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Can econometric analysis make (agricultural) economics a hard science? Critical remarks and implications for economic methodology

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DISCUSSION PAPER

Institute of Agricultural Development in Central and Eastern Europe

CAN ECONOMETRIC ANALYSIS MAKE (AGRICULTURAL) ECONOMICS A HARD SCIENCE?

**Critical remarks and
implications for economic methodology**

MARTIN PETRICK

**DISCUSSION PAPER No. 62
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ABSTRACT

This paper argues that mainstream economics does not follow the positivist research tradition it (often implicitly) claims to follow and expresses some serious doubt that econometrics in particular can make economics a hard science. Mathematical rigour and sophisticated statistical techniques may be regarded as persuasive analytical tools in economics, but their mere application does not guarantee good research practice. The paper outlines an alternative methodological view of pragmatic instrumentalism in which the well known tools of economic analysis can find a new place. Instead of relying on a narrow rule that purports to produce universal truth, this view encourages to increase the target area for questioning and probing. Much more attention is paid to the communicative aspects of scientific methodology – in which terms an analysis is framed, and under what conditions, to whom, by whom, and by which means it is articulated.

JEL: B 41, C 10.

Keywords: economic method, econometrics, philosophy of science, pragmatic instrumentalism

ZUSAMMENFASSUNG**MACHT DIE ÖKONOMETRIE DIE (AGRAR-) ÖKONOMIE ZU EINER HARTEN WISSENSCHAFT?**

Der Beitrag vertritt den Standpunkt, dass die Wirtschaftswissenschaften nicht der positivistischen Forschungstradition folgen, der sie (oft implizit) vorgeben zu folgen. Darüber hinaus wird Zweifel geäußert, dass die Ökonometrie im Besonderen in der Lage ist, die Ökonomie zu einer harten Wissenschaft zu machen. Mathematische Stringenz und anspruchsvolle statistische Verfahren mögen als überzeugende Analyseinstrumente der Ökonomie gelten, allein ihre Anwendung garantiert jedoch noch keine gute Forschungspraxis. Der Beitrag skizziert eine alternative Betrachtungsweise, die einem pragmatischen Instrumentalismus folgt und in der die bekannten Instrumente der ökonomischen Analyse einen neuen Platz finden können. Anstatt auf eine enge Regel zu vertrauen, die vorgibt, universelle Wahrheiten hervorzubringen, sollte die Zielfläche für das Hinterfragen und Nachforschen vergrößert werden. Wesentlich mehr Aufmerksamkeit wird den kommunikativen Aspekten wissenschaftlicher Arbeit gewidmet – mit Hilfe welcher Begriffe eine Analyse vorgenommen wird, und unter welchen Bedingungen, im Hinblick auf wen, durch wen und auf welche Weise sie artikuliert wird.

JEL: B 41, C 10.

Schlüsselwörter: Methoden der Wirtschaftswissenschaften, Ökonometrie, Wissenschaftstheorie, pragmatischer Instrumentalismus

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1 INTRODUCTION¹

"Economics as a hard science: realistic goal or wishful thinking?" was the title of an article by Thomas MAYER published in 1980 in which he argued that economics appeared to be far from a hard science (one that is able to empirically test hypotheses), but that this insight could have valuable implications for everyday research practice. The aim of this paper is to take up MAYER's argument from a current perspective and to introduce the interested reader into some of the lines of reasoning that are behind economists' struggle for a hard science. Particular attention is given to the role econometric analysis plays and can play in applied economics. Since this is a paper written for agricultural economists by an agricultural economist trained in Germany, the reader will find several references to this background. It is not in my competence to present a balanced survey of contemporary economic methodology. I rather wish to highlight some of the difficulties inherent in a traditional, positivist view of economic methodology that still seems to underlie much academic teaching in economics, and to point out some alternative ways of approaching methodology. My hope is that this contributes to a sharpening of the readers' sensitivity with regard to these issues and that they find some of the references useful for further studies.

The argument of the paper runs as follows. I start with a brief explanation of the historical background that inspired the early economics profession in its scientific endeavour and how this led to the positivist research tradition in economics, which has been predominant in the post-war period. Next I discuss the current relevance of this tradition and present the criticism that has been raised from several directions. In particular I will question whether econometric analysis does offer a tool that makes economics a hard science and will put forward several arguments why this might not be the case. In the final sections, I outline the ingredients of an alternative, more pragmatic and instrumentalist approach to economic methodology and finish with some conclusions.

