Limits of Econometrics for Testing Economic Hypotheses

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1. INTRODUCTION

David A. Freedman’s (DF hereafter) fascinating methodological survey article explains very concisely the principal challenges faced by economists and other social scientists when they aim to test hypotheses using statistical (or econometric) models. He also discusses the principal responses to these challenges. Let me state from the beginning that I agree with most of DF’s central methodological assessments, in particular his well-reasoned “considerable scepticism about disentangling causal processes by statistical modelling”.

Arnold Zellner’s (AZ hereafter) important observations on DF’s survey follow directly from the principles of his well-known philosophy of econometrics. I will provide additional commentary on David Freedman’s survey and views, by discussing the various proposals how (not) to use econometric methods to test (hypothesised) causal structures in economics. In this context, I will also consider several of AZ’s very interesting and helpful methodological recommendations.

2. THE START OF THE DEBATE ON THE LIMITS OF THE SCIENTIFIC USE OF ECONOMETRICS

As noted by DF, the debate on the limits of the scientific use of econometrics (i.e. testing economic hypotheses using econometric methods) started very early on. J. M. Keynes raised in 1939, at the peak of his fame, fundamental questions about econometrics as a serious, at that time new, scientific discipline. To that end, John Maynard Keynes drew one of its pioneers of econometric modelling, the Dutch economist and physicist Jan Tinbergen¹, into a methodological debate on the scientific usefulness of econometrics. Keynes was perhaps the first influential scholar with global reach who expressed a strong disbelief that econometric (or statistical²) methods can be used “to disentangle complex causal processes” in economics.

Ever since, challenges related to testing causal relationships using econometric methods have been subject of serious debate. I would argue that many of these discussions (summarised conveniently by DF) are serious enough to label them part of the ongoing debate on the “Philosophy and Methodology of Econometrics”.

¹ Jan Tinbergen received in 1969 (together with Ragnar Frisch) the first “Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel”. He studied mathematics and physics at the University of Leiden under Paul Ehrenfest. During 1929 he earned his PhD degree at this university with his thesis entitled “Minimumproblemen in de natuurkunde en de economie” (Minimisation problems in Physics and Economics).
² I will use these terms as synonyms in this article.
3. SEMANTICAL INSUFFICIENCY COMPlicates GReATLY TESTING ECONOMIC HYPOTHESES IN ECONOMETRIC FORM

Modelling challenges manifest themselves via the absence of “strong prior theory to limit the universe of possible (statistical or econometric) models.” Economists (and other social scientists) have to operate with theories that share a number of key epistemological characteristics that limit strongly their empirical content (or predictability). As a result, economists are usually facing the problem of under-identification (Liu, 1960; Sims, 1980). The key source of this problem is in my view the inherent semantical insufficiency (SI) of economic theories (Blommestein, 1985). Semantically insufficient theories have difficulties (or are totally incapable) in identifying and predicting causal patterns or explanations in a precise fashion. As a result, SI creates a situation in which the deduction of the empirical implications of an economic theory becomes to an important degree an arbitrary exercise.

The empirical content of economic theories is therefore systematically lower than those from physical theory. Testing an economic theory in quantitative form requires the introduction of all sorts of ad hoc statistical or econometric modelling assumptions so as to arrive at a fully specified empirical model (Blommestein, 1985). This ad hoc nature of economic model-building generates a significant degree of specification uncertainty. For example, as noted above, the empirical pricing models for structured products such as CDOs and CDSs is hampered by a considerable degree of specification uncertainty. Semantically insufficient theories make it therefore very hard to formulate reliable empirical models. In other words, the big problem with economic theories is not that they are too simplistic or that so-called ‘unrealistic’ assumptions are being used, but it is their semantical insufficiency (low degree of testability). By not taking the specification uncertainty caused by semantical insufficiency seriously enough, testing competing theories becomes a wasteful exercise. At the minimum one needs statistically reliable econometric models, although technical virtuosity can never substitute for increasing the information content of economic theories.

DF discusses various proposed solutions to the problem of specification uncertainty. In doing so, he summarises much of the critical literature on the limited ability of advanced econometric (statistical) methods to make causal inferences from observational data. He also discusses many of the principal responses by modellers and economic theoreticians to several of these critical comments.

4. TAXONOMY OF PRINCIPAL RESPONSES BY MODELLERS AND THEORETICIANS

I will make the following distinction (in a highly stylised fashion) among the various responses by modellers and theoreticians.

