

# Inherent World Views

Narrative-Induced Research in Economics

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# 1 Introduction

Milton Friedman's essay "The methodology of positive economics" from 1953 is the most important piece on economic modelling to date (see Hausman 1994 or Mäki 2009). It is not only the rare case of a twentieth century neoclassical economist writing about the methodology of economics, but also one by a scholar with remarkable influence and respect within the community of mainstream economists.<sup>1</sup>

Friedman emphasises two main points. Both rely on the assertion that "a consensus on 'correct' economic policy depends [...] on the progress of a positive economics yielding conclusions that are, and deserve to be, widely accepted." (Friedman 1953, p. 148) Now that he has declared the aim of economics to be a *positive* science, he firstly argues that the most important feature of a model is its predictive power and that any model should be evaluated considering this goal. That entails secondly that "unrealistic" assumptions may well be tolerated, even accepted, if only the model performs well in terms of prognosis. He criticises that due to the impossibility of conducting (especially macroeconomic) experiments, economists have begun to evaluate the quality of a model by the closeness to reality of its assumptions.

Now, there has been critique of these positions (Hausman 1989, Spiegler 2015). However, most economists do not seem to be too narrow-minded about methodology. McCloskey (1983) famously summarises that "a watered down version of Friedman's essay is part of the intellectual equipment of most economists, and its arguments come readily to their lips". At the same time there are abundant examples of economic work that base on this justification but use, defend, and interpret models in a way Friedman (1953) clearly forbids; above all, there is a tendency to use language that suggests causal relations or the assumption of certain motives that lead agents to some specific behaviour, plainly put: they give explanations where Friedman constrains economists to mere predictions. Although I do not want to defend Friedman's methodology, I want to shed light on the issue of theoretical papers from economic literature deploying narratives to cover their models. As an example, a typical paper from the banking literature will be introduced below.

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<sup>1</sup> Milton Friedman was to be awarded the Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel for his research in 1976.

## 2.1 Models and Discussions: an Example from Banking

There is no clear methodological successor to Friedman's essay, neither does a clear definition of how economic modelling may be undertaken exist, nor is there an assessment of what conclusions those models ultimately allow. One might argue that there are also few physicists, biologists, et cetera, who care about methodology in their field. The difference is that (especially macro-)economic theory is constantly challenged by its repeated failure in the real world context; the most recent and prominent example being the phenomenal aberration of mainstream economics before the financial crisis of 2008. How come that economists would boast themselves about the completeness of their scientific endeavour on the eve of the worst crisis since the Great Depression? Krugman (2009) gives a *who is who* of economists that during the 2000s all declared the science to be in good shape. Caballero (2010) points out that core macroeconomists have become so captivated by the theoretical model world they predict that they have forgotten about the incompatibility with the real world that these predictions exhibit.

Even though aspects of Friedman's philosophy of economic science are easily adapted when it suits the needs (e.g. the above mentioned dealing with assumptions), methodology seems to be dealt with rather nonchalantly. To be precise, models seem to have been reduced to the service of narratives, i.e. there is a tendency to put models into economics papers although actual reasoning takes place in a verbal part; the attached model serves little. To illustrate my reasoning, I would like to introduce a model constructed by Hellmann et al. (2000) describing risk behaviour of banks under capital regulation. One of the authors of this paper, Joseph Stiglitz, received the Nobel Memorial Prize in Economic Sciences, so this is all but esoteric research. In the abstract of "Liberalization, Moral Hazard in Banking, and Prudential Regulation", it is stated that capital requirements "have a perverse effect of harming banks' franchise values, thus encouraging gambling". The initial idea is that capital requirements are costly for the bank, which is identical with lowered proceedings in every (future) period. Hence, the present value of all future revenues is lowered. The value of all future revenues equates to the value of the bank licence. With lowered present value (due to capital regulation), the cost of losing the bank licence itself decreases. This has implications for the behaviour of the bank, as it changes the attitude towards gambling, with gambling being the

investment in a high risk portfolio, i.e. conducting financial operations that yield outstanding gains with low(er) probability. Losing such a gamble entails bankruptcy and the loss of the bank licence. Thus, a bank licence lower in value means lower cost in case of bankruptcy. Then, gambling in the current period may become rational: with low, but positive probability, the bank will earn a lot of money. With high probability, the gamble will be lost and the bank bears the sunk opportunity cost of losing its licence.<sup>2</sup>

It should be noted that this paper is not giving an idea about mere economic behaviour in a global context, but that it is a precise explanation of a specific economic problem as the authors perceive it - namely equity regulation and adverse incentives. The model is preceded by a large verbal part, where the idea about bank behaviour is explained and defended. This is much more than the prediction Friedman has in mind. The authors give *reasons* for why bankers will likely act this way. In the end, the model merely serves to show that the given proposition can be depicted in mathematical terms. The crucial part of the paper is the essay-like introduction. Hellmann et al. undoubtedly make a good point why equity regulation might have adverse effects. However, their argument is just as valuable without the model. Why add it then? Would there be any difference for Hellmann et al.'s argumentation if the paper was published without any model? The next section shall illuminate this issue.