2 NATURAL SCIENCE METHODOLOGY AND ITS IMPACT ON ECONOMICS

In 1776, Adam SMITH published his thoughts on how individual behaviour of economic agents steers the resource allocation of society in the classical text "The Wealth of Nations". This is commonly regarded as the birthday of modern economic science and happened at a time where successful research in the natural sciences begun to provide the basis for the industrial revolution and thus had a large influence on daily life. Not by chance, the practice to discover physical laws – of which the axioms of Newtonian physics are a prominent example – by experimental research had a lasting effect on the young profession of economics. There was a widely held view among the classical economists that the social and economic system of society should obey general laws similar to those found in the natural sciences (ALBERT 1979, pp. 52-53). The research methodology of the natural sciences thus soon became a blueprint for economics as a scientific enterprise. In the early post-world war II period, it appeared to be the most influential epistemology among economists.²

¹ The paper is based on an invited lecture for a workshop on the Philosophy of Social Science held at Humboldt-Universität zu Berlin, Faculty of Agriculture and Horticulture, Chair of Resource Economics, on 10 March 2004. Most parts draw on PETRICK (2003, section 3.1.2). I am indebted to the workshop participants and to Martin DAMGAARD, Vladislav VALENTINOV, and Peter WEINGARTEN for helpful comments.

² RICHTER (1994, p. 592), in a retrospective on the methodological views held among economists in Germany in the 1960's, reports the following: "We were marked by the desire to apply the analytical style of the natural sciences [...] to our subject area, so as in that way to emulate the great successes of the natural sciences".

According to this methodology, for example as formalised in the famous scheme due to HEMPEL and OPPENHEIM, the process of scientific explanation involves the logical deduction of a statement about some real world event from a description of a set of initial conditions plus a *universal law* (see BLAUG 1992, pp. 4-5).³ This methodological view holds that any scientific explanation at the same time allows the *prediction* of future events, as soon as the initial conditions and the universal law are known. As a consequence, a first crucial question is under what circumstances a postulated law may claim to be universally true. An influential view in the philosophy of science maintained by representatives of a positivist tradition is that there is only *one* criterion decisive for the truth content of a proposition: the *empirical evidence*. Only statements that are backed by empirical observation or experiment may claim to be universal laws and thus deserve to be called 'scientific'. This implies a second important question: how can the required empirical evidence be furnished? Again, there is a widely acknowledged position laid down in Karl POPPER's 'Logic of Scientific Discovery' ('Logik der Forschung' in German, (1994 [1934])) and known under the labels of 'falsificationism' or 'critical rationalism', which will be outlined briefly.

The key to POPPER's methodology is his assertion that the empirical content of theories can only be tested by attempting to *falsify* them, since conclusive *verification* is logically impossible (see BLAUG 1992, pp. 12-26). A theory is the more corroborated and hence scientific the more empirical tests it has resisted without refutation. Although (or because) there are no 'crucial experiments' which once and for all refute theoretical propositions (the so-called Duhem-Quine thesis), POPPER recommends to be as precise about the circumstances (empirical events) under which a theory can be regarded as falsified. In a Popperian view, it is thus good scientific practice to maximise the empirical testability of scientific statements. On the other hand, such auxiliary hypotheses are to be avoided which reduce the applicability or testability of the theory, for example in the form of unspecified *ceteris paribus* clauses or *ad hoc* modifications.

3 ARE ECONOMISTS 'GOOD' SCIENTISTS?

A cursory glance at the major economics journals today leaves the impression that the precision of mathematically formulated, universal laws is what indeed has been characterising the research process in economics. Furthermore, the branch of *econometrics* apparently offers the promising opportunity to subject any theoretical reasoning in economics to a rigorous empirical test. Probably for one or both of these reasons, the majority of economists seems to implicitly or explicitly accept POPPER's critical rationalism as the relevant research methodology.⁴

³ For example, the universal law may take the form: "in all cases where event A occurs, event B also occurs." The initial condition may be: "event A occurs". Taken together, by way of deductive logic, it is possible to *explain* the occurrence of an observed event B by referring to the initial condition and the universal law.

