REALISM, INSTRUMENTALISM AND THE TIME ASPECT OF THEORY BUILDING

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Abstract:
This paper contributes to the debate about the role of realism and instrumentalism in social science. It asserts that the debate should distinguish between *ex-ante* and *ex-post* perspectives. *Ex-post*, *i.e.* once the research is done, instrumentalism might be given priority because in this situation, significant predictive power is the key. On the other hand, *ex-ante*, *i.e.* before any model revealed its predictive power, only realism may give a hint of which direction is promising to pay off the effort. Thus both realism and instrumentalism play their role in the scientific activity, depending on the stage of the research. In this paper I argue that in order to develop economic realism, we should have recourse to phenomenology. Phenomenology analyses the thought constructs of ordinary people, classifies them and seeks the underlying invariant structure. Realistic social science should build on these invariant structures; only then the link to social reality is not lost.

Keywords: realism, instrumentalism, *ex-ante*, *ex-post*, phenomenology, economic methodology
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1. Introduction

Julian Reiss (2012) in his recent paper argues against realism, bringing many insightful arguments in favour of instrumentalism in economics. In the present paper I intend to critically comment on his view; I find a deficiency in his line of argument and offer a viable way to reconcile realism and instrumentalism in economics. The paper is organized as follows: in the second section, the definition of realistic models is questioned, and it is shown that "reality of models" means only their conformity with a paradigm. In the third section, the time aspect of scientific inquiry is analysed, coming to the key difference of *ex-ante* and *ex-post* realism/instrumentalism of scientific theories, through which the need for realism is revealed. In the final section, a workable form of realism in economics is suggested, in the form of the phenomenological approach.

2. What Does Make Models Realistic?

Reiss (2012, p. 367) correctly reminds us that in a sense, no scientific models are realistic, as their goal is not to mirror, but to *map* reality. A map is designed to enable our orientation in the world, so it contains only relevant information about reality. If it contained all information accessible, it would be useless. That is also why there is an immense number

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of mapping methods of reality, none of them more realistic than the others. Methods are chosen according to the nature of relevant information, and relevancy of information is derived from the goals we pursue.

According to Reiss, realism must have a form of "partial" realism: "The realist therefore has to define a set of aspects of a model the truth or truthful representativeness of which he thinks is valuable…" (Reiss, 2012, p. 367), where the "truth" must mean something more than mere predictive accuracy. Reiss then concentrates on particular forms of truth requests (especially claims for causal relationships) but leaves open the question that should have been answered first: what does make models realistic[1]?

The problem of realism is not that it requires scientific models to be true representations of reality; the problem is *how to assess* this truthfulness. The realistic approach tacitly assumes that we have another measure of truth than mere predictive accuracy and significance; it assumes that we have another access to reality besides scientific models – direct access to (pure) facts. But is such an access possible? Recall the mapping analogy. All mental representations of reality are like maps – scientific models as well as concepts of a common man. In this sense, their nature is the same. Schütz (1971a, p. 5) illustrates the point clearly:

> All our knowledge of the world, in common-sense as well as in scientific thinking, involves constructs, *i.e.* a set of abstractions, generalizations, formalizations, idealizations specific to the respective level of thought organization. Strictly speaking, there are no such things as facts, pure and simple. All facts are from the outset facts selected from a universal context by the activities of our mind.

If we accuse a model of not being realistic, we are not comparing it with reality; we are comparing it with another model – the model which we *believe* is the accurate map of reality. Again, we may ask: what is the basis of our belief? What makes us feel that such a belief is the privileged one?

Some authors claim that the privileged status stems from the fact that we are unable to abandon it, as is the case of Ludwig von Mises and his praxeology: "Everybody in his daily behaviour again and again bears witness to the immutability and universality of the categories of thought and action. ... We cannot think of a world without causality and teleology." (Mises, 1963, p. 35).