## 2.2 Why models?

An often-heard argument in favour of modelling is that it averts logical flaws. Others also state that modelling facilitates the communication of a point, that it is a scientific language that circumvents the ambiguity of spoken languages; Sudgen (2000) speaks of "*metaphors*".

However that may be, models clearly are no epistemic tool as the endeavour of describing truths about the world is carried out by the narrative presented *beforehand*. It is not the case that prerequisites are formulated, a model is built, and subsequently the model's results

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<sup>2</sup> In a second part, the paper further explores optimal banking regulation and proposes implementation of deposit-rate controls. As the analysis of bank behaviour under capital regulation is concluded, there is no need to discuss that second part here.

are accepted whatever the model yields. Quite differently, it is the result that is formulated as a narrative and then replicated by a model. A publication in a renowned economics journal would also be borderline impossible without mathematics, at least not in the case of theoretical papers; but apparently, no one cares for the math once some equations have been put down. A dazzling example of this practice was exposed by Romer (2015) who showed that a flawed proof in a 2009 paper by Robert Lucas remained unnoticed throughout the long and wearisome process of peer review a whole year until publication.<sup>3</sup> Consequently, he states that “[n]either colleagues who read working papers, nor reviewers, nor journal editors, are paying attention to the math.” If no one (but Romer) checks the math, then peers obviously assess the quality of a paper by the strength of the argument put forward in the narrative section, i.e. the model has *no importance* to fellow scholars apart from *being there*. Again, this is certainly the case with the above mentioned paper on bank behaviour by Hellmann et al.: the narrative in the first part is defining for the whole paper.

Such practice entails severe issues. Firstly, there are general downsides of introducing mathematical models as a convention for the whole domain such as (i) the incompatibility with other social sciences, or (ii) the limitation to what can be described in mathematical terms. Secondly, and even more importantly, when economic research relies on narratives, it is the economist’s *worldview* that will define what he or she writes about and states as theory. Those two ensuing problem areas - issues inherent to models and others that are of external nature - are discussed in the following sections.

## 2.2.1 Problems With Models

Economics has a tendency to exclude itself from other social sciences with separate faculties and a method that alludes to the natural sciences, despite the fact that it deals

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<sup>3</sup> A proponent of mathematical modelling in economics might object here that in this case, after all, review has worked in the sense that a peer found the mistake. However, we would need another discussion about the review processes (which already lingers on various blog pages in the Internet for a while). The fact remains that a year of reviewing did not see anyone checking the math. On a side note, this example should be an alarm to think about all the flaws that have not been uncovered by attentive readers, if even such elemental errors can pass the review process.

with phenomena that belong to the realm of the social sciences rather than to the natural sciences. In this context, Arnsperger & Varoufakis (2006) point out a possible explanation considering the omnipresence of methodological individualism in economics: as agents act individually, their actions precede any (social) structure. In this way, neoclassical economics swiftly circumvents the issues imposed by a social environment. The widespread use of models complicates the communication with all the other social sciences. Although I argue that those models in many cases do not contribute to the advocated notion, this is clearly neither what social scientists expect nor economists purport. Consequently, mathematically inexperienced non-economists who want to enter the discourse are distracted if not excluded by simultaneous equations and greek letters.

Then there is the allusion of economic modelling to the natural sciences, especially to physics. However, apart from the difference in that narratives precede any model - which I will discuss later - a common justification is that mathematisation would enable precise assessment of models and theories via empirical testing. However, economic models come with such far-reaching assumptions and unclear implications that hardly a (macroeconomic) model is ever rejected.<sup>4</sup> Rather, limited data and the impossibility of experiments in macro helps little to permanently reject theoretical hypotheses - unlike in physics, where experiments enable final falsification of theoretical models. The discussion about the effects of a minimum wage may serve as an example: there are influential empirical papers that purport not only insignificance, but a positive employment effect such as Card and Krueger (1993), with a fair amount of subsequent papers trying to rebut those claims as for example Neumark and Wascher (2000). Then, there are studies that present empirical evidence for negative effects such as Burkhauser et al. (2000). There is a lot at stake in this discussion as neoclassical economics could hardly explain a minimum wage that does not result in a reduction of employment, *all things equal*.<sup>5</sup>

At the same time as economic modelling allegedly enables an assessment of theories with empirical evidence, it significantly reduces the expressive power of the economic

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<sup>4</sup> Except, of course, the above-mentioned Lucas (2009) which should certainly be revised if not rejected. The course of the next years will show if flawed mathematics can bar a paper from remaining in the canon. The paper is still available on his web page by September 2016.