⁴ This might be exemplified by a look at an arbitrary selection of widely used economic textbooks. LIPSEY and CHRYSTAL (1995), in the eighth edition of 'Positive Economics', write (p. xii): "Economic theory is meant to be about the real world. Economists seek, by the use of theory, to explain, understand, and predict real-world phenomena, and theory must therefore be related to, and tested by, empirical observations." PINDYCK and RUBINFELD (1995), in the third edition of their 'Microeconomics', note that (pp. 4-5; italics in the original): "Like any science, economics is concerned with the *explanation* and *prediction* of observed phenomena. [...] The usefulness and validity of a theory depend on whether it succeeds in explaining and predicting the set of phenomena that it is intended to explain and predict. Consistent with this goal, theories are continually tested against observation. As a result of this testing, theories are often modified or refined or even discarded. The process of testing and refining theories is central to the development of economics as a science." German textbooks are even more explicit. WOLL (1993), in the eleventh edition of 'Allgemeine Volkswirtschaftslehre', ascertains (p. 13): "Unsere Kenntnis von der Realität wird vor allem dadurch gefördert, daß an die Stelle

But *do* economists in fact *practice* what they preach? And *can* they? There is an ongoing debate concerning both questions, and the alternative methodological positions are more or less fundamentally different from the mainstream, depending on whether only the first or both questions are answered negatively. I will briefly present a number of positions in the following.

BLAUG (1992) is one of the most articulate contributors in favour of a stronger commitment to falsificationism in economics.⁵ Though he obviously believes that economists *can* practice falsificationism (p. xv), he complains that they don't (p. 111). After rigorously examining the methodological background of many important fields of economics in the sequel, he sums up his criticism in the form of two major conclusions (p. 238): (a) economic theories are not formulated in such a way that they yield empirically refutable implications, and (b) there generally is too little empirical testing of economic theories. BLAUG's position may be illustrated by the following passage on general equilibrium (GE) theory, which is widely regarded as a landmark of economic thought (BLAUG 1992, p. 169):

"Enormous intellectual resources have been invested in its [GE theory, M.P.] endless refinements, none of which has even provided a fruitful starting point from which to approach a substantive explanation of the workings of an economic system. Its leading characteristic has been the endless formalization of purely logical problems without the slightest regard for the production of falsifiable theorems about actual economic behaviour, which, we insist, remains the fundamental task of economics. The widespread belief that every economic theory must be fitted into the GE mold if it is to qualify as rigorous science has perhaps been more responsible than any other intellectual force for the purely abstract and nonempirical character of so much of modern economic reasoning."

In contrast, *agricultural* economists were supposed to have always maintained a close relationship to the research field they were studying; probably more than economists of other branches. This was made almost proverbial by the assessment of LEONTIEF (1971, p. 5), according to whom agricultural economists, "[w]hen they speak of crop rotation, fertilizers, or alternative harvesting techniques, they usually know, sometimes from personal experience, what they are talking about."⁶ Agricultural economists hence can hardly be accused of being entirely abstract and non-empirical. Despite this commendatory assertion, there has been a growing scepticism in this applied tradition with regard to the value of economic theory for the understanding and prediction of real world phenomena. A prominent example in the German context is BRANDES (1985; 1989).

A major aim of BRANDES is to point to the limitations of what he calls 'armchair economics' ('Schreibtisch-Ökonomie' in German). By taking the agricultural sector as an example, he demonstrates that current economic theories of farmers' behaviour are not capable of accurately predicting individual response to changing environments (BRANDES 1985, pp. 60-62; 1989, p. 334). On the sectoral or aggregate level, the reliability of forecasting may improve, however, also the danger of mutually compensating errors in the analysis increases (1985, p. 110).

bisher unsicherer Theorien neue, empirisch hinreichend bestätigte Aussagensysteme treten. Dazu ist es nötig, die vorhandenen Theorien ständigen Falsifizierungsversuchen zu unterwerfen." He continues on p. 27: "Die empirische Überprüfung ökonomischer Hypothesen [...] fällt in das Aufgabengebiet der Ökonometrie." Finally, KOESTER (1992), in the second edition of 'Grundzüge der landwirtschaftlichen Marktlehre' spends more than six pages (pp. 5-11) on explaining how POPPER's methodological positions define the empirical approach of agricultural economics. He opens his considerations with the statement (p. 5; italics in the original): "Nach den grundlegenden Ausführungen von *Popper* wird heute allgemein mit Hilfe der deduktiven Methode gearbeitet und das im folgenden dargestellte Vorgehen akzeptiert."

