Philosophia 80/1 ▌2020 ▌pp. 9 a 39

CONTROVERSIES ON THE EMPIRICAL SIGNIFICANCE OF AUXILIARY ASSUMPTIONS*

Controversias en torno a la significación empírica de los supuestos auxiliares

María CAAMAÑO ALEGRE
UNIVERSIDAD DE VALLADOLID (ESPAÑA)
mariac@fyl.uva.es

Abstract: Theoretical laws need to be conjoined with auxiliary assumptions in order to be empirically testable, whether in natural or social science. A particularly heated debate has been developing over the nature and role of these assumptions in economic theories. The so called "F(riedman)-Twist" ("the more significant the theory, the more unrealistic the assumptions", Friedman 1953) as well as some later criticisms by authors like Musgrave, Lawson, Mäki and Cartwright will be examined. I will explore the apparent conflict between the Popperian desideratum to pursue the independent testability of auxiliary assumptions and the idealizational theoretical means needed to isolate causal variables.

Keywords: Friedman-Twist, auxiliary assumptions, realism in economics, idealization.

Resumen: En cualquier ámbito científico, las leyes teóricas deben combinarse con supuestos auxiliares para poder contrastarse empíricamente. En economía, se ha venido desarrollando un debate particularmente acalorado sobre la naturaleza y el papel de estos supuestos en las teorías económicas. Se examinarán el llamado "F(riedman)-Twist" ("the more significant the theory, the more unrealistic the assumptions ", Friedman 1953), así como algunas críticas posteriores de autores como Musgrave, Lawson, Mäki y Cartwright, atendiendo al aparente conflicto entre el desideratum popperiano de buscar la contrastabilidad independiente de los supuestos auxiliares y los

 ▌ISSN 0328-9672 (impresa) ▌ISSN 2313-9528 (en línea)

procedimientos de idealización necesarios para aislar las variables causales.

Palabras clave: Friedman-Twist, supuestos auxiliares, realismo en economía, idealización.

1. Introduction

Auxiliary assumptions have been under discussion over the last decades − particularly in the field of economics−, where special attention has been paid to the risk of misusing of idealizations. Idealizations often operate at different although interrelated levels, in the very formulation of theoretical laws and in auxiliary assumptions usually accompanying the latter. Idealizing assumptions in economics have often been the target of criticism due to their highly unrealistic nature. Yet, Popper's frequently invoked emphasis on specification and refutability is not at odds with idealizations, as it is sometimes suggested, it is just at odds with epistemically unjustified idealizations, not helpful to uncover any interesting truths. My goal is to explore the apparent conflict between the Popperian desideratum to pursue the independent testability of auxiliary assumptions and the idealizational theoretical means needed to isolate causal variables. I will argue that heuristic assumptions or idealizations must be evaluated by methods other than the merely derivational ones, combining different resources, like the

bidirectional method of empirical approximation,[1] and dialectical methods such as contrast explanation and replacement of assumptions.[2]

I will start with some clarifications on the debate around auxiliary assumptions (section 2), then I will discuss what is called the "F(riedman)-Twist" in economics, that is, Milton Friedman's influential (and controversial) vindication of unrealistic assumptions (section 3), and comment on Alan Musgrave's critical respond to it (section 4). After that, I will consider some methodological insights by Tony Lawson, Uskali Mäki and Nancy Cartwright that could be regarded as challenges affecting both sides of the debate (section 5). Finally, some possible answers to the challenges will be sketched (section 6).

2. Main features of the debate

The nature of auxiliary assumptions is a subject that has been addressed very early in the contemporary philosophy of science, already in Pierre Duhem's discussion of the holistic features of confirmation.[3] He convincingly argued that it is impossible to test a hypothesis in isolation, since, in order to derive empirical consequences from a hypothesis, the latter needs to be conjoined with many other assumptions and hypotheses about

* I am thankful to Valeriano Iranzo and other members of the Valencia Philosophy Lab for valuable feedback on an earlier version of this work. This research was financially supported by the research projects "Laws and Models in Physical, Chemical, Biological, and Social Sciences" (PICT-2018-03454, ANPCyT, Argentina), and "Stochastic Representations in the Natural Sciences: Conceptual Foundations and Applications (STOCREP)" (PGC2018-099423-B-I00, Spanish Ministry of Science, Innovation and Universities).

1) Igor Hanzel, "The Inherent Type of Scientific Law, The Idealized Types of Scientific Law", in *Idealization XIV: Models in Science, Poznań Studies in the Philosophy of the Sciences and the Humanities*, Volume 108, ed. Giacomo Borbone & Krzysztof Brzechczyn (Boston: Brill/Rodopi, 2016), 43-62.

2) See, respectively, Tony Lawson, "Applied Economics, Contrast Explanation and Asymmetric Explanation", *Cambridge Journal of Economics*, 33/4 (2009): 405–19, "Central Fallacies of Modern Economics", in *Economic Objects and the Objects of Economics. Virtues and Economics*, vol. 3, ed. Peter Róna & László Zsolnai (Cham: Springer, 2018), 51-68, and Uskali Mäki, "Realistic Realism about Unrealistic Models", in *The Oxford Handbook of Philosophy of Economics*, ed. Harold Kincaid & Don Ross (Oxford: Oxford University Press, 2009), 68-98.

3) Pierre Duhem, *The Aim and Structure of Physical Theory* (Princeton (NJ): Princeton University Press, 1906/1991).

the world, the functioning of measuring instruments, the environmental conditions, etc. For example, in testing hypotheses from thermodynamics, we need to be able to empirically determine changes in temperature by correlating changes in temperature with changes in some other quantity. If we use a mercury thermometer to this end, we need to assume that changes in the length of the strand of mercury is what is relevant to be able to establish changes in temperature and to endorse numerous assumptions about how mercury expands or contracts as the temperature rises or falls. According to Duhem, this type of measurement depends on the assumption of certain laws of nature, like linear expansion, according to which, change in length is directly proportional to the change in temperature. Also, there are assumptions on the conditions under which a temperature reading as given by a mercury thermometer should be disregarded, for example, if the mercury thermometer is placed in a strong magnetic field.

