

ALL THAT GLITTERS IS NOT GOLD:
THE CASE OF MAINSTREAM PLURALISM

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ABSTRACT

It has been recently noted a new pluralistic tendency *within* mainstream economics, purportedly characterized by an eclectic attitude towards some traditional tenets of neoclassical economics. Anyway, mainstream literature continues to rely on the appropriateness of partial equilibrium analysis and on a comparatively intensified engagement in applied economics. It is my contention here that any appraisal of the (supposed) new mainstream pluralism cannot abstain from a careful discussion of the somewhat problematic consequences of both these prominent features.

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Although this may seem a paradox, all exact science is dominated by the idea of approximation. When a man tells you that he knows the exact truth about anything, you are safe in inferring that he is an inexact man.

Russell 1931, 65

INTRODUCTION

The debate on the desirability of scientific pluralism originated about four decades ago among philosophers of science, mainly as a critical reac-

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tion to previous auspices in favour of the unity of science (Suppes 1978). Since then, such a debate naturally spread among various disciplines (cp. Kellert *et al.* 2006) with economics being no exception.¹ Discussions about pluralism within our discipline, however, stemmed from at least three different fields of study. We may indeed recognize, in chronological order, a first strand of literature among the 1980s mounting interest in economic methodology when the locution “methodological pluralism” was a brand name originally proposed by Caldwell (1982: chapter 13),² although different approaches in many ways similar to it were at that time variously christened by other authors.³

Methodological pluralism, roughly speaking, is the view that in the course of scientific practice it is legitimate to take advantage of multiple methodological approaches as well as of multiple theoretical models.⁴ More precisely we may make a distinction between a *stricto sensu* methodological pluralism (the thesis that a plurality of methods is more than advisable, due to the absence of decisive criteria for selecting the best style of analysis and/or the best procedure of theory appraisal) and a theoretical pluralism (the idea of the possibility of different but equally plausible narrative of economic phenomena, hence the legitimacy of different “schools” within the discipline of economics). All this paved the way for a propagation of the quest for pluralism among heterodox economists of various persuasions:⁵ in a sense, indeed, we might say that the bulk of the literature on pluralism in economics has arisen within the heterodox literature.

More recently, however, a few authors – among whom David Colander and John Davis are perhaps the most committed supporters – envisaged a new pluralistic tendency *within* mainstream economics, characterized by an eclectic attitude towards some traditional tenets of neoclassical economics.⁶

¹ For recent overviews on the subject, see GARNETT *et al.* (2010) and COURVISANOS *et al.* (2016).

² See also CALDWELL (1988).

³ For a sample of the different positions endorsed in those times, let me refer to the various essays in SALANTI and SCREPANTI (1997). HOOVER (2018) offers an interesting (and critical) appraisal of CALDWELL (1982) thirty-five years later from its publication.

⁴ This does not imply, however, that we are compelled to embrace Feyerabend’s epistemological anarchism. Instead of maintaining that “anything goes”, we might more modestly recognize that different theoretical approaches can be reasonably grounded on different methodological grounds.

⁵ In this respect see, for instance, SENT (2006) and HANDS (2015).

⁶ Cf. COLANDER (2000, 2003 and 2010), COLANDER *et al.* (2004 and 2007), and DAVIS (2006a, 2007, 2008a, 2008b and 2016). For further references on the recent debate on this subject, see CEDRINI and FONTANA (2018).

In what follows I will argue that: *i*) in *all* these cases advocacy (or, as more appropriate in the most recent case, practice) of pluralism come up from difficulties encountered in dealing with some fundamental problems of one kind or another; *ii*) the purported new fields of research within mainstream go through two shortcomings generally unnoticed, namely a pervasive reliance on partial equilibrium analysis (whose legitimacy is generally simply taken for granted) and an engagement in applied pieces of research whose relevance is often open to question.

