

# METHODOLOGICAL ASSUMPTIONS IN ECONOMICS: THE ARGUMENT AGAINST INSTRUMENTALISM

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**Abstract:** *This paper considers critically Milton Friedman's instrumental view of methodological assumptions in economic theory and argues that these assumptions need to be assessed, along with the rest of the theoretical apparatus, in terms of their theoretical fit to a particular context of application, predictive success and descriptive relevance. It also highlights the inconsistency of the instrumentalist position and claims that the attempt to formulate scientific methodology in purely instrumental terms will sooner or later reveal an underlying commitment to realism.*

**Keywords:** *economic methodology, assumptions, realism, instrumentalism, neoclassical economics.*

Is mainstream economics still dominated by the neoclassical tradition? Did John Hicks and Paul Samuelson's attempted synthesis of neoclassical theory and Keynesianism<sup>1</sup> largely retain the main assumptions of the former, with its focus on instrumental (means-to-ends) rationality, calculability and equilibrium conditions? Or is it possible to speak of a gradual departure from neoclassical orthodoxy?

The last decades have witnessed the rising influence of alternative approaches as behavioral economics, neo-institutionalism or ecological economics. It can be argued that some elements of these approaches have been, to a certain extent, integrated into the new mainstream economics: "Evolutionary and institutional themes have been primarily associated with heterodox economics during the period of neoclassical dominance, but there is good reason to think they may be emerging as central themes in recent mainstream economics. For example, one important dimension of behavioral economics is that it reverses a century-long history in neoclassical economics in which the psychological characteristics of individuals were increasingly de-emphasized."<sup>2</sup> At the same time, neoclassical theory itself has evolved and integrated bounded rationality and results from behavioral economics. One may wonder if (and to what degree) this evolution has extended to the level of methodological assumptions.

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<sup>1</sup> See Samuelson, Paul A., 1955, *Economics*, 3rd ed., McGraw-Hill and Hicks, J.R. (1937). "Mr. Keynes and the 'Classics': A Suggested Interpretation," *Econometrica*, 5(2)

<sup>2</sup> Davis, John B., *The turn in economics: neoclassical dominance to mainstream pluralism?*, *Journal of Institutional Economics*, 2: 1, 1-20, 2006, p. 3.

Methodological debates in economics have often focused on ways of *operationalizing* one theory or another, for instance by specifying unambiguous assessment criteria and setting clear thresholds. Questioning the methodological basis of the dominant approach was episodic and the dialogue between rival schools seems to have remained limited to refining each other's positions or building elaborate defenses rather than attempting a major revision or an integrative effort. A famous example is the "Cambridge capital controversy" in the '60s, which focused on the role of capital and capital aggregation problems. However, for mainstream economics the implicit priority was not so much to justify its theoretical assumptions (it was somehow taken for granted that this had already been done successfully, roughly between 1850 and 1950), as to connect the theoretical apparatus to its context of application. It was generally taken for granted that the apparatus itself is adequate for the description and explanation of a certain domain of facts. Often, it also assumed that it can be extended beyond its initial domain of application, under certain conditions. However, these assumptions are by no means unproblematic. In particular, the so-called "homogeneity assumption" of neoclassical economics has received much criticism. Simply put, homogeneity assumes that the abstractions or simplifications made in a certain context are in principle translatable to other similar contexts, that they retain their relevance across domains, through time and space, due to ontological and epistemological *universalism*. "For example, the behavior of consumers is, at least initially, discussed in abstraction from their gender, age and culture; the relationship between trade and growth is discussed in abstraction from issues such as historical context, culture and so forth. Of course, these issues can be and are brought into the analysis, but normally as a second step, and there is a strong tendency within economic theory to use models that are highly 'universal'."<sup>3</sup> The 'homogeneity assumption', however, is itself grounded in a particular understanding of how theoretical assumptions work and interact with the rest of the theoretical apparatus – namely, their *instrumental* role. Here we are focusing on this particular understanding and on its undesirable and (largely unintended) consequences.