As it is well known, Duhem emphasizes as an important implication of his view that confirmation holism precludes the possibility of performing crucial experiments. He famously denied that there had been a crucial experiment leading to the rejection of the particle theory of light in favor of the wave theory of light. According to him:

> (…) in fact, what the experiment declares stained with error is the whole group of propositions accepted by Newton, and after him by Laplace and Biot, that is, the whole theory from which we deduce the relation between the index of refraction and the velocity of light in various media. But in condemning this system as a whole by declaring it stained with error, the experiment does not tell us where the error lies. Is it in the fundamental hypothesis that light consists in projectiles thrown out with great speed by luminous bodies? Is it in some other assumption concerning the actions experienced by light corpuscles due to the media in which they move?[4]

4) Ibid., 187.

Willard Van Orman Quine took Duhem's argument a step further and asserted that a theory can always avoid refutation by changing the auxiliary assumptions conjoined with it.[5] While accepting the very fact of confirmation holism, Popper rejected the implications drawn from the so called Duhem-Quine thesis, in particular, the idea that, when a false prediction is derived from a hypothesis conjoined with auxiliary assumptions, it is not possible to identify where the mistake lies.[6] Against this "holistic dogma", as he calls it, he claimed that it is always possible to pinpoint the logical connections between hypotheses or assumptions and refuted predictions. The way to do that would be similar to the one applied to prove the independence of axioms in axiomatic systems, which would involve finding out a model that satisfies all axioms but the independent one. When some refuting evidence is gathered, such evidence may provide a model that satisfies several assumptions while not the main hypothesis that happens to be conjoined with them. If so, even in non-axiomatized systems, we could identify the source of error by conjoining a different hypothesis to the same assumptions and check whether the previously refuting evidence is now a model of the new system sharing the same auxiliary assumptions with the old system. In that case, if a positive result is obtained, we have good grounds to infer that the assumptions were not the source of error in the first place, that is, when conjoined with the old hypothesis. As a consequence, the more analyzed a theoretical system is, the better for methodological purposes.

Earlier, Popper had also objected to the idea that *ad hoc* modifications, replacements or additions of auxiliary assumptions is an acceptable scientific practice.[7] Good scientific practice would require an effort to uncover

5) Willard Van Orman Quine, "Two Dogmas of Empiricism", in *From a Logical Point of View*, by Willard Van Orman Quine (Cambridge, Massachusetts: Harvard University Press, 1951/1953), 20-46.
6) Karl Popper, Conjectures and Refutations: The Growth of Scientific Knowledge (London: Routledge, 1963), 322-325.
7) Karl Popper, *The Logic of Scientific Discovery* (London: Routledge, 1935/2002), 19-20, 59-61.

mistakes, not the opposite, and so, according to Popper, auxiliary assumptions should be modified whenever there is refuting evidence undermining them, but not when the refuting evidence rather undermines the main hypothesis the assumptions are conjoined with. A case in point is the *ad hoc* assumption that phlogiston has negative weight in order for phlogistonians to accommodate the anomaly of the increase in weight of calcined metals, despite that fact that no independent evidence supported the introduction of such assumption. Some typical, textbook examples of auxiliary assumptions may also help to get a sense of how the Popperian test for the acceptability of auxiliary assumptions could work. To test the hypothesis that puerperal fever was caused by cadaveric contamination, it was assumed that certain substance used to remove such contamination had indeed disinfecting power, an assumption that was clearly testable independently of the hypothesis it was conjoined to. The testing of Copernican astronomy on the basis of the lack of observable stellar parallax is a less clear-cut case, since here the independent test of some auxiliary assumptions has its own difficulties. The lack of observable stellar parallax can only be acknowledged as evidence refuting Copernican astronomy if it is assumed that stellar parallax can be observed regardless of the distance between the Earth and the stars. On the contrary, if the magnitude of the distance between the Earth and the stars is assumed to rule out the possibility of observing the stellar parallax, then, obviously, the lack of observable stellar parallax cannot be considered as evidence refuting Copernican astronomy. Now, here we have a case where confirmation holism comes together with some limitations in the independent testability of auxiliary assumptions, ultimately resulting in a strong (although historically transient) underdetermination of theory by observation.

Interestingly, while the problem of auxiliary assumptions has mainly been approached indirectly in general philosophy of science, most often in

discussing confirmation holism, underdetermination and adhocness,[8] a particularly heated and detailed debate has developed over the role of auxiliary assumptions in economic theories. As a consequence, the issue of auxiliary assumptions has been addressed more thoroughly in the literature in philosophy of social science.[9] It is certainly very common that some philosophical and methodological discussions about different aspects of science are developed in more detail in social sciences, where the problematic side of those aspects appears more clearly. The problem of validity of experiments and the very issue of auxiliary assumptions are cases in point, falling out of the focus of attention in general philosophy of science, traditionally very oriented towards the study of natural science, where experiments and assumptions seem to be less problematic if compared to social science. Still, the progress or insights that philosophers of social science have made in these subjects can be extended beyond social science to reach also natural science.

In the middle of the 20th century, Milton Friedman made a very influential defense of the use of idealizations in economics, leading to what has been labeled "the F(riedman)-Twist", which can be comprised in his famous statement that: "the more significant the theory, the more unrealistic the

8) Duhem, The Aim and Structure of Physical Theory, Popper, The Logic of Scientific Discovery, Popper, Conjectures and Refutations, Quine, "Two Dogmas of Empiricism", Imre Lakatos, The Methodology of Scientific Research Programmes (Cambridge: Cambridge University Press, 1978).

9) Ernest Nagel, "Assumptions in Economic Theory", *The American Economic Review*, 53/2, May (1963): 211-219, Alan Musgrave, "'Unreal Assumptions' in Economic Theory: The F-Twist Untwisted", *Kyklos,* 34/3 (1981): 377-87, Mäki, Uskali & Piimies, Jukka-Pekka, "Ceteris paribus", in *The Handbook of Economic Methodology*, ed. Davis, John B., Hands, D. Wades & Mäki, Uskali (Edward Elgar, Cheltenham, 1998), 55-59, Uskali Mäki, "Kinds of Assumptions and Their Truth: Shaking an Untwisted F-Twist", *Kyklos*, 53/3 (2000): 303-322, Uskali Mäki, "Ceteris Paribus: Interpretaciones e Implicaciones", *Revista Asturiana de Economía,* 28 (2003): 7-32, Nancy Cartwright, "Are RCTs the Gold Standard?", *BioSocieties* (Special Issue: The Construction and Governance of Randomised Controlled Trials) 2/1, March (2007a): 11-20, Nancy Cartwright, "The Vanity of Rigour in Economics: Theoretical Models and Galilean Experiments", in *Hunting Causes and Using Them: Approaches in Philosophy and Economics*, by: Nancy Cartwright (Cambridge; New York: Cambridge University Press, 2007b), 217-261.

assumptions".[10] The F-Twist implied that economics follows the example of physics in applying the Galilean "paradigm", thus including, in economic theory, assumptions equivalent to those about frictionless planes, perfectly rigid bodies, mass points in physics. In both fields, the predictive fruitfulness of idealizations would provide the epistemic justification of these literally false assumptions. In a paper from 1981, Alan Musgrave tried to untwist the F-Twist by showing that, in his own words: "[in economic theory] the more unrealistic domain assumptions are, the less testable and hence less significant is the theory".[11] According to him, there has been a systematic misuse of the Galilean "paradigm" in economic theory, a misuse related to both the neglect of the empirical nature of auxiliary assumptions and the failure to distinguish the different purposes of negligibility, domain and heuristic assumptions respectively.

