Poznań Studies in the Philosophy of the Sciences and the Humanities
1994, Vol. 38, pp. 43-53

Kevin D. Hoover

SIX QUERIES ABOUT IDEALIZATION IN AN EMPIRICAL CONTEXT[1]

ABSTRACT. ~~Inventarization~~ Investigation of the main questions for research into the idealizational character of economic theories, focusing on the new classical macroeconomic research programme. It suggests a method to discriminate between rival idealized models.

Idealization as a fundamental element of scientific method is treated by Leszek Nowak (1980) as an almost entirely formal process of setting complicating conditions to their limiting values in order to isolate the essential, idealized relationship that they mask. In Theo Kuipers' (1985) discussion of the van der Waals' law, the law is seen as a step in the process of concretizing the ideal gas law. But the van der Waal's law in Kuiper's account derived from theoretical considerations. And its derivation is not necessarily motivated by empirical considerations such as the lack of fit of Boyle's law to experimental data gathered at low temperatures or high pressures. The relationship between idealized laws or theories and real-world data is one that appears not to have been fully explored. In this essay, I wish to use the new classical macroeconomics as a vehicle for exploring this relationship.[2] The new classical macroeconomics is the most unrelentingly formalized school of macroeconomic thought. Its models seem to provide perfect exemplars of idealization. Yet, from its earliest days, these models have been confronted with empirical data, and debates over how best to relate data to theory are prominent in its history.[3] It would seem, then, that the new classical economics would be well suited to the role of test-bed for conjectures on the relationship of idealization and empirical data. Despite this, shall we say, "ideal" test case, the questions raised will outweigh the tentative answers suggested in this essay.

[1] This paper extends my informal presentation on the problems of idealization in the new classical macroeconomics made to the conference on *Problems of Idealization in Economics*, Tilburg University, The Netherlands, 1-3 July 1991.

[2] Hoover (1988) provides a relatively complete account of new classical doctrine.

[3] Hoover (1988, ch. 8; 1991 forthcoming) discuss these debates in some detail.

The single most important document in the new classical macroeconomics is Robert Lucas's article "Econometric Policy Evaluation: A Critique" (1976). The central message of this article is that econometrically estimated macroeconomic relations (e.g., consumption functions, demand-for-money functions or Phillips curves) cannot, in general, be expected to remain invariant to changes in policy regimes. For example, since people will incorporate the central bank's money-supply rule into the process by which they form their expectations, a change in the money-supply rule would result in a change in the slope of the estimated Phillips curve.

There are at least two different interpretations of the Lucas critique. On the narrow interpretation, the problem arises – as it does in the Phillips curve example – from the existence of rational expectations, which generates an interdependence between the coefficients of different estimated equations.[4] On a wider interpretation, the omission of any relevant factor (i.e., releasing any *ceteris paribus* condition) generates a lack of invariance.[5] On either interpretation, the recommended solution is to get behind (or below) the macroeconomic relation to the so-called "deep parameters", to the tastes and technology that guide and constrain individual economic decision-makers.

Recall that the original context of the Lucas critique is the problem of evaluating policy. Policy evaluation requires quantitative advice, which in turn raises the central issue of the relationship between empirical data and the highly idealized models that are as close as the new classical macroeconomics comes to capturing deep parameters.

There have been two principal strategies for dealing with the Lucas critique within the context of the new classical macroeconomics. Each raises its own questions about the role of idealization. The first strategy is clearly exemplified in the work of Sargent (1981) and Hansen and Sargent (1980). This strategy embraces the traditional econometrics of simultaneous systems. The investigator begins with a theoretical model and derives estimatable equations from it. The new twist is to account for the effects of the rational expectations hypothesis on the interrelationships between the individual equations. Once this is done, the investigator proceeds with the usual (but now technically more complicated) methods of structural estimation.

The models that are estimated under the program of structural estimation are idealized. Typically, they are representative-agent models in which a single agent, characterized by a utility function, consumes one

[4] Hendry (1988) is an example of such an interpretation.

[5] Hoover (1988, ch. 8; 1992) argue for such an interpretation.

type of good during an infinite life span. The agent faces perfectly competitive product and labor markets and forms expectations rationally. The models thus idealize with respect to the heterogeneity of goods and agents, mortality, market structure, informational imperfections and so forth. Such idealization appears to be similar to Nowak's schema illustrated by Marxist economic models. Nevertheless, careful examination of these models raises the following questions.

Query One: What sort of simplifications count as idealizations?