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3 It is a separate (but related) question to what extent economists and other social scientists are really able to formulate competing economic theories that can be identified on the basis of different structural parameters (Blommestein, 1985).

4 See section VII and VIII for a brief overview of suggested improvements in economic theories and/or econometric methods. D.F. Hendry (Ericsson, 2004) provides an overview of the many efforts made since the 1970s to deal in a more satisfactory way with specification and identification problems.

5 Summers (1991) concludes from his critical analysis of advances in econometrics that it is much easier to demonstrate an author’s capacity for statistical ‘pyrotechniques’ than to make a contribution to economic knowledge.
(Strategy A) Improvements (I) of economic theories (TE) with the same (S) quantitative methods (QM) including econometric modelling approaches;
(Strategy B) Same economic theories with improvements in QM;
(Strategy C) Improvements in both theory and QM.

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Table 1: The Growth of Knowledge: Improvements (I) in Theory (TE) and Econometric Methods (MA)

Source: Author

Strategy A covers the bulk of the ‘regular’ research output in economics (or, at least, it is the intention of many stated research programmes that they seek to improve theories using existing methods or econometric modelling approaches). This strategy covers de facto also the position of those researchers, like J.M. Keynes, who do not think improvements in econometric methods are useful for testing or disentangling complex causal processes in economics.

From the perspective of the growth of knowledge, the status-quo (SQ) covers in principle straight-forward applications of existing TE and QM (e.g. for public policy purposes or consultancy in the private sector). Clearly, the concept of the SQ as such is a dynamic one in the sense that is related to knowledge at specific points-in-time that will change gradually or by leaps-and- bounds under the influence of scientific progress (regarding ‘theory’ and/or ‘methods’). For example, in the 1950s and 1960s, most policymakers used empirical macro models based on the following simultaneous equations modelling (SEM) framework of a set of variables $Z = \{Y, X\}$ developed by the Cowles Commission research programme:

$$BY_t = \Gamma X_t + \varepsilon_t$$

(4.1)

With $Y_t = a (g \times 1)$ vector of endogenous variables; $X_t = a (k \times 1)$ vector of explanatory variables; $\varepsilon_t = a (g \times 1)$ vector of unobserved error terms; and $\Gamma$ and $B$ matrices with to be estimated parameters of dimension $(g \times k)$ and $(g \times g)$, respectively. Most of the original Cowles work was based on the additional assumptions that error terms are IID (Independent and Identically Distributed) and that functions are parameters are LCC (Linear with Constant Coefficients). Tellingly, DF refer to them as “articles of faith” that had (and continue to have) a considerable influence on the applied literature.

The empirical and theoretical failings of the Cowles programme weakened this SQ-approach and encouraged the development of alternative theories and/or econometric modelling strategies (see below). Changes in modelling strategies often included the relaxation of the IID or LCC assumptions.

The principal focus of the remainder of this rejoinder is on econometric modelling efforts that constitute part of strategies B and C. I will elaborate on them by discussing several methodological problems from academic finance models with a few examples related to the recent global credit crisis. In doing so, I will argue that a great deal of the (financial) econometric model-building efforts have in my view gone astray by not acknowledging sufficiently or adequately the implications of economics as a social science.
5. WHAT ARE THE MODELLING IMPLICATIONS FOR ECONOMICS AS A SOCIAL SCIENCE?

Improvements of econometric methodologies and techniques should be based on the notion that economics (finance) is part of the social sciences. A thorough epistemological and methodological analysis of the nature and limitations of economics as a social science leads to the core conclusion that the empirical (or predictive) content of economic theories is relatively low while specification uncertainty of empirical models (of economic hypotheses) is very high (Blommestein, 1985). For example, the foundation of the pricing of risk in structured products such as CDOs and CDSs is based on the key theoretical notion of perfect replication. Naturally, perfect replication does not exist in reality and has to be approximated by historical data which in many cases is very incomplete and of poor quality. Instead, researches and practitioners had to rely on simulation-based pricing machines. The input for these simulations was very shaky as they were based on “relatively arbitrary assumptions on correlations between risks and default probabilities” (Colander et al., 2009). Another weak part of the chain from theory to practice was the adoption of an industry-standard for simulation-based pricing machines by the leading rating agencies.