I suspect that much of our inability in this sense is based on habit and lack of imagination. We rely on our *paradigm*; however, as Kuhn (1996) points out, paradigms may shift. There are many examples of "essential truths" that were abandoned later on. Should not that be a reason for caution against realism? In a sense, there is no "realism", but only "paradigmism".

1 It should be mentioned that a model can be considered realistic or unrealistic in manifold ways. It may be said that a model is realistic in the sense that it refers to a real entity; or a model might be considered realistic in the sense that it is successful in empirical tests; or it might be realistic in the sense that it is plausible, *etc.* (For a detailed list of different types of "realisticness" of economic theories, see Mäki, 1998.) Reiss concentrates only on the "veristic realisticness" (*i.e.*, whether the statements of the theory are true or false). For the sake of the argument, let us adopt his view.

3. Is There Any Need for Realism?

The critique of realism offered above does not bring a clear victory to instrumentalism[2]. There is a common confusion hidden in the plea for instrumentalism, which stems from mixing up *ex-ante* and *ex-post* instrumentalism. *Ex-ante* instrumentalism means instrumentalism with respect to the starting point of scientific research programs and models. *Ex-post* instrumentalism means instrumentalism with respect to the performing of research.

Most advocates of instrumentalism implicitly concentrate on *ex-post* instrumentalism. Friedman (2008) serves as a classical example of the *ex-post* instrumentalism defence, where the question of *ex-ante* instrumentalism is not addressed. Reiss (2012) concentrates on *ex-post* instrumentalism too. But such a defence could be superfluous[3]. It is of no use to ponder about the reality of models *once* they are employed. For that matter, practising scientists are not interested in this problem at all; they do science rather than ponder its assumptions. Practicing economists do not need to care whether people really follow their goals, or whether it only seems so. Utility maximization is simply a scheme used to predict human behaviour. Economists define certain "utility function", the range of variables influencing human behaviour, and empirically test the explanatory power of their model. The "ontological status" of the model does not need to be addressed[4]. Its use is justified purely by its applicability.

Methodologists would be mistaken if they thought that this approach eliminated the concept of truth from the scientific work (since accuracy of predictions is the objective, there is no need to speak about truth). But practicing scientists do not deny the concept of truth; they only do not reflect it. A reflection of the problem of truth, *i.e.* a reflection of the ontological status of models, is not necessary for carrying out the scientific research. More likely, it could hamper the work. Any possible success of a scientist stems mainly from the fact that he uses his model as an interpretative scheme anytime and anywhere. He is passionate in his work and sees the world through his "scientific glasses". Therefore he finds relevant principles (biological, chemical, economic, *etc.*) and especially their application, where no one else would even think of them. Such a "distortion of mind" may sometimes seem ridiculous and it definitely does not always lead to new discoveries. However, thanks to these people, our scientific knowledge of the world multiplies and deepens every year. Actually, it is *inappropriate* to ask a chemist why he sees chemical bonds and reactions everywhere, or to ask an economist, why he sees motivated human actions behind everything. Chemists and economists are doing their job; questions about principles would just delay them.[5]

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2 Like realism, instrumentalism also needs further explanation. Generally, instrumentalist position holds that scientific theories are instruments for pursuing scientists' ends. They should not to be judged by their "realisticness"; their value is pragmatic, measured by their ability to contribute to scientists' ends. The conventional approach considers *successful prediction* to be the goal of science; however, scientists might have other goals as well (such as the aesthetic pleasure derived from simple theories). Here we adopt the former approach – instrumentalism as predictionism.

3 As far as this is concerned, my position here is in line with Reiss's (2012) "cheer 1" for instrumentalism.

4 *Cf.* the footnote No. 4 in Hausman (1992, p. 119).

5 Among quantum physicists, this is summarized in a popular claim: "Shut up and calculate." (Mermin, 1990, p. 199).

On the other hand, even the practising scientist must have had a starting point *before* he began to construct his models. How did he set his initial hypotheses? Could he do that instrumentalistically – with respect to predictive power only, regardless of reality (more precisely, regardless of any paradigm)? These are the questions addressing *ex-ante* instrumentalism, which lead to much fruitful debate.