Idealizing steps for Nowak are ones that set parameters to extreme values: zero or infinity. So, for example, in gravitational models objects are massive but dimensionless. In contrast, new classical models in particular and economic models in general often involve simplified or stylized features that result from stipulating definite values or forms, and not from removing features altogether by setting characteristics to extreme values. For example, such models often stipultate forms for utility functions: e.g., Cobb-Douglas, constant-relative-risk-aversion, or constant-elasticity-of-substitution forms. Such forms are chosen not to isolate the model from certain complicating features of reality, but to give those features a definite and tractable form. Which functional form is chosen depends in part on what issues a model is meant to address – e.g., the constant-relative-risk-aversion utility function is preferred to the Cobb-Douglas in modeling financial markets because it permits a range of values for the coefficient of relative-risk aversion rather than determining it uniquely. A second question, then, is closely related to the first.

Query Two: Is there a difference between setting aside a disturbing factor (or friction) and setting an essential factor to a stipulated value?

To illustrate the issue with a case that is not from the new classical macroeconomics, consider Baumol's (1952) model of the demand for money. Here the optimal amount of money for an individual is computed from the minimal cost of managing a financial portfolio in which there are two types of assets (money and interest-bearing bonds), costs of management related to the frequency of conversion from bonds to money, and income/expenditure flows in which identical increments to income are received at regular intervals and expenditures are made at a uniform rate. The model appears to be idealized. Yet, is the particular pattern of income and expenditure an idealization or a *stipulated concretization*? Clearly, one could always cast the issue in such a way that the pattern assumed in the model was the limit point of some sort of index of income and expenditure patterns. But the ease with which *any* pattern of income and expenditure could be placed at an extreme of index suggests that there remains a fundamental difference between this type of simplification and that, for example, incolved in assuming dimensionless points.[6]

Whether we call it an idealization or a stipulated concretization, this type of simplification abounds in new classical macromodels. The program of structural estimation typically takes the simplified models and estimates them directly using real-world data without any attempt to concretize and account for the deforming idealizations.[7] The most common test of the aptness of the model is to check whether the overidentifying restrictions (i.e., the relationships between the estimated coefficients beyond the minimum number needed to isolate them) hold. These are routinely rejected on standard statistical tests. Of course, such rejection is precisely what Nowak would expect: it is a hallmark of an idealized model that it does not accurately represent real-world data in the absence of concretizing steps.

Some new classical economists are not surprised by the failures of the program of structural estimation. These are the so-called "calibrators" (Kydland and Prescott, 1982, 1991). Like the structural estimators, calibrators use representative-agent models. They do not, however, take direct econometric tests of those models seriously, precisely because they believe that the contingencies of necessary, yet unaccounted for, concretizations would generate deformed parameter estimates that could not be expected to reveal deep structure. For the calibrator, to solve the Lucas critique, an idealized model must be *essentially* correct. Conclusions from simulating idealized models must be restricted to a range defined by the idealizing assumptions. Again, this would appear to closely parallel Nowak's view that not just any arbitrary simplification counts an idealization. Rather, an idealization is a simplification that isolates an *essential* core.

[6] Bert Hamminga has suggested to me that one needs to distinguish as Nowak does not between *technical* idealizations made for reasons of tractability (mathematical or statistical) and *essentialist* idealizations made to isolate the fundamental from the secondary. Clearly both sorts of simplifications occur. The issue is whether there is enough of a family resemblance between them to class them both as idealizations.

[7] Estimators of consumer demand systems take a similar approach; see Keuzenkamp (intra).

Recall that the starting point of all macroeconomics is policy analysis, and policy analysis requires quantification. Calibrators eschew macroeconomic parameter estimates, because the models are insufficiently concretized. In their place, they attempt to substitute estimates based on microeconomic studies, accounting considerations, *a priori* economic theory and informal searches of the parameter space for the "best" matching parameters on limited key dimensions. These procedures, however, raise the following general issue.

Query Three: Under what circumstances can coefficients of an empirical model be legitimately used to parameterize an idealized model?

To say that it is over legitimate poses difficulties. It seems to suppose a notion of robustness against alternative specifications that is problematic.[8] Why should the estimated coefficient representing, say, the marginal propensity to consume, be relatively invariant to misspecifications of functional form? It also suggests something like Nancy Cartwright's (1989, ch. 4) notion of causal capacities – i.e., of dispositions that are invariant in themselves, but that manifest themselves differently in different particular contexts. Cartwright's discussion in terms of linearity and orthogonality of regressors suggests a linkage with Leamer's views on robustness; yet, neither linearity nor orthogonality appear required by her more general views on causal capacities. Finally, it suggests an *essentialist* view of idealized models (i.e., idealization isolates real essences). This view is, I think, quite compatible with Nowak's account. Cartwright (1983), who regards scientific laws instrumentally, should find it harder to swallow.

