Nordic Journal of Political Economy

Volume 33 | 2007 | Article 3

# Back to the Future – the Marginal Utility of History in Economics

Eivind Merok*    Nils August Andresen‡

*Researcher at the Department of Archeology, Conservation and History, PO Box 1008 Blindern, 0315 Oslo, Norway, eivind.merok@iakh.uio.no
‡ Researcher at Econ Pöyry, PO Box 5, 0051 Oslo, Norway, naa@econ.no

*Eivind Merok, Nils August Andresen*

# Back to the Future – the Marginal Utility of History in Economics[1]


## Abstract

Economics and economic history share many fundamental research problems and have a rich shared intellectual history. Still, works by economic historians are rarely read or referenced in economics. In this essay we attempt to identify the cost of this negligence. In particular, we argue that a restrictive understanding of the economic research programme excludes available evidence and precludes analysis of complex situational constraints on economic decision-making.


*JEL classification:* A12, B1, B2, B4

## 1. Back to the Future – or the Marginal Utility of History in Economics

Economists and economic historians share many fundamental research questions and have a rich common history, intellectually and practically. After World War II, however, historical analysis has been largely absent from leading journals in economics, and seldom constitutes an important part of the curriculum taught at universities. To economic historians, then, it seems that a large body of information from a great variety of sources with potential significance for the explanation of economic phenomena is left out of the academic discipline of economics. This restrictive view on what counts as evidence in economics has in our view had significant ramifications for economic methodology more broadly. Stated differently, our topic in this article is whether economics can manage without history.

Whereas economic theory is a point of departure for much analysis, economic history would not have been a separate academic discipline if it did not add something else. That "else", we believe, is contextual explanation, explanation that looks specifically at the geographical and temporal location of the phenomena, in the belief that history

[1] This article is based on a presentation at a seminar at the Institute for Social Research, Oslo, 18.09.2007.

matters. In this essay we argue that predominant research strategies applied in economics are biased against drawing upon the kind of evidence needed to provide contextual explanations in the analyses of key contemporary problems. However, economics was not always like this. Earlier in the century, many leading economists practiced contextual explanation in a way seen much more rarely in the latter half of the century. One root cause of the change and the bias against historical analysis, we argue, is that a distorted application of methodological individualism came to dominate the economic research programme.

We further argue that this bias has some costs: The reluctance to take empirical detail into account too often hampers the formulation of empirically testable hypotheses, and leaves the economist to choose between hypotheses without empirical relevance on the one hand and statistical descriptions of the world, unable to capture the details of causal mechanisms, on the other.

The remainder of this essay is structured as follows: In the first subchapter we describe how what we would now recognize as mainstream economic research developed as a distinct research programme from the 1930s. This programme – often described as the econometric revolution –marked a significant departure from the broader tradition of political economy and epitomized a distinctive understanding of ideal methodology in economics. In the second subchapter we seek to investigate how this methodological programme influences current mainstream economics as practised in economic journals today, looking specifically at publications in Nordic Journal of Political Economy (NOPEC). Our observation is that papers published seldom combine theoretical analysis with empirical testing, but most often accept either purely theoretical discussion or reviews of secondary evidence.

The third subchapter of the paper discusses some of the challenges that arise from the findings in NOPEC. Finally, the last subchapter indicates an alternative research strategy. Rather than launching an all-out attack on economics, we formulate what we believe are realistic amendments to the research programme to deal with the shortcomings pointed out above.

## 2. How Economics Forgot History

Modern economics, to quote a present commentator, has definitively forgotten about history, and economic history is rarely read by students of economics, referenced in leading journals or read by scholars (Hodgson, 2001). Consulting a contemporary edition of a leading journal in economics one would be surprised to find articles making extensive reference to historical events or processes.

This situation is dramatically at odds with how economics was practiced and conceived prior to the post-war period. Classical contributors to economic theory as diverse as Smith, Marshall, Schumpeter and Keynes all drew widely on published and unpublished historical sources when developing their theories. Indeed, within the broader field of political economy, scholars up until the 1950s applied a wide range of strategies for analysing problems, including strategies that we today would identify as historical analysis.

This is perhaps most clearly illustrated by one of the most prominent contributors of modern economics. Alfred Marshall, often viewed as the leading propagandist of the marginalist revolution in the British Isles, demonstrated a staggering mastery of the historical development of the British economy, and several of his publications testament competent analysis of historical material. In particular, the publication of *Industry and Trade* in 1919 documented Marshall's ability to conduct historical analysis through what we today might call a comparative institutional analysis.

Even though Marshall's *Principles of Economics* marked a significant advancement in economic theory, the style of argument in his seminal contributions remained close to modern economic history. It is important to note that his style of economic reasoning was based on firmly held methodological beliefs. According to Marshall, successful economic analysis demanded that the economist mastered both the relevant analytical tools and had a grasp of the empirical intricacies presented by the problem at hand. Marshall was careful to warn his readers about the limitation of deductive reasoning for economists. Comparing economics with mechanics and chemistry, he warned:

> *But even in mechanics long chains of deductive reasoning are directly applicable only to the occurrences of the laboratory. By themselves they are seldom a sufficient guide for dealing with the heterogeneous materials and the complex and uncertain combination of the forces of the real world. For that purpose they need to be supplemented by specific experience, and applied in harmony with, and often in subordination to, a ceaseless study of new facts, a ceaseless search for new inductions.*[2]

The development of theory is thus seen as a vehicle for devising explanations of concrete events or developments. The symmetry between deduction and induction is stressed repeatedly by Marshall in his writings, and continuing his observation he argues:

> *The function then of analysis and deduction in economics is not to forge a few long chains of reasoning, but to forge rightly many short chains and single connecting links. This however is no trivial task. If the economist reasons rapidly and with a light heart, he is apt to make bad connections at every turn of his work. He needs to make careful use of analysis and deduction, because only by their aid can he select the right facts, group them rightly, and make them serviceable for suggestions in thought and guidance in practice; and because, as surely as every deduction must rest on the basis of inductions, so surely does every inductive process involve and include analysis and deduction.*[3]

The lengthy quotations from *Principles* serve to underline the balanced view on induction and deduction that Marshall held, and his inclusion of historical analysis as an integral part of economic analysis. However, Marshall's advice did not have a sustained impact on modern economics in the 20th century. Indeed, the developments after Marshall's death have confirmed a firm separation between economics and economic

[2] Marshall, 1920: Appendix C.5
[3] Marshall, 1920: Appendix C.9

history at most universities, and the disciplines emerged as wholly distinctive fields of inquiry in the 1950s (Coleman, 1987).

One important explanation for this development was the lack of credibility that the field of political economy or economics experienced after the great depression. In several countries this gave a strong impetus for change within economics. One of the most significant changes was the emergence of what we may call the econometric school of researchers, marked by the establishment of the Econometric Society in 1931 and the publication of the first edition of the journal *Econometrica* two years later (Louçã, 2007; Morgan, 1990). A leading proponent of this school, and the first editor of *Econometrica*, Ragnar Frisch, formulated the common views held by this school of researchers in the first editorial of the journal. In the most general terms, the econometric society sought to improve the state of economics through the formulation of quantitative economic theory, the application of mathematics to economics, and utilization of economic statistics (Frisch, 1933).