<sup>5</sup> Although most theory and a fair amount of research leans towards a negative effect of a minimum wage on employment, Germany has introduced a minimum wage in 2016. However, this is another discussion needed to be lead elsewhere.

language. Caballero (2010) differentiates between core and periphery macroeconomics. The first deals with neoclassical stochastic growth models where households optimise by equating the marginal substitution between labour and leisure with the real wage. Firms optimise the use of labor and capital according to a production function. The author describes the power or powerlessness of these models with the following words:

After much trial and error, these core models have managed to generate reasonable numbers for quantities during plain-vanilla, second-order business cycle fluctuations. However, the structural interpretation attributed to these results is often naïve at best, and more often is worse than that. For example, while these models have been successful in matching some aggregate quantities, they have done much more poorly on prices. But in what sense is it a good general equilibrium if the quantities are right but not the prices? (Ricardo J. Caballero 2010, p. 89)

Periphery macroeconomics tries to explain specific phenomena like fire sales, flights to quality, or as Hellmann et al. do: bank behaviour under equity regulation. Caballero rightly states that a fair amount of those explanations are quite convincing.<sup>6</sup> However, there is no way that this peripheral (peripheral in concentrating on specific events, not in the sense of irrelevant) work can be put together with core macroeconomics. An overall approach on macroeconomics including such characteristics of an economy - maybe like the General Theory by John Maynard Keynes - does seem infeasible with the current framework.

### **2.2.2 Inherent World Views**

In 2.2, the proposition was introduced that the driving force behind most theoretical papers in economics is a conclusive narrative, a convincing story - and not its model. On the contrary, it seems like models are often added to a piece of academic research in order to be accepted in a journal. The subsection was ended with the summary that the use of models itself introduces severe problems - I sketched some pressing ones in 2.2.1 - although it does little for the point the paper tries to make.

However, there is a more relevant question regarding the development of economics, once the notion that models merely serve the narrative is established: It is the question of

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<sup>6</sup> Again, I would argue that this is due to their well-working narratives.

what is the driving force behind explanatory approaches to economic phenomena. If models are a mere addendum, it is the underlying narrative that is crucial. This may seem trivial, but would come as a shocking message not only to lots of students of economics. It would furthermore entail that research is immensely formed by the view the researchers have about the world, i.e. their worldview becomes inherent in the narratives they purport in their research. Economics would not stand alone with this phenomenon, though. There have been comparable effects in physics, where even famous physicists built their models upon their perception of physical phenomena. Take for example the Bohr-Einstein-debates about quantum mechanics: throughout the debate and his life, Einstein refuted the completeness of the theory of quantum non-locality, condensed in a prominent phrase he wrote in a 1926 letter to Max Born: “Jedenfalls bin ich überzeugt, daß *der* [Gott] nicht würfelt.”<sup>7</sup>. He was not willing to accept the theory as it would entail the rejection of determinism. However, as I pointed out above, falsification works in physics and worldview-driven models are put aside once they are falsified. Also, plainly put, physics deals with objects, whereas economics deals with subjects, meaning that opinions about the world and much more about its inhabitants play a considerable, if not crucial, role. Take alone the unresolved conflict surrounding methodological individualism (Rolle 2005): the proposition makes most social scientists and philosophers shake their heads, but economists cling on to it. Even in the light of the recent financial crisis, significant efforts are made to *safe* a version of this conception by slightly, cosmetically altering it (Lindner 2015).

The importance of such opinions in constructing theories is also visible in the above introduced work by Hellmann et al. (2000). Likely the main prerequisite for the narrative, equity is stated to be *expensive* to banks. If equity is not significantly more expensive than debt, the present value of all future earnings is not reduced and thus no gambling incentives prevail. However, the contention that equity is expensive is disputed. An important player in this discussion are large banks themselves, that use the argument of expensive equity to take on massive amounts of debt in order to augment their balance sheets to unseen sizes. In turn, a default becomes more likely as less equity is there to compensate losses, but even more important, a default entails huge losses for all creditors of the bank. The insolvency of Lehman Brothers in 2008 is an example for the ensuing

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<sup>7</sup> “I, at any rate, am convinced that He [God] does not throw dice” (Einstein et al. 1969)



consequences: just the same, the other highly leveraged banks are incapable of compensating the loss inflicted by the first fallen bank. In order to save the financial system from the brink of collapse for a second time, governments will save those huge banks from failing. These implicit government guarantees mean substantial savings on interest for those banks (Ueda & di Mauro 2013).