⁵ In Germany, a methodologically similar position is held by BRINKMANN (1997).

⁶ In a recent comment on the broadening of research fields agricultural economists deal with, this favourable assessment was however called into question by HANF (1997, p. 573).

4 WHY ECONOMETRICS CANNOT MAKE ECONOMICS A HARD SCIENCE

In his considerations on scientific methodology, BRANDES' initial position seems to be basically Popperian since he emphasises the search for an empirically grounded, scientific explanation of economic behaviour (1985, pp. 114-120; 144). At the same time, he is fully aware of the fact that it is fraught with problems to follow this path in economics. I list three major groups of arguments why it is so difficult to make economics a 'hard science', since they are of relevance for the further discussion (I refer to BRANDES 1985, pp. 76-81; 123-130; 156-159): (a) it is usually regarded as being impossible to devise controlled experiments in economics, it is hence quite difficult to test a given hypothesis in isolation from the surrounding auxiliary assumptions; (b) in economics, there are no constant numerical relationships as in physics, and any 'laws' are only of a stochastic nature; (c) there is a wide range of possibilities to manipulate empirical models in order to obtain a desired result, a practice also called 'data mining'. The latter means the arbitrary and intransparent choice of the researcher to publish his favourable result out of a pool of estimates with possibly largely diverging implications.

All three points in some respect concern the role *econometric analysis* plays in the methodological approach of economics. As a supplement to the assessment by BRANDES, I want to highlight a number of reasons why it must be seriously doubted that econometrics can play the empirical judge for economic theories (see SCHOR 1991, pp. 54-56; 151-152):

1. The first step of econometric analysis concerns the operationalisation of the theory to allow its connection with observable data. Often the data employed in the empirical model corresponds *only crudely* to the concepts in the theory, or relevant and reliable data may *not be available* at all. Several frequently used concepts have *no measurable counterpart*, for example risk aversion or technical progress. Astonishingly, the various *sources of error in data collection* are often neglected even by applied economists (BRINKMANN 1997, pp. 176-189; a comprehensive description of these errors is GROVES 1989).
2. In a second step, the econometric model to be tested is specified, that is the precise qualitative structure of the relationship between right- and left-hand variables is determined. A premise for this specification is the *assumption* that *one knows* the true qualitative nature of the underlying structure. However, this is usually not the case. Correlation is not causation (DEATON 1997, p. 65), and a given theory may be represented by a variety of models or functional forms. There is a substantial danger that relevant exogenous variables are omitted or that irrelevant ones are included, which compromises any regression analysis due to *specification error*. LEAMER (1978) has criticised the intransparent search for the 'best' fit (the one that confirms the theory – or falsifies it?) as 'ad hoc inference' without any clearly articulated procedure. If there is no criterion determining which of the models is correct, it is impossible to conclusively reject any theory based on the obtained estimates.
3. Next, the parameters of the empirical model are quantified by employing an appropriate estimator. The estimation may often be impossible without various *auxiliary assumptions* or simplifications. An example is the *identification problem* if systems of equations are underidentified, which necessarily requires additional theoretical assumptions (HSIAO 1983). It is clear that the validity of a theory cannot be tested if it is the constituting assumption of the test. Identification may also be increasingly difficult if, as a measure to improve model precision, formerly exogenous variables are endogenised and hence explained by other exogenous variables.

4. A final remark concerns the use (or abuse) of significance tests in econometrics. It must be stressed that there is *no objective criterion* that allows to maintain or reject a certain hypothesis. If probabilistic relationships are examined, there is a clear trade-off between the risk of committing a type I error (reject a correct hypothesis) and a type II error (accept a false hypothesis). *Both are relevant*, though the latter is usually neglected (see MAYER 1993, pp. 132-151). The commonly used levels of statistical significance are only based on *convention*, and thus a completely arbitrary choice. MCCLOSKEY (1985), by asking the question 'how large is large?' (p. 142), stresses that whether or not a coefficient is significant is not a statistical problem (p. 163). Instead, he emphasises the importance to evaluate the *benefits or costs for society* if a certain hypothesis is rejected or accepted, for example by specifying a loss function (p. 158). This, in turn, will clearly involve several normative judgements. As a consequence, there is no 'objective' line between science and non-science left.