The emergence of econometrics as a defined strategy for studying economic phenomena ranks as one of the most significant changes in economics during the 20th century. In several manners, the introduction of this research programme marked a complete departure from how the subject had been perceived. In Norway, for instance, the academic field of economics was remoulded in the design of Ragnar Frisch. In his extensive methodological writings from the 1930s, Frisch formulated a research programme for economics that would strongly shape the discipline. In formulating the methodological strategies for economics, Frisch openly expressed his admiration for modern physics. In his inaugural lecture at the University of Oslo, he promised a quantum leap forward for economics if the subject followed physics' path towards quantification.

As Frisch's influence grew in academic circles, traditional approaches more inspired by historical analysis and law lost prominence (Bergh and Hanisch, 1984). Interestingly, though, the diagnosis Frisch and many of his contemporaries presented of economics had a strong resemblance with Alfred Marshall's worries presented above. As we have observed, Marshall was careful to warn his peers of venturing into theoretical exercises without making extensive use of empirical material to identify the relevance of theoretical arguments, and test their empirical usefulness. Frisch's diagnosis shared many of the same concerns. In his view, previous developments in economics had been hampered by a lack of dialogue between theoretical analysis and empirical studies. This was largely believed to be a problem of coherence, understood as a lack of dialogue between theories and empirical observations. To Frisch, though, this coherence problem was about to be resolved with the advent of modern analytical theory, as the development of a precise observational language would allow for a convergence between internal (theoretical) and external (empirical) work.

In order to achieve a stronger coherence between theoretical and empirical work, Frisch would look to the natural sciences for inspiration. According to Berg and Hanisch (1984), Frisch's position reflected an inherent scepticism towards the ability of historically oriented scholars to reach valid empirical generalizations. The empirical world was in principal unattainable to the researcher, as if the economic world was in a constant flux, composed of an indefinite number of decisions and processes, and thus inaccessible for direct observation by the economist. According to Frisch:

> *The world of experience as a whole is, by means of its boundless complexity and mass of details, impossible to understand. One may say that it, in its immediate form as sense impressions, looks like a "jelly-like-mass". It does not supply any fixed point on which to base any thinking. In order to create such fixed points we make [use of] an intellectual trick: we create a small world in our mind, a model.*[4]

This view bears strong resemblance to the justification for a general reductionist programme in economics, as presented by Irving Fisher in the 1890s. In this setting it is sufficient to note that the epistemological position taken by Frisch had direct influence on the methodology that he would suggest for economics. Being disillusioned with the established field of Political Economy, and sceptical towards the formulation of empirical generalizations as practiced by institutionally or historically oriented scholars, Frisch would through his writings, and perhaps most importantly through his practice, launch an alternative solution that consisted of two strategies: First, to develop analytical models in order to identify and study complex economic processes, and, secondly, to combine this model apparatus with precise quantifiable empirical observation, preferably in large-scale time series of economic variables.

Frisch maintained a view that economic theories ideally should seek verification through empirical studies, and he would himself devote significant efforts and resources into large-scale empirical studies. Compared to Alfred Marshall, though, Frisch's position would attach a higher value to developing theoretical models as a strategy for understanding economic phenomena. In this view, models should be viewed as primarily analytical constructs, and the development of models should not be constrained by reference to empirical facts. According to Frisch: "The model world shall have a purpose. It shall help us adopt a way of thinking that will ultimately be useful in our fight for control over nature and social institutions" (Bergh and Hanisch, 1984:157). While Frisch seemed to accept that economic models often had to make unrealistic assumptions, he justified this with a sceptical view on the possibilities of establishing firm empirical foundation for generalization.

The Oslo School's methodological orientation, as represented by Frisch, was largely in line with views published later by dominant American economists such as Paul A. Samuelson and Milton Friedman. The common feature of this programme was that it attached a high priority to mathematical reasoning as an essential tool for formulating theory. To what extent the theoretical exercises would mature into a fully scientific programme where the theories would yield predictions testable in empirical studies, was largely an open question.

---

[4] Quoted in Andvig (1986)

The advances of econometrics in the 1930s and early post-war period left some room for optimism for this programme. Another leading representative of the Oslo-school, and a leading contributor to the emerging field of econometrics, Trygve Haavelmo, would become increasingly sceptical. Originally, the development of precise econometric methods was believed to be necessary in order to facilitate precise testing of the now mathematically formulated theory. In his late career, however, Haavelmo came to hold the position that the problem was turned on his head – it was now the theory that was insufficiently precise to allow the empirical testing that econometrics now allowed. In his lecture notes, published by his students and assistants, Haavolmo would lament over the imprecision of the established theory, and argued repeatedly that the advances of econometrics had yet to produce coherence between theory and empirical observations. Despite enormous advances in econometric methods, Haavelmo still felt that the coherence problem remained (Bergh and Hanisch, 1984:211ff).

A particular understanding of methodology and philosophy of science, then, motivated Ragnar Frisch and others to develop methodological alternatives to traditional analysis in political economy. The combined effort of the Oslo-school and other parallel developments in America, Great Britain and the continent, led to a total reshaping of economic analysis. The legacy for economics was a strong recommendation to prefer mathematical language for theory formulation and from this followed a strict preference for quantifiable evidence for empirical testing. While we may be correct in identifying how leading figures perceived the proper methodology of economics, it is still an open question whether or not this methodological ideal holds sway in contemporary economic research. This is the question we turn to in the next sub-chapter.

## 3. What do Economists do?

In order to discuss dominant methodological traditions in economics, we here make a limited attempt to identify some specific features of how economists do research. In order to do this, we have surveyed a series of economic journals published in Scandinavia. Here we present our finding based on a full sample of articles published in the Nordic Journal of Political Economy (NOPEC) in the eight years from 1995 to 2003.

Our main concern was to identify the research strategies employed in the papers accepted in the journal. For this purpose it is sufficient to define research strategy in a very open-ended way. With the term research strategy we understand the strategy that the researcher uses in order to answer the problem and the style of argument employed to convince the reader. Defined in this way we would include purely theoretical papers as representatives of a particular research strategy, i.e. answering a problem and convincing the reader through logical analysis.

The question is then what research strategies economists employ when publishing in NOPEC? In order to classify papers with widely varying research problems we devised a simplified "questionnaire" identifying the paper's style of argument and use of empirical evidence. We first asked about the relationship between empirical evidence and economic theory in the paper. To what extent were the theoretical arguments presented confronted with empirical testing, or, to what extent was the empirical material presented devised in a manner that would be informative for some theory of interest. Second, we asked whether the papers used original evidence, that is, if the paper presented empirical evidence gathered to support its argument. Finally, if original evidence was used, we investigated what type of evidence was used.