If one takes the view that the point of the idealized model is to isolate the essential from the secondary deforming factors, then a purely theoretical description of the concretizing process would appear to assume rather too much. Nowak's account of Marx's economics from the idealized pure labor theory of value to the actual process of the formation of prices in the real world assumes that the ramifications of each idealizing step – or of the correlative sequence of concretizing steps – can be worked out from some principles. Similarly, Kuipers (1985) sees the addition to the ideal gas laws of the van der Waals law, deduced from theoretical principles, as a paradigm for the concretization

[8] Leamer (1978, 1983) defend such a notion of robustness. Cartwright (1991) questions its soundness.

process. And again, Cools, Hamminga and Kuipers (intra) take the simplest form of the Modigliani-Miller theorem as the idealized case and regard the theoretically understood complications of introducing taxes and bankruptcy as concretizing steps. In practice, however, we idealize — at least in the context of quantifying models — precisely because we lack a principled way of concretizing the idealized model. We generally do not know all the factors that are relevant or in just what way they are relevant. This suggests the following assertion.

***Assertion*: the bridge between quantified idealized models and the real world is atheoretical.**

The point of the assertion is this: if idealized laws are linked to real phenomena only through concretizing steps; and if we cannot provide all of the concretizing steps theoretically; then part of the linkage must be composed of atheoretical quantified information, something like Cartwright's (1983, Essay 6) "phenomenal laws". These phenomenal laws provide the fudge factors to match ill-fitting idealizations to data. Although, I regard this view as an assertion, I think that it can be defended through another question.

Query Four: If the assertion that the bridge between the quantified idealization and the world must be atheoretical is false, then why does the idealization have any special status?

It is sometimes argued that a Walrasian barter model is an idealization of the operation of the economy. Adding money to such a model could be regarded as a concretization. But if a complex economy could never function without money, then one might argue that the barter model distorts the *essence* of the economy. It is clear that the Modigliani-Miller theorem without taxes and bankruptcy is simpler than the theorem that accounts for those complications. Given that those complications are theoretically understood, is the simpler theorem really more of the essence?

When theoretical understanding of the implications of various complications is relatively complete, idealization would appear to be a fairly sterile formal relationship between simpler and more complicated models in nested sets. In an empirical context, however, in which the bridge to the real world is atheoretical, the idealization is in effect a substantive claim about how to take the world apart at the joints. Of course, to assert such a claim is one thing, for it to be true is quite another.

It is an adventitious fact that within the new classical macroeconomics the proponents of the program of structural estimation have, at least sometimes, favored monetary-equilibrium-business-cycle models, while proponents of the calibration program have favored real-equilibrium-business-cycle models. Both sorts of models are idealizations, yet they contradict each other at fundamental points. If the linkage between idealizations and the real world is atheoretical, and if we are committed to an essentialist rather than an instrumentalist view, we need some means of discriminating between such models.

Query Five: Can we test idealized models?

Nowak (1980, ch. 5; in press) attempts to subvert the Popperian, falsificationist approach to scientific method in part by using disconfirming instances as a source for further idealizing assumptions and further concretizations. This has the probably accidental effect of raising the initial idealized model to the status of a Lakatosian hard core (Lakatos 1970). The new classical macroeconomics provides a clear example. When Kydland and Prescott's (1982) original real-business-cycle model was quantified to best match the variance of GNP, it seemed to miss the covariance of productivity and hours worked by a large margin. Gary Hansen (1985) dealt with this disconfirmation by recognizing that the original Kydland and Prescott model idealized when it assumed continuity of the supply of labor. Hansen then showed that, if one takes a concretizing step of introducing indivisible labor (workers work fixed eight-hour days), then the model better matches the covariance. Hansen's fix, leaves the idealized core of Kydland and Prescott's model untouched. Were such a strategy also followed by proponents of the competing monetary-business-cycle models, the idealized models at the center of each program would be incommensurable, not because of incommensurability in principle, but because the strategy of idealizing disconfirming instances would mean that we would never seek a resolution to the question of which core idealization was correct.

Query Six: Is there a way to approach such a resolution?

I believe that the answer is yes. I conclude with a concrete suggestion based on linking idealization and the econometric strategy known as encompassing (Hendry and Richard 1987; Hendry 1988b).