The purpose of this is of course not to banish econometrics from the toolbox of the economist. Econometric analysis remains a powerful instrument to confront economic theories with empirical facts. What should be acknowledged, however, is the following:

It seems expected far too much of econometrics to be the fundamental benchmark for the falsification of theories. Because econometrics is regarded as the only convincing tool to potentially falsify theories (BLAUG 1992, p. 245), the methodological standpoint of critical rationalism becomes *untenable for economics*. Furthermore, even if the demand on econometric analysis is more modest, a good deal of openness and honesty is required to make any results credible.

5 IMPLICATIONS AND CONSEQUENCES

At this point of the examination, it appears useful to refer to the conclusions BRANDES arrives at, which are in my view as follows:

1. It is necessary to overcome the existing *theoretical deficit* by developing theories that are more capable of explaining economic behaviour in the real world (BRANDES 1985, p. 167). At the same time, economists should become more humble with respect to the performance of their profession (1985, p. 216).
2. More attention should be paid to *empirical work*. This includes on the one hand more transparency and openness in the publication of conventional approaches, particularly in econometrics (1985, pp. 183-188). On the other hand, it also implies to increase efforts to directly investigate economic behaviour, for example by field studies or experiments (1985, pp. 168-172; 188-200).
3. The aim of economics to be a *hard science* that produces objective knowledge must be questioned (1989, p. 336) because it is regarded as impossible to follow the strict rules of falsificationism (1985, p. 124). The belief in a universally accepted, 'scientific method' should thus be openly replaced by a more subjectivistic and pragmatic attitude (1985, pp. 130-136; 162-164).

It would be a rewarding task to analyse the development of the past twenty years since BRANDES' (1985) publication in the light of his previously summarised critique. However, I confine myself to the following remarks.

With regard to theory, there appears to be an increasing willingness of economists to adapt their models to the 'irritating observations of the real world' (WITZKE 1993, p. 53). Of particular

importance for much current research in agricultural and resource economics is the rapidly growing field of the 'New Institutional Economics' (NIE) (see FURUBOTN and RICHTER 1997; HAGEDORN 1996). Unfortunately, the hope that the closer proximity to real world phenomena improves the testability of theoretical approaches of the NIE seems unjustified. As TERBERGER (1994, pp. 143-151) argues, the empirical testability of NIE concepts may even have decreased in comparison to that of the orthodox neoclassical tradition: (a) unobservable factors play an even larger role in NIE, for example the general assumption of utility instead of profit maximisation, the informational status of agents, or the possibility of bounded rationality; (b) as a result of these modifications, even less observable events can be excluded by theoretical arguments (many results from NIE analysis seem to be even more dependent on the specific assumptions one is willing to make than in the neoclassical approach, for example with regard to the information status of agents; see HART and HOLMSTROM 1987, pp. 105-106, as cited by FURUBOTN and RICHTER 1997, p. 202); (c) concepts that appear to be simple to operationalise at first sight turn out to be rather vague after a second thought (see the reflections on 'transaction costs' by SCHNEIDER 1987). In addition, several reservations concerning econometric analysis as expressed above apply even more for models derived from NIE theories. This particularly concerns the risk of specification errors, since postulated relationships may be highly non-linear, and even less might be known about functional forms in institutional economics than it is, for example, in applied production analysis.

On the other hand, it seems fair to say that contemporary econometricians care a lot more about the sources of error outlined above than it was the case a few decades ago (see MAYER 1993, p. 145). It is now an emerging standard in introductory econometric textbooks to refer to the danger of data mining and to acquaint the reader with available testing procedures (see for example JOHNSTON and DiNARDO 1997, chapter 4, particularly pp. 109; 112). At the same time, expectations with regard to the power of causal interference in econometrics have become more modest, and increasing attention is paid to the procedures of survey design and data collection. This is particularly true for researchers who deal with large sets of micro-data. A case in point are the huge amounts of data collected by the World Bank in the framework of the Living Standards Measurement Study. DEATON (1997, pp. 65-66), in his textbook on survey data analysis, comments on the problems involved by citing FREEDMAN (1991): "[...] statistical technique can seldom be an adequate substitute for good design, relevant data, and testing predictions against reality in a variety of settings." DEATON continues his introduction to econometric methods as follows:

"One of my aims [...] is to clarify the often rather limited conditions under which the various econometric techniques work, and to indicate some more realistic alternatives, even if they promise less. A good starting point for all econometric work is the (obvious) realization that it is not always possible to make the desired inferences with the data to hand. Nevertheless, even if we must sometimes give up on causal inference, much can be learned from careful inspection and description of data [...]."