We limited the categorization to four broad categories of papers. Firstly, we identified papers that developed arguments on a purely theoretical level without extensive reference to empirical facts. This group of papers, which could include both verbal and formal expositions, and mathematical modelling exercises, was labelled theoretical papers. The second group of papers was named theory-driven empirical studies. These papers developed empirical hypotheses from theory, and then tested the hypotheses and the theories they relied on with empirical facts. Thirdly, we identified papers that analysed empirical material without extensive reference to any theoretical hypothesis. These papers, labelled data-driven studies, utilised data to answer questions that were not directly developed from existing theories.

Finally, the last category of papers was labelled reviews. Being somewhat a residual category, we applied this label to papers that made surveys of existing theoretical or empirical literature, without presenting a novel problem or analysis. The papers classified as reviews included mere summaries of contributions in a particular theoretical field, substantive summaries, and policy debates. The results of our classification are presented in table 1.

**Table 1: Classification of articles accepted in the Nordic Journal of Political Economy, 1995-2003**

| | N | % |
|---|---|---|
| Theoretical and conceptual work | 29 | 37,7 |
| Theory-driven empirical studies | 7 | 9,1 |
| Data-driven studies | 9 | 11,7 |
| Reviews* | 32 (11)* | 41,6(14,3) |
| | | |
| N | 77 | 100 |

* Reviews of theories are included in the parentheses

We initially wanted to distinguish the papers on the basis of what type of evidence that was utilised. The survey demonstrated that the need to distinguish between different kinds of evidence was very low: Almost all the papers that analysed empirical material were based on quantitative evidence, while only two papers made extensive use of alternative sources. Additionally, the papers drawing on quantitative data were almost without exception “large-N studies”, i.e. studies with a large number of observational units, but with information on few variables. Compared to the methodological styles found in other fields of the social sciences, this finding documents that economists in fact do stand out in the field, with an extreme reluctance to apply alternative methodological strategies, as for instance case study strategies found among historians and other social scientists, or qualitative research strategies (Ragin, 2000).

It is, of course, prudent with a careful disclaimer before we continue our discussion. We do not claim that the results from our sample of articles published in NOPEC give a necessarily representative picture of the research activities of most economists. Obviously, a much larger sample with a broader range of publications would be needed. We do believe, however, that the findings are indicative of the allocation of resources made by economists.

Based on our restrictive sample, it is striking how few contributions that actually apply research strategies that allow for a dialogue between theory and data according to standard ideals in the philosophy of science. Falsification of theoretically deduced empirical sentences, or even testing of basic intuitions, seems like a little sought after research activity. The coherence problem, as identified by Marshall, Frisch and Haavelmo, seems to loom very strong in our sample of papers. Strikingly, only 7 papers according to our classification made use of evidence in this matter, representing a mere 9.1 percent of the total contributions. This finding falls in line with a broader tendency that rather few papers utilize empirical evidence to strengthen their arguments. Combined with the papers that we have categorised as data-driven analysis, only about one fifth of the papers in the sample made use of original empirical evidence to present their argument.

Unmistakably, the majority of contributions to NOPEC offer some variant of a pure theoretical analysis. Papers developing theory or dealing with conceptual issues amount to 37.7 percent of the papers accepted in the period. Within this category we find both model-building exercises and verbal expositions. Typically, therefore, a contribution to NOPEC answers to research problems by devising a model. We will have more to say about this tendency later in the paper. Here it is sufficient to note that this style of argument seems acceptable as satisfactory answers to core problems in the subject. Model-building, it seems, has its own value, regardless of whether the models could lead to testable hypothesis.

Despite the limited sample, three findings stand out in our restrictive sample of papers, and to us would seem indicative of the research strategies employed in economics. First, the ambition to combine theoretical analysis and empirical studies seems to be difficult to achieve, as indicated by the few papers actually attempting to do this. Secondly, theoretical analysis is revealed as somewhat of a favoured research strategy among economists. Marshall's warning against long chains of deductive reasoning seems unnoticed among a large segment of the papers accepted in NOPEC in the period. Finally, to the degree that the papers utilised empirical evidence, their strict reliance on a particular kind of evidence is striking. Despite a broad range of available evidence, economists seem to prefer a strict diet of quantitative data, with a large number of observational units, but with a restrictive sample of available variables.

Moreover, our findings seem to be largely confirmed by studies of articles published in the most prestigious journals of the profession. A similar analysis was conducted by Herbert G. Grubel and Lawrence A. Boland (1986). They reviewed articles accepted in leading economic journals and found that papers presenting "Pure Mathematical Models", defined as papers presenting several equations but no empirical results presented in tables, charts or regressions, had increased in all categories of journals. For instance, in their analysis of articles in the American Economic Review, they found that the space allotted to mathematical reasoning had increased from a mere 2.2 percent in 1951 to 44 percent in 1978 (Brubel and Boland, 1986:425). Not surprisingly, then, our sample confirms a broader pattern in economics, wherein reasoning within models, without extensive empirical corroboration, seems to be a central strategy for answering questions. In the following subchapter we will comment on this tendency further.

## 3. Reasoning Without Constraints: The Difficult Case of Tax Evasion

An article by Helmutch Cremer and Firouz Gavari from NOPEC 1997 developing economic theory on tax evasion and tax competition can serve as a typical example for our discussion. The article is neither better nor worse than most of the other theoretical papers surveyed; it is typical as to the kind of statements about the world it offers.

In their article "Tax Competition and Tax Evasion", Cremer and Gavari argue that in a situation of tax harmonization, governments have an incentive to compete through allowing tax evasion. (Cremer and Gahvari, 1997) The argument takes as its point of departure the literature on tax competition, i.e. that governments seek to attract business investment by offering lower taxes. In a globalised world, the loss of government revenue caused by lower tax rates is compensated for by the influx of businesses from abroad which would otherwise not have paid taxes at all. This literature is inspired by the experiences of several countries and is a recurring issue in the EU, where Great Britain and Ireland have lower corporate taxes than other EU countries, and has a plausible and sound empirical footing. However, Cremer and Gahvari take the argument one step further. In a situation of harmonised taxes, they argue, Governments will have an incentive to compete not on tax rates, because they can't, but on compliance measures, in particular the probability of tax audits. Looser compliance measures will allow for lower levels of compliance, and thus to lower real tax rates.

This relatively simple theoretical argument is built on a model of tax competitions. Even this model, which is inspired by empirically observable trends, is built on assumptions which do seem particularly plausible: "Consider two neighbouring countries, *h* and *f*. The population in each country is uniformly distributed over the space it occupies. The two countries are of the same size, populated with persons of identical tastes, and have the same production technologies. (…) There is one private good and one public good in each country. (…) Consumers have preferences which are linear in the private good and logarithmic in public goods."