It is possible to set the initial hypotheses instrumentalistically only if they are already framed in an established model or in an established interpretative scheme. However, if a scientist is to choose an interpretative scheme itself (before it manifested its predictive power), what does he have at hand? Recall Kuhn (1996, pp. 157–158):

> The man who embraces a new paradigm at an early stage must often do so in defiance of the evidence provided by problem-solving. He must, that is, have faith that the new paradigm will succeed with the many large problems that confront it, knowing only that the older paradigm has failed with a few. A decision of that kind can only be done in good faith.

Kuhn's words apply to the situation of scientific revolution; but even in a normal science situation, scientists face the choice of models without having enough information about their predictive power. What to choose then? Feyerabend (1993) would suggest anarchism ("anything goes") and especially counterinduction (use of hypotheses that contradict well-confirmed theories or well-established experimental results). According to him, only such an approach has a chance to promote progress.

On the other hand, every human action has its costs; even pondering about possible hypotheses and models does have costs. The "expected utility" (expected predictive power) of hypotheses that have been chosen arbitrarily is highly doubtful. For example, in an effort to explain the price movements at the stock market, it is surely possible to test the influence of sun spots, or shifts in natality, or the spin of electrons, or the change of curvature of bananas, *etc.* The number of possible explanations is infinite, but only some of them are promising to pay off the effort.

> The search for assumptions on the part of the scientist is subject rather to a whole host of *ex-ante* (or as we might also say: *a priori*) controls. For he seeks only those assumptions which will give him a justified expectation of predictive power. The sun-spot example fails to awaken such expectations precisely because we can find no intelligible reason why sun-spots should cause stock exchange movements. Even if we were to accept such a correlation as a fact, we would still be unsatisfied with the corresponding hypothesis, because we would feel no certainty that this correlation might not at any moment cease to obtain. Certainty of this sort is acquired only where we have some explanation as to *why* these and those stock exchange phenomena are associated in a non-arbitrary fashion with these and those sun-spot phenomena (Smith, 1996, pp. 183–184).

This excerpt is also the answer to Reiss' (2012, pp. 371–2) example of two models, both equally significant, but only one of them being true. Reiss asks what value added does the truth bring? The answer is: if we were not able to translate correlation into (genuine) explanation, "we would feel no certainty that this correlation might not at any moment cease to obtain". Now this is not only a critique of the *ex-ante* instrumentalism; it shakes the position of the *ex-post* instrumentalism as well. The reason is that in two ways,

the scientific inquiry is never complete: first, every model is partial (recall the mapping analogy again) and its predictions are partial too. They do not cover all they could and they are never faultlessly precise. Hence we may discriminate models by their *promise* of successful extension and improvement. Needless to say that the promise should not be supported only by the past prediction success, since "past performance is no guarantee of future results"[6]. Second, the reality is constantly changing; scientific models must be able to incorporate it and remain stable when changing their parameters. However, the instrumentalism does not give us a clue whether we should tune up the existing model or build another one (and in that case, what model to build?). We are coming back to the problem of *ex-ante* instrumentalism. Indeed, there is always a hidden realism in the scientific practice. It might not be touched on explicitly (since the explicit formulation is not necessary for carrying out the research) but it is there implicitly contained (for otherwise, the choice of future paths of the research would be too arbitrary and costly).

If we want to describe the behaviour of scientists, we must acknowledge the existence of their inner "*a priori* controls". In other words, we must acknowledge realism which is hidden in scientific practice and which manifests in situations when a scientist seeks for assumptions with a *justified* expectation of predictive power. It frames the scientific research; it is a measure of *plausibility* of models and hypotheses. Thus realism and instrumentalism both play a role in the research activity:

1. At the beginning, realism takes part: it comes up with plausible, justified hypotheses.
2. Instrumentalism joins in: practising scientists diverge from realistic assumptions, either incidentally, or intentionally. They seek for significant predictive power.
3. The question of realism arises again, if a successful *and* unrealistic theory occurs. It should be reconciled with our view of reality; otherwise we would not be sure, how firm and permanent the predictions are.[7]
4. The answer then is either to provide a competing theory (that would be both successful and realistic), or to correct our view of reality (to refine realism) in order to be able to go back to point 1.