Academic economists should have loudly objected to these developments and practices (in their capacity as scientists) as they constitute an unsound scientific basis for the reliable pricing of risk. It is therefore essential to acknowledge both in theoretical and empirical work that economics (or finance) as an academic discipline has no relationship to the epistemology and methodology of the physical sciences. But this insight is not apparent from academic practices. There is a formidable collection of economic and finance studies in which mathematics and advanced statistics play an as important (or sometimes even more prominent) role as in physics, while (again like in physics) empirical regularities are often considered or treated as if they reflect constant structural economic laws and parameters.

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However, economics and physics are qualitatively different enterprises, and always will be. Economics or finance as a social science differs in at least two important respects from the physical sciences. First, physical theories often formulate scientific predictions in the form of individual events, while predictions in economic theories (and other social sciences) are usually specified as patterns of a certain kind or type (Hayek, 1967; Blommestein, 1985:86-90). Second, theories of economic behaviour need to take into account reflexivity – the human capability for self-reference. Self-referential calculations have major (complicating) implications for economic explanations and predictions. For example, financial asset markets have a reflexive nature in the sense that prices are generated by the expectations of traders. But the latter expectations are based on the basis of the anticipation of others’ expectations. This implication precludes the formation of expectations using deductive rules (Spear, 1989).

In a fundamental way, economic issues (and social science topics in general) are harder and far more complex to analyse scientifically than topics from the physical sciences. The result of this insight should not necessarily be more emphasis on quantification and the use of ever more sophisticated statistical techniques (i.e. Strategy B) as more technical sophistication has too often functioned as a veil. Instead, it should encourage the development of better theories (with a higher empirical content) [i.e. strategies A and C] that allow the “credible”

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6 W.B. Arthur (1994) shows why an asset market equilibrium cannot exist when all economic agents use the same meta model for predicting the state of the market (implying homogeneity in forecast rules). The implications of reflexivity for identifying rational expectations equilibria was studied by Sargent (1993).
identification of structural parameters (Blommestein, 2009). A key question is how to attain economic theories with higher empirical content. Unfortunately, academic economists give very different answers to this question. Behind the veil of the working of our academic journals, the unity of economic science is missing.

6. THE UNITY OF ECONOMIC SCIENCE

AZ emphasises in his methodological work the principle of the unity of sciences. Can we reconcile this principle with our conclusion that there are specific and complicating modelling challenges for economists and other social scientists? AZ advocates Karl Pearson’s ‘unity of science principle’. This methodological principle embodies the notion that “any area of study……can be scientific if scientific methods are employed in producing, analyzing and learning from data” (Zellner, 2009:1). It seems likely that most academic economists are in agreement with this principle.

The problems and disagreements begin when we have to agree about what is exactly meant by ‘scientific methods’. AZ argues that Harold Jeffreys’ Theory of Probability is a fundamentally important framework “for scientific research not only for the physical sciences but also for all areas of study, including the biological sciences and social sciences…” But, unfortunately, not everybody seems to agree (either implicitly or explicitly).

For example, Debreu (1991) notes that economic theory cannot follow the role model offered by physical theory because economics is “[b]eing denied a sufficiently secure experimental base…”. For that reason, “economic theory has to adhere to the rules of logical discourse…” He advocates therefore allocating research efforts mainly (or even exclusively) to the formulation of deductive structures in mathematical form to check for internal consistency (Debreu, 1991). This is in my view a too minimalist research strategy. Debreu claims that his recommended approach has not only led to (the development and use of) better quantitative methods but also to improvements in economic theory (Strategy C in Table 1). However, it is doubtful whether we can have genuine knowledge growth without empirical testing. A theory that consists of a set of tautologies cannot predict (Friedman, 1953). Naturally, a correctly formulated mathematical model of complex economic interactions is to be preferred over an econometric model that is ‘identified’ using spurious “a priori” restrictions. But to give-up all attempts at empirical testing because we cannot replicate the role model offered by physical theory seems too extreme (and misguided). In effect, we would get stuck in strategy B. Respectable and perhaps impressive, but not good enough. Unfortunately, Debreu’s echo in economics is still fairly strong as evidenced by key concepts from modern macroeconomics (including many areas of macro finance) such as the rational expectations equilibrium concept, time inconsistency, optimal policies framed as Ramsey and mechanism design problems, foreseen versus unexpected policy actions, and reputation and commitment under rational expectations.