By 'assumptions' we understand here the underlying beliefs, commitments and values that guide and constrain the methodology of a particular theory. Although not directly applicable to 'facts', assumptions are influential in the overall setup of a theory, since they delimit the 'conceptual space' in which a particular methodology can develop. We can distinguish between methodological assumptions, 'core assumptions' and epistemic values.

The *methodological assumptions* represent the condensed and general formulation of the theory's methodological rules. They represent the first level of abstraction and the 'interface' between the higher-level assumptions and the operational level. As they usually identify significant features of the theory's main 'unit of analysis' – in this case, the individual economic agent – they are essentially behavioral assumptions. At a deeper level, the *core assumptions* set up

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<sup>3</sup> Pforde, Adam, Persuasion: Reflections on economics, data, and the 'homogeneity assumption', *Journal of Economic Methodology* 12:1, 63–91 March 2005, p. 64.

the conceptual framework underlying theoretical construction. It is at this level that Thomas Kuhn's concept of scientific paradigm comes into the picture: paradigms are differentiated not only by distinct methodologies, but also by meta-theoretical presuppositions concerning the way their contexts of application are structured and delimited into analyzable units. Finally, *epistemic values* such as impartiality, epistemic justification, critical inquiry or the intrinsic value of knowledge are normative commitments guiding the scientific enquiry at the most general level.

Different theories will most likely be based on (partially) different assumptions, but how far does this divergence go? While alternative theoretical discourses may give an impression of radical divergence, at the level of assumptions the situation may look significantly different. In the case of neoclassical economics, one could identify at methodological level assumptions derived from a concept of 'economic rationality' (focused on self-interest, constrained maximization, stable preference functions, perfect information, unlimited processing capacity) and from the 'marginalist logic' developed by economists like Leon Walras or Carl Menger. Although more recent developments largely integrate models of bounded rationality, it can be argued that the relaxation of core assumptions is limited and – in this respect – there is a theoretical fracture between the methodological adjustments and the underlying presuppositions. For instance, Herbert Simon, who has played a major role in integrating bounded rationality in mainstream economics, is not interested in challenging key assumptions such as self-interested and maximizing behavior. For him, bounded rationality implies the maximization of individual utility functions under specific constraints (information, time, budget etc.) in pursuit of self-interest, as reflected in the theory of subjective expected utility. He therefore 'relaxes' those assumptions which deal with the cognitive capacities of the real decision-maker, as well as with the scarcity and cost of information, but the core edifice of background assumptions remains largely untouched. These background assumptions would include, for instance, a firm delimitation between facts and values and a positivist requirement of normative neutrality, as well as linearity, calculability, equilibrium and a largely tacit assumption of homogeneity in the application of the theory across domains.

Identifying what those core assumptions are at the level of each particular theory remains, of course, a matter of debate. However, when one considers the set of theoretical approaches that usually go under the name of "neoclassical", there is a surprising convergence on the essential background assumptions (if not on the methodological assumptions as well). This convergence is even more significant given the fact that it comes from both proponents and critics of the neoclassical paradigm. For instance, E. Roy Weintraub<sup>4</sup> proposes the following three assumptions as forming the foundation of neoclassical economics: (1) People have rational preferences among outcomes that can be identified and associated with a value, (2) individuals maximize utility and firms maximize

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<sup>4</sup> E. Roy Weintraub, 2007, Neoclassical Economics. The Concise Encyclopedia of Economics. Retrieved September 26, 2010, from <http://www.econlib.org/library/Enc1/NeoclassicalEconomics.html>.

profits and (3) people act independently on the basis of full and relevant information. These are all behavioral assumptions supporting a certain methodological approach. In this particular case the background assumptions are not difficult to identify: one meta-theoretical claim is that the *individual* is the relevant unit of analysis at all levels, another one is that utility-maximizing rationality implies choosing the best means to achieve given goals (or to pursue given preferences).