4. What Form of Realism to Develop?

Practicing scientists do not state many of their assumptions explicitly. It is the task of methodologists to analyse their conceptual framework, to refine and to develop it further, in order to facilitate the next research activity (see the point 4 mentioned above).

Take the example of bounded rationality of economics. Does it pose a threat to the overall validity of the theory of rational choice? Or, is rational choice a useful but limited tool, and both approaches complement each other? Or, is it advisable to rationalize all bounded-rationality models (as rational responses to "frictions")?[8] The empirical results of either part are not conclusive enough to put an end to such questions. Therefore

6 See also Hausman (1992, pp. 70–73).

7 See also Schotter (2008, p. 72): "It is only when choice is consistent not only with the predictions of a theory but also with the reasons stipulated for that choice that we can be confident that the predictions of the theory will remain valid when the parameters of the model change." Cf. also Long (2006, p. 4).

8 See Spiegler (2011) for the recent discussion of the latter.

the realisticness of models plays a role, and economists adhere to this characteristic as to the important one. The service of economic methodology is needed and economic methodology should concentrate on deeper analysis of economic realism.

The starting point of this form of realism is well-known: Economics looks on a man as on a being who has goals and strives to achieve them. Since people live in a world where the means to their ends are often scarce and having alternative uses, people must decide; they have to choose between competing ways to achieve their goals. Obviously, the basic approach of economics is based more on common-sense understanding of human beings or folk psychology (Rosenberg, 1998) than on the natural-science approach. The teleological structure of human behaviour is derived from natural experience (it is "phenomenological") and economists intend to use it as their interpretative scheme.

Certainly, this was not their only option. Economists could alternatively adopt the naturalistic view, according to which human beings are to be treated in the same way as any other living creatures.[9] But they did not choose the option. Most economists *really believe* that men pursue their ends and choose the means to achieve them.[10] They really believe that this teleological structure provides us with a key to the understanding of human behaviour and its consequences. On the other hand, it is not enough to take the teleological structure of human behaviour in the form of the plain statement that "man is a creature who pursues ends and looks for (the best) means to achieve them." We understand ourselves in a much richer structure. There is much more to be done in the examination of teleological structure of the human world. What should such examination look like?

I have labelled the teleological structure "phenomenological" intentionally. We cannot discover the teleological structure of human action by using methods of natural sciences. We can reveal it only as *subjective phenomena* of our being-in-the-world; as a necessary part of *meaningful* human experience. Traditionally, this is a task of phenomenology, as in the works of Edmund Husserl (1978, 1999). Phenomenology seeks to disprove our above mentioned objection that there is no privileged access to reality. In fact, there is some – it is our natural position in our life-world. The world, as well as the things in the world, is not given to us in an arbitrary way. If we ponder our experience it shows that while some things can be changed, without anything significant happening, others cannot be thought of in a different way. Some things are an *indispensable*[11] base of our being-in-the-world, a prerequisite of our experience.[12]

9 The fact that people are endowed with consciousness is simply the result of evolution, and so is their ability to represent reality (*i.e.* their use of language, their use of concepts). This ability is probably related to the complexity of the human organism as a new way to solve complex problems. It may, however, show that creatures without self-consciousness are evolutionarily more successful. Our mental states are the same fact as any other physical data. They are part of a causal world. They are nothing more.

10 When Paul Samuelson, in his famous article (Samuelson, 1938), freed the consumer-choice theory from the utility concept, he did so in order to get rid of a concept with no explanation power, not in order to deny rational, motivated human behaviour. Even the name "revealed preference theory" clearly refers to our belief that human choice is underlain by motivated people.