Against the backdrop of this analysis, bringing together the insights from different parts of economics such as macroeconomics and financial economics is not necessarily the best response to the recent wave of criticism of academic finance. The end result [of this intended merger of theories] would still not go beyond disguised mathematics or sets of tautological structures. For example, the underlying behavioural paradigm of macroeconomic/macro finance models is that economic agents have rational expectations, meaning that the

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7 C. Sims (1980) explains that many so-called identified structural parameters are not credible but, instead, the result of spurious “a priori” restrictions.
“representative” agent is at all times and everywhere completely informed, while fully understanding the complexities of the world (De Grauwe, 2009). This would imply that bankers and their clients at all times fully understand the complexities of the new financial landscape. It is difficult to reject the impression that these behavioural assumptions have been introduced to facilitate the underlying math and the formulation of quantitative models with closed-form solutions.

From Debreu’s perspective, this mathematization of economic theories has been very successful. But most economists have ambitions beyond the formulation of an elegant set of tautologies. I agree therefore with Friedman’s observation that economic theory must be more than “disguised mathematics” (Friedman, 1953:11-12). In other words, economic theory must be more than a structure of tautologies if it is to be able to predict...

This perspective puts the spotlight on the second key methodological requirement: external or empirical consistency. On this second condition more research efforts have been spent than on logical or internal consistency (condition 1) which, by itself, is a good allocation of research resources since economics is a social science. Unfortunately, this empirical ‘testing’ of theories by economists was to an important extent a waste of time. Interestingly enough, already several decades ago Arrow noted that testing for statistical significance as practiced by economists is in many instances a non-scientific activity (Arrow, 1959). As summarised some time ago by McCloskey (1998): …..“Leaders of the economics profession think that the main empirical rhetoric in economics is nonsense.” Arrow (1959) argues that economists should spend scientific time on arguing, observing and estimating rather than pseudo-testing by Student’s-\(t\).

Against this backdrop, I will give additional comments on some of the proposed alternative theories and/or modelling approaches that are meant to deal more adequately with the problem of testing economic hypotheses. DF classifies them as part of the ‘critical literature’.

### 7. SAME (SEMANTICALLY INSUFFICIENT) ECONOMIC THEORIES WITH IMPROVEMENTS IN ECONOMETRIC METHODS (STRATEGY B)

Liu (1960) and Sims (1980) argue that the a priori exclusion of variables from causal equations can seldom be justified on economic theory grounds. As a result, “there will be typically more parameters than data” (Freedman, 2009:3). Sims’s response to the resulting fundamental problem of under-identification (an important example of the semantical insufficiency of macroeconomic theories) is to employ low-dimensional time-series methods. His vector autoregression (VAR) approach replaced the ‘spurious’ a priori restrictions on behavioural equations by covariance restrictions on time-series processes. In doing so, “innovation accounting replaced the causal analysis “of the Cowles Commission research programme (Heckman, 2000).

Judging from the many academic articles that use VAR, Sims’s (1980) approach is quite successful. The \(\text{VAR}\)-approach starts with modelling the variables \(Z_t\) from (4.1) by means of an unrestricted VAR model with innovation process \(E(\varepsilon_t / Z_{t-1}, Z_{t-2},...,)=0\): \[ C(L) Z_t = \varepsilon_t \] \[ \text{(6.2)} \]

Where \(C(L)\) is a matrix polynomial in the lag operator \(L\) of order \(n\). The choice of \(n\) is not determined by economic theory but, instead, is data-driven to ensure that the model-derived \(\{\varepsilon_t\}\) is an innovation process. In other words, a crucial notion in Sims’s VAR-approach is that
VAR residuals are serially uncorrelated by construction (and not by assumption as in the Cowles SEM model (4.1)).

C. Sims, T. Sargent and other time-series analysts made great efforts in justifying the errors \{e_t\} as innovation shocks with a structural interpretation. In the VAR-approach the ‘structural’ counter-part to the Cowles model (4.1) is obtained via the specification of a moving average representation (MAR):

\[ Z_t = C(L)^{-1} \epsilon_t \]  

Innovation accounting involves estimating the impact of innovations in \{\epsilon_t\} on the time-paths of the \(Z_t\). The effects of \(\epsilon_t\) through the transmission mechanism \(C(L)^{-1}\) are interpreted as the key generator of macroeconomic dynamic responses of exogenous shocks (or impulses) on the outcome variables (Qin and Gilbert, 2001).