Again, this is not to say that causal interference in econometrics is an undesirable goal. However, it demonstrates the possible limitations of empirical analysis in economics.⁷

⁷ It should be noted that conducting *laboratory experiments* has become a quite widespread methodological approach in economics which clearly has left the stage of infancy (for an overview see KAGEL and ROTH 1995). However, in my opinion, an important problem not yet solved in a satisfying way is how results gained in the laboratory can be generalised to be valid in given real world situations (the problem of external validity, see FRIEDMAN and SUNDER 1994, pp. 15-16).

6 PRAGMATIC INSTRUMENTALISM AS AN ALTERNATIVE APPROACH

If critical rationalism cannot be the benchmark for economists, the natural question arises what will take its part. I will not review the entire and rather dispersed field of current thinking about economic methodology (the interested reader is referred to surveys such as BLAUG 1992 or CALDWELL 1994).⁸ However, I want to point the reader to an influential strand of thought introduced by MCCLOSKEY (1983) as 'rhetoric of economics' and developed further towards a methodological position of 'pragmatic instrumentalism' by SCHOR (1991).

MCCLOSKEY (1983; 1985) shares most of the critique of traditional methodology in spirit of the natural sciences as outlined above; a methodology he calls 'modernist'. He insists that the 'modernist' rules of scientific reasoning are not cogent, are counterproductive to any scientific progress in economics, and, in particular, are not followed by economists (1983, pp. 484-493). The problem he sees is that economists *do not recognise* the fact that the prescriptions of traditional methodology "are apparently not the grounds for their scientific conviction" (p. 482). What economists in fact practise, MCCLOSKEY argues, is *persuading their auditorium* by a *specific rhetoric of economics*. Following BOOTH, he understands rhetoric as " 'the art of probing what men believe they ought to believe, rather than proving what is true according to abstract methods.' " In his view, this 'unofficial methodology' is neither sufficiently acknowledged nor examined more closely by the profession, although it should. A central claim of MCCLOSKEY is that economists should become aware of the rhetoric they are using, which to a substantial extent consists of metaphors, even if the language of mathematics is used. He takes the production function as an example (pp. 505-506):

"Consider [...] the theory of production functions. Its vocabulary is intrinsically metaphorical. 'Aggregate capital' involves an analogy of 'capital' (itself analogical) with something – sand, bricks, shmooos – that can be 'added' in a meaningful way; so does 'aggregate labour,' with the additional peculiarity that the thing added is no thing, but hours of conscientious attentiveness; the very idea of a 'production function' involves an astonishing analogy to the subject, the fabrication of things, about which it is appropriate to think in terms of ingenuity, discipline, and planning, with the modifier, a mathematical function, about it is appropriate to think in terms of height, shape, and single valuedness."

If metaphors are essential to economic thinking, MCCLOSKEY continues, these metaphors should be openly recognised and examined, but not be banished. For him the consequence to draw from a better understanding of economists' rhetoric is to avoid any discrimination of allegedly 'non-scientific' arguments, and to open economics "to a wider range of discourse" (p. 493). What counts in the end is the persuasiveness of argument: "There are some subjective, soft, vague propositions that are more persuasive than some objective, hard, precise propositions" (p. 511). MCCLOSKEY concludes that the recognition of this fact will, due to more transparency and honesty, also contribute to a better quality of research and teaching (pp. 512-515).

SCHOR (1991) takes these rather provocative claims as a starting point for the development of a more fully articulated, methodological position of what he calls 'pragmatic instrumentalism' (pp. 103-140). According to its very name, the key characteristics of this position are (a) a close *proximity to the object of investigation* (in opposition to an abstract 'scientific rule' of positivist methodology), and (b) the recognition of scientific reasoning as an *instrument of argumentation* within the scientific community (rather than a universally valid explanation of the real world). A major stepping stone in SCHOR's argumentation is the so-called 'linguistic turn' in the philosophy of science, according to which there is no real world existing independently of a system of language, and the relationship between language and real world cannot be examined

⁸ A brief and up-to-date overview is HANDS (2001).

without referring to the context in which a speech product originated (SCHOR 1991, p. 106).⁹ It is hence regarded as impossible to assess an abstract theory without reference to the individuals by whom it is constructed and employed. The purpose of theories is no longer to formalise truth claims, but, more modestly, to structure and communicate relevant problems identified by the scientist *in a way that others find plausible or persuasive*.