Focusing specifically on the core assumptions that are shared by different neoclassical approaches, Arnsperger & Varoufakis<sup>5</sup> also identify three “axioms”: methodological individualism, methodological instrumentalism and methodological equilibration. The axiomatic nature of these assumptions implies that the focus is not on how the agents actually behave, but on what could be inferred about their behavior starting from a set of (presumably) relevant assumptions: “Note that methodological equilibration is equivalent to avoiding (axiomatically) what ought to be the behaviorist’s central question: Will rational agents behave according to the theory’s equilibrium prediction? Instead, the question becomes: If rational agents are behaving according to the theory’s equilibrium prediction, will they have cause to stop doing so?” (p. 5). This is perfectly consistent with an instrumentalist view of theoretical assumptions, as defended by Friedman, according to which they are ‘useful fictions’ intended to generate successful predictions, not to represent reality.

It is worth noticing that epistemic values and methodological assumptions are intertwined and interdependent. A theory which values linearity, formalization and equilibrium will obviously be quite different in its methodological setup from one focusing on complexity and irreducible uncertainty. Epistemic values and meta-theoretical assumptions will continue naturally into methodological assumptions regarding the way markets work or the way individuals acquire and process information. For instance, assumptions of individual maximization and macro-equilibrium will be consistent with a perspective of ‘self-healing markets’, which provide a strong incentive to participants to correct their cognitive and behavioral imperfections. TEV (Total Economic Value) approaches will implicitly assume that economic / monetary valuation is relevant across domains and that it is possible to quantify different types of valuation.

However, this mutual dependency does not necessarily imply *consistency* of assumptions. Theories are not purely deductive systems; rather, they represent an evolving body of knowledge which can include divergent (or even contradictory) claims. This suggests an interesting area of inquiry, not very well researched: notwithstanding the perception of theoretical homogeneity, the divergence between what a theory *says* and what it *assumes* may be significant. These divergences can go a long way in telling us why a theory has low explanatory power or is normatively irrelevant. The problem is compounded by the relative reluctance of neoclassical theory to question or adjust its core methodological

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<sup>5</sup> Arnsperger, Christian & Varoufakis, Yanis, What is neoclassical economics? The three axioms responsible for its theoretical oeuvre, practical irrelevance and, thus, discursive power. Post-autistic economics review, Issue no. 38, 1 July 2006.

assumptions. Arguably, this reluctance can be partly traced back to a certain understanding of the role of assumptions in the overall architecture of a theory, which emphasizes their *instrumental* function at the expense of their *descriptive* function. If assumptions are acceptable as long as they provide a useful means to generate successful predictions, and if they are not meant to represent reality<sup>6</sup>, then it may appear justified to consider them in isolation from the rest of the methodology (which is amenable to empirical check) and treat them as given. However, a problem arises when this methodological move (taking the assumptions for granted as long as the theory works) is transferred at epistemological level. The adoption of a version of ‘ontological and epistemological universalism’ that is disconnected from empirical justification generally leads to downplaying ‘differences that other disciplines treat as central: cultural meanings, socially constructed institutions, gender, historical change’<sup>7</sup>. Ostrom<sup>8</sup> also argued against the tendency to impose ‘panaceas’ in explaining complex and multi-layered systems (particularly socio-ecological systems) or in proposing policy measures to deal with complexity.

These issues of context demarcation reflect deeper problems at methodological and epistemological level, namely an insufficient ability to shift between local and broader explanatory frameworks and to deal with contingent variables. This is particularly risky early on in the analysis, when the adoption of overly generalized assumptions may help in downplaying the diversity of phenomena under study. The under-consideration of contextual differences is apparent in rational choice theory, with its universalistic behavioral assumptions concerning the agent’s capacity to acquire and process information, form preferences, and use information and preferences to maximize utility.