1. THE ADVOCACY FOR PLURALISM AS A SECOND BEST OPTION

To start with the advocacy of methodological pluralism, it can be observed that it has been an evident case in point. That it emerged from some discomfort with the then current state of affairs within either the philosophy of science or economic theory was openly admitted at the time, for instance, by two of its more prominent advocates, as in the following passages:

My own path to methodological pluralism was prompted by the dual discovery of, first, disarray within the philosophy of science [...] and, second, an equally strange situation in economics, where it appeared that a dominant paradigm prevailed, but one which had very little in common with any account within the philosophy of science, a possible exception being Popper's situational logic, which was then being ignored (Caldwell 1997: 100).

The case for methodological pluralism ultimately rests on the necessity of choice in the absence of a single conclusive final methodological or epistemological principle. We have to choose between alternative methodologies each of which has its own internal limitations and there is no single unequivocal, conclusive meta-principle on which to make that choice (Samuels 1997: 67).

The post-Popperian “disarray within the philosophy of science” has been so much extensively discussed in the past decades⁷ that I do not think to have anything new worth to be added here. However, some further remarks about the reasons why economics seemed to be recalcitrant to any ready-to-use methodological appraisal available at the time may instead be in order, if only because there does not seem to be a general consensus in this respect among economic methodologists themselves.

The ultimate source of such difficulties rests, in my opinion, on the uncertain epistemological status of those first (or fundamental) principles

⁷ With reference to economic methodology see, for example, BACKHOUSE (1998), SALANTI (1989), HANDS (2001), HAUSMAN (2013).

upon which economic theory is supposed to be firmly grounded, combined with the contention of economics having to be, after all, an empirical discipline. This tension, to be sure, is by no means new. Already in the 1950s, hence well before the upsurging of literature which characterized the methodological debate in economics since the 1970s, the intrinsic contradiction between the aprioristic defence of the supposed first principles *à la* Robbins and the “as if” justification of manifestly unrealistic assumption *à la* Friedman was more than evident. It was so much so that even an author usually sober in his judgments as Tjalling Koopmans openly admitted that:

After more than a century of intensive activity in scientific economics, two economists who have made outstanding substantive contributions to our science and whose positions on questions of economic policy are moreover not far apart, seek the ultimate basis of economic knowledge in considerations which (a) contradict each other and (b) are each subject to strong objections. One is led to conclude that economics as a scientific discipline is still somewhat hanging in the air (Koopmans 1957: 141).

Now, if we look over the more recent introductory textbooks, companions or handbooks on economic methodology, we may surely appreciate many interesting appraisals of particular methodological issues within the discipline and its subfields,⁸ but cannot be denied that the same conclusion about “the ultimate basis of economic knowledge” might be agreed today as well as sixty years ago. As Hoover (2017: abstract) puts it: “Commitment to first principles risks emptying economics of its empirical content, while commitment to empirical content entails violating supposed first principles and muddles the boundaries of the discipline”.

All this, in a sense, is the heritage of the explicit recognition of the need to isolate a particular aspect of human behaviour from all the other activities that characterize human beings. It was a risky, albeit courageous, methodological choice, subject as it might have been, and indeed it actually was, to the most varied criticisms: from those directed at the realism of the fundamental assumptions, to the moral strictures towards a discipline which seemed to have elected the utilitarian calculus as the ultimate principle governing human action. Instrumental to such a methodological decision was indeed the portrayal of the economic agent as a sort of *homo oeconomicus*.⁹ Apart from old fashioned critiques, even if we adopt the present

⁸ Cf. GUALA (2006), DAVIS (2006b), KINCAID and ROSS (2009), BOUMANS and DAVIS (2010), DAVIS and HANDS (2011), REISS (2013) and MAAS (2014).

⁹ The corresponding English expression “economic man” was probably firstly adopted by INGRAM (1888) with reference to the Millian methodological perspective, while the Latin version is due to PARETO (1892), who then repeatedly used it in several other writings.

view of the “economic man” as implying nothing else than the adoption of the principle of rationality, we remain with the problem of justifying the disciplinary boundaries of economics.