The model is then modified to allow for competition not in tax *rates*, but in tax *audit probability*. The premise is that firms try to evade taxes at a level which depends on the probability of being caught. By taking into account maximizing behaviour on behalf of the companies, the government can maximize the optimal level of tax audit probability, and the paper derives closed-form solutions for cases with open and closed borders. In short, the modelling is intended to show that since the level of tax affects the price of goods, (which, in the absence of tax evasion, is assumed to be production cost plus the cost of tax), a country could, through lowering the probability of tax audit, thus allowing for tax evasion, lower the tax burden, and thus make the private goods produced in that country internationally more competitive. Selling more of the goods, they would expand their tax base, and thus be able to uphold the consumption of public goods paid for by taxes. Since the government is supposed to wish to maximize the welfare of their country, they will choose an audit probability that allows for optimal tax evasion.

The paper concludes: "Again tracing the equilibrium values of the variables, the paper has shown that as a result of fiscal competition, public good supplies will continue [in the face of tax harmonization] to be less than optimal. However, fiscal competition can now be waged not just through tax instruments but also through audit strategies. The outcome is less than optimal tax rates *and* audit probabilities." The paper lasts 16 pages, and consists of 20 equations and a number of improbable assumptions to arrive at this result, before continuing: "Whether or not the countries do in fact engage in fiscal competition, is of course an empirical question."

It is worthwhile pausing and inspecting what exactly has been achieved in the paper. The paper has demonstrated that under given assumptions, there exists an equilibrium solution to the model where governments' response to an opening of the economy accompanied by a harmonization of tax rates would be to reduce their tax enforcement efforts below the optimal level. The model may thus serve as a predictive device for politicians considering introducing tax harmonization measures, or it may provide an explanans for an expected pattern of decreasing effective tax rates in the face of tax harmonization (explanandum).

Note, however, that several alternative models might be constructed to predict the consequences of tax harmonization, where harmonization can be envisaged to have different, and sometimes opposite effects.

Our first objection has to do with predicting or explaining actual behaviour on the part of politicians. The paper assumes that governments act as a single agent with well defined, consistent and stable preferences. Even if one accepts this assumption, their hypothesis about the government's decision-making can be contrasted with an at least as plausible alternative hypothesis. First, it can be argued with support from anecdotal evidence that most politicians have at least some normative views on the significance of compliance with laws. Second, even those who do not have such moral constraints, might rationally fear that engaging in policies enabling tax evasion could lead to negative media coverage jeopardizing re-election. Thirdly, politicians and bureaucrats might rationally fear that if they encourage non-compliance through low levels of enforcement in one particular area, non-compliance with regulations might spread among the population to other areas, and a perception of unfair treatment on behalf of those who cannot evade taxes might reduce levels of trust in the government. An economic model which does not even consider the kind of externalities that might arise from the model, which immediately suggest themselves to a reader, seems to be a questionable guide to explain actual behaviour, since that behaviour will be informed by the actor's own belief of possible externalities.

Before empirical evidence is introduced, there exists no prima facie reason to expect that governments act in the way the model assumes, and there exists several alternative theoretical formulations that would not need to imply any deviations from rationality.

Even if we find evidence of tax audit policy strategies facilitating tax evasion on behalf of governments, there is a myriad of other rational or quasi-rational reasons why politicians introduce measures that might reduce the efficiency of enforcement of tax legislation, such as budgetary considerations; misapprehensions of the funding necessary to ensure a certain level of collection; or corruption by some specific industries or companies. These, and other, situational constraints faced by policy-makers are simply left out of the model, but anecdotal evidence suggests that they may be pivotal in public decision-making processes.[5]

These objections resemble the standard objections of the non-economist; however, our point is neither to argue against use of rational choice or against this or that assumption in particular; it is against the inclusion of such assumptions in mathematical modelling in a way that isolates the theoretical arguments from available evidence, and in some instances renders empirical verification of the model impossible. Particularly troubling is the tendency to place agents at one end of a possible behavioural spectrum, as for instance when Cremer and Gahvari assume that politicians are led solely by their view on the long term economic welfare of the citizens, or when they assume (albeit implicitly) that there are no negative regulation compliance externalities from one area to another. Very often, we believe, the interesting question is not *whether* politicians have regard for the long term welfare of citizens, but *to what extent*, not *whether* there are externalities, but how serious they are. This general tendency in much economics to use a "0/1-logic" as a basis for mathematical modelling has been brutally criticized by economist Deirdre McCloskey, who sees them as constituting one of the most important obstacles for economists in seeking the empirical sources that they would need to do the math that

[5] A recent example is found in Norway, where employees at the Norwegian tax administration report limited resources has led to significant reduction in audits, as reported by NRK on their webpage: http://www.nrk.no/nyheter/okonomi/1.5293082 (accessed at 12.05.2008)

would actually be needed in order to describe many important real-world problems. (McCloskey 2005)

As a result of the kind of modelling and kind of mathematics which is employed, the crucial questions that would make the model useful can, in our view, not be answered satisfactorily. How would we know whether a given level of tax enforcement results from a conscious attempt to reduce the tax burden or from other factors? How do we know when or whether a change from an open to a closed economy, or a change in tax levels in neighbouring countries, will effect a change in tax enforcement efforts? Does the model help us in devising research strategies to map the relevant beliefs and preferences of the relevant decision-makers? Empirical evidence looking at whether a certain tax enforcement level is introduced or not can be used neither to verify nor to vilify the model, since we do not know whether the model bears any resemblance to reality. If it turns out that there is no empirical backing, the economist would conclude that some of the assumptions have not been met. If it turns out to be empirical backing, the economist would surely claim victory, but since numerous other motivations and circumstances could result in the same policy, and the model does nothing to distinguish between them, the methodological basis for doing so would be extremely shaky.

Our objection is, therefore, put simply: How does such mathematical modelling help us further than a simple verbal (or other) exposition of the basic logic? Why is most of the article spent on this particular type of reasoning, when it is not quite clear what good comes out of it?

Cremer and Gahvari's paper illustrates, in our mind, important features of a predominant research strategy found in economics. The use of models in the social sciences have been studied extensively by philosophers of science (Morrison, 1999; McCloskey, 1994), but the findings from this literature seems to have had very limited impact in economics. There are at least some obvious lessons from this literature that are almost embarrassing to put in print. Firstly, proving something on the blackboard does not prove anything about the world. Cremer and Gahvari seem to accept this when they state that "whether or not countries do in fact engage in fiscal competition, is of course, an empirical question". Still, the authors seem to draw wide implications for policy makers from their modelling exercise: "The paper has shown that, contrary to the case without tax evasion, one can no longer rely on tax harmonization alone to achieve efficiency. Banned from competing in tax rates, the countries continue to engage in tax competition by cutting their audit rates. That is, they implicitly encourage tax evasion! […] These are serious policy questions which must further be investigated." (Cremer and Gahvari, 1997:102)

Although no empirical evidence has been presented to substantiate their claims the authors reach firm conclusions, and the line between reasoning about model-outcomes and real-world outcomes seems, at least, blurred. This feature is not unique to the example discussed here; rather, it seems to be a predominant research strategy. The objection to this strategy is obvious: "Proving" something in a model merely demonstrates the existence of a solution within a model, given the defined assumptions. These existence theorems, however, add no substantive knowledge about the world. That there exists a theoretical model that can demonstrate that under the given assumption A, C follows, is equally interesting as finding a model that deduces D from a set of different assumptions. Neither can, in any reasonable sense, be said to provide knowledge about any given subject. In order to provide explanatory claims about the economy, any satisfactory exposition would have to put forward arguments backed by evidence (McCloskey, 1994).

