perceived to be wrong, with actors switching to the opposite behaviour – although that, too, was notably influenced by an economist, viz. Lord Keynes: So far, actors had thought in terms of an economic "equilibrium" rapidly reached and had thought at best in terms of risk, i. e. cases where all possible "states of the world" and the probability of the occurrence of those various states are known. Suddenly one reverted to the never fully analysed world of Lord Keynes with the likelihood of long-run "disequilibria" and uncertainty (instead of risk, which, in contrast to uncertainty, is quantifiable). Keynes' analysis, however, is short-run and what it leaves out are the long-run consequences of actions.

2. Contradictions in exchange rate theory

2.1 My first example of fundamental economic confusions is one where the different aspects of the financial markets analysed just do not fit together, so that we are left with serious contradictions. Can there be anyone who would assume for international capital markets that millions of different actors in various countries and environments will see the future, and often the very distant future in quantitatively the same terms, especially as there are often no invariant future prices to guide them? Unfortunately, there is an influential strand in present-day – above all US-American – economic thinking that takes all economic outcomes to be the result of the decisions of one single all-understanding "rational" actor.

This makes for one of the major methodological puzzles in present-day economics. The puzzle is, in fact, a not untypical example of a teaching tradition prevailing over established facts: It is fairly easy to calculate and to teach what prices and quantities will result from bargaining in a small well-understood and isolated market with mostly traditional behaviour. Some of those "teachers" go on to

[8] J. M. Keynes, *The General Theory of Employment Interest and Money,* London 1936, p. 172.

[9] Remark of R. Klump during discussion of his paper "Geldzins, Güterpreise und das Gibson Paradoxon: Besonderheiten und Implikationen des Erklärungsansatzes von Kurt Wicksell"; in: *Studien zur Entwicklung der ökonomischen Theorie* XVIII, E. Streissler (ed.), Berlin 1998, pp. 39–62.

[10] See St. F. Le Roy, "Efficient Markets and Martingales", Journal of Economic Literature, Dec. 1989, XXVII, pp. 1583–1621.

conclude that things must be more or less the same on more complex markets, e. g. on the hazy and huge financial market of Euros against US-dollars. As an instructive exercise I suggest you consider how the recent political difficulties and uncertainties in a tiny country, Greece, have affected the Euro-Dollar exchange rate: they have certainly done so out of all proportion. Does that impress you as an effect of one unitary rationality?

2.2 Let us look at present exchange rate modelling. On the one hand, an exchange rate can be explained as determined on the goods market, the market for exports and imports. This explanation is called the purchasing power parity theory. It states: If home and foreign have different rates of inflation, then one is indifferent over time between buying at home or in foreign only if and when the difference in inflation rates is just compensated by changes in the exchange rate. If, e. g., foreign has a higher rate of inflation than home, this difference has to be exactly compensated by a corresponding depreciation of the foreign currency. When one buys in foreign one sees that foreign prices have risen more over time because of the inflation, but one acquires the foreign currency correspondingly cheaper on the market. But at present, inflation differences within the developed world amount to at most one per cent a year, therefore exchange rates should be practically constant, changing just by that one per cent a year.

The exchange rate is also determined on the market for financial capital. Here, the equally plausible theory is called the uncovered interest parity theory. It says that in order for one to be indifferent between letting one's financial capital "work" at home or in foreign, any difference in interest rates between the two currency areas must be exactly compensated by changes in the exchange rate. E. g., if foreign offers 3 per cent higher interest rates than home, then the foreign currency must depreciate by just these 3 per cent in order for one to be indifferent between investing at home or in foreign. Lately, however, in most countries of the highly developed world, long-term interest rates (i. e. on ten year government bonds) have differed within a range of less than one per cent only, therefore exchange rates should not change, or again only by one per cent a year.

Thus, because of similar goods market inflation rates and similar interest rates within the developed world, in particular between the USA and core Euro-Europe, the exchange rate of the Euro against the dollar should not change. But since its introduction in 2002 the Euro has appreciated by some 80 per cent if we look at its peak value in the summer of 2008; or by some 40 per cent if we look at the low point of the Greek scare in May 2010. It has thus appreciated on average by at least 5 per cent a year or up to 10 per cent a year; thus it is certainly not nearly stationary, as both uncovered interest parity theory and purchasing power parity theory would have it. And its annual fluctuations are quite large: in a few months about the beginning of 2010 the Euro-Dollar exchange rate dropped by 15 per cent[11].

2.3 On the other hand, a counterfactual stationary climate for exchange rates is assumed in many long-term financial contracts, where forward exchange rates hardly differ from spot rates. For the most advanced economies, exchange rates pegged to the US-dollar ended in March 1973. Now, however, 38 years later, this past exchange rate regime still tacitly dominates much of long-term financial market reasoning! The dollar is considered by many, in particular financially unstable countries, e. g. Russia, Argentina etc., not only as a "safe" haven, but also as a currency with an in-the-long-run stable value. Because of this belief the dollar shows a long-term (10 year) interest rate on government

[11] Actually purchasing power parity and uncovered interest parity theory would contradict each other, unless real interest rates, i. e. inflation corrected interest rates, do not differ between currency areas. For "PPP" says that the difference in the change of real exchange rates should be zero, while "UIP" says that this difference should equal the difference in real interest rates. Amusingly, at present when real interest rate differences hardly exist and thus the condition for conformity of the two theories is more or less fulfilled, both theories are, as pointed out, empirically overthrown particularly often.

bonds only one fifth of one per cent higher than that of Germany – notwithstanding the fact that its relative annual depreciation against the Euro of at least 5 per cent (or up to 10 per cent) should have resulted in an interest rate at least 5 per cent or even 10 per cent higher – according to the theory of uncovered interest parity.