The persistence of heterodox approaches within the discipline may be regarded, after all, as one of the ultimate consequences of this old chapter in the history of economic thought. However, advocacy of pluralism by many heterodox economists appears to be somewhat instrumental. As perceptively noted by Davis (1997: 209):

Their motivation ... is not that their own theoretical approaches are *also* correct – a theoretical pluralist view – but rather that neoclassical economics is mistaken and misguided in its most basic assumptions, and that their own approaches remedy the deficiencies of neoclassicism – a theoretical monist view [...] Heterodox economists and neoclassical economists dispute the worth of each other’s approaches and sometimes also the worth of their associated methodological strategies, but tend to maintain an openness on the latter score, because methodological disputes appear irresolvable while economic theories are thought distinct and non-overlapping.

For the sake of completeness it must be added that other commentators seem nonetheless to be more inclined to concede the possibility of some degree of theoretical pluralism within heterodoxy variously defined.¹⁰ Be that as it may, all the previous remarks indicate that in any case the advocacy of methodological (and to a lesser extent, theoretical) pluralism derives from some kind of dissatisfaction given the impossibility of achieving some ideal of economic scientificity. Indeed, the above referred endorsements of a pluralist attitude stem, in a way or another, from a perceived weakness of the methodological justification of mainstream economics, and in some cases – especially from the diverse heterodox perspectives – of its substantive content.

2. ON THE SO-CALLED PLURALISM WITHIN MAINSTREAM

More recently, the emergence *within* the mainstream of an assortment of new research programmes (sharing the adoption of research methodologies somewhat extraneous to the tradition of neoclassical economics) has been interpreted as the sign of an emergent “mainstream pluralism”. As already mentioned in the introduction, the two scholars more actively engaged in exploring the implications of these new directions in economic research are John Davis and David Colander.

¹⁰ Cf. SENT (2006), DOW (2008), LEE (2010 and 2011), HANDS (2015).

According to Davis (2008a: 349) these new fields of research include “classical game theory, evolutionary game theory, behavioural game theory, evolutionary economics, behavioural economics, experimental economics, neuroeconomics and agent-based complexity economics.” In this respect one of Davis’s major points seems to be that some developments in these “new” fields signal a trend reversal of economic imperialism expansion and a parallel shift towards multidisciplinary,¹¹ with some relevant consequences on the definition of the boundary of the discipline.

Colander, for his part, insists on the advisability of abandoning the “rhetoric of pluralism” because of its ineffectiveness in promoting a constructive confrontation between mainstream and heterodox economists, which could put at risk even the survival of the dissenting schools,¹² as is openly stated in Colander *et al.* (2007: 308):

We argued in [Colander *et al.* 2004] that change in economics was unlikely to come through a Kuhnian paradigm shift that replaced a neoclassical orthodoxy with a heterodox alternative. Instead, the change would come from within, and it is already ongoing. If heterodox economics wants to affect that change, it must deal with that reality, and see that its ideas get a hearing at the edge of economics where the new ideas are sprouting.

We may agree or disagree with the view that a new mainstream pluralism is supplanting the traditional neoclassical approach. This depends, after all, on how we define neoclassical economics, and such a definition is far from being uncontroversial. What is undeniable, however, is that the new fields that are supposed to play a part in the development of the new mainstream are among the most interesting contributions to microeconomics of the last two decades. As Hands (2015: 71-74) aptly remarks:

Although I am not quite as convinced as many others that the mainstream is no longer neoclassical, I do think the trend is clearly in that direction, and more importantly here, I definitely believe that a substantial change has taken place within *economic methodology*. [...] The bottom line is that one does not need to be completely convinced that neoclassical economics has been displaced from its dominant position within the mainstream to recognize that the most interesting and important methodological questions are no longer about either traditional neoclassical or heterodox economics, but rather, are about precisely the fields most often identified as representing a new more pluralistic mainstream.

In any case, with the possible exception of the agent-based complexity economics, all fields usually indicated as constituents of the new plural-

¹¹ See, in particular, DAVIS (2006a and 2016).