Sims’s (1980) critical attitude vis-à-vis the usefulness of the structural Cowles model (4.1) is based on his diagnosis that the identification of structural parameters in (4.1) has insufficient credibility. This conclusion created quite a stir. Especially during its earlier stages, Sims’s attack led to a polarised methodological debate. There were also confusions about the epistemological meaning of VAR results caused, I believe, by different interpretations of the roles of economic theory and/or economists’ beliefs that were associated with the identification conditions in the VAR-approach. For example, Heckman (2000) argued that there is “an air of mystery and controversy” surrounding the information generated by the unrestricted VAR model (6.2).

A second point often made by those sceptical about the alleged superiority of the VAR-approach vis-à-vis the Cowles Commission framework, concerns the nature of ‘identification’ in the two modelling approaches. Sceptics note that also the VAR-approach requires the imposition of (ad hoc) restrictions to be operational, notably in identifying the MAR (6.3) as a ‘structural’ model. This has prompted ironic comments that Sims’s substitute sources of identification “look no more credible and often appear to be of the same character” as in the Cowles model (4.1) (Heckman, 2000). Some authors even refer to Simss’s (1980) VAR-manifesto and its critical notion of ‘incredible’ identification as an almost “nihilistic rejection” of the rich Cowles Commission legacy (Davidson, 1991). Davidson (1991:406) concludes from Sims’s position that “structural modelling is taboo…..”

These characterisations of the VAR-approach are, at best, unhelpful rhetorical exaggerations. In fact, the VAR model cannot only be formally linked to the Cowles model (4.1) (by restricting the MAR model (6.3)) but, based on the historical investigations by Qin, Gilbert and others, it can even be concluded that the VAR approach is (or evolved into) a ‘methodological revision and renovation of the Cowles structural modelling approach.” (Eichenbaum, 1985; Qin and Gilbert, 2001; Qin, 2008).

An alternative econometric modelling strategy generally considered closer to conventional structural modelling than the (S)VAR approach, is the Equilibrium Correction Modelling (ECM) approach to dynamic modelling developed by Sargan, Hendry and others. This approach uses a particular ‘theory of interest’ to decompose the variables \(Z = \{Y, X\}\). To that

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8 See, for example, Heckman (2000).
9 Reliance on (weak) restrictions from economic theory for orthogonalising structural shocks forms the core of the ‘structural’ VAR (SVAR) approach (Qin, 2008).
end, the joint probability distribution \( F(Z) \), underlying the unrestricted VAR model (6.2), can be factorised into:

\[
F(Z) = F(Y/X) F(X)
\]  

(6.4)

Assuming that (6.4) is a valid factorisation of the hypothesised data generation process of the variables \( Z = \{Y, X\} \), the modeller can discard the marginal distribution \( F(X) \) to produce a (statistically) valid reduced density. This essential selection modelling step of the ECM approach is guided by the ‘theory of interest’ in question whereby a distinction is made between (to be tested) ‘parameters of interest’ \( \varphi_1 \) and auxiliary or background parameters \( \varphi_2 \).

For example, by formulating a testable specific causal pattern in the form of a statistical model. Cross-restrictions on \( \varphi_1 \) and \( \varphi_2 \) are absent:

\[
\{\varphi_1, \varphi_2\} \in \Phi_1 \times \Phi_2 , \quad \text{with } \Phi_i \text{ the set with permissible values of } \varphi_i.
\]

This model selection approach involves a formal search for a data-coherent structural model (see David Hendry, 1995, for details), where the error term is regarded as a (model-derived) mean-innovation process. The selected data-coherent model, \( \tilde{M} \), must also obey the encompassing principle (i.e., \( \tilde{M} \), must be able to explain the successes and failures of rival empirical models).

8. IMPROVEMENTS IN ECONOMIC THEORY AND ECONOMETRIC METHODS (STRATEGY C)

Sims’s (1980) VAR manifesto elicited the methodological criticisms that the VAR-approach is too much data-driven and that “the link to an explicit dynamic economic theory is at best weak.”

In response to these types of criticism, VAR researchers moved to a modelling strategy that relies (weakly) on a priori economic theory for orthogonalising structural shocks, called the SVAR approach. These steps in the direction of a stronger structural interpretation of the error terms correspond to the accentuation by VAR modellers of Dynamic Stochastic General Equilibrium (DSGE) models, with Bayesian VAR’s playing the role of standard of fit (Sims, 2006).