Analysts are faced with a basic contradiction: Prevalent theories, e.g. uncovered interest parity, visualize a long-established stationary probability system from which an unchanging mean value, the long-run unchanging average exchange rate, is well-known to all economic actors. This assumed unchanging and commonly understood mean value provides the informational basis for assumed "equilibria". Actually, however, because of relatively rapid changes in markets we have to deal with constantly shifting economic constellations, which have not yet settled into any kind of equilibrium and are not fully understood by economic actors; and this is typically so for all macroeconomic constellations and, in particular, for international financial markets. Furthermore, the more – heterogeneous – actors there are in the world's markets, the less likely is it that all of them hold the same theories.

2.4 This becomes glaringly obvious when one has to explain fundamental economic theories like the above exchange rate theories to students and feels obliged at the same time to relate those theories to present-day empirical facts and figures. Right away, one stumbles on crass inconsistencies. It is a big challenge for a teacher plausibly to reconcile established theory and rapidly changing facts. In attempting such a reconciliation, not only students learn, teachers do, too. My experience in over half a century of university teaching is: *Docendo discimus.* We learn by teaching. Unchecked by facts, economic theory is insufficient. It is fascinating to see how many facts assumed for decades are by now outdated, e.g. the implicit assumption that all international capital movements just provide short-run finance for exports and imports. Examples of theories or theorems abound that are hailed and applauded for a long time before being put to the test of reality – and dismally failing.

Thus we arrive at the central topic of this paper: economics tries to be a quantitative science and a quantitatively correct science. But in times of fairly rapidly changing environments it tends to become a quarry of relics of quite different past circumstances. The attempt at precision in theory often leads to notions that are historically explainable and were correct once, but do not apply to the present.

3. Even Nobel Laureates in economics are fallible

3.1 Among the first cases of such conflict between theory and empirical facts was Milton Friedman's central notion that profitable speculation in markets for durable goods or for assets would reduce intertemporal price variation and would thus be price-stabilizing, a notion Friedman developed in 1950 and published in 1953[12]. His highly plausible argument for price stabilization by speculation ran as follows: If buying a commodity, holding it for a time and then selling it is to be profitable, purchase has to be made at a lower price than the eventual selling price. Therefore the experienced speculator will buy an asset when the price is temporarily lower than the average price. But by buying when the price is below average and thereby creating additional demand the speculator drives the price towards the average, thus reducing intertemporal price variations; i.e. he acts in a price-stabilizing way. On the other hand, the experienced speculator will sell when the price is above average, pushing the price down towards the average, once more reducing intertemporal price variation. Ergo: Profitable speculation is price-stabilizing by reducing intertemporal price fluctuations.

[12] M. Friedman, "The Case for Flexible Exchange Rates"; in: M. Friedman, *Essays in Positive Economics,* Chicago and London 1953, pp. 157–203. Especially in II A 3, "Speculation in foreign exchange markets will be destabilizing", Friedman argues exactly to the contrary of that caption.

But actually this plausible model, a favourite of many economists, is wrong and extremely misleading – for a number of reasons:

(1) It is true only for a stationary probability system with an unchanging mean value well-known to the actors. (Note the tacit assumption of a well-known average price in the above argument.) In fact, postulating an equilibrium in any model with a time dimension assumes a long-run, well understood stationary climate of the generating system. Friedman was soon advised that his model had actually been advanced already by John Stuart Mill in 1848 for the special case of the British wheat market[13] – for which the model was correct, because then supply changes were only caused by variations of the weather, a fact generally known. Demand was also stationary over time, or rather: In the long run, because of technical progress, supply increased in step with demand increase due to population growth. But a stationary probability system is not enough.

(2) The pricing function relative to the quantity must also be linear[14]. Otherwise, with a monotonically convex or concave price function relative to quantity the reduction of the remaining quantities on the market over time would change the average price, which had been assumed to be constant.

(3) Even worse, there has to exist a well-known average price. But there is none in the case of a random walk of prices, the random walk being highly relevant for the exchange rate of the dollar, for which Friedman used his argument in particular: With a random walk an average price does not "exist" in the sense that the variance of the average goes to infinity over time or, to put it differently, the best estimate of the average keeps changing with every new price, this best estimate being just the value of each past price quotation.

(4) Perhaps of least importance: If the volume of speculation becomes very large, it would nearly or even completely eliminate all price variation so that speculation would become unprofitable. So we arrive at a contradiction: If the volume of speculations becomes very large, there is no longer any reason for speculating, and speculation has to cease. The price would always be pushed to the average price or nearly so, and there would be no profit left in speculating. There would be no stationary average towards which speculation could move. But the lack of speculation would once more increase the need for it and we would arrive at a perpetual creation and then self-destruction of speculative behaviour.

3.2 The biggest problem of all is the first one in conjunction with the third: Does there exist a stationary probability system with an unchanging mean value, well known to the actors? Friedman's argument about stabilizing speculation was only subsidiary to his plea for flexible exchange rates[15] (rates determined freely on the markets – in contrast to fixed exchange rates, which are set by political agencies). So when in March 1973 the US-dollar left its former fixed value and was allowed to float, the majority of US academic economists joined Friedman in expecting the resulting exchange rate to be highly stable. Actually, in March 1973, everyone knew that the US dollar was overvalued but no one knew by how much. Thus, the appropriate mean value was not known, and due to the rapid large changes in basic economic parameters it has never been found out in the 38 years since. How do you learn in a rapidly changing system? Actually, as already pointed out, markets still expect (contrary to the facts) that in the very long run the dollar value relative to the Deutsche Mark, later – since 1999 – renamed the Euro, would remain at the value it had had before 1973. In the short run, however, they expect a constantly changing real dollar value. Short-run and long-run expectations are in conflict!