Criticisms regarding the perceived descriptive deficit of assumptions in neoclassical economics are often countered by stating that the role of theoretical assumptions is not to offer a realistic description of facts or an approximation of truth. M. Friedman famously claimed that assumptions are simply the instruments we use to develop testable hypotheses and successful predictions: “Truly important and significant hypotheses will be found to have “assumptions” that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions (in this sense). The reason is simple. A hypothesis is important if it “explains” much by little, that is, if it abstracts the common and crucial elements from the mass of complex and detailed circumstances surrounding the phenomena to be explained and permits valid predictions on the basis of them alone.”<sup>9</sup>

Friedman’s assertion amounts to the fact that descriptive inaccuracy is necessarily coupled with explanatory power, since explanation implies abstracting

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<sup>6</sup> See Friedman, Milton, *Essays in Positive Economics*, Chicago: Chicago University Press, 1953.

<sup>7</sup> Pforde (2005), *op. cit.*

<sup>8</sup> Ostrom, Elinor, A diagnostic approach for going beyond panaceas, Workshop in Political Theory and Policy Analysis, Indiana University, US, 2007  
[http://www.indiana.edu/~workshop/colloquia/materials/papers/ostrom\\_paper.pdf](http://www.indiana.edu/~workshop/colloquia/materials/papers/ostrom_paper.pdf).

<sup>9</sup> Friedman, 1953, *op. cit.*, pp. 14-15.

from the complexity of reality and identifying the ‘essential’ variables enabling prediction. On his part, Karl Popper also famously described science as ‘the art of systematic over-simplification — the art of discerning what we may with advantage omit’.<sup>10</sup> But he also warned against the temptation of taking certain assumptions for granted, as being self-justified. How will one proceed about discerning the essential from the secondary? Our claim here is that assumptions need to be subjected, along with the rest of the theoretical apparatus, to some sort of reality check and that the instrumentalist position is incoherent: the attempt to formulate scientific methodology in purely instrumental terms will sooner or later reveal an underlying commitment to realism. ‘As-if’ assumptions turn out to be meaningless, along with the associated requirement of predictive success, if at a certain level they do not make reference to the *realism* of theoretical entities used in the description.

It is somehow ironic that, although Friedman’s bold claim (baptized the “F-twist” by Paul Samuelson) has been met with strong criticism, it has nevertheless managed to make its way into the methodological foundations of much of mainstream economics. While Ernest Nagel<sup>11</sup> set out to show that his conclusion is sound despite the inconclusiveness of his arguments, others chose to attack the very idea that truth and usefulness can be rigidly separated. Thus the debate was usually framed in terms of “realism” versus “instrumentalism”, although this is slightly misleading, given the multiple distinct understandings of these terms in philosophy and economic theory. Herbert Simon is more radical in his assessment: “The expressed purpose of Friedman’s principle of unreality (or as-if hypothesis) is to save Classical theory in the face of the patent invalidity of the assumption that people have the cognitive capacity to find a maximum”.<sup>12</sup> For Simon, the descriptive deficit of assumptions can be temporarily accepted as an imperfect and perfectible consequence of cognitive and epistemic limits – a methodological ‘necessary evil’. He reformulates it as the “principle of continuity of approximation”, whereby “if the conditions of the real world approximate sufficiently well the assumptions of the ideal type, the derivations from these assumptions will be approximately correct.”<sup>13</sup>

In one of the most cited criticisms to the F-twist, Alan Musgrave<sup>14</sup> differentiates between *negligibility*, *domain* and *heuristic* assumptions and claims that Friedman’s assertion is only partially true for the first category of assumptions, not for the other two. Negligibility assumptions generally state ‘the hypothesis that some factor F which might be expected to affect (the) phenomenon (under investigation, say P) actually has no effect upon it, or at least

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<sup>10</sup> Popper, K. R., *The Open Universe : An Argument for Indeterminism*, Routledge, 1992, p. 44

<sup>11</sup> Ernest Nagel, *Assumptions in Economic Theory*, *The American Economic Review*, Vol. 53, No. 2, *Papers and Proceedings of the Seventy-Fifth Annual Meeting of the American Economic Association*. (May, 1963), pp. 211-219.