¹² Cf., for instance, COLANDER (2000 and 2010).

ist mainstream (that is, classical game theory, evolutionary game theory, behavioural game theory, evolutionary economics, behavioural economics, experimental economics, and neuroeconomics) explore territories that overlap the ones that have been, for more than a century, the typical domain of traditional (neoclassical) microeconomics.

At first sight, in comparison with traditional approaches the recent literature in these new fields (as well as, for that matter, the more orthodox recent literature in microeconomics) exhibits at least two remarkable differences, namely an unequivocal (more often implicit than otherwise) reliance on the appropriateness of partial equilibrium analysis and a comparatively intensified engagement in applied economics. It is my contention here that any appraisal of the (supposed) new mainstream pluralism cannot abstain from a careful discussion of the consequences of these two prominent features.

3. PARTIAL EQUILIBRIUM (TO BE) RECONSIDERED

David Colander, alongside his contention of the materializing of a sort of mainstream pluralism within recent economic literature, has also been one of the few scholars who have explicitly pointed out the dismissal of general equilibrium theory as a compulsory frame of reference within the new mainstream, as in the following passages:

Another aspect of modern applied policy modelling is that ... these models pay almost no heed to consistency with general equilibrium theory. New work in micro emphasizes the development of a variety of practical models ... that are relevant for specific problems, but make no claim that, and give little thought to whether, they are general-equilibrium consistent. [...] By the 1970s economists recognized that the Arrow/Debreu general equilibrium work was not going to get to the promised land. That recognition freed economists to deal with practical policy models that were inconsistent with general equilibrium theory (Colander 2000: 138-139).

To speak about models “inconsistent with general equilibrium theory”, however, is merely a scholarly tactful and a bit elusive way for not to say, in a more straightforward manner, that modern (micro)economics must extensively rely on analyses of a partial equilibrium kind.^{13,14} Just to have a

¹³ In AKERLOF (2003: 1-2), for example, we find the following more candid judgment: “At the beginning of the 1960s, standard microeconomic theory was overwhelmingly based upon the perfectly competitive general equilibrium model. By the 1990s the study of this model was just one branch of economic theory. Then, standard papers in economic theory were in a very different style from now, where *economic models are tailored to specific markets and specific situations*. In this new style, economic theory is not just the exploration of deviations from the

glimpse of how much today perspective differs from what was quite common even before the Arrow/Debreu fashion season, we may recollect the following derogatory comment that Schumpeter (1941: 241) ventured to insert in his commemorative essay for the fifty years from the publication of the first edition of Marshall's *Principles*:¹⁴

[T]he text of the *Principles* suggests and the appendix proves that Marshall had fully grasped the idea of general equilibrium, discovering "a whole Copernican system, by which all the elements of the economic universe are kept in their places by mutual counterpoise and interaction" [Keynes 1933: 223]. But in order to display the working of that system he forged and extensively used a different model that was much easier to manage though its field of application was also much more restricted.

In all fairness to such a great economist, it is nonetheless difficult to understand in which sense the "field of application" of partial equilibrium analysis should be "much more restricted" than general equilibrium analysis. One might rather say that it is more flexible but less rigorous, but by no means "much more restricted". An argument of this kind cannot but originate from a belief in the primacy of the requirement of formal rigor in economic theorizing.¹⁵

Such a belief has persisted for some decades and we may detect a similar perspective behind some subsequent attempts to identify exact conditions under which partial equilibrium analysis would be theoretically legitimate.

In this vein, for instance, it has been authoritatively argued that "a proper understanding of general equilibrium [is necessary] ... to attain... an understanding of partial equilibrium" (Samuelson 1967: 113).¹⁶ After

single model of perfect competition. Instead, in this new style, the economic model is customized to describe the salient features of reality that describe the special problem under consideration". (italics added). More recently, in the same vein, RODRÍG (2015) forcefully maintains that there is not a "right model", but rather many potentially useful context-specific models among which economists must choose the most fitted to the task at hand. In a sense, the strength of economics would definitely reside in her wide collection of models which allows its practitioners to deal with a broad array of economic observable fact.