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[13] J. St. Mill, *Principles of Political Economy* with Some of their Applications to Social Philosophy, London 1848, book IV, ch. II, § 4,5.

[14] This has been shown by G. Orosel, "Profitable Speculation and Price Stability", Jahrb. Nationalökon. Statist., 1999 (6), November 1984, pp. 485–501.

[15] See the title of his essay given in FN 12: "The Case for Flexible Exchange Rates"!

Anyhow, contrary to Friedman's expectations the average exchange rate variations of the various currencies did not decrease with floating rates, but rather, as Mussa (1986)[16] showed, increased by three to eight times. Speculation which, with a known mean value, might be stabilizing, tends much rather to be destabilizing when the mean is unknown. And furthermore, the type of monetary policy then matters especially in the long run. In 1953, Friedman had explicitly assumed that the economic world shows stationary probabilities, while at least in monetary matters little-understood frequent changes are evident, largely due to international capital movements.

Markets still have not realized that the type of monetary policy does matter for the long-run real value of an exchange rate. E. g., with the D-Mark and the Euro the sole legally fixed long-run policy aim is the prevention of inflation. Since Alan Greenspan (governor of the Federal Reserve from 1987 to 2006), the policy pursued for the US dollar has been aiming at the prevention of unemployment (without success since 2008) and the stimulation of economic growth. With fundamentally different policies in the respective currency areas, the assumption that the relative prices are everywhere stationary (i. e. a stationary exchange rate) is highly unlikely.

3.3 While Friedman would never admit his mistake about the (non-)existence of a stable mean value, the mistake of another Nobel Laureate, Edward C. Prescott, was quite primitive and was furthermore corrected by himself – although 18 years later, when his Nobel Prize, partly due to the resonant acclaim of the original methodologically incorrect article, was already assured. Typically, US-American theory looks at market prices unadulterated by government action. In other words: Effects of taxation are simply ignored. Also, for the sake of mathematical convenience, preferably only costs proportional to the quantity bought and sold are considered, while costs that are independent of those quantities – so-called "fixed costs" – are ignored: They would lead to considerable non-linearity in models. But actually the prevalence of such fixed costs increases over time.

Let me sketch that mistake: The "equity premium puzzle", presumably uncovered by Mehra and Prescott (1985)[17], was that "the average return on 'U.S. common' stock has far exceeded the average return on short-term U.S. government debt ... the average difference was 6.2 per cent per year in the 1889–1978 period". "They tried to account for this difference by construing it as a premium for bearing non diversifiable aggregate risk, but found that risk accounts for only a tiny fraction of the difference"[18]. The first problem is that Mehra and Prescott took as their measuring rod not the long-term but the short-term interest rate on US government debt, which is a rate relevant only for banks and similar institutions, while practically irrelevant for investors in stocks and shares, i. e. "equities". But Mehra and Prescott had also forgotten about taxation and the fixed costs of holding equity capital: "Taking into account some factors ignored by Mehra and Prescott (taxes, regulatory constraints, and diversification cases) and focussing on long-term rather than short-term savings instruments", the puzzle is gone, "the average real debt return [is] almost 4 per cent and the average real equity return somewhat under 5 per cent", the difference very modestly being "less than 1 per cent", easily accountable for by the higher risk of equity capital.

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16 M. Mussa, "Nominal Exchange Rate Regimes and the Behavior of Real Exchange Rates: Evidence and Implications", Carnegie- Rochester Conference Series on Public Policy, vol. 25 (1986), pp. 117–213. See also already M. Mussa, "Empirical Regularities in the Behavior of Exchange Rates and Theories of Foreign Exchange Markets", Carnegie-Rochester Conf. Ser. on Publ. Pol., vol.11 (1979), pp. 9–57.

17 R. Mehra & E. C. Prescott, "The Equity Premium: A Puzzle", Journal of Monetary Economics, 15 (2), March 1985, pp. 145–161.

18 This quotation and all the quotations following in this paragraph are taken from E. R. McGrattan & E. C. Prescott, "Average Debt and Equity Returns: Puzzling?", American Economic Review PP 93/2, May 2003, pp. 392–397, here p. 392.

Thus, Prescott had corrected his own mistake just before receiving the Nobel Prize one and a half years later, in October 2004. But actually some other puzzles remain: Equity returns were much lower than average in the 1970s: Evidently, referring back to the discussion of Friedman, average investors take very long to learn in how far averages in returns change.

And the average returns of stocks went up quickly from the later 1980s until 2000: Average investors thus tended to overestimate the effect of booms as well. "The high volatility of stock market returns is puzzling". There is an "excessive stock price volatility puzzle"[19]. Thus, it is not only mean values that are misjudged for long periods by market actors, but also variances. "Low volatility of productive capital returns" and "high volatility of stock-market returns" show that (information-wise) similar markets are not well coordinated. We are forced to use different theories for very similar economic phenomena.