A question largely left unanswered so far is *what makes* an argument plausible. If producing plausible statements about some real phenomena is the purpose of economic reasoning, it is natural to ask what kind of benchmark distinguishes plausible from less plausible statements. SCHOR (1991, pp. 119-124) claims that a theory is plausible if its user *recognises the correspondence between the theory and his subjective knowledge or understanding of the real world*. Subjective knowledge consists both of individual perception (for example based on intuition or introspection) *and* empirical facts (for example based on experimental measurement or observation). A plausible theory connects its content with the user's subjective knowledge by means of analogy or metaphors. There are no formal or objective criteria determining whether an analogy is valid, this is decided upon by the subjective perception of the auditorium alone. However, according to SCHOR, a theory is to be preferred among others with otherwise the same properties if it has a *larger target area* for *questioning its premises and implications* (p. 134). A good theory hence *promotes the interaction process* between its content and the auditorium, for example by the use of well chosen metaphors.¹⁰

Against this background, the 'traditional' methods of economic reasoning are relieved from the burden of being imperfect tools for a positivist methodology. However, in the view of pragmatic instrumentalism, their specific strengths is now ascribed a new quality (SCHOR 1991, pp. 162-176). The widespread use of *mathematical models* in economics, for example, may be regarded as an obvious hint for the reader that this is no description of reality but a fiction, an idealising metaphor of real world events. By formulating a theory in mathematical terms, the degree of unambiguity and consistency is clearly raised, which in turn increases the target area for questioning and probing. Furthermore, mathematical language has the advantage of being easily communicated and widely understood. All this is desirable for a methodology of pragmatic instrumentalism. Similarly, *econometric modelling* is now assessed with regard to its persuasiveness and plausibility, but no longer seen as the fundamental judge for the falsification of theories. The assumption that one knows the real structure of an econometric model simply serves as a means of argumentation, but does no longer claim any universal validity or truth. However, econometricians should be aware of the rhetoric they are using, which implies to make transparent the limitations of the employed statistical techniques. Furthermore, not only those statements are scientifically relevant that can be underpinned by an econometric analysis. More abstract concepts – such as transaction costs or social capital – may bear persuasive power although they are hard to operationalise. On the other hand, econometrics also has a value in explorative work, even when it is not used to validate existing theories.

⁹ Since my exposition at this point has more the form of a digression, it must necessarily be crude and simplifying. The reader interested in the broader background of the philosophy of social science is referred to the recent introductory survey by BENTON and CRAIB (2001).

¹⁰ To elaborate on the philosophical debate about the interconnection between social reality and language is beyond the scope of this paper. Inter alia, SCHOR (1991) draws on work by HABERMAS who, roughly speaking, takes language as a model for democracy. The properties of language can be used to derive an 'ideal speech situation', which creates a standard for assessing real world communication processes (BENTON and CRAIB 2001, p. 115). See also HABERMAS (1999).

7 SUMMARY AND CONCLUSIONS

This paper argued that mainstream economics does not follow the positivist research tradition it (often implicitly) claims to follow and expressed some serious doubt that econometrics in particular can make economics a hard science. Mathematical rigour and sophisticated statistical techniques may be regarded as persuasive analytical tools in economics, but for sure their mere application does not guarantee good research practice, even not in a falsificationist perspective. The paper outlined an alternative methodological view in which the well known tools of economic analysis can find a new place. Instead of relying on a narrow rule that purports to produce universal truth, it encourages to *increase the target area for questioning and probing*. Much more attention is paid to the *communicative aspects* of scientific methodology – in which terms an analysis is framed, and under what conditions, to whom, by whom, and by which means it is articulated. Scientific discourse is an act of conversation which should make transparent its internal structure and the rhetoric that is being used.

It is my hope that the reader realises the relevance of these aspects of economic methodology and got some understanding why it may be useful to step beyond the traditional boundaries of positivist methodology. In the end, this may shed some new light on the way we see ourselves as scientists and lead to a more honest and productive mode of economic inquiry.

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