However, not all economists share this view held by commercial banks. Two prominent economists in this area are Anat Admati and Martin Hellwig who in several papers have argued against the contention of costly equity (see Admati et al. 2013 or Admati & Hellwig 2014 amongst others). Their account is both readable and conclusive. Surely, the financial crisis from 2008 has done much to shift opinions towards a more favourable view of equity regulation, especially as the disciplinary effect of debt can hardly be held up any longer. But had the financial crisis - by sheer luck - not occurred, scholarly opposition to equity regulation would likely still be significant. Unsurprisingly, banks are still lobbying against it - with considerable success.<sup>8</sup>

Thus, what we see is an instance of a model or theory being founded in a mere contention, a belief about the world. Another instance of this phenomenon was observable in 2013, when both Eugene Fama and Robert Shiller received the Prize in Economic Sciences in Memory of Alfred Nobel. That year's prizes were particularly interesting for the question of inherent world views as Fama and Shiller represent two contrary, even contradictory positions on financial markets. Robert Shiller argues that instances of irrational behaviour occur rather regularly, and that participants in financial markets have a tendency to gamble; see for example Case & Shiller (2003), which is also an early example of an economist discussing the looming danger of a house price bubble. Eugene Fama's name, on the other hand, is strongly associated with the Efficient Markets Hypothesis (EMH, see Fama & Malkiel 1970) which is a strong supporter of the notion that all available information is contained in (especially stock) prices on financial markets. The EMH is also much more compatible with the overall neoclassical framework, even derived from standard neoclassical assumptions. Fama also opposes Shiller in maintaining that the financial crisis from 2008 *has not* undercut his theory of efficient markets (Miller et al. 2013).

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<sup>8</sup> See for example the appeasing amendments to the Dodd-Frank Act achieved through Wall Street lobbying.

Coming back to where this paper set out beginning with Milton Friedman, some observations on his proper work can be made. He demands academical economics to be of *positive* nature. However, *his* political conceptions are in line with both his theoretical and empirical works (MacIntyre 1981). Regarding the role of his convictions in his research, taking a look at Snowden and Vane (2005, p. 169) seems fruitful:

Friedman (1958) sought to re-establish an important independent role for money through a study of time series data comparing rates of monetary growth with turning points in the level of economic activity for the USA. [...] Friedman concluded that this [his findings] provided strong suggestive evidence of an influence running from money to business. Friedman's study was subsequently criticised by Culbertson (1960, 1961) and by Kareken and Solow (1963) on both methodological and statistical grounds. First, the question was raised as to whether the timing evidence justified the inference of a causal relationship running from money to economic activity (see also Kaldor, 1970a; Sims, 1972). Second, statistical objections to Friedman's procedure were raised in that he had not compared like with like. When Kareken and Solow reran the tests with Friedman's data using rates of change for both money and economic activity, they found no uniform lead of monetary changes over changes in the level of economic activity.

True, the later Friedman & Schwartz (1963) was a groundbreaking work on monetary policy and widely acclaimed. It has retained its importance until today and served central bankers in their handling of the financial crisis of 2008.<sup>9</sup>

### 3 Conclusion

It is surprising that there is not much more discussion on the importance of narratives in economic research and the subsequently introduced inherent world views, especially given events like the above described contradictory situation with the Prize in Economic Sciences in Memory of Alfred Nobel. Although the awarding jury appointed by the Sveriges Riksbank is not the High Council of Economics it sheds light on the introduction of diverging world views into economic research - albeit unwillingly. As economics is still expected to be an advisor of politics, this has serious implications: Firstly, depending on the background of an economist, it is feasible if not common to introduce normative opinions into economics. This often happens under the guise of the *positive* economics that were the leitmotif purported by Milton Friedman. Secondly, all theory is built on a fundament, a school of thought. This very fact remains undiscussed and unaccepted.

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<sup>9</sup> "Perhaps the single most important piece of economic research that provided guidance to Federal Reserve Board members during the crisis was Milton Friedman and Anna Schwartz' Monetary History of the United States" Kroszner (2010)

Maybe, this is related to a determination to retain economics' allusion to the natural sciences. The orientation towards "science" (in the form of natural science as opposed to the social sciences) entails (i) prestige and authority and (ii) frees from the necessity to acknowledge the mainstream school of thought *as* a school of thought and thus from entering into a discourse with other approaches to economics.

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