3.4 While it is well-known that, as above, stock market prices and their changes are closely linked with what might be called the business cycle, be it as to its effect or its cause or both, the next Nobel Prize winner to be discussed, Robert E. Lucas, Jr., views the business cycle quite differently – and extremely narrowly so: He presents[20] "a simple [?] example of ... the central feature of the [?] modern [?] business cycle: a systematic relation between the rate of change in nominal prices and the level of real output"[21]. Typically, Lucas considers an extreme version of his and his Chicago colleagues' theorizing as "the" sole possible theory and, of course, the only one that is "modern". Reading on, we find that he thinks he "provides an explicit elaborate example ... of the characteristics attributed to the U.S. economy by Friedman"[22]. Maybe so. Looking closer into his model, we realize that Lucas assumes a given and unchanging demand for money and that, actually, by only one single person (for, what Lucas considers as "rational" action is only one unique possibility of action). The only thing that varies is money supply, in a stochastic way. That basically presupposes a dictatorial central bank and is at variance with any democratic regime controlling it. Lucas also assumes a goods market with a given aggregate supply function with only stochastic variations of output. Traditionally, this would have been assumed as the effect of harvest variations, but with Lucas it is due to the chance variation of the number of inhabitants – in its turn due to something (by assumption) close to immigration or maybe to variations in the labour supply of each individual depending on the expected "harvest". Apart from these stochastic variations aggregate supply – of only one single commodity, the "consumption good" – is stationary over time: There is only one factor of production: labour; and workers do not learn any new skills over time. In fact, after the beginning of the period individuals do not learn anything, while living in two different states: first as workers and income earners (in the real world perhaps for about 40 years) and then as old-age spenders of their savings and in addition of anything the government (whose sole activity is the creation of additional money) might add in proportion to those savings (in the real world for about 20 years). During those 20 or 40 years individuals learn nothing of economic relevance (for which reason I call Lucas' economy a "fruit fly" economy – fruit flies being organisms that live for a few days only, during which they learn virtually nothing).

What about money demand? Here Lucas presents us with a remarkable sleight of hand. He says[23]: "In the next section we show [!] that" Lucas' central equation (4.2) "has a unique [!] solution". Actually at the bottom of that p.lll he says: "It is a plausible conjecture [?] that solutions to (4.2) assume the form ..." and then writes down one version of the quantity theory of money. His equation would have an <u>infinite</u> number of solutions, but Lucas <u>assumes</u> only <u>one</u> "unique" solution, because that is

[19] McGrattan-Prescott (2003), loc.cit. p. 396.

[20] R. E. Lucas Jr., "Expectations and the Neutrality of Money", J. Eco. Theory, 4 (2), April 1972, pp. 103–124.

[21] Lucas (1972), loc. cit. p. 103.

[22] Lucas (1972), loc. cit. p. 104

[23] Lucas (1972), loc. cit. p. 111.

"a plausible conjecture", a mathematical hocus-pocus that has one shuddering: There is one unique money demand function assumed, and that is the money demand of the by now more than 300 years old quantity theory of money.

But that equation is only a statement of a final equilibrium under special circumstances and an equilibrium which David Hume in his extensive treatment in 1752[24] – Lucas quotes him copiously in his Nobel Lecture of 1995[25] – had called (not quoted by Lucas) a state "not easily to be accounted for"[26]. The question is whether there are equilibrating tendencies at all and how and when an equilibrium is reached! Thus, Lucas assumes (a) a certain form of money demand as "a plausible conjecture", (b) production of the "consumption" good with labour only, therefore (c) no capital which would be the link between past and future decisions in a capitalist economy. The not-considered capital would embody information most relevant to economic change, i. e. innovation in the production process. Thus, Lucas once more uses an essentially stationary model (and a rather primitive one, at that) to describe development!

Finally, the very essence of his model, the money side of it, is at least as primitive as the production side without capital or innovation: Lucas knows only cash, thus leaving out the money multiplier, which would show how banks change the money supply and, in addition, how individuals vary their demand over the business cycle between different types of money. Thus, even the money supply is wrongly specified. And the production process – quite lengthy in reality – remains non-financed with Lucas: The consumption sector, the sole production sector with him, is paid only at the moment of the eventual sale of the consumption good.

Lucas' main point, that individuals will have difficulties in deciding what actually happens when there is a mixture of "real" and monetary changes is not relevant when monetary changes per time period are so evidently much larger, become effective more rapidly, and are measured more precisely. Or would anybody suggest that Greece's problems of 2010 were mainly due to Greek workers suddenly asking for more leisure time?

Could the Lucas model perhaps be employed for an elucidation of the secular US boom of 1995–1999? Hardly, as that boom was due above all to a sectoral shift in production to computer hardware and software, a "new" sector in a double sense: an innovative sector that increased greatly in volume and was so profitable because it needed very little capital (a highly "capital saving" innovative sector). Could the Lucas model explain the US crash of the year 2008? Not at all: Lucas ignored all relevant real-world problems. In his model there is nothing like a "bankruptcy", there is no faulty investment due to an overestimation of expected future price increases, as in the housing sector, and there are no banks that have completely miscalculated their financing decisions. There is no over-stimulation by monetary policy, as in the USA in the autumn 2008,because with Lucas there is no monetary policy. And above all, in Lucas there is no other money than the US-dollar, there cannot be a huge current account deficit, there is no effect of foreign finance, there are no international capital movements: "Forget the 75 years between 1914 and 1989", was the advice of Robert Mundell;[27] so perhaps Lucas 1972 should best be forgotten, too. But was he not forgettable even before 1989, since he did not at all present, as he claimed, "the central feature of the modern business cycle"?

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[24] D. Hume, *Political Discourses,* Edinburgh 1752.

[25] R. E. Lucas Jr. "Nobel Lecture: Monetary Neutrality", Journal of Political Economy 104 (Aug. 1996), pp. 661–682.