¹⁴ According to a suggestion by one of the two anonymous referees, it may be worth pointing out that in what follows the discussion of partial equilibrium analysis refers to the neoclassical interpretation of Marshall rather than the very different evolutionary interpretation of it.

¹⁵ This, however, does not prevent Schumpeter from recognizing, a few lines below the quoted passages, that: "...if we frankly recognize that this method is essentially one of *approximation* ... then we are at liberty to enjoy the rich harvest of results which it turned out and for the sake of which Marshall, deviating from strict correctness, developed what was really much more bold and novel than his method of presentation suggests." (italics added).

¹⁶ As quoted in PANICO (1991: 558).

all, in spite of their utterly different research strategies and opinions on general equilibrium theories, and on neoclassical economics overall, Sraffa (1926) and Samuelson (1971) pursued the same objective, that is to define the strict conditions for Marshallian partial equilibrium to be theoretically legitimate, *i.e.* suited to reach rigorously exact conclusions. They were attempting, in other words, to reconstruct in a logically consistent way the Marshallian partial equilibrium model of pure competition markets, in order to single out the logically admissible accounts of empirical situations to which those models could be applied and those situations to which they could not.¹⁷ The problem, as I see it, is to understand of which use such very strict conditions (of one kind or another) could ever be, because it is undeniable that they are plainly impossible to be satisfied for any real existing market.

Be that as it may, as Backhouse (2011: 87) perceptively observes, it surely happened that:

Given the dominance of the Walrasian paradigm in the post-Second World War era, Marshall's star waned, except as the creator of partial equilibrium analysis that was useful to applied economists and suitable for introductory courses in microeconomics. [...] Though there have been recent defenders of Marshall, the move towards game theory rather than general equilibrium as the dominant foundation for economic theorizing, and interest in formal models of imperfect competition may have led to partial equilibrium analysis being taken more seriously but it has hardly caused a change of attitude towards Marshall's method.

To the observation that there has been no changes of attitude towards Marshall's method I would add that within current literature, in spite of its overwhelming reliance on partial equilibrium investigations, the issue of its legitimacy has been simply put under the carpet (or, in more polite terms, just taken for granted).¹⁸

A satisfactory solution for this unpleasant state of affairs is admittedly far from easy. On one hand, as hinted before, any attempt to find *exact* (sufficient?) conditions to be satisfied in order to assure the absolute consistency of partial equilibrium analysis is doomed to result in a newer wave of

¹⁷ A summary of the disagreements between Samuelson and a number of Sraffian scholars in this respect may be found in PANICO (1991) and SAMUELSON (1991) rejoinder. Note, by the way, that even in this case (as happened in many other occasions, including the controversy on the theory of capital of the 1960s) both of them shared exactly the same methodological attitude, that is, a strong inclination towards formalism. It is the same formalist attitude that made possible to conceive, for instance, a book like WALSH and GRAM (1980).

¹⁸ Note, by the way, that under the carpet it has been equally put the methodologically fundamental question of the explanatory power of general equilibrium theories: cf. HAUSMAN (1981) and ROSENBERG (1992).

“empty boxes”. On the other hand, to circumvent the problem by simply ignoring it does not seem to be an advisable rule of conduct.

My tentative suggestion in this respect is to try to rediscover the virtue of *approximation*. As it has been observed with reference to Marshallian supply functions, “further reflections on Viner’s (1931) and Lipsey’s (1963: 404) claim that the main justification of partial analysis, rather than its logical validity, is the assumption that the errors it implies are negligible”¹⁹ (Panico 1991: 568).

With reference to the other vexed question concerning Marshall’s formal analyses, *i.e.* the constancy of marginal utility of money,²⁰ analogous considerations might well be in order.