I will first examine the Friedmanian arguments in favor of "unrealistic assumptions" and later will focus on some criticisms by authors like Musgrave, Uskali Mäki, Tony Lawson and Nancy Cartwright to conclude with some positive proposals invoking the role of empirical approximations.

3. The F(riedman)-Twist: the vindication of unrealistic assumptions

Friedman's argument in favor of unrealistic assumptions hinges on the distinction between descriptive accuracy and analytical relevance. The former would require a detailed empirical correspondence between theoretical assumptions and the target domain, while the latter would involve an explanatory and predictive effectiveness usually dependent, in turn, on the endorsement of unrealistic assumptions. According to Friedman, descriptive accuracy is not compatible with analytical relevance, that is, an empirically detailed theory would defeat its own purpose, namely, explaining

10) Milton Friedman, "The Methodology of Positive Economics", in *Essays in Positive Economics,* by Milton Friedman (Chicago: University of Chicago Press, 1953/1966), 3-16, 30-43, 14.
11) Musgrave, "'Unreal Assumptions' in Economic Theory: The F-Twist Untwisted", 382.

and predicting phenomena in certain domain by identifying a few variables as the main one responsible for them. The identification and selection of a limited set of explanatory variables, as well as the formulation of fundamental conjectures on how they operate, force us to go beyond realistic descriptions into the domain of unrealistic, idealizing assumptions. Granting that any genuine explanation would require this move, the methodological *desideratum* of explaining more with less would accentuate it. Friedman's somehow sarcastic rejection of realistic assumptions, endorsed by virtue of their descriptive accuracy, is clear when he states:

> A completely "realistic" theory of the wheat market would have to include not only the conditions directly underlying the supply and demand for wheat but also the kind of coins or credit instruments used to make exchanges; the personal characteristics of wheat-traders such as the color of each trader's hair and eyes, his antecedents and education, the number of members of his family, their characteristics, antecedents, and education, etc.; the kind of soil on which the wheat was grown, its physical and chemical characteristics, the weather prevailing during the growing season; the personal characteristics of the farmers growing the wheat and of the consumers who will ultimately use it; and so on indefinitely. Any attempt to move very far in achieving this kind of "'realism" is certain to render a theory utterly useless.[12]

A theory like the one described in the above quote would be unmanageable in its detail and hence would lack any focus that could enable us to uncover the (often) hidden causes determining the phenomena under study. Without a careful choice of a few variables applicable to theoretically represent a wide range of phenomena, we are left with no explanatory resources to make causal inferences, and thus ultimately, with no means to make predictions. In Friedman's view, the similarity gap between theoretical variables and empirical phenomena is simply the natural consequence of what theorizing takes, namely, covering a great number of heterogeneous, complex phenomena with a few simple concepts providing a homogeneous

12) Friedman, "The Methodology of Positive Economics", 32.

representation. Theories, according to Friedman, must then be unrealistic, and their acceptability depends entirely on their predictive success, which includes, not only future events, but also past events not known to the person making the prediction.[13] (M. Friedman, 1953/1966, p. 8). Moreover, he has forcefully rejected the idea that even a theory with highly unrealistic postulates can still be made indirectly realistic by conjoining descriptively accurate or realistic auxiliary assumptions with it –a view that he considers as harmful as widely spread in the mid-20th century economics. From his standpoint, not only the same arguments that hold for the unrealism of theoretical postulates hold for the unrealism of auxiliary assumptions, but also the same test for validity (i.e., predictive success) must be simultaneously applied in both cases. To put it differently, auxiliary assumptions should not be empirically tested independently of their conjoined theory, but rather together with it, since their validity is to be evaluated according to the purpose that they are expected to fulfill, namely, to make the conjoined theory predictively successful. The mutual dependence between theory and auxiliary assumptions for them to be empirically tested follows, as a consequence, from Friedman's account. He, on the one hand, (at least implicitly) assumes that a theory holistically depends, for its confirmation, on its auxiliary assumptions, and on the other hand, points out that auxiliary assumptions depend on their conjoined theory for testing their validity as auxiliary devices enabling the confirmation of that very theory. While the first kind of holistic dependence is usually associated with the widely accepted Duhem-Quine thesis, the second line of dependence is peculiar to Friedman's approach, where auxiliary assumptions are presented as heuristic devices meant to increase the analytical relevance of a certain theory, rather than as inherited truths about the domain of application of a theory or the experimental conditions required

13) Ibid., 8.

for its testing. Descriptive accuracy would certainly be a valuable feature for the latter and no relativity to the conjoined theory would emerge in that case, which implies that auxiliary assumptions should be testable independently of the conjoined theory. By contrast, if such assumptions are mere heuristic devices intended to maximize the explanatory and predictive capacity of a conjoined theory, their validation becomes relative to the theory, and descriptive accuracy, for reasons already explained, need not be acknowledged as a valuable feature. To put it bluntly, in order to serve the purposes of their unrealistic conjoined theory, auxiliary assumptions would have to provide new unrealistic resources to cope with extremely complex, heterogeneous domains. Again, we can see how Friedman states his view:

> To put this point less paradoxically, the relevant question to ask about the "assumptions" of a theory is not whether they are descriptively "realistic," for they never are, but whether they are sufficiently good approximations for the purpose in hand. And this question can be answered only by seeing whether the theory works, which means whether it yields sufficiently accurate predictions. The two supposedly independent tests thus reduce to one test.[14]

Note that when Friedman talks of “the two supposedly independent tests” he is referring to the very idea of distinguishing testing a theory from testing its auxiliary assumptions, a distinction motivated by the purpose of making sure that the second are “realistic”. In his view, on the contrary, no such distinction makes sense. Both test are inextricably united by their shared heuristic, idealizational nature.