[26] Hume (1752), loc. cit, Discourse III, "Of Money", p.47.

[27] R. A. Mundell, "A Reconsideration of the Twentieth Century", American Economic Review 90/3, June 2000, pp. 327–340, here p. 327.

3.5 The fundamental methodological problem is this: The question of the empirical relevance of a model is normally not raised at all in present-day US economics. Its exponents are immune against criticism: Any critic would simply be told to improve his own knowledge of economics by coming to study in the USA! American economic theory is above all propaganda for one of the country's largest industries: university studies – not least in Lucas' Chicago.

While the problem with the ideas reviewed so far is that they assume precisely defined equilibrium situations with precisely defined, but wrongly specified complete or near-complete information, there has also existed – for nearly half a century now – a path-breaking literature on various types of incomplete information situations. That literature also depends on very fine differences in its assumptions, differences that make for considerable differences in their results. This strand of thought started already with the first pertinent model by the 1981 Nobel Laureate, George Stigler, "The Economics of Information" (1961)[28]. Stigler raised the question of the search for the best price for a given product. He assumed, e. g., constant marginal search cost for price, but falling marginal gains in terms of ever smaller price reductions the longer one searches. The optimum is reached when (in the last search step undertaken) marginal search cost just equals marginal lowering of the price. Thus Stigler determines the <u>optimal quantity</u> of search or the optimal number of search steps to undertake.

By the time he was awarded the Nobel Prize, standard opinion actually considered Stigler's solution wrong. For if, e. g., one happens to be offered a very good (=low) price for the intended purchase, why should one search on and not accept that first offer? Soon, standard opinion had changed again and took Stigler to be correct – although for one special problem: When (those were pre-Internet-times!) one searched by mail for the best price offer, then one searched in Stigler fashion with the need for determining the optimum number of letters to be mailed. If, however, one searches by actually visiting one shop after another in order to ask for the price of the desired good one will <u>optimally determine the acceptance price,</u> and the quantity of search, the number of shops one visits, will become a random variable. The question thus is whether one receives additional information in each search step or not.

3.6 In this case two correct optimum solutions for two slightly different problems were found. Quite otherwise in the case of the Nobel Prize winning article of Joseph E. Stiglitz (Nobel Laureate of 2000), "Credit Rationing in Markets with Imperfect Information" (co-authored by A. Weiss)[29]: 28 years after its publication it was shown that the solution suggested for the optimal credit policy of banks was actually non-existent! Stiglitz is famous for his exceptional inventiveness, but just as famous for never fully working out his ideas and depending on varying co-authors for that. In this case we find at the very beginning of the article in Figure 1 the picture of a nicely domed = "hump-shaped" curve with a unique maximum with the caption: "There Exists an Interest Rate which Maximizes the Expected Return to the Bank"[30], the existence of such a maximizing interest rate being again and again assumed, but never proved. In the meantime, it has been shown that the assumed unique maximum does not exist.

This proof was given in a paper by Arnold and Riley[31]. Note that Arnold teaches in Regensburg, Germany: He is not a US-American insider. Arnold and Riley show that there will be a "nonmonotonic [!] expected lender revenue function"[32]. They conclude: "For the central model analyzed by

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[28] G. J. Stigler, "The Economics of Information", Journal of Political Economy 69, June 1961, pp. 213–225.

[29] J. E. Stiglitz & A. Weiss, "Credit Rationing in Markets with Imperfect Information", American Economic Review 71 (3), June 1981, pp. 393–410.

[30] Stiglitz & Weiss (1981), loc. cit. p. 394.

[31] L. G. Arnold & J. G. Riley, "On the Possibility of Credit Rationing in the Stiglitz-Weiss Model", American Economic Review 99 (5), Dec. 2009, pp. 2012–2021.

[32] Arnold & Riley, (2009), loc. cit., p. 2014.

[Stiglitz and Weiss] there can never [!] be a hump-shaped expected profit function for borrowers, and so rationing with a single [!] equilibrium loan rate is impossible. Instead, either the expected profit function increases with the borrowing rate and there is no rationing, or there must be at least two [!] turning points. High-risk borrowers demand loans at high interest rates, so any equilibrium with rationing will involve market clearing at a high loan rate and rationing at a second lower rate"[33]. Thus, potential customers not served at the low interest rate (when there is rationing at that rate) will indeed be offered credit, but at a much higher interest rate, while no credit will be given at the in-between interest rates. What Arnold and Riley show contradicts the very definition of credit rationing by Stiglitz and Weiss[34]. The latter defined an economic situation that can <u>never exist</u>: "We reserve the term rationing for circumstances in which either (a) among loan applicants who appear to be identical some receive a loan and others do not, and the rejected applicants would not receive a loan even if they offered to pay a higher interest rate; or (b) there are identifiable groups of individuals ... who ... are unable to obtain loans at any interest rate, even though with a larger supply of credit, they would". The former case was shown by Arnold and Riley not to exist, and the latter case remains unanalysed.

3.7 There is also a deeper methodological problem buried in the Stiglitz-Weiss model of credit rationing. We had long been familiar with the phenomenon that certain <u>well-distinguishable types</u> of potential bank customers do not get credit, particularly after a financial crash and in an economic depression. It is a commonplace that among applicants known to be different not everyone is necessarily served. For the market for used cars the Nobel Laureate Akerlof (1970)[35] had shown that under specified conditions, without legal safeguards and with asymmetric information of the two sides of the market (= prospective customers being less informed than suppliers), the market would break down while with equal information of both sellers and prospective buyers it would function satisfactorily. Stiglitz tried to show (without success, as it turned out) that in the case of credit rationing – this time the "suppliers", the banks, being less well informed how their customers are going to use their credits – there would be a <u>partial market breakdown</u>, some applicants receiving loans, but otherwise undistinguishable others getting nothing. The fascination of the model sprang from the idea of only a partial satisfaction of demand.