¹⁹ Viner’s statements in this respect are especially neat: “The analysis which follows is based on the usual assumptions and presuppositions of the Marshallian type of economics. ... [I]t contents itself with examination of the conditions of a partial equilibrium of a special sort, and does not inquire into the repercussions of the postulated changes in cost or demand conditions on the general equilibrium situation. Like all partial equilibrium analysis ... it rests on assumptions of the *coeteris paribus* order which posit independence where in fact there is some degree of dependence. For such logically invalid assumptions there is the pragmatic defense that they permit of more detailed analysis of certain phases of economic interdependence than would be possible in their absence, and that to the extent that they are fictions uncompensated by counterbalancing fictions, it is reasonable to believe that the errors in the results obtained will be almost invariably quantitative rather than qualitative in character, and will generally be even quantitatively of minor importance” (Viner 1931: 24).

²⁰ The formal analysis of utility and demand as developed in Marshall (2013: Book III and Mathematical Appendix, notes I-IX) is usually presented as characterized by:

- 1) the use of and additive utility function, that is

$$U(x_1, x_2, \dots, x_n) = U(x_1) + U(x_2) + \dots + U(x_n);$$

- 2) decreasing marginal utility of every good, so that we have

$$\frac{\partial U(\cdot)}{\partial x_i} = \frac{dU(x_i)}{dx_i} > 0 \quad \text{and} \quad \frac{\partial^2 U(\cdot)}{\partial^2 x_i} = \frac{d^2 U(x_i)}{d^2 x_i} < 0;$$

- 3) constant marginal utility of income (or of “money”);

- 4) consumer’s demand curve obtained under the assumption of an income effect equal to zero.

If this were really the case, however, Marshall (and a number of his subsequent commentators, for that matter) would have incurred two hardly understandable analytical lapses. The first is due to the fact that (1) and (2) imply that all goods are normal (cp., for instance, Green 1976: 91-94), so that (4) cannot be consistently maintained. The second is ever more surprising because (3) would require the (additive) utility function to be homogeneous of degree one, *i.e.*

$$U(ax_1, ax_2, \dots, ax_n) = aU(x_1) + aU(x_2) + \dots + aU(x_n), >$$

and therefore

$$\frac{\partial^2 U(\cdot)}{\partial^2 x_i} = \frac{d^2 U(x_i)}{d^2 x_i} = 0$$

this would contradict the assumption (2) of decreasing marginal utility of every good.

Even in this respect Samuelson (1942) looked for conditions of consistency. He argued that by “marginal utility of money” Marshall in fact meant “marginal utility of income”, assuming its constancy with regard to changes in prices. Given Marshall’s assumption of an additively separable utility function, it follows that:

In words, *the seemingly innocent assumption that there exist a utility index for which the marginal utility of income is constant with respect to price changes results in the empirical restriction of unitary income elasticity of demand, or that the consumption of each and every good is exactly proportional to income. [...] the combined assumptions of constancy of the marginal utility of income and independence of utility imply that the elasticity of demand be always unity.* (Samuelson 1942: 43-44, italics in the original).²¹

In formal terms (with obvious notation) each consumer maximizes

$$U(x_1, x_2, \dots, x_n) = U(x_1) + U(x_2) + \dots + U(x_n) \quad [1]$$

subject to the usual constraint $\sum_i x_i p_i = M$.

Because of the composite commodity theorem (cf., for instance, Green 1976: 111-112 and 308-310), the comparative statics of a change in one price (ρ_i) can be conveniently illustrated with reference to

$$U(x_i, x_c) = U(x_i) + U(x_c) \quad [1']$$

From the Lagrangean $L = U(x_i) + U(x_c) + \mu(M - x_i p_i - x_c p_c)$ we have the following first-order conditions

$$\frac{dU(x_i)}{dx_i} - \mu p_i = 0 \quad [2]$$

$$\frac{dU(x_c)}{dx_c} - \mu p_c = 0 \quad [2']$$

$$M - x_i p_i - x_c p_c = 0 \quad [2'']$$

and, differentiating them with respect to ρ_i , we obtain

$$p_i \left(\frac{\partial \mu}{\partial p_i} \right) - \left(\frac{d^2 U(x_i)}{d^2 x_i} \right) \left(\frac{\partial x_i}{\partial p_i} \right) = -\mu \quad [3]$$