In Friedman’s approach, assumptions of ideal conditions like “perfect competition” and “perfect monopoly” underlying neoclassical economic theory are to be evaluated with regard to their analytical relevance, i.e., by their contribution to the predictive success of such theory. Predictive success would play a twofold role: as the purpose of auxiliary assumptions and as

14) Ibid., 15.

the criterion to evaluate them, ultimately providing also the criterion for acceptable departures from realism, since assumptions would need to deviate from realism in order to fulfill their purpose. The question of what to neglect in studying economic phenomena could only be answered by checking what choice of neglect proves more helpful in terms of predictive power. According to Friedman, the difference in the contribution to predictive power that an auxiliary assumption (or a set of them) can make constitutes all the available evidence to judge whether the idealized features represented in the auxiliary assumptions make more difference to the phenomenon under study than the neglected features. He thereby implicitly acknowledges that the predictive contribution of auxiliary assumptions plays a key role in guiding causal inference, which is the cornerstone of scientific theorizing. The core of his argument is presented in the following quote:

> What is the criterion by which to judge whether a particular departure from realism is or is not acceptable? Why is it more "unrealistic" in analyzing business behavior to neglect the magnitude of businessmen's costs than the color of their eyes? The obvious answer is because the first makes more difference to business behavior than the second; but there is no way of knowing that this is so simply by observing that businessmen do have costs of different magnitudes and eyes of different color. Clearly it can only be known by comparing the effect on the discrepancy between actual and predicted behavior of taking the one factor or the other into account.[15]

The above quote suggests that auxiliary assumptions prove analytically relevant in so far as they contribute to identify the prevalent causal factors involved in the phenomenon under study, an identification that, in turn, can only be achieved by comparing different (sets of) assumptions with respect to their relative contribution to the predictive power of their conjoined theory. Now, given that the scope of a theory is always restricted in at least two ways, namely, by the specific problems under study and by the circumstances under which it holds, the test of prediction for analytical

15) Ibid., 32-33.

relevance is itself relative to both restrictions. To put it differently, the analytical relevance of "unrealistic" or ideal assumptions is always relative to the problem addressed and the circumstances under consideration. The pursuit of analytical relevance amounts to the pursuit of a correspondence between the ideal and real entities in a particular problem and under particular circumstances, and this implies that the choice of variables used to define such correspondence is strongly restricted by pragmatic and contextual factors. Certainly, without those restrictions, auxiliary assumptions could be established in a "realistic" way and make the same contribution whatever the theory. But, again, without those restrictions, all theorizing would become pointless, either too trivial or unmanageably complex. As emphasized by Friedman, the choice of assumptions only makes sense relative to a problem:

> Everything depends on the problem; there is no inconsistency in regarding the same firm as if it were a perfect competitor for one problem, and a monopolist for another, just as there is none in regarding the same chalk mark as a Euclidean line for one problem, a Euclidean surface for a second, and a Euclidean solid for a third.[16]

Circumstances of application of a theory are equally important. For instance, the evolution of retail prices of cigarettes affected by an increase of the federal cigarette tax during a war period would be very different from their evolution if the tax increase had occurred before that period. War circumstances may make it more convenient to replace the ideal assumption of perfect competitors by the ideal assumption of perfect monopoly, for in such circumstances each firm may prioritize their prestige and keeping their share of the market,[17] thereby adjusting their prices with other firms and making sure that the quantity produced could satisfy the demand.

16) Ibid., 36.
17) Ibid., 36-37.

Friedman's reference to unrealistic assumptions does not seem to fit well with the examples of auxiliary assumptions mentioned earlier, in connection with Duhem's account. Those examples where directly concerned with background knowledge involved in the use of experimental instruments or in the acknowledgment of certain conditions for observation. It seems utterly absurd to vindicate the unrealism of such assumptions, which are empirical in nature. Therefore, even if, as we will see in the following section, the notion of auxiliary assumption includes very different kinds of assumptions, it appears plausible that Friedman is primarily referring to idealization assumptions. This is still a very broad category, but certainly one that does not overlap with empirical assumptions on experimental conditions. So we will later narrow down the discussion to the issue about the justification of ideal assumptions. Friedman himself is certainly not explicit about these distinctions and so, in order to clarify the different roles of assumptions, it will be useful to take into account Musgrave's taxonomy as well as his objection to what he describes as the unnoticed change in the status of auxiliary assumptions in economic theory.[18]

Before turning to Musgrave's criticism of Friedman's view, I would like to highlight a few aspects of the latter's account. First, Friedman is far from holding an antirealist or merely instrumentalist view of science, his vindication of false assumptions instead being related to their essential role in uncovering the truth behind the appearances. Second, according to him, the only way to check whether false auxiliary assumptions are acceptable is by deriving successful predictions from the hypothesis the assumptions are conjoined to. The following sections raise some concerns about the validity of Friedman's criterion for the acceptability of auxiliary assumptions, not about his general idea that false assumptions are necessary to achieve some theoretical truths. As later criticisms by Lawson, Mäki and Cartwright

18) Musgrave, "'Unreal Assumptions' in Economic Theory: The F-Twist Untwisted", 385-6.

will show, the derivational method advocated Friedman is too limited, in different respects. The "test of prediction", as Friedman calls it, does not only overlooks the importance of the bridge principles providing an empirical interpretation for auxiliary assumptions, but also the tradeoff between predictive power and scope of application of a hypothesis, which is also connected with the contrast between ideal conditions in the experimental setting and real conditions in the target domain.

4. Musgrave's criticism: advocating the independent testability of auxiliary assumptions.

In contrast to Friedman, Musgrave vindicates both the empirical significance of auxiliary assumptions and their testability independently of their conjoined theory.[19] According to the second, most economist (including Friedman) would have failed to distinguish between three kinds of auxiliary assumptions:

- negligibility assumptions, i.e., empirically testable assertions regarding the low influence of certain variables on the phenomena under study;

- domain assumptions, i.e., empirically testable assertions expressing restrictions on the domain of application of a theory; and

- heuristic assumptions, i.e., empirically evaluable assertions intended to enable successive approximations to the phenomena under study.