The 1970s saw the start of significant new theoretical developments in macroeconomics (presented as major improvements by its proponents) supported by innovations in econometric modelling. Theoretical work focused on providing macroeconomics with a foundation in microeconomic and general equilibrium theory. These theoretical developments served as building blocks for extending the Cowles framework by explicitly accounting or searching for the ‘deep parameters’ associated with technology, preferences and expectations. The dominant driving force was the doctrine of rational expectations and its implications for the development of new econometric methods (see Lucas and Sargent, 1981, for an early overview).

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11 That is, \( \varphi_1 \) and \( \varphi_2 \) are variation free.
12 J. Heckman (2000:26) argues that a fully specified dynamic economic theory (he made in this context a reference to Hansen and Sargent (1991)) can rarely be cast in the form of models” (6.2) and (6.3).
13 Sims (1986) and Qin (2008). Sims and other time-series analysts responded with new modelling suggestions to deal with various identification problems with VAR systems such as lack of uniqueness in imposing a causal ordering. The technical problem is that there is no unique way for the triangularisation of the leading terms of \( C(L)^{-1} \). Sims and others suggest to deal with this situation by imposing ad hoc restrictions suggested by (weak) economic theory, thereby moving closer to the spirit of the original Cowles framework.
14 R. Lucas and T. Sargent were the prominent leaders of this intellectual movement. See, for example, Lucas and Sargent (1981).
Theoretical work in the 1970s and 1980s resurrected time-series models developed by Slutsky, Frisch and Wold (Qin and Gilbert, 2001). This resulted in an augmentation of the Cowles framework by including a richer array of dynamic models. For example, the ECM framework entails a dynamic specification approach to structural models in which the dynamics of selected and specified variables are decomposed into short-run shocks, disequilibrium shocks and innovation residuals.

An even more comprehensive augmentation of the conventional Cowles framework is based on the work of Zellner and Palm (1974, 2004). They developed a structural econometric, time-series analysis (SEMTSA) approach by integrating uni- and multi-variate time-series models and dynamic stochastic theory into the SEM framework of the Cowles Commission. AZ notes that the SEMTSA approach uses economic theory to formulate causal multi-variate dynamic economic models that **mathematically** imply uni-variate and multi-variate time-series models (Zellner, 2009:3).

### 9. CONCLUSION

The main econometric modelling approaches reviewed above as part of strategies B and C, are grounded in theory-based structural models. In different ways, they represent extensions or augmentations of the conventional SEM framework of the Cowles Commission.

VAR modelling has evolved into a strategy that follows a structuralist approach that combines improvements in data-based model comparisons with the use of a (better) theory-bound compass. Also the ECM framework and the SEMTSA approach have in common that they rely (albeit in different degrees) on economic theory in their modelling strategies.

These developments reflect the (in my view correct) response to DF’s key insight that “there is no way to infer the ‘right’ model from the data unless there is strong prior theory to limit the universe of possible models.” (Freedman, 2009:2). This view is consistent with the role of economic theory that AZ (Zellner, 1979) attributes to restricting the scope of an investigation a priori.

An optimistic view would conclude that advances in economic theory (notably DSGE and real business cycle theories) have indeed led to the identification of ‘deep’ structural or causal parameters that are more precisely defined in terms of preferences, expectations and technology, while newly developed sophisticated econometric methods played an important supporting role. Unfortunately, most of these recent economic theories are semantically insufficient. This means that there is no strong prior theory to limit the range of possible empirical models, which is making the scope of most investigations very wide indeed. Testing economic hypotheses, even when using the latest, technically advanced econometric modelling strategies, is then a much less straightforward exercise than often suggested. Moreover, the lack of unity of economic science guarantees fundamental disagreements about the best way forward.

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15 Sims (2006) and Qin (2008). Clearly, a crucial test is to what extent these improvements in the theoretical compass can overcome the semantically insufficiency of economic theories, including DSGE.

16 In Blommestein (2009), *ibid*, I analysed the responsibility of academic finance for the global financial crisis. On the basis of this study, 5 principles for future economic research were suggested, including taking more seriously the modelling implications of economics as a social science, paying more attention to institutions, and putting specification uncertainty at the forefront of empirical research using econometric or statistical models.
REFERENCES