<sup>12</sup> G. C. Archibald; Herbert A. Simon; Paul A. Samuelson (1963) *Discussion*, *The American Economic Review*, Vol. 53, No. 2, *Papers and Proceedings of the Seventy-Fifth Annual Meeting of the American Economic Association*. (May, 1963), p. 230.

<sup>13</sup> *Ibid.*

<sup>14</sup> Musgrave, A., “Unreal assumptions” in economic theory: The F-twist untwisted’, *Kyklos*, 34: 1981, 377–87.

no detectable effect'.<sup>15</sup> Domain assumptions are concerned with the theory's domain of applicability and affirm that theory T applies only if certain conditions obtain (usually, only if factor F is present or absent). Finally, heuristic assumptions are first-step, preliminary presuppositions that will be adjusted or relaxed as additional information is considered.

From Musgrave's description, it is not clear why there should be a separate category of heuristic methodological assumptions; if one recognizes that no assumption is set in stone and it can be adjusted under certain conditions, then all assumptions should be treated as heuristic. Are all assumptions subject to revision, just as methodological rules are supposed to be? A methodology is successful as long (and to the extent in which) it generates explanations and predictions than are considered better than those engendered by other methodologies. However, one could ask if even epistemic values such as those emphasizing the centrality of unbiased inquiry, argumentation and empirical testing are debatable. It could be argued that these are *foundational values*, in the sense that departing from them would render the very concept of scientific knowledge meaningless. Musgrave's description of heuristic assumptions certainly fits the methodological level, where confrontation with facts is unavoidable. But meta-theoretical assumptions or epistemic values cannot be *directly* subjected to empirical validation.

Friedman's "F-twist" is based on the premise that the truth of assumptions is not relevant as long as one has a straightforward way of assessing their consequences – as long as they are successful in generating confirmed predictions. But the success of predictions made on the basis of certain assumptions can only be evaluated contextually – that is, under a particular set of constraints and in comparison with how well other assumptions would work under similar conditions. Such a comparison is unfeasible in most situations, given the way paradigms function and interact. The most we can do is study how other paradigms did in similar circumstances or analyze the logical consequences of adopting particular assumptions. Although this does not render the whole enterprise of evaluating the success of some particular assumptions useless, it does offer good reasons for cautiousness when talking about 'success'. Also, one has to deal with the ambiguity of defining 'success' in operational terms – for instance, identifying the relevant timescale to consider. Short-term confirmation is not necessarily a reliable marker of long-term success.

Musgrave's argument is taken over and refined by Uskali Mäki,<sup>16</sup> who takes issue with the very dichotomy between realism and instrumentalism. He notices that, even if we take instrumentalism at face value, there are higher level epistemic assumptions that bring the truth (or at least the realism) of assumptions back into the picture. For instance, a negligibility assumption implies making a "hypothesis

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<sup>15</sup> *Op.cit.*, p. 378.

<sup>16</sup> See Mäki, Uskali (2000). "Kinds of Assumptions and Their Truth: Shaking an Untwisted F-Twist", *Kyklos*, V. 53, Fasc. 3, 317–336 and Uskali Mäki (2003) *Unrealistic assumptions and unnecessary confusions: Rereading and rewriting F53 as a realist statement*, to appear in *The Methodology of Positive Economics: Milton Friedman's Essay After Half a Century*, edited by Uskali Mäki. Cambridge University Press.

that some factor F that might be expected to affect the phenomenon under investigation actually has an effect upon it small enough to be neglected relative to a given purpose". One is not warranted to make all-encompassing and undifferentiated negligibility assumptions on a whole domain of facts; the purposes of cognitive subjects and the exact area of application have to be clarified. This way, "a negligibility assumption is a true or false assertion about a relationship between real effects and the purposes of cognitive subjects"<sup>17</sup>. Instrumentalist assumptions can be reformulated as meta-level statements that have a truth value (even if establishing their truth *in practice* may be very difficult).