11 It may remind us of the above mentioned Mises's approach. We, however, will not pursue his line of reasoning. Špecián (2012) and Svoboda (2014) show that it is encumbered by methodological difficulties which make it inferior to the phenomenological position.

12 It is common to use the term "*a priori*" structures to stress the fact that the knowledge of them

To the area of social science, the phenomenological analysis was brought by Alfred Schütz. Schütz (1967), knowledgeable with phenomenology, tried to refine Max Weber's theory of social action and his interpretive sociology, which has common roots with economics:

> [Sociology is] the science whose object is to interpret the meaning of social action and thereby give a causal explanation of the way in which the action proceeds and the effects which it produces. By 'action' in this definition is meant the human behaviour when and to the extent that the agent or agents see it as subjectively *meaningful*… (Weber, 2007, p. 7).

In order to understand social (and all human) action, we must classify individual events under "ideal types", thus constructing a model of rationally purposive action. However, Weber did not offer an unambiguous way of constructing ideal types, giving a space for the objection that ideal types were rather arbitrary constructs of a scientist, which limited their universal validity. According to Schütz, this was so because Weber took the meaningful action as a primitive for his analysis; nevertheless, crucial was the analysis of the phenomenon of meaning itself. The analysis of its origin and structure could reveal the non-arbitrary process of the ideal-types construction.

Our life-world is full of meaning. There are no raw data; we always classify the world and things somehow. Our world is the world of meaningful concepts – there are no pure facts but always interpreted facts. These naïve, pre-scientific interpretations of a common man underlie the concepts of social science. Phenomenology may function as an important link between the life-world constructs and objects of science. Phenomenology ponders the everyday man's thought objects, classifies and organizes them; it varies them in order to reveal their invariant features (*eidos* in Husserl's terms). Through the analysis of this kind, it helps social scientists to build on typical constructs (ideal types), and not on arbitrary ones.[13]

Knowledge of invariant structures of the human world, provided by phenomenology, should be compulsory for every economist, since it suggests which questions and ways of research are promising. Those, who want to avoid those suggestions, risk their work being useless. Their conclusions would be too flat and not very fitting.

5. Conclusion

In the current debate about realism and instrumentalism, it is often neglected that we have no access to pure facts. We never compare scientific models with reality, but only with some other models of reality – our paradigms. In fact, realism means "paradigmism".

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necessarily precedes any other empirical knowledge (for only with their use *meaningful* experience is possible). However, even if we perceive *a priori* structures as essential, we must resist the temptation to label the knowledge of them as "certain" or "indisputable". Firstly, every one may make a mistake while doing research. Secondly, the certainty, with which *a priori* structures are found, may not be perceived as perfect, but it can be certain to some extent. This is related to how we find *a priori* structures – as something without which it is *practically* impossible to think about the world. As for some things, it is impossible "at all"; as for others, it is only "well-nigh" impossible. Thus even the *a priori* knowledge is not infallible. Cf. Smith (1996).

13 For the detailed description of the process through which the scientific thought constructs should be derived from the thought constructs of a common man, see Schütz (1971b) or Svoboda (2014).

This is not to say that realism is to be completely rejected. Its importance varies according to the time aspect of the scientific research. If we inquire *ex-post*, *i.e.* once it is being carried out, instrumentalism might be given priority because in this situation significant predictive power is the key. (No one would prefer a more realistic model if its predictions were inferior.) Nevertheless, if we look into the scientific research *ex-ante*, *i.e.* before any model revealed its predictive power, only realism may give a hint which direction is promising to pay off the effort. Thus both realism and instrumentalism play their role in the scientific activity, depending on the stage of the research.

Realism in economics stems from common-sense understanding of humans as motivated, active beings. In this paper I argue that if we want to develop economic realism, we should have recourse to phenomenology. Phenomenology analyses the thought constructs of ordinary people, classifies them and seeks for the underlying invariant structure. Realistic social science should build on these invariant structures; only then is the link to the social reality (*i.e.* to the thought objects of the life-world) not lost.

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