Actually, it is less important that theoretically this case, as modelled, could not exist, than that in practice this case is most unlikely anyway: Is it not weird to assume that a bank so far has had no experience of giving credit and that all the customers showing up are essentially first time customers without a credit history?

As Dell'Ariccia et al., 2008, have shown that was only plausible for <u>investment</u> banks which entered the housing market only late and then got the worst, mostly first time applicants.[36] But then it was highly incautious to charge into an unknown market, and for established banks to finance newcomer banks.

Even about new customers established banks will have some easily available information, above all whether the applicant's firm is big or small. Small firms are more likely to be rationed than large firms. For the present US crisis a recent empirical study[37] has shown that out of the 75 per cent of small firms in a sample 22 per cent said they were "very affected" by credit constraints, while only 16

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33 Arnold & Riley, (2009), loc. cit., p. 2019.

34 Stiglitz & Weiss, (1981), loc. cit. 394 f.

35 G. A. Akerlof, "The Market for 'Lemons': Quality Uncertainty and the Market Mechanism", Quarterly Journal of Economics 84 (3), Aug. 1970, pp. 488–500.

36 I owe the reference to Dell'Ariccia to G. Tichy: Dell'Arricia et al, 2008, Credit booms and lending standards: Evidence from the subprime mortgage market, Centre for Economic Policy Research Discussion Paper No. 6683.

37 Nber Digest April 2010, "The Real Effects of Financial Constraints".

per cent of (the 25 per cent) large firms said so. As I know from practical experience with large firms that were turned down, a number of them were actually already bankrupt, some without being aware of it (or at least pretending to know nothing about it).[38]

The other criterion would be that firms that have a well-established longer-term business connection with a bank (and are therefore known to the bank) will get credit while unknown new applicants will be turned down. It is for this reason that well-established firms will hold a credit account or at least provide themselves with a credit line with one bank – if not with several banks at the same time. But the typical disapppointment is that in a recession when firms sorely need credit or more credit, credit is cut. The most important case of credit rationing, neither analysed by Stiglitz-Weiss nor in the critical article by Arnold-Riley, is that firms do receive bank credit but often much less credit than would be optimal for them, with the macroeconomic consequence that recovery from a recession is considerably postponed. And in the recent US "subprime" credit crisis the problem was actually just the opposite: too many credits were granted, not too few.

3.8 Finally let us have a look at the influential article by Nobuhiro Kiyotaki and Randall Wright, "Money as a Medium of Exchange"[39]. The authors look at a non-centralized economy, where trades are achieved only occasionally. By assumption there are at least three individuals, each producing one of three goods but consuming one of the other two. In addition to useful goods there is intrinsically worthless money on the market. Goods are distinguished by the amount of storage cost per period and by their likelihood of being accepted ("acceptability") by trading partners. Money has zero (or at least the lowest) storage cost and the highest acceptability, so that, though by itself useless, it is accepted in order to get the desired good more quickly, i. e. at less search cost. The set-up is thus very close to that of C. Menger (1871)[40], who had modelled money as the commodity with the highest marketability or saleability, as is mentioned by Kiyotaki-Wright[41]. Menger had used the term "Marktgängigkeit". It is interesting how old ideas sometimes crop up again and again.

In the model it is assumed that individuals are always fully provided with only one commodity at a time or that only money is held. Thus the possible states of the world are finite and countable, which makes analysis (comparatively) easy. Furthermore, it is assumed that the economy has already reached and remains in "a steady state Nash equilibrium"[42]. The analysis only of equilibria without considering whether they can actually be reached is not untypical and the above 0/1 analysis of storage appears a legitimate simplification. But unfortunately neither one nor the other assumption was critically investigated.

Fifteen years later Shevchenko and Wright[43] – one of the original authors plus a new collaborator – showed that using up all one's assets that were held in the form of one single commodity (or as money) would make for an unstable equilibrium that could never be reached. To establish this the authors used a deeper mathematical analysis: topological fixed point analysis. Happily, they also found an alternative solution to the one to be rejected: In order to save the basic idea of the model,

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[38] How can one estimate a minimum credit worthiness? G. Tichy names as such an estimate the FICO scores, a mechanical projection of the credit risk based on customers' previous records, named after the designing firm, the Fair Isaac Corporation, FICO for short.

[39] N. Kiyotaki & R. Wright, "On Money as a Medium of Exchange", Journ. of Political Economy 97 (4) Aug. 1989, pp. 927–954.

[40] C. Menger, *Grundsätze der Volkswirthschaftslehre,* Vienna, 1871, ch. 8, ed. 1968, pp. 249, 252. The same, *Principles of Economics,* New York, London, 1976, pp. 256, 259.

[41] Kiyotaki & Wright (1989), loc. cit., p. 935.

[42] Kiyotaki & Wright (1989), loc. cit., p. 932.

[43] A. Shevchenko & R. Wright, "A simple search model of money with heterogeneous agents and partial acceptability", Economic Theory 24, Nov. 2004, pp. 877–885.

one has to assume that individuals differ not only in their final commodity demands, but also in other basic respects. Such additional distinctions are highly plausible in practice, and then several interesting equilibria result. So the basic idea of the model can be retained.