$$p_c \left(\frac{\partial \mu}{\partial p_i} \right) - \left(\frac{d^2 U(x_c)}{d^2 x_c} \right) \left(\frac{\partial x_c}{\partial p_i} \right) = 0 \quad [3']$$

$$\left(\frac{p_i}{x_i} \right) \left(\frac{\delta x_i}{\delta p_i} \right) + \left(\frac{p_c}{x_c} \right) \left(\frac{\partial x_c}{\partial p_i} \right) = -1 \quad [3'']$$

²¹ WEINTRAUB (1942: 541 n. 12) went so far as to attribute to Marshall himself the assumption of unitary elasticity of the demand curve. For a different point of view, see FRIEDMAN (1949).

According to equation [3''], $\epsilon_i = \left(\frac{p_i}{x_i}\right) \left(\frac{\partial x_i}{\partial p_i}\right) = -1$ implies that $\frac{\partial x_c}{\partial p_i} = 0$.

Consequently (because of equations [3'] and [3]), it must be $\frac{\partial \mu}{\partial p_i} = 0$ and $\left(\frac{d^2 U(x_i)}{d^2 x_i}\right) \left(\frac{\partial x_i}{\partial p_i}\right) = \mu$. This means that, with $\epsilon_i = -1$, the demanded quantities

of all other goods (x_c) are unaffected by changes in p_i .²² Unitary elasticity is not only a sufficient, but also a necessary condition for this result. Simple inspection of equations [3], [3'] and [3''] confirms that, for $\epsilon_i = \left(\frac{p_i}{x_i}\right) \left(\frac{\partial x_i}{\partial p_i}\right) \neq -1$, there must be $\left(\frac{\partial x_c}{\partial p_i}\right) \neq 0$ and $\left(\frac{\partial \mu}{\partial p_i}\right) \neq 0$. Otherwise, with $\epsilon_i \neq -1$ and $\left(\frac{\partial \mu}{\partial p_i}\right) = 0$, the budget constraint would not be met for changes in p_i .

This may well be why Marshall, in dealing with the assumption of “constant marginal utility of money”, takes care to specify that he is assuming the total expense for x_i to be a “small” fraction of consumer’s budget,²³ as it has been repeatedly acknowledged by a number of authors.²⁴

I argued above that an alternative equivalent assumption could be to assume $\epsilon_i \approx 1$ when actually it is, in general, $\epsilon_i \neq 1$

This would have the advantage, apart from giving analytical consistency – albeit for a very particular case – to the Marshallian demand theory, of obtaining a formalization that legitimizes, at least as far as the demand side is concerned, the use of partial equilibrium method and makes somewhat superfluous that notion of marginal utility of income (or of “money”), which has always been so troubling for all commentators of Marshall. Obviously, in this case the caution about the part of the whole expenditure that should be “small” has to be substituted by a parallel caution about changes in prices (and, therefore, in quantities) that should be equally “small”.

²² Note that, since $\frac{\partial \mu}{\partial p_i} = 0$ when $\epsilon_i = -1$, we ought to be cautious in interpreting μ as the “marginal utility of money”, excepted for changes in M all p_i being equal.

²³ “When a person buys anything for his own consumption, he generally spends on it a small part of his total resources; while when he buys it for the purposes of trade, he looks to re-selling it, and therefore his potential resources are not diminished. In either case there is no appreciable change in his willingness to part with money” (Marshall 2013: 279), and “But these changes of consumers’ rent (being of the second order of smallness) may be neglected, on the assumption, which underlies our whole reasoning, that his expenditure on any one thing, as, for instance, tea, is only a small part of his whole expenditure” (*ibid.*: 693).