The encompassing notion can be understood as a way of selecting between rival econometric specifications. If two specifications purport to account fully for the same phenomena, then one can apply statistical tests to see whether either is dispensable in a joint specification which nests them both. Hendry and Richard propose a framework in which the econometrician begins with a very general, unrestricted dynamic specification and searches using a mixture of theoretical insight and statistical criteria for a parsimonious representation of the phenomena as a funcion of limited set of variables. The successful parsimonious representation must possess residual errors with properties that suggest no easily remediable misspecification. For example, the errors should be mean zero, serially uncorrelated, without heteroscedasticity, and normal. Furthermore, the parsimonious specification should be a statistically valid restriction of the more general specification, i.e., it must be nested in the more general form and carry all the information carried in the general form.

Encompassing might appear contrary to the spirit of idealization/concretization. A model gains purchase on real phenomena as it becomes more concretized, more complex. An econometric specification begins general – i.e., complex – and is reduced to a simpler form. The contrariness is illusory. Econometric specifications (I have been careful not to follow the vernacular and write "model") belong to the sphere of phenomenal laws – the atheoretical relationships that I earlier asserted form the bridges from idealized models to the real world. Hoover (1994) distinguishes between *econometrics as observation* and *econometrics as measurement*. While the terminology is my own, the distinction is evident in the history of econometrics. Morgan (1990) contrasts the early econometrics of business-cycle research (an example of observation) with the early econometrics of demand curves (an example of measurement). The general-to-specific specification strategy is an observational strategy – an attempt to characterize the data rather than to measure a theoretically important parameter precisely.

Using the general-to-specific framework, I propose the following strategy for discriminating between rival idealized models.

Consider two rival idealized models. To be concrete, consider Baumol's model of the demand for money referred to above as a rival of an even simpler model in which portfolios are not managed at all. In Baumol's model, the demand for money can be written as

Model I. $M = (bY/2r)^{0.5}$,

where M is the stock of money, b is the brokerage cost of transferring money out of bonds, Y is the level of income, and r is the market rate of interest. The rival model is simpler.

Model II. $M = Y/2$.

These models provide starkly different predictions. In Model I the elasticity of money with respect to income is 1/2: i.e., a 10 per cent increase in the level of income increases the demand for money by 5 per cent. The elasticity of money with respect to the interest rate is -1/2. In contrast, in Model II, the elasticity of money with respect to income is unity and with respect to interest is zero. Both models are idealized, and neither fits the data well on its own. Nevertheless, we may be able to choose between them in the following way:

(A) Generate a time series for the variable(s) of interest using each rival (calibrated) idealized model. E.g., for Model I, pick a value for b from microeconomic or other evidence, and use available data on Y and r to generate predicted values for M; proceed similarly for Model II.

(B) Estimate a general econometric specification for the variable(s) of interest including the generated time series. E.g., estimate a model for money of the following form:

$$M_t = \sum_{t=1}^{n} \alpha_j M_{t-j} + \sum_{t=0}^{n} \beta_j Y_{t-j} + \sum_{t=0}^{n} \delta_j r_{t-j} + \sum_{t=0}^{n} \varphi_j Z_{t-j} +$$

$$+ \mu \mathrm{MI}_t + \tau \mathrm{MII}_t + \epsilon_t,$$

where MI is the time series generated from Model I, MII is the time series generated from Model II, Z stands for all other relevant variables, ϵ is a residual random error, and t is a time index. The lag length n is chosen empirically to insure that the ϵ possess the requisite statistical properties to indicate adequate specification.

(C) Conduct a general-to-specific specification search to form a parsimonious representation of the specification in B with the restriction that both predicted time series always be retained in the final specification.

(D) Use standard statistics to test for the possibility of omitting each of the model-based predicted time series in turn and jointly.

If one model but not the other cannot be omitted from the final specification, then it clearly dominates the other. If neither can be omitted, both apparently carry information essential to the idealized model, which should be helpful in reconstructing a more suitable model. If both can be omitted, then the idealization exercise was not empirically helpful: either a different sort of model, or a different level of theoretical concretization would appear to be needed.

The new classical macroeconomics presents many examples of the quantification of apparently idealized models. Although, I have ended this essay with a concrete suggestion of how one might discriminate between rival idealized models – a task which would appear to be essential if the method of idealization is not to collapse into sterile formalism – it is clear that far more issues have been raised than satisfactorily answered. The scope for future research is obvious.

Kevin D. Hoover
Department of Economics
University of California
Davis, California 95616-8578
U.S.A.

REFERENCES

Baumol, W.J. (1952). The Transactions Demand for Cash: An Inventory Theoretic Approach. *Quartely Journal of Economics*, **66**, 546-556.