Musgrave claims that in none of the three cases it is true that, the more significant the theory, the more unrealistic the assumptions. Conversely, in all three cases, the role assigned to the assumptions could only be successfully played by them if they prove empirically sound. Yet, each kind of assumption plays a different role, one that is not compatible with the others. Negligible factors do not restrict the domain of application of a theory as do domain assumptions, precisely by pointing to some factors as not

19) Musgrave, "'Unreal Assumptions' in Economic Theory: The F-Twist Untwisted".

negligible. In none of these two cases the assumptions are meant as fictions for purposes of approximation, as happens with heuristic assumptions. But even in this third case, the role of ideal assumptions is to be judged by their contribution to empirical approximation. Contrary to what is argued by Friedman, the lack of "realism" of auxiliary assumptions, closely connected to their lack of independent empirical evaluation, would hamper progress in economics. The problem increases due to the unnoticed change in the role of such assumptions in economic theory. To use Musgrave's own example, "assume that the budget is balanced" may mean:

1. Whether or not the budget is balanced makes no detectable difference to the phenomena under investigation;
2. If the budget is balanced, then the following applies;
3. Let us temporarily assume that the budget is balanced.

Each meaning is not compatible with the others and calls for a different empirical evaluation. Musgrave points out that heuristic assumptions can be understood as negligibility assumptions turned into heuristic devices allowing for successive approximation and, thus, for taking steps towards precise predictions. His view is similar to Ernest Nagel's in this respect, for both understand the heuristic role of idealizations primarily as enabling empirical approximation, and therefore as leading to more descriptively accurate formulations of a theory.[20] Nagel's earlier discussion of Friedman's account moreover suggests that the latter conflates three different senses of 'unreal' applied to assumptions: descriptive inaccuracy due to abstraction, descriptive inaccuracy due to falsity and descriptive inaccuracy due to idealization. As Nagel notes, there is no genuine debate on the relevance of the first sense as denoting an essential feature of scientific assumptions. It is also uncontroversial that falsity is to be avoid unless it successfully serves idealization purposes. The question, then, is again on what basis ideal

20) Nagel, "Assumptions in Economic Theory", 215-17.

assumptions are acknowledged as valid and whether the validity criterion itself involves a move towards realism. Friedman would have failed to realize that the application of his test of prediction for ideal assumptions involves such move after all, because no predictive progress is possible unless the idealized conditions stated in the assumptions are gradually relaxed and different interfering factors omitted in the ideal assumptions are included in subsequent formulations of auxiliary assumptions conjoined with a theory, whether they are assumptions on friction conjoined with Galileo's law or assumptions on bounded rationality conjoined with the rational maximization of returns hypothesis.[21]

The dynamical nature of the status of auxiliary assumptions is another important aspect stressed by Musgrave, who draws attention to the fact that the development of inquiry often requires moving from one kind of assumption to another. For instance, to explain the mechanical features of the Solar System, Newton initially neglected the inter-planetary gravitational forces. In particular, his initial formulation of Kepler's planetary hypothesis includes the negligibility assumption that the actions of the planets one upon another are so small that they can be neglected. Later on, once astronomical observations became more refined, Newton's negligibility assumptions turned into heuristic ones regarding inter-planetary gravitational forces and, ultimately, those assumptions systematically developed in his theory of perturbations.

Friedman and Musgrave clearly agree that ideal assumptions play an important role in scientific theorizing, but both disagree on how to understand their empirical significance. The method of successive approximation that Musgrave regards characteristic of heuristic assumptions involves a constant evolution towards "realism" (or descriptive accuracy) that clashes with the picture emerging from Friedman's account, where the lack of realism

21) Ibid., 217-18.

is a feature preserved by assumptions repeatedly subject to the test of prediction. In fact, even if both authors invoke prediction as a key evaluative means for assumptions, Friedman, as opposed to Musgrave, acknowledges no progression towards descriptive accuracy as the result of systematically applying the predictive test. Next I will explore some limitations and challenges affecting both approaches.

5. Challenges to Friedman's and Musgrave's views

Let us examine some relevant contributions to the debate after Musgrave's paper, many of them pointing to difficulties shared by Friedman's and Musgrave's views. In particular, Tony Lawson and has questioned the gold standard of derivational methods, whether applied to auxiliary assumptions directly (as suggested by Musgrave) or indirectly (as advocated by Friedman). On the other hand, the main issue raised by Uskali Mäki's account of idealizations as theoretical isolations conflicts with Friedman's and Musgrave's views in different ways, for it entails a vindication of idealizations even when no predictive test is applicable. Finally, despite the fact that Cartwright has to some extent endorsed Friedman's view, especially when arguing that in order to be explanatory and predictively fruitful, theories must lie, she has raised some important objections to the use of ideal assumptions in economics, a use that would systematically preclude external validity.

5.1 Lawson's objection to the limitations of the derivational approach to empirical significance

Let us focus on the heuristic use of auxiliary assumptions and suppose that economic theories do pass the predictive test, thus enabling us to derive empirical consequences. It could then be argued, *à la* Friedman, that the empirical significance of auxiliary assumptions and of idealizations in general can only be assessed by evaluating the overall explanatory/predictive power

of the theory including such idealizations. This derivational view of empirical significance, often associated with the idea that simplifying and fictionalizing are the cornerstones of scientific explanation, has been vigorously criticized by Lawson, who provides the following illustrative example:

> It may be true that 'all polar bears are white'. But if this apparent truth is deductively generated from the assumptions that 'all polar bears eat snow' and 'all snow-eaters are white', we have added nothing to our understanding of polar bears, snow or whiteness; and nor have we provided explanatory support for the proposition that 'all polar bears are white'. All deductive exercises that are so based on known absurd fictions, and this inevitably includes almost all mathematical modelling exercises in modern economics, are just as pointless.[22]

The use of ideal assumptions in economics would be hampered by the peculiar use of mathematics in economics, which, according to Lawson, is too influenced by Hilbert's reconsideration of math as concerned with "providing a pool of frameworks for possible realities", rather than being regarded as the language of nature. As shown in the example above, absurd fictions may play a role in inferring true empirical consequences from a theory, thereby fulfilling Friedman's requirement for empirical significance. Yet, their empirical contribution would have more to do with the triviality of the empirical features they are associated with than with their correspondence with relevant hidden features of the real events under study. Ultimately, Lawson's overall criticism of traditional economics is related to the mismatch between the method of isolation, atomization and mathematical modelling, on the one hand, and conditions of application (open systems marked by internal-relations, process, emergent totalities, meaning, value) on the other.[23] He calls for dialectical methods such as contrast explanation, more sensitive to the ontological complexities of the social domain and conducive to an evaluation of assumptions based on their