3.9 Thus, the problem of many recent US-American models is that they seek theoretically "interesting" and very specific conclusions without ever asking whether the assumptions to be made bear any relation to the world of facts. Stigler's original model had the methodologically interesting conclusion that one should not only look at averages but rather at extreme value distributions (the cheapest price!); but the model is applicable in only one special case. Friedman's model happened to contain a serious mathematical mistake. Lucas' model is so extreme in its assumptions that one cannot say when and where it may be applicable. Prescott had originally just forgotten about taxes; Stiglitz had made an impossible assumption and looked at an empirically virtually irrelevant case; and Kiyotaki-Wright (the only non-Nobel-Prize article reviewed here) employed an illicit simplification. Dominant macroeconomic models are hard to test. To quite an extent, economics has been turned into dogmatic reflection. Macroeconomics, in particular, has become in its most highly esteemed models a subject taught, but not tested. More and more one gets the impression of l'art pour l'art exercises divorced from practically useful elucidations of real life. Or, to put it differently, can these models serve as foundations for policy advice?

4. Probabilistic model specifications

4.1 We have seen that people at the very top of the profession tend to offer economic models that are too highly specialized, using assumptions that invalidate their conclusions. We now turn to the probabilistic specification of models for estimation purposes: And here once more we find that far too simple assumptions are used, which, if generalised, mean that there are no precise conclusions to be arrived at. We remain in limbo.

Their central modelling assumption provided the title for this paper: It is assumed, usually without reasons being given, that the error term of the model to be estimated is "i.i.d.", i. e. the error term is "independently and identically distributed". Actually, even more is usually assumed: that the error term is normally and therefore symmetrically distributed with mean zero and a given variance σ^2, which then is also independently and identically distributed (e. g. over time), to be abbreviated as $N(0,\sigma^2)$. This assumption is not unrealistic in sampling of economic data at a given moment, but highly problematic in macroeconomic models over time: Why should the errors in a model of a very typical slow adjustment process be independent of what has happened in earlier periods? And why should the distribution not change over time, sometimes being very regular with small fluctuations, in other cases with hectic and large changes?

The assumption of the symmetric normal distribution is a standard one. So one can quote many instances of its completely unthinking application even for samples at a given moment. It is e. g. well known that the distribution of the population size of towns is heavily skewed to the left, there being many small towns and only a few big ones. Assuming a symmetric distribution in this case would generate nonsense. I well remember a thesis where the assumption of a symmetric size distribution of towns led to the conclusion that there existed a large number of towns with a negative population!

4.2 The first case to be discussed here is of an article distinguished by the invention of five "P"s where normally only three would be used: the "purchasing power parity persistence paradigm" by Christian J. Murray and David H. Papell[44]. The authors take it as agreed upon that real exchange rates

[44] C. J. Murray & D. H. Papell, "The Purchasing Power Parity Persistence Paradigm", Journ. of International Economics 56 (2002), pp. 1–19.

show "high short term volatility ... with extremely slow convergence to purchasing power parity"[45]. In fact, the question is whether there is any such convergence at all or whether real exchange rates do not rather show random walk behaviour. Many authors reject the random walk, but then their "techniques are not appropriate for measuring persistence for three reasons. First, virtually all existent studies ... are biased downwards ... Second ... the methods chosen tend to be unreliable or even invalid. Third, most of the studies ... do not account for serial correlation" (p. 17)[46]. "Most important, the least squares estimates of the half-lives are biased downward in small samples. This imparts a 'double' bias ... they are constructed using biased estimates from a biased parametrization" (p.3)[47]. This is typical for slow economic processes: One would need a very large number of data, which one does not have and can never get, due to changes over time, particularly with major changes in political systems. Due to historical change one can hardly ever draw sufficiently large, but still sufficiently homogeneous samples.

The authors find: "Once the bias in least squares estimates is corrected ... the confidence intervals of half-lives of PPP deviations are too wide to place any credence in the point estimates ... all the upper bounds are infinite"[48][!]. An infinite upper bound of confidence intervals implies that the random walk model cannot be rejected for exchange rates. Careful assumptions thus permit the conclusion that exchange rate forecasts are in general impossible. This is shown by the authors for 20 countries, but in all cases for the exchange rates against the US-dollar. It is exactly the dollar whose value is unpredictable, because in that currency the largest amount of international capital transactions takes place and thus the dollar is so far "the" major reserve currency. Careful estimation proves that the exchange rates against the dollar cannot be forecast.

Murray-Papell rely on the fact that it is wrong to assume a normal distribution in case of a small sample of values, when the parameters of the normal distribution are not yet known and therefore have still to be estimated from the small sample at hand. In this case, as Weitzman[49] pointed out, it ought to be remembered that one has to assume a Student t-distribution, which converges to the Normal Distribution for large (actually: infinite) samples, but has much larger variance for small samples, and which converges to infinite variance for very small samples. Thus, the wide confidence intervals required make precise distinctions impossible.

4.3 So far, I have been concerned with the appropriateness of the assumption of the normal distribution of the error terms without yet turning to the question whether these will be independently and identically distributed ("i.i.d."), as is often assumed. (In the cases analyzed by Murray-Papell the error term also had time structures that did not show independent errors over time.) The first problem here is that with all types of assets the variance of asset prices is not at all constant over time: In the case of US common stock prices, incorporated in the Dow-Jones-Index, stock price variance made three big jumps upwards within the last 110 years: in 1931–33, in 1987 and in 2008. The three jumps were of about equal size: to a level of an app. 15 times higher variance than normal, i. e. larger changes were about 4 per cent in one day instead of about 1 per cent; and on a few days even 10 per cent price changes occurred (see the essay on "Rare Events" as well as the next essay). Such large price changes require large reserves; typically, they caught many banks unprepared. As these upward jumps in variance occur only rarely and with long time intervals in between, they require much caution on the part of financial investors, caution as recommended by historical experience.