Cartwright, N. (1983). *How the Laws of Physics Lie.* Oxford: Clarendon Press.

Cartwright, N. (1989). *Nature's Capacities and their Measurement.* Oxford: Clarendon Press.

Cartwright, N. (1991). Replicability, Reproducibility, and Robustness: Comments on Harry Collins. *History of Political Economy*, **23**, 143-156.

Cools, K., B. Hamminga and Th.A.F. Kuipers (infra). Truth Approximation by Concretization in Capital Structure Theory.

Hansen, G.D. (1985). Indivisible Labor and the Business Cycle. *Journal of Monetary Economics*, **16**, 309-328.

Hansen, L.P. and T.J. Sargent (1980). Estimating and Formulating Dynamic Linear Rational Expectations Models. *Journal of Economic Dynamics and Control*, **2**, 7-46.

Hendry, D.F. (1988a). The Encompassing Implications of Feedback versus Feedforward Mechanisms in Econometrics. *Oxford Economic Papers*, N.S. **40**, 132-149.

Hendry, D.F. (1988b). Encompassing. *National Institute Economic Review*, August, 88-92.

Hendry, D.F. and J. Richard (1987). Recent Developments in the Theory of Encompassing. In: B. Cornet and H. Tulkens (Eds.) *Contributions to Operations Research and Econometrics. The XXth anniversary of CORE.* Cambridge, MA: MIT Press, pp. 393-440.

Hoover, K.D. (1988). *The New Classical Macroeconomics: A Sceptical Inquiry.* Oxford: Blackwell.

Hoover, K.D. (1994). Econometrics as Observation: The Lucas Critique and the Nature of Econometric Inference. *Journal of Economic Methodology*, **1**, forthcoming.

Hoover, K.D. (1991). Calibration versus Estimation: Standards of Empirical Assessment in the New Classical Macroeconomics. *Research Program in Applied Macroeconomics and Macro Policy, Working Paper Number 72*, Institute of Governmental Affairs, University of California, Davis, January.

Hoover, K.D. (forthcoming). Quantitative Evaluation of Idealized Models in the New Classical Macroeconomics. In: N. Cartwright and M. Jones (Eds.). *Correcting the Model (Poznań Studies in the Philosophy of the Sciences and the Humanities*, forthcoming). Amsterdam: Rodopi.

Keuzenkamp, H. (infra). What it Idealization is Problematic? The Case of the Homogeneity Condition in Consumer Demand.

Kuipers, T.A.F. (1985). The Paradigm of Concretization: The Law of van der Waals. In: J. Brzeziński (Ed.) *Consciousness: Methodological and Psychological Approaches (Poznań Studies in the Philosophy of Sciences and the Humanities*, **10**). Amsterdam: Rodopi, pp. 79-99.

Kydland, F.E. and E.C. Prescott (1982). Time to Build and Aggregate Fluctuations. *Econometrica*, **50**, 1345-1369.

Kydland, F.E. and E.C. Prescott (1991). The Econometrics of the General Equilibrium Approach to Business Cycles. *Scandinavian Journal of Economics*, **93**, 161-178.

Lakatos, I. (1970). Falsification and the Methodology of Scientific Research Programmes. In: I. Lakatos and A. Musgrave (Eds.). *Criticism and the Growth of Knowledge.* Cambridge: Cambridge University Press, pp. 91-196.

Leamer, E.E. (1978). *Specification Searches: Ad-hoc Inference with Nonexperimental Data.* New York: Wiley.

Leamer, E.E. (1983). Lets Take the Con out of Econometrics. *American Economic Review*, **73**, 31-43.

Lucas, R.E., Jr. (1976). Econometric Policy Evaluation: A Critique. In: K. Brunner and A.H. Meltzer (Eds.). *The Phillips Curve and Labor Markets.* Carnegie-Rochester Conference Series on Public Policy, vol. 1. Amsterdam: North-Holland, pp. 19-46.

Morgan, M. (1990). *The History of Econometric Ideas.* Cambridge: Cambridge University Press.

Nowak, L. (1980). *The Structure of Idealization: Towards a Systematic Interpretation of the Marxian Idea of Science.* Dordrecht: Reidel.

Nowak, L. (1991). The Method of Relevant Variables and Idealization. In: E. Eells and T. Maruszewski (Eds.). *Rationality and Probability (Poznań Studies in the Philosophy of the Sciences and the Humanities*, **21**). Amsterdam: Rodopi.

Sargent, T.J. (1981). Interpreting Economic Time Series. *Journal of Political Economy*, **89**, 213-248.