To be precise, our contention is not simply that the authors should have constructed an alternative model; it is that the returns to the margin of the effort put into these modelling exercises seem to be quite low. We are, in fact, no more knowledgeable about how tax harmonization affects tax rates after reading Cremer and Gahvari, and an alternative set of models constructed along similar lines would demonstrate nothing but the fact that there exists several alternative possibilities for thinking about how governments set tax audit policies in the face of tax harmonization.

Our contention, therefore, is that the return to the margin of this exercise is potentially lower than if the researcher could also follow alternative lines of research. It is difficult to argue that providing empirical evidence on these processes is very costly. Changes in tax audit policies are public events and can easily be mapped. There also exist historical experiments with economic unions and tax harmonization.

To be sure, we are ourselves not experts on tax evasion competition. Maybe Cremer and Gahvari are right, maybe this really *is* how politicians think. It would indeed be interesting to learn that leading politicians believe that it is possible and beneficial to cut levels of tax enforcement in the particular case of tax harmonization between countries. However, Cremer and Gahvari do not investigate that question by asking politicians, present or former, or their economic advisers. They do not investigate whether politicians have strong norms against such behaviour or whether they believe it will result in more widespread non-compliance with laws; neither do they investigate how actual compliance norms are affected by such behaviour by politicians in different societies at different times. Instead, they provide an empty exercise in mathematical logic.

We now turn to the question of why economists employ these research strategies, and why, in our view, these strategies predictably fall short of being able to enhance our understanding of many important questions.

## 5. Is Economic Theory Based on Methodological Individualism?

Discussions about the methodological underpinnings had at least until the 1980s been remarkably cut off from general discussion on social science methodology. Daniel Hausman commented on the situation in 1984 in the following manner: "Most methodological writing on economics is by economists. Although the bulk is produced by lesser members of the profession, almost all leading economists have at one time or another tried their hand at methodological reflection. The results are usually poor […]

Although this literature is heavily influenced by philosophy – both current and, especially, out-dated – it is cut off from philosophical discourse." (Hausman, 1984:231).

Both our own criticism of Cremer and Gahvari and similar literature, and Hausman's of the broader field of economics, may seem harsh, and the usual response from economists seems to be that our diagnosis may have been true in the 1950s and 1960s, but that the developments in the subject since then have altered the practices in the field significantly.

And to be sure, several important alternative perspectives exist, as witnessed by taking a look at the list of Nobel laureates over the last 25 years which includes economists such as Herbert Simon, James Heckman and Daniel Kahneman. However, their programmes have yet to make a great impact on the majority of research published in journals such as NOPEC. Nonetheless, we shall return to these developments in economics towards the end of the next section, to discuss to what extent they address the methodological challenges that much of the economic literature faces. First, however, we need to scrutinize some of the reasons behind the problems we have discussed above.

In this section, therefore, we will deal mainly with the methodological underpinnings of neoclassical economic theory. At least some of the problems we identify here, moreover, apply also to more hetherodox economists such as Simon, Heckman and Kahneman.

So, what is this beast, neoclassical economics? If you ask a methodologically aware economist today, she will usually tell you that economic theory is based on methodological individualism. However, in short, we believe that the common failure of economists to explore contextual explanation is facilitated by a misunderstanding of what methodological individualism is and why it matters. As understood by philosophers of science outside economics, explanations based on methodological individualism assume as their point of departure that it is individuals who act, and that social phenomena can be understood in terms of the actions of individuals, rendered understandable to us by reference to their preferences, and by their perceptions of the world they live in.[6] In order to make economics a methodologically individualist discipline, then, we need a good grasp of people's perceptions and of their preferences of the world they live in– and by implication, we also need to know quite a bit about that world in general. Additionally, of course, we also need a way of translating perceptions and preferences into actions, that is, we need an understanding of the decision-making process, which for instance could be understood in terms of rational choice theory, but which also might be treated in other ways.

Economists usually employ a very specific version of this paradigm, taking as their point of departure for explaining behaviour stable, well-defined preferences and rational choice[7] – and often not reflecting at all over whether the actors share the same

[6] For a definition of methodological individualism, see Elster 1992

[7] For such a definition of neoclassical economics, as opposed to various heterodox methodologies, see Eggertson 1990: 6.

perceptions as those that are implicit in the model. The variable inputs to the equation have been the material world surrounding the economic agents – as seen by the economist – and the task of economists has been to provide those inputs and then, usually mathematically, deduce actions. As we have seen above, this research programme has been firmly established since the late interwar-period onwards.

Interestingly, an early formulation of this specificity of economic theory, and perhaps a precondition for the epistemological considerations found in Frisch and Friedman, is that made in 1932 by Lionel Robbins, who defined economics as "the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses."(Robbins, 1945[1932]:12) The point here is to establish a certain methodology by viewing economics not as a subject describing a specific activity (such as wealth seeking), but rather as a point of view from which to examine activities, namely that they take place in a condition of scarcity (Kirzner, 2000). Robbins wanted to specify that the task of the economist was to focus on the *relationship*, rather than on the *ends* or the *means*. This conceptualization has been extremely influential ever since, and seems to be implicitly or explicitly in use by most modern economists.

It should be noted that Robbins himself did not advocate that any specific ends or means be assumed by the economist; merely that the study of these is not her subject (Kirzner, 2000). He was educated at a time when economists still routinely read a much wider social science literature than subsequently became fashionable, and believed that the necessary input on these issues would come from other field. But the moulding of preferences and perceptions has subsequently largely been absent from economists' attempts to explain economic phenomena. Most economists would probably agree that the formation of perceptions and preferences is a complex study about which current economics has little to say – although attempts have been made to say something about parts of this field by economic psychologists in recent years. Interestingly, Robbins, although admonishing the economist to work theoretically on describing relationships between ends and means, believed that economic theory's "applicability to a given situation depends upon the forces operating in that situation". Robbins accepted the need for systematic investigation to determine these forces; but did not equip his followers with tools to undertake the task.

The problem with the definition is not what it includes, but what it excludes: Seeing economics as (modelling) only the *relationship* between given ends and means, pushes out of focus any systematic attempt at understanding the changes in and development of both the ends or preferences that people hold; and, crucially, at grasping how they think about the use of means through which they can achieve them. Explanation of the world – or prediction – based on methodological individualism, is more than dubious if there is not a sound foundation on these counts. Thus, in the case of economics, methodological individualism necessitates that the empirical context be part of the explanation.