[45] Loc. cit. p. 1.
[46] Loc. cit. p. 40.
[47] Loc. cit. p. 3.
[48] Loc. cit. p. 15.
[49] M. L. Weitzman, "Subjective Expectations and Asset-Return Puzzles", American Economic Review 97/4, Sept 2007, pp. 1102–1130.

Actually, large price changes and thus increases in their variance are rare, but still much more frequent than a normal distribution of price changes would imply: Contrary to simple models the distribution of relative price changes exhibits what is graphically called "fat tails", as compared with the normal distribution. In particular, large upward movements of prices are much too frequent. These large price increases show, as I demonstrate, a Pareto distribution, and not a Normal Distribution, causing a significant deviation from "i.i.d.".

While price changes are thus not identically distributed over time the fact that often they are also not at all independently distributed either, particularly on financial markets, creates even greater problems. Also it is well-nigh impossible to distinguish empirically between two quite different phenomena: on the one hand, variable (1) causing variable (2) and, on the other hand, an involved lag structure of variable (1) determining that same variable over time.

4.4 An important article by Engel and West, "Exchange Rates and Fundamentals"[50] demonstrates that nicely: The authors argue "an asset price manifests near-random walk behaviour if fundamentals are I (1) and the factor for discounting future fundamentals is near one ... this ... helps explain ... [why] fundamental variables such as money supplies, outputs, inflation and interest rates provide little help in predicting changes in floating exchange rates ... [but] that the exchange rate helps predict these fundamentals"[51]. To put it crassly in quasi-theological terms: the exchange rate is the uncaused cause of other macroeconomic variables while the exact opposite had been argued for a long time, viz. exchange rate movements being caused by changes within the internal economy.

Engel-West show this empirically for the period 1974:1–2001:3 for the US exchange rate vs. Canada, France, Germany, Italy, Japan and the United Kingdom. They find "there is almost no evidence of causality from the fundamentals to the exchange rate"[52], while quite a bit of evidence exists for causation the other way round. It has to be admitted, however, that "a vast part of the movements in exchange rates [is] not tied to fundamentals"[53] at all! In addition, "all our empirical results are consistent with at least one other explanation, namely that exchange rate movements are dominated by unobserved shocks that follow a random walk"[54]. In a word: At least for international financial markets, time structure and causal interrelationship of variables are so intricately intertwined that we can conclude practically nothing for sure.

And, after all, why should a complicated world be easy to explain just to suit simple-minded economists? Independent error distributions over time simply cannot be assumed once models become just the least bit complicated.

4.5 The result is not really surprising. Why, it has to be asked, should the factors determining exchange rates not change considerably over time, and the lag structure as well? What happens e. g. if vast amounts of additional international reserves are created and bought up by previously unsupplied agents, and these then start to speculate with their holdings? But changing structures cannot be derived from models that assume unchanging structures. Furthermore, in a financially integrated world, exchange rates are greatly influenced by third world repercussions as well. And are the variables usually embodied in exchange rate analyses still the most relevant ones? One thing at least seems clear: <u>For internationally integrated financial markets, the "i.i.d." assumption is by now illicit</u>! Quite apart from the fact that appropriate models would by now probably be non-linear. Non-linear models have

[50] C. E. Engel & K. D. West, "Exchange Rates and Fundamentals", Journ. of Political Economy 113/3, (June 2005), pp. 487–517.

[51] Loc. cit, p. 485.

[52] Loc. cit., p. 506.

[53] Loc. cit, p. 512.

[54] Loc. cit, p. 488.

often been suggested, but were seldom tried out. There is only one single linearity, but an infinite number of non-linear models to choose from!

The structure of world-wide integrated economic systems is far too complicated to be known in precise detail by any one agent. At best we can hope for partial understanding, which proves often, but by no means always, correct. Economic systems change over time and human learning is too slow to catch up with changing reality, especially as actors on financial markets are not at all interested in encountering well-informed trading partners. The less knowledgeable provide more opportunities for profit.

We have witnessed the development of ever more explicit economic models that have not been (or: are not being) tested against reality, because once-and-for-all testing is nearly impossible in economics since the basic probability systems keep changing over time – possibly in a circular way, too (as we see right now when the Crash experience of the 1930s has become relevant once more). Instead of being tested, some of those models have rather been turned into dogmas.

The question of the empirical relevance has more and more receded into the background of American economics, which is a profitable teaching discipline, but no longer policy-oriented. So what to do then, after 2008? Should one discard nearly all one knows and just continue with policies of the late 1930s and 1940s? That would mean forgetting completely that those policies were fiercely argued over and considered theoretically invalid from the late 1960s to the 1980s – quite apart from the fact that the framework of economic policy has changed fundamentally since the 1930s, with highly open economies and free capital movements now.

In this last part, we looked into the empirically testable structure of economic models – and discovered that models for testing purposes, too, had often been careless. The stochastic structure of a testable model is often just stuck on, like an afterthought: One finds an abundance of small sample problems, error terms that are neither identically nor independently distributed over time, and structures that cannot be identified but allow for various explanations. Therefore, it is perhaps best to end with the standard assertion of economic articles, although in a more pessimistic vein than usual: Further research is needed, but not necessarily along the routes taken so far. Advanced theoretical models and the search for higher precision have resulted in less objectivity, a decline in validity and greater uncertainty about practically relevant conclusions.