22) Lawson, "Central Fallacies of Modern Economics", 62.
23) Ibid., 62-63.

contribution to understanding real events rather than to predicting some trivial facts. In contrast explanations, the goal is to explain unexpected differences in outcomes, i.e., to explain why, in outcomes assumed to share the same causal history − and thus to be the same− we find a surprising difference. This kind of explanations should provide an answer to questions of the form "why x rather than y?" like, for example, why unemployment is falling everywhere in a region except in one area?[24] A key advantage of contrast explanations would be that they can be equated to experiments occurring outside the laboratory, as they enable us to standardize for all causal factors except one over a particular domain, hence allowing for causal explanation without artificial simplification.[25]

5.2 Mäki's vindication of idealizations as theoretical isolations

A central idea underlying Mäki's approach is that the highly complex and intertwined nature of social interactions requires their theoretical decomposition by means of idealizing assumptions, whose purpose consists in isolating causal variables, often by making false simplifying assumptions.[26] Only by endorsing literally false assumptions regarding some complex domains would we be able to gain access to (isolate) some simple hidden truths about the causal connections operating in them. The explanatory requirement of theoretical isolation would then justify the methodological use of non-transient (*pace* Musgrave) and non-predictive (*pace* Friedman) idealizations. However, as suggested by Mäki, false assumptions are often kept even though they do not contribute to the isolation of any real causal variable, thereby losing or betraying their purpose. This inadequate use of idealizing assumptions results in a lack of

24) Lawson, "Applied Economics, Contrast Explanation and Asymmetric Explanation", 408.
25) Ibid., 409.
26) Mäki, "Realistic Realism about Unrealistic Models", 78.

connection between them and real systems —or, in in Mäki's terms, in using 'substitute models' as if they were 'surrogate models'—, and even in imposing isolations precluded in real systems.[27] For example, excluding the role of institutions when representing economic systems could dramatically limit the explanatory capacity of the corresponding representation. By imposing isolations precluded in real systems, theoretical models in economics may end up devoid of empirical and explanatory significance.

While recognizing the three different roles that, according to Musgrave, auxiliary assumptions may play,[28] Mäki vindicates the methodological role of false assumptions in the form of idealizations, and not merely as a transient heuristic device to be discarded in the future. In order to identify and represent causal connections, we would need to find a way to isolate those causal links from (usually) a highly complex, open and uncertain range of interfering variables. Even if no predictive power is gained by employing idealizations, the explanatory power would require idealizations. In Mäki's view, idealizations are justified, neither on the basis of predictive effectiveness (contrary to Friedman), nor as gradual approximations (contrary to Musgrave), but as devices to uncover some hidden truths about domains whose complexity precludes the chances of generating predictions. Even if ideal assumptions are often not intended as empirical approximations,[29] they manage to uncover real, identifiable tendencies or causal connections existing beneath the surface of interference factors.

Let us get a clearer view of how idealizations should work according to Mäki by considering one of his own examples.[30] In vindicating the Galilean kind of idealizations in economics, he compares Galileo's idealizations supplementing mechanical laws to idealizations employed in von Thünen's

27) Ibid., 85.
28) Mäki & Piimies, "Ceteris paribus", Mäki, "Kinds of Assumptions and Their Truth: Shaking an Untwisted F-Twist".
29) Mäki, "Ceteris Paribus: Interpretaciones e Implicaciones", 21.
30) Mäki, "Realistic Realism about Unrealistic Models", 78-80.

model of agricultural land use in the Isolated State. This model successfully isolates distance (or the associated transportation cost) as the major causal factor that shapes land use patterns in agriculture, leaving aside a wide variety of heterogeneous interfering factors like the proximity of other cities, the dimension of the city, geographical accidents like mountains or rivers, and assuming uniform fertility and climate, no trade, and so on. The derivational or predictive approach to the evaluation of ideal assumptions would not be applicable in cases like the above. The inapplicability of such approach is here not related to the Duhem-Quine problem –for even a complete set of theories and hypothesis would be affected by exceptions and provisos in their application−, but to the very nature and role of ideal assumptions, essentially consisting in the isolation of variables in the discovery context.[31]

Now, what happens when neither successful predictions, nor successful explanations are obtained despite the massive use of idealizations? What is the justification supporting the use of idealizations in those cases and, therefore, on what grounds can they be kept as valid research devices? Mäki warns against the risk that mere tractability (or heuristic) assumptions overrule meaningful idealizations, giving rise to ontologically ungrounded idealizations. The risk of arriving at ungrounded idealizations is stressed in the following quote:

> Just as biologists will fail in representing a system such as the human organism if they consistently exclude the brain or the heart from their theory, economists might fail in representing an economic system for certain explanatory purposes —such as for explaining the performance of a developing economy— if the isolations they employ exclude the role of institutions.[32]

31) Mäki, “Ceteris Paribus: Interpretaciones e Implicaciones”, 25-26.
32) Mäki, “Realistic Realism about Unrealistic Models”, 85.

In order to revert the tendency towards ontologically vacuous idealizations, Mäki suggests some replacement of assumptions in economic theory. Some of the replacements that he advocates entail moving from assuming symmetric information to assuming asymmetric information, from zero to positive transaction costs, from certainty to uncertainty in decision making, from unbounded to bounded rationality, from maximization to satisficing, from asocial and amoral agents to ones with social and moral awareness; and so on.[33]

5.3 Cartwright's tradeoff between internal and external validity

Despite being sympathetic with Friedman's vindication of ideal assumptions,[34] Cartwright thinks that an inadequate use of idealizations is often made in economics, where false assumptions are kept even if they do not play the important methodological role that Galilean assumptions would play in physics.[35] In particular, they often do not enable interesting experimentation – 'interesting' in the sense that allows for successful causal inferences. On the contrary, unreal assumptions would become a mere device for purposes of deriving consequences from a theory, whether or not such consequences can be tested in a way that guarantees the generalizability of the results to the target domain. Contrary to Galilean assumptions, these inadequate assumptions would overconstrain the applicability of the theory and, thus, the experimental conditions needed for its testing.