The next few decades after Robbins' definition of economics saw a development from a situation where context was admitted as important, although not a main part of the economist's task, to a situation where context was taken out of economic thinking altogether. This was done through assuming stable preferences and perfect information, as well as *ceteris paribus*-clauses. The paradigm seems to have been accepted on mainly pragmatic grounds; the thought was that without these devices, the task of mathematically modelling complex relationships would be rendered almost impossible. This argument has over the past half century repeatedly been brought forward to defend the use of assumptions (Eggertson, 1990: 9).

Second, economists believed that they achieved good results with these assumptions, even if they might not be entirely accurate. As an explicit methodological recommendation, this thought was initially developed through essays published by Armen Alchian and Milton Friedman in the early 1950s (Alchian, 1950; Friedman, 1953). Friedman likened the rational choice assumption in economics to a description of the laws governing the growing of leafs on a tree: We learn that leafs grow on a tree *as if* they sought to maximize the amount of sunshine each leaf receives. Of course, leafs do not deliberately seek to maximize – but the result fits the predictions nonetheless. To Friedman, the justification of neoclassical economic theory was to be found in its yielding correct predictions. Friedman's position has been reformulated more recently by Debrah Satz and John Ferejohn, who claim that structures – for instance the discipline of the market place or internal company procedures, produced by a Darwinian survival of the fittest in the market place – ensure such things as stable preferences and rational choice, even if individual psychological states vary (Satz and Ferejohn, 1994).

For the methodological individualist, there are two serious objections to accepting Friedman's practice. First, as philosopher of science Daniel Hausman has pointed out, that predictions sometimes conform to observations is an insufficient reason for the methodological individualist to rest content with his explanation. If outcomes conform to rational choice, whereas psychological states do not, then an explanation must include a specification of how the actual psychological states are translated into that outcome (Hausman, 1995). If our models are built on rational people or firms with stable, well-defined preferences – even when we have reason to believe the assumptions to be false – whereas outcomes actually are explained by some other features which make people act "rationally" in the sense the model prescribes, we need to explain those features. Otherwise, it will be impossible to know when predictions will work and when they won't.

Second, and related, Friedman's belief that economic theory always yields correct predictions is ludicrous, and economists today know it. There was a time when economists would respond that economic theory *works*. Even if they could admit that strictly speaking, Hausman's objection to Friedman is correct, they believed that the success of economic theory suggests either that assumptions moreover are sufficiently accurate, even if economists don't bother to investigate them empirically, or that the theory consistently works according to some other logic, for instance a functionalist principle ensuring substantively rational behaviour (Satz and Ferejohn, 1994), even if the economist doesn't bother to explore that logic in detail. However, those times should now be long past. Economics has not succeeded in several of its major fields: Predicting or explaining policy consequences in developing countries; understanding transition economies; or explaining long term consequences of Western economic policies such as the welfare state can serve as examples, in addition to all the small conundrums of every day life, such as stock market crashes, housing prices, business cycles or technological development.

Whatever mechanisms Friedman or Satz and Ferejohn believed would compensate for the often false underpinnings of economics, they have proved not to hold. Philosopher of science Alexander Rosenberg has pointed out how inept the comparison between Darwinism and economics is (Rosenberg 1992), and also that neoclassical economic theory in its current state – a theory that sometimes works and sometimes does not – in the natural sciences would have been considered an abject failure, whose basic tenets would be in need of critical scrutiny and overhaul (Rosenberg, 1986: 130). That economists too often fail to do so when their theories fail, and rather come up with *ad hoc* hypothesis, is testimony to a problematic relationship to the methodological underpinnings of the existing paradigm.

By paying too little attention to the significance of the veracity of the assumptions employed in economic theory, and by the unwillingness to discuss the basic tenets of the theory, economics has relegated itself to the fringes of methodological individualism. The reason is that methodological individualism is built precisely on the veracity of its statements about preferences and perceptions. If we do not care about actual individual reasons there is nothing left of the paradigm. This view is not an attack on rational choice, or even stable preferences. We believe that rational choice is often a sufficiently accurate description of human decision-making. We believe that for certain time spans, in certain countries, in well-defined contexts, preferences are indeed relatively stable. We also believe that in many contexts, it is relatively obvious what people believe about the world, and how they think about the means through which they can reach their goals. In other words, sometimes the "extra curricular" work of the economist would be rather trivial; she would just need to confirm that indeed, the standard assumptions seem to be relatively accurate.

However, in many other contexts, it is not obvious how people think about ends and means. Even if we accept that most people tend to behave rationally in most circumstances, our models should take into account *what they believe to be rational.* Philosopher of history Geoffrey Hawthorn has forcefully argued this point: "even if we believe that a "real interest" […] is guiding people's reflections, we start from what we take these reflections to be. If we do not, it is not clear in what sense we are talking about the practical reflection which is theirs. Social and political theorists […] have usually resisted [this]. Theoretical reasoning, they have believed, has been sufficient to explain why people do what they do." (Hawthorn 1991:162).

Economists have not been entirely oblivious to some of these problems. Promising developments, in our view, are found in research conducted in micro-econometrics and behavioural economics, represented for instance by Nobel laureates James Heckman and Daniel Kahneman respectively. Heckman has demonstrated the need to put the representative agent, utilized in a majority of modelling exercises, to serious scrutiny. To quote Heckman, a central result of the research in micro-econometrics has been that ”the long-standing edifice of the representative consumer was shown to lack empirical support” (Heckman, 2001 :674). A mass of empirical research in economic psychology has similarly demonstrated in various ways how typical respondents fail to meet the criteria of decision-making assumed in standard models of choice. The challenge, to quote Daniel Kahneman is “the construction of formal models based on “common sense psychology of the intuitive agent” (Kahneman, 2003:1470).

The work of Heckman and Kahneman and others goes some way to address some of our concerns – though not all of them. This research does take methodological individualism seriously – however, in our view, it still downplays the role of historical context in building explanations based on methodological individualism. Kahneman's work on decision-making procedures, for example, draws the economist's attention to several well-known mechanisms affecting decision-making, such as the use of heuristics or the effects of cognitive dissonance on perceptions. His focus, however, is rather different than ours: Kahneman seeks to identify *universal* aberrations from rational choice procedures, that is aberrations that are independent of context. Thus, he adheres to Robbins' admonition to work to understand the *relationship* between means and ends, but not those means and ends in themselves. Our focus is that the economist needs to know the *specific* context of the situation in order to understand for instance what kind of heuristics actually makes sense.

Dealing with perceptions and preferences implies paying attention to the reasons for which economic agents make their choices. Put differently: the methodological individualist needs quite a bit of insight into how people look at the world in order to understand how they can rationally make the decisions they make. There seldom exists only one rational response to a challenge; however, when we look at people around us and the context they are in, their actions can normally be rendered subjectively rational and objectively *reasonable*. Explanations based on “reasonable choice” should therefore be part of the economist's repertoire alongside with rational choice.[8] And that would necessarily include extensive references to historical context.