The very action of instituting negligibility, domain and heuristic assumptions implies ontological and epistemological commitments which, at a certain point, make the issues of realism and truth impossible to evade. These are not simply additional presuppositions that commit the scientist to a certain epistemological position (broadly called 'realism'), but meta-theoretical assumptions that make the scientific discourse meaningful and communicable to begin with. At the level of epistemic values and meta-theoretical assumptions, we can differentiate between those which have an 'enabling' role (that is, they enable the very possibility of science as an attempt towards a knowledge that is inter-subjectively validated) and the rest. The requirement of logical consistency belongs to the first, the assumptions of linearity or equilibrium to the second.

Maki takes the example of negligibility assumption (N) "The government has a balanced budget", which is then rephrased as (NB) "A given budget imbalance has negligible effects on the phenomena under investigation". The instrumentalist may well claim that it is irrelevant whether real governments *really* have balanced budgets, because the role of (N) is not to represent reality. However, insofar as we recognize that (N) boils down to (NB), we are compelled to consider the *empirical adequacy* of the assumption. Even if in some cases it may be difficult to settle the matter, it is obviously an empirical matter with a definite answer. Maki also notices that "(B) is a statement about economic reality, while (NB) is a statement about economic reality plus certain purposes of the economist making the statement. (B) is a statement about the existence of a 'factor', while (NB) is a statement about its causal powers, viewed from a pragmatic perspective, that is, from the point of view of the purposes of the users of the assumption."<sup>18</sup>

Meta-theoretical or 'core' assumptions *could* be used to delimit the context of applicability of a specific theory: "One may state, for example, that a theory involving it only applies to domains where divergences from calculative maximization behavior are negligible in their consequences – but not to certain individuals or organizations within a given culture, or to actors in some other cultures or to certain realms within a given culture (such as the realm of kin and friendship), where the divergences are not negligible."<sup>19</sup> However, in practice this usually happens the other way around: the domain of applicability is given, and the assumptions are implicitly assumed to be all-encompassing.

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<sup>17</sup> Maki 2000, op. cit., p. 320

<sup>18</sup> Ibid., pp. 322-326

<sup>19</sup> Ibid., p. 329



The instrumentalist approach posits a ‘methodological black box’ that should miraculously take us from unlikely assumptions to likely outcomes. Musgrave and Maki, among others, have argued that the ‘F-twist’ has a shaky theoretical justification and that not all reference to facts can be eliminated from the assessment of assumptions themselves. One does not need to be a (methodological) realist in order to acknowledge this. Even an instrumentalist recognizes that assumptions can be assessed by how well they perform in generating successful predictions – that is, in uncovering *real* causal connections. (S)he just does not take the extra step of assuming that generating good predictions has anything to do with how things really are. But once we recognize that predictions (unlike lucky guesses) are built on knowledge about causal relations, the reference to states of fact cannot be avoided. Moreover, this ‘consequentialist’ approach to assessing assumptions need not be limited to the success of predictions. Past successes or failures in offering an adequate description of a certain context or in proposing non-reductionist explanations has to be taken into account. This could be used not so much as a confirmation, but rather as an falsification device: theories which have systematically failed in grasping the specificity of a certain context of application should be questioned, along with the meta-theoretical assumptions they are based upon.