She mentions several examples of the overconstrained nature of economic models –which would actually compensate for their meager number of general theoretical principles−, among them, Lucas's models from

33) Ibid., Mäki, "Ceteris Paribus: Interpretaciones e Implicaciones".
34) Cartwright, "The Vanity of Rigour in Economics: Theoretical Models and Galilean Experiments", 217.
35) Ibid., 226.

his 1973 “Expectations and the Neutrality of Money” and the skill-loss model of Pissarides, that would contain around sixteen assumptions.[36] Like in other cases, the problem would be that theories lack or hardly have bridge principles, that is, principles that provide links between the theoretical concepts and the empirical concepts. In the ideal gas theory, for example, we would find the bridge principle identifying the theoretical concept of mean kinetic energy of the molecules with the empirical concept of temperature. This sort of principle establishes a correspondence between theoretical constructs and empirical phenomena providing some grounds for justifying our belief in the correspondence between theoretical constructs and real entities. What we find in economics, when bridge principles are missing, is a proliferation of auxiliary assumptions meant to fill the gap between general theoretical postulates and their concrete applications. This proliferation is far from serving purposes of theoretical isolation or empirical approximation, both extremely useful in making good experimentation possible−again, ‘good’ in the sense that favors both internal and external validity of the experiment. The overconstrained nature of economic models, on the contrary, only makes it possible, at best, to maximize the internal validity of experiment, that is, the evidence that the covariation between the presumed independent and dependent variables results from a causal relationship. External validity would be systematically precluded by the very overconstraining nature of assumptions. As a consequence, the predictive power of a theory may be at odds with its scope of application and, therefore, with the external validity of experiments testing the theory conjoined with the assumptions−provided that external validity requires the generalizability from results obtained in a research setting to phenomena out of such setting. According to Cartwright, the problem of the overconstraining nature of

36) Ibid., 227-8.

assumptions affect also experimentation through ramdomized control trials, whose deductive nature in combination with the inclusion of overconstraining assumptions inevitable results in narrowness of scope.[37]

6. Facing the methodological challenges

After recalling the main contributions to the debate on ideal assumptions, a question remains as to how the empirical significance of heuristic or ideal assumptions can be evaluated if not merely by derivational methods. Addressing this question implies going back to the issue of how it is possible to attain independent empirical support for idealizations and what alternative assumptions should be considered as empirically more significant than the prevalent ones. As pointed out earlier, Lawson and Mäki have suggested some dialectical methods to deal with the second issue, let us now address the first issue by considering some other relevant contributions.

The vast literature on idealization from *Poznań School of Methodology*, and, more in particular, Igor Hanzel bi-directional method of empirical approximation, provide some interesting clues. Leszek Nowak's (1943-2009) foundational ideas on the idealizational nature of scientific models, which have been further developed by the *Poznań School of Methodology*, emphasize the contrast between generalization or abstraction in the Aristotelian sense and idealization, the latter entailing a deletion and/or deformation of properties conducive to the creation of ideal (not real) objects. Following Nowak's ideas, Giacomo Borbone and Krzysztof Brzechczyn take the combination of systematic idealization and concretization to be the main mechanism underlying mature science, scientific modelling or the very possibility of bridging the gap between essence and appearance.[38] The

37) Cartwright, "Are RCTs the Gold Standard?"
38) Borbone & Brzechczyn, "The Role of Models in Science: An Introduction", in *Idealization XIV: Models in Science, Poznań Studies in the Philosophy of the Sciences and the Humanities*, Volume 108, ed. Giacomo Borbone & Krzysztof Brzechczyn (Boston: Brill/Rodopi, 2016), 1-10, 2.

dynamics of mature science would involve three stages: the introduction of ideal assumptions, the formulation of ideal laws and the gradual concretization of the laws to the point where completely factual laws, free from ideal assumption, are obtained.[39]

Interestingly, the idealization-concretization mechanism involves more than mere derivational evaluation of idealizations, since concretization, or the possibility of de-idealize assumptions, is a precondition for prediction and can operate in different directions. In a 2016 paper by Igor Hanzel,[40] the author questions the usual reading of Newton's second law and draws attention to the bi-directionallity of the method favored by Newton. According to Hanzel, mass and acceleration are not the main factors (grounds) determining the phenomenal effect to be equated with the force. Rather, force would be the main factor causing the phenomenal effect of acceleration in bodies with certain mass. In Newton's bi-directional method, he goes from the effect of forces to forces and from forces to their effects. Before the formulation of laws makes it possible to go from force (as cause) to some of its effects (change of movement along time), some definitions are established so that force can be determined on the basis of some of its attributable effects (change of state of a body, proportionality between the magnitude of the generated force and that of generated motion).

Hanzel emphasizes the relevance of the distinction between two kinds of phenomenal effects (or conditions of modification of the ground): a) forms of appearance of the ground (main explanatory cause), which would be made explicit in definitions; and b) forms of manifestation of the ground, which would be made explicit in laws.[41] According to him, the empirical approximation in terms of "forward" concretization (thus in the direction from laws to applications, or from causes to conditions where concrete effects can

39) Ibid., 4.
40) Hanzel, "The Inherent Type of Scientific Law, The Idealized Types of Scientific Law".
41) Ibid., 49, 56.

be identified as manifestations of the causes) should be supplemented by an empirical approximation in terms of "backward" concretization (in the direction from appearances of the causes to the definitions of the causes). Note that the appearances of the causes can be, either effects other than the manifestations of the causes, or (observable) causes of such causes. Given that 'ground' is here understood as the main factor that plays an especially relevant explanatory role, Hanzel argument implies that grounds should be empirically supported by both kinds of effects, which, if determinable in quantitative terms would provide an immanent (law dependent) and external (law independent) measure respectively. As Hanzel points out, contrary to Newton's second law, Marx's law of value is not only explicit about the forms of manifestation of the ground but also about the forms of appearance of the ground, which in this case are not certain effects but the phenomenal causes of the ground, in particular, the amount of time involved in producing a good would be the cause of its value. Value, in turn, would interact with the value of other products thereby causing the phenomenal effect of price.[42]

Hanzel bi-directional method of empirical approximation implicitly amounts to recognizing the importance not only of predictions (derivations from laws or "forward" concretization) but also of prior evidences (basis for definitions or "backward" concretization). The first kind of approximation essentially involves deductive inference leading to predictions, the second sort of approximation, by contrast, operates through abduction, resulting in definitions intended to best explain some salient empirical features of the domain under study. The need to evaluate theoretical concepts −as something different and more basic than the evaluation of theoretical laws− and the related resort to abductive inference have been only very recently acknowledged in economic methodology. These, however, are important

42) Ibid., 51.