These points are illustrated well by the article on tax evasion described above; no empirical evidence is put forward about the how politicians think about tax evasion and its effects; no empirical evidence is put forward to support the arguments about how firms decide on their level of tax evasion or compliance; no arguments are made about how attitudes might change over time, or whether such change might be related to policies.

[8] See Andresen, 2006: 45 for a discussion on how historians can be said to base explanations on a form of “reasonable choice”-theory.

Theoretical reasoning, largely in the form of mathematical modelling, based partly on unrealistic assumptions, is what is offered instead. The argument thus goes as follows: If the world were as it certainly is not, then it would have been as it isn't.

Sometimes, of course, the assumptions are sufficiently close, and the world described by the model is recognisable to us. However, in our opinion, economic theory, although seemingly adhering to the principles of methodological individualism, too often ignores a crucial tenet of that research paradigm, namely that the assumptions we make about perceptions and preferences be true. If they are not, or if we do not care whether they are, economic theorizing is reduced to *etudes* in logic.

## 6. When Does it Matter?

An important question, we believe, is then to have some guidelines as to when the theoretical modelling currently employed in economics actually goes someway to describe the world, and when it simply constitutes exercises in logic. Since perceptions and preferences have everything to do with the context of the decision-makers that make up the economic phenomena to be explained, we believe that when economic theory sometimes works well, it is because the assumptions made on these issues sometimes correspond sufficiently well to the context of the economic phenomena to be explained. What we need to know, is when we have reason to believe that the assumptions currently employed by mainstream economics are insufficient to provide meaningful insights about the real world.

In brief, economic models will tend to work less well in two important circumstances. The first one is when *actors do not in fact hold the perceptions or preferences imputed to them by the model*, that is, when the initial assumptions are untrue. We believe that whereas preferences often, though not always, sensibly can be understood in terms of maximizing or satisficing, perceptions of the means through which preferences can be achieved are often more complex than and different from what economics implicitly presupposes in theoretical models. In general, it is probably the case that the more complex is the relationship between the action perceived suitable and the preferred outcome, the greater will be the variety of perceptions of optimal actions to achieve a given preference. Furthermore, it is probably the case that the further removed the economic actors to be understood are in time and space from those who seek to understand, the greater is the risk of a very different set of perceptions altogether, even as these actors seek rationally to maximize their utility. If Norwegian or American economists write about India, Nigeria or Russia, they would be wise to read a great deal more about those countries than quantitative economic indicators alone.

The second case is when *perceptions or preferences change over time*, that is, when the *ceteris paribus*-clause is untrue. If purposes are stable during the time span of interest, understanding their formation is not necessary to providing an explanation of an economic phenomenon; it is enough to establish what they are – although economists rarely do that either. However, we know that norms, worldviews, social practices and business practices change over time, sometimes quickly, sometimes slowly. Particularly when economists consider situations of some duration, there is little reason to believe the *ceteris paribus*-clause will hold true, although sometimes the consequences are not serious for economic theory.

Such change can happen due to ideographic factors that have nothing to do with the logic of the model itself, i.e. breach of the condition of temporal isolation of the phenomenon; or it can be causally related to actions in the model, i.e. breach of the condition of causal isolation.[9] In the former case, economists need to be aware of the changes that can plausibly be predicted, even if they are not part of the model. Say, for instance, that we could plausibly know that prospective parents become less and less willing to reduce their living standard to have children, but that this trend is unrelated to policies to support families with children. The effect of such policies on child birth would then diminish over time, and if this is predictable, it needs to be taken account of in modelling the effect of policies. To take into account all the changes that can happen in the world outside the model, is of course not easy; the world is an unpredictable place, and economists should not be blamed for not always getting it right. But neither should they disregard evidence that the environment is changing in ways that would affect the outcomes of their models. The more important case, however, is when changes in perceptions and preferences should have been endogenous in the model, whereas economists assume they are exogenous. Say, for instance, that prospective parents become less and less willing to reduce living standards precisely *because* of policies supporting families with children, which increase their expectation of future support and change their view on the responsibilities to be born by parents and the government respectively. A model not factoring in this change in preferences would be grossly misleading. As pointed out in the discussion about tax evasion above, it could plausibly be believed that a policy promoting tax evasion could have lasting changes on norms and beliefs about tax payments more generally. If that is the case, a model not taking such change into account will fail.[10]

One might ask: How could the economist possibly know when there might be such exogenous or endogenous changes in perceptions and preferences that might affect the validity of economic modelling? Our argument is that with today's research practices, she couldn't, and she doesn't. However, if economists displayed a methodological awareness of and interest in questions pertaining to perceptions and preferences, and if they were more willing to read many more sources to get a grasp of the economic agents whose actions they ultimately seek to explain, we believe they would be in a much better position to understand when such issues might arise when they do not. To us, it seems obvious that whereas simple price changes in a spot market normally might be believed

[9] The problems related to causal isolation and the use of ceteris paribus-clauses in economics were discussed by Lucas in his pioneering article 'Econometric Policy Evaluation: A Critique' in 1976.
[10] For an elucidating discussion of the problem of endogenous preferences, see Bowles, 1998.

not to affect perceptions and preferences too much, most broad policy questions usually have impacts on discursive practices and social relations beyond their effects on immediate economic outcomes.

## 7. Conclusion

What we propose, then, is a strategy paying attention to contextual detail as part of the explanation of economic phenomena. Shorter chains of formal mathematical arguments would need to be supported by narratives explaining the situation economic agents are in, their view of that situation and the changes in that view over time. The observant reader will notice that our suggestions have much in common with the research strategies prescribed by Alfred Marshall a century ago. The great advances made in economic theory and econometrics over the past century notwithstanding, we believe that the future of economics lies in incorporating better the insights of its own past.

We want to underline again that this article is not meant as an all-out attack on economic theory. We believe in the usefulness of rational choice; in the usefulness of the *ceteris paribus*-clause and other assumptions such as perfect information. However, we also believe in the necessity to understand more about the context in which economic theory is meant to apply. Only by doing so is it possible to know when assumptions are justified and when they are not; only by doing so is it possible to regain a better balance between induction and deduction in economics. We thus believe in reinvigorating the tradition of the fathers of modern economics, such as Alfred Marshall and Lionel Robbins, of short chains of deductive reasoning, interacting with constant reference to various aspects of the real world.

It is not a question of supplanting existing knowledge and practices with something entirely different; but of supplementing it with research strategies which offer a better chance of being both relevant to real world problems and methodologically sound. What we propose, is that economists should start taking methodological questions seriously, by devoting sufficient time to the study of perceptions and preferences, and the way in which they change and interact; and when necessary, spend somewhat more time on understanding context and somewhat less time on modelling a world which does not exist. What does this admonition imply?