Maki also notes that “core assumptions seldom function as early-step assumptions in a sequence of models within a given framework of analysis: they are the stable ‘all-step’ assumptions”.<sup>20</sup> Therefore higher-level assumptions are presumed not to be amenable to further questioning and adjustment. This enables the overextension of a certain methodology to different domains of application where they may prove inadequate. In fact, this tendency to postulate and uphold certain ‘fundamental’ assumptions may be more important than the *content* of the assumptions themselves for explaining ‘methodological absolutism’. Obviously, this does not amount to undermining the theoretical fundamentals of scientific knowledge. It only reminds the general principles of critical inquiry and falsifiability, marking the demarcation between science and non-science. Everything that is needed in order for these principles to be applicable here is to acknowledge the fact that higher-level assumptions are not isolated from the rest of the theory; they are solidary with, and related to, methodological and behavioral assumptions, as well as on details of theory implementation (we can call this the *principle of continuity*). Despite their level of abstraction or their ‘intuitive’ fundamental character, there is no reason to grant core assumptions a different epistemic status: at the bottom, they are still conjectures about how things are. As conjectures, they are fallible and can be falsified. One can justifiably question the legitimacy of an assumption which can be so construed as to escape any possible attempt to falsification, just as one would question an unfalsifiable theory. Equilibrium is not intrinsically written in the DNA of the markets, in the sense that it is not automatically derived from an empirical description of how the markets function. Markets approximate equilibrium under certain conditions and seem to evade equilibrium under different conditions. It is a problem of

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<sup>20</sup> Ibid.

contextual characterization, of delimiting the contexts of application for which different methodologies (and their underlying assumptions) may prove adequate. Even if higher-level assumptions are not theory-dependent (they can be shared by several theories), their validity in extending beyond the context of a certain group of related theories (or paradigm) is limited. The possibility of extending it to new contexts has to be evaluated on a case by case basis and remains an empirical issue (even if no direct empirical assessment of core assumptions is available).

It is worth noticing that Friedman's view subordinates explanation to prediction and largely ignores contexts in which one can explain but not predict (which is often the case in complex, self-organizing systems). The instrumentalist approach explains much of the neoclassical theory's reluctance in questioning its core assumptions. Once 'success' is defined in terms of prediction and explanatory power is ultimately reducible to predictive power, the scientific basis for evaluating a theory's validity becomes quite shaky. Predictive, ex-post verification is insufficient if not considered in the context of alternative, competing predictions: 'success' is meaningless if not considered in the context of competing theories – if not characterized with respect to how *other* theories would deal with the same empirical domain. Moreover, explanatory power cannot be reduced to predictive power: while a prediction is implicitly based on a (tentative) explanation, not all explanations enable prediction. One can only propose an explanation after a certain event took place, but in the case of complex systems with a high number of variables this will not necessarily provide reliable knowledge about the circumstances under which the event will take place again. This is why *verification* is supplemented by *falsification* in the overall evaluation of a theory. A theory must be so construed as to make falsification possible (that is, have results which can be verified experimentally and specify under what conditions a certain result counts as a falsification). One which cannot be falsified under any circumstances can explain anything – and thus it explains nothing.

Predictive success can, by itself, be construed as an unfalsifiable concept if predictions are kept general enough, by conveniently adjusting the description of facts in order to suit the predictions. Markets can be shown to be in equilibrium by assuming that the 'natural state' is equilibrium and then describing all apparent contradictions as temporary deviations from the natural state. With sufficient generosity in considering what 'temporary' means and with carefully chosen caveats in delimiting a theory's domain of application, one can arguably explain away any market behavior, no matter how far from equilibrium. One could also claim that "the discrepancies which are asserted to exist between the experimental results and the theory are only apparent and that they will disappear with the advance of our understanding".<sup>21</sup> Predictive success may not be enough to ensure that methodological assumptions themselves are also questioned: not only were those very assumptions used to define 'success', but the way predictive success is understood can be adjusted in order to safeguard the assumptions. There is no reason why methodological assumptions should not be themselves subjected to

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<sup>21</sup> Popper, K. R., *The Logic of Scientific Discovery*, London and New York: Routledge, 2005, p. 28.

critical inquiry as regards their empirical relevance, theoretical fit to the particular context under study *and* predictive success. The instrumental view of assumptions, with its uncritical reliance on a self-serving concept of predictive success, limits the scope of this inquiry.

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