aspects of the non-derivational, not merely deductive view of the empirical evaluation of ideal assumptions. As recently argued by James J. Heckman and Burton Singer, the rigid separation of the processes of model generation and model testing, despite its analytical convenience, is artificial and misleading in different ways.[43] In invoking abduction in economics, they reach a conclusion in agreement with the view suggested here, namely, the insufficiency of the predictive test *á la* Friedman to evaluate assumptions:

> This approach addresses the problem of using the same data to formulate and test hypotheses. Analysts are advised to test provisional models on fresh data, possibly of a different character than the data used to formulate initial hypotheses, and to draw new testable implications from hypotheses that survive an initial stage of scrutiny.[44]

If we apply these ideas to our subject, it becomes clear that the very generation of ideal assumption needs, indeed, to be justified, and obviously such justification cannot be obtained through predictions inferred from already generated, accepted assumptions. Yet, there is no reference to generation requirements in Friedman's discussion and no elaboration on the problem of choosing or accepting certain concepts instead of others. On the other hand, Mäki's advocated method for deciding about the replacement of assumptions does include a combined process of de-idealization and reisolation very in tune with the above mentioned generative purposes and the iterative bidirectional method of concretization-idealization put forward by Hanzel.[45]

7. Concluding remarks

The different approaches to auxiliary assumptions discussed in the previous sections have shed light on the different roles of idealization. The

43) James J. Heckman & Burton Singer, "Abducting Economics", *American Economic Review: Papers & Proceedings*, 107/5 (2017): 298–302.
44) Ibid., 301.
45) Mäki, "Realistic Realism about Unrealistic Models".

main roles of empirical approximation and theoretical isolation are neither always simultaneously attainable, nor always evaluable by the same means. Friedman's test of prediction for auxiliary assumptions, as well as the rejection of both descriptive accuracy and independent testability associated with it, face serious limitations and leaves the expected correspondence between assumptions and reality unexplained.

There have been important contributions to overcome the failure to distinguish between different kinds of auxiliary assumptions and, also, the limits of the predictive or derivational account of empirical significance. Dialectical methods and bi-directional empirical approximation represent two promising venues to explore in the future.

References

Borbone, Giacomo & Brzechczyn, Krzysztof, "The Role of Models in Science: An Introduction", in *Idealization XIV: Models in Science, Poznań Studies in the Philosophy of the Sciences and the Humanities*, Volume 108, ed. Giacomo Borbone & Krzysztof Brzechczyn (Boston: Brill/Rodopi, 2016), 1-10.

Cartwright, Nancy, "Are RCTs the Gold Standard?" *BioSocieties* (Special Issue: The Construction and Governance of Randomised Controlled Trials) 2/1, March (2007a): 11-20.

Cartwright, Nancy, "The Vanity of Rigour in Economics: Theoretical Models and Galilean Experiments", in *Hunting Causes and Using Them: Approaches in Philosophy and Economics*, by: Nancy Cartwright (Cambridge, New York: Cambridge University Press, 2007b), 217-261.

Duhem, Pierre, *The Aim and Structure of Physical Theory* (Princeton (NJ): Princeton University Press, 1906/1991).

Friedman, Milton, "The Methodology of Positive Economics", in *Essays in Positive Economics,* by Milton Friedman (Chicago: University of Chicago Press, 1953/1966), 3-16, 30-43.

Hanzel, Igor, “The Inherent Type of Scientific Law, The Idealized Types of Scientific Law”, in *Idealization XIV: Models in Science, Poznań Studies in the Philosophy of the Sciences and the Humanities*, Volume 108, ed. Giacomo Borbone & Krzysztof Brzechczyn (Boston: Brill/Rodopi, 2016), 43-62.

Heckman, James J. & Singer, Burton, “Abducting Economics”, *American Economic Review: Papers & Proceedings*, 107/5 (2017): 298–302.

Lakatos, Imre, *The Methodology of Scientific Research Programmes* (Cambridge: Cambridge University Press, 1978).

Lawson, Tony, “Applied Economics, Contrast Explanation and Asymmetric Explanation”, *Cambridge Journal of Economics*, 33/4 (2009): 405–19.

Lawson, Tony, “Central Fallacies of Modern Economics”, in *Economic Objects and the Objects of Economics. Virtues and Economics*, vol. 3, ed. Peter Róna & László Zsolnai (Cham: Springer, 2018), 51-68.

Mäki, Uskali & Piimies, Jukka-Pekka, “Ceteris paribus”, in *The Handbook of Economic Methodology*, ed. Davis, John B., Hands, D. Wades & Mäki, Uskali (Edward Elgar, Cheltenham, 1998), 55-59.

Mäki, Uskali, “Kinds of Assumptions and Their Truth: Shaking an Untwisted F-Twist”, *Kyklos*, 53/3 (2000): 303-322.

Mäki, Uskali, “Ceteris Paribus: Interpretaciones e Implicaciones”, *Revista Asturiana de Economía,* 28 (2003): 7-32.

Mäki, Uskali, “Realistic Realism about Unrealistic Models”, in *The Oxford Handbook of Philosophy of Economics*, ed. Harold Kincaid & Don Ross (Oxford: Oxford University Press, 2009), 68-98.

Musgrave, Alan, "'Unreal Assumptions' in Economic Theory: The F-Twist Untwisted", *Kyklos,* 34/3 (1981): 377-87.

Nagel, Ernest, "Assumptions in Economic Theory", *The American Economic Review*, 53/2, May (1963): 211-219.

Popper, Karl, *The Logic of Scientific Discovery* (London: Routledge, 1935/2002).

Popper, Karl, *Conjectures and Refutations: The Growth of Scientific Knowledge* (London: Routledge, 1963).

Quine, Willard Van Orman, "Two Dogmas of Empiricism", in *From a Logical Point of View*, by Willard Van Orman Quine (Cambridge, Massachusetts: Harvard University Press, 1951/1953), 20-46.

La autora es Profesora Titular de Filosofía de la Ciencia en la Universidad de Valladolid (España). Sus principales áreas de interés son la filosofía general de la ciencia, la metodología de la ciencia, la filosofía del lenguaje y la epistemología. Su investigación se ha centrado en cuestiones limítrofes entre dichos campos, como la inconmensurabilidad, la validez experimental, la evaluación de teorías y las relaciones interteóricas.

Recibido: 15 de diciembre de 2019.

Aprobado para su publicación: 10 de enero de 2020.