First, it should be noted that the study of perceptions and preferences is an extremely wide subject. Understanding preferences implies understanding norms, social institutions and psychology, which each in its own right is the specialty of other disciplines such as sociology and anthropology. Understanding perceptions implies understanding the way in which people think, which again entails a hoist of difficult questions that is the hard toil of discourse analysts and cognitive psychologists. Naturally, the economist cannot be equally eloquent within all traditions that have insights to offer about social phenomena. Might one not object to our admonition that the economist cannot plausible master all these disciplines? Perhaps – but she might not need to. What she needs to understand, is when and how perceptions and preferences matter for economic theory, and what disciplines she needs to be able to engage with in order to improve her research. In order to do that, however, the economist needs better knowledge of the context of the subject of study. Such knowledge can be achieved, but it requires some effort, akin to that which historians undertake when they start researching a topic.

As we see it, our view on the state of economics would also suggest that changes be considered both in education and in research communities. Researching and working effectively with economic perceptions, ideographic events and broad social contexts requires skills that are not taught extensively in most economic departments. An important challenge, in our view, is to develop in new economists skills that allow them to communicate and engage more efficiently with other social science disciplines.

Of course, *all* economists should, as today, be supposed to master formal modelling, and engage in arguments based on rigid, theoretical analysis, or statistical material. What we suggest is that, in addition, *all* economists should have sufficient knowledge about the methodology of contextual explanation to be able to engage in discussions – and cooperation – with sociologists, political scientists and historians about the possible importance of endogenous or exogenous social, political or other influences on their models and explanations. Finally, *at least some* economists should engage in contextual explanation themselves, which could serve the purpose of a more meaningful interaction between model and reality, deductive and inductive thinking.

As a result, we would hope that articles in economic journals would see less space devoted to long chains of deductive modelling without reference to fact; a slide towards a form of mathematics better able to take into account empirical detail; and greater variety of sources accepted. By doing so, the future of economics would incorporates some of the best parts of its past, even as developing the field further and in new directions. As stated in the introduction, we believe the marginal utility of obtaining knowledge of historical processes in economics would be higher than obtaining more knowledge on mathematical modelling. Better still, the cost of buying this good is actually quite low: As some might find out, figuring out how things *really* are, can actually be quite fun.

## Bibliography

Alchian, A.A. 1950, "Uncertainty, evolution and economic theory", *Journal of Political Economy*, Vol. 58.

Andresen, N.A. 2005. "As safe as the Bank? Household financial behaviour and economic reasoning in post-soviet Russia", *Working Paper 687,* Norwegian Institute of International Affairs, Oslo.

Andvig, J. C. 1986. *Ragnar Frisch and the Great Depression. A study in the Interwar History of Macroeconmic Theory and Policy*, Norsk utenrikspolitisk institutt, Oslo.

Bergh, T. and T. J. Hanisch, 1984. *Vitenskap og politikk: linjer i norsk sosialøkonomi gjennom 150 år*, Aschehoug, Oslo.

Blaug, M. 2001. "No History of Ideas, Please, We're Economists", *Journal of Economic Perspectives,* 15:145-164.

Bowles, S. 1998. "Endogenous Preferences: The Cultural Consequences of Markets and other Economic Institutions", *Journal of Economic Literature*, 36: 75-111.

Bucholz, T. 1998. *New Ideas from Dead Economists,* New American Liberty, New York.

Coleman, D. C. 1987. *History and the economic past: an account of the rise and decline of economic history in Britain*, Clarendon Press, Oxford.
Cremer, H. and F. Gahvari, 1997. "Tax Competition and Tax Evasion", *Nordic Journal of Political Economy,* 1997:89-104.

Eggertson, T. 1990. *Economic Behaviour and Institutions*, Cambridge: Cambridge University Press.

Elster, J. 1992, *Political Psychology*, Cambridge University Press, Cambridge.

Friedman, M. 1953. *Essays in Positive Economics*, University of Chicago Press, Chicago.

Frisch, R. 1933. "Editor's note." *Econometrica*, 1(1): 1-4.

Grubel, H. A. and L. A. Boland, 1986. "On the Efficient Use of Mathematics in Economics: Some Theory, Facts and Results of an Opinion Survey", *Kyklos*, 39:419-442.

Hausman, D. 1984. 'Philosophy and Economic Methodology', in *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association*, Vol. 1984, Volume Two: Symposia and Invited Papers, (1984), pp. 231-249.

Hausman, D. 1995. 'Rational Choice and Social Theory: A Comment', *Journal of Philosophy*, Vol. 92, No. 2.

Heckman, J. J. 2001. "Micro Data, Heterogeneity, and the Evaluation of Public Policy: Nobel Lecture", *The Journal of Political Economy*, 109(4): 673-748.
Hodgson, G. M. 2001. *How Economics Forgot History: The Problem of Historical Specificity in social science,* Routledge, London.

Kahneman, D. 2003. "Maps of Bounded Rationality: Psychology for Behavioral Economics" *The American Economic Review*, 93(5): 1449-1475.

Kirzner, I.M. 2000. 'Human Nature and the Character of Economic Science. The Historical Background of the Misesian Perspective', *The Harvard Review of Philosophy VIII.*

Louçã, F. 2007. *The years of high econometrics : a short history of the generation that reinvented economics*, Routledge, London.

Lucas, R. 1976. "Econometric Policy Evaluation: A Critique.", *Carnegie-Rochester Conference Series on Public Policy* 1: 19–46.

Marshall, A. 1920. *Principles of Economics,* (Eight edition), Macmillan and Co., Ltd., London.

McCloskey, D. N. 1994. *Knowledge and persuasion in economics*, Cambridge University Press, Cambridge.

McCloskey, D. 2005. 'The Trouble with Mathematics and Statistics in Economics', *History of Economic Ideas XIII* (3,2005): 85-102.

Morgan, M. S. 1990. *The History of Econometric Ideas,* Cambridge University Press, Cambridge.

Morrison, M. and M. S. Morgan, 1999. *Models as mediators: perspectives on natural and social science*, Cambridge University Press, New York.

North, D. C. 1993. "Nobel Prize Lecture: Economic Performance Through Time"(Dec. 9, 1993)

Ragin, C. 2000. *Fuzzy-set social science,* Chicago University Press, Chicago.

Robbins, L. 1945 [1932]. *Essay on the Nature and Significance of Economic Science*, Macmillan, London.

Rosenberg, A. 1986. 'What Rosenberg's Philosophy of Economics Is Not', *Philosophy of Science*, Vol. 53.

Rosenberg, A. 1992. 'Neoclassical Econoimcs and Evolutionary Theory: Strange Bedfellows?', *Proceedings of the Biennial Meeting of the Philosophy of Science Association 1992,* Vol. I.

Satz, D. and J. Ferejohn, 1994. "Rational Choice and Social Theory", *Journal of Philosophy*, Vol. 91, No. 2.