²⁴ GEORGESCU-ROEGEN (1968: 176-177, italics added), for instance, remarks: “That Marshall did not have in mind a constancy in the strict sense of a function which has exactly the same value for every value of its argument, is beyond doubt. [...] The quasi-constancy of $w(m)$, he asserts, follows from the fact that the ‘expenditure on any one thing... is only a small part’ of the budget”. For a recent detailed reconsideration of the notion of Marshallian demand curve, see HUDIK 2017.

APPLIED ECONOMICS: THE ROLE OF EMPIRICAL RESEARCH

In their Introduction to the collection of papers delivered at the 2016 *HOPE* conference on “Becoming Applied: The Transformation of Economics since 1970”, the guest editors appropriately notice that:

The twenty-first century is the age of the applied economist. Applied work dominates the top economics journals. Citations of ten out of the last twelve John Bates Clark Medal winners describe the recipients as being “applied”, “empirical” or as doing work of “practical relevance.” [...] The empirical practices in which economists are now engaged include not only traditional econometric work but also laboratory experiments, randomized control trials, analysis of natural experiments and the building of databases that can be used in different ways (Backhouse and Cherrier 2017: 1-3).

In the following pages they reveal an appreciative attitude towards these changes within the discipline and end up announcing that “It had become the age of the applied economist” (*ibid.*: 28). It is a conclusion which it would be surely difficult to disagree with,²⁵ if only because the existence of such a trend is also witnessed by a number of practicing economists. Dani Rodrik (2015: 200-201), for instance, has recently offered the following account:

In my own experience, I have seen economics change drastically over a period of three short decades, [...] At the time I was working on my dissertation, the best and brightest in these fields focused on applied theory, producing mathematical models that attempted to shed light on a particular facet of the economy. Evidence was used to motivate the model, and sometimes to buttress their results. But it was unusual to devote the bulk of the work to empirical analysis. Only the lesser students, the ones without bright ideas and theoretical skills, would attempt empirically testing this or that model. [...] The standards of the profession now require much greater attention to the quality of the data, to causal inference from evidence, and to a variety of statistical pitfalls. All in all, this has been good for the profession.

This is undoubtedly what is actually happening within the economics profession. The problem, to some extent, is what we can expect from such an “empirical turn” in economic research. Even if on the basis of admittedly scanty reading on these subjects, I must confess to be a bit less optimistic (to say the least) about what may we expect from this turn in economic research.

²⁵ Cf., for instance, HAMERMESH (2013) and EINAV and LEVIN (2013).

The standard format of papers in contemporary “applied economics” might be summarized, with the appropriate caveats due to possible differences in the various subfields, as follows: *i*) a “theoretical” conjecture; *ii*) an empirical “test” with regard to a “new” (and as big as possible) database; *iii*) some conclusions in which the conjecture is somewhat validated and possible “anomalies” are taken as requiring further research. It seems, methodologically speaking, as if we have moved from the “innocuous falsificationism”, which Mark Blaug four decades ago repeatedly protested against,²⁶ to a sort of “innocuous verificationism”.

I prefer this slightly different characterization for essentially two reasons. First, *verificationism* instead of falsificationism because the so called Duhem-Quine thesis (asserting the impossibility of precisely identifying the component in charge for a falsifying instance among an exceedingly complex and heterogeneous set of explicit theoretical conjectures, auxiliary hypotheses, initial conditions, implicitly assumed items of background knowledge, and so on) hampers in any case the feasibility of a serious falsificationist methodology in economics (cf. Salanti 1998). It is even more so concerning most of applied (micro)econometrics, given the huge amount of assumptions that making use of econometric tools inescapably involves. Second, *innocuous* because the problem of the underdetermination of scientific theories (cf. Stanford 2017) makes all too easy to “explain” any empirical result according to the theoretical presuppositions on whose basis the particular empirical application has been designed, not to speak of the examples of “duh” science reported below.

The abandonment of any falsificationist rhetoric is also detectable in Rodrik (2015), who goes as far as to say that:

In economics, context is all. What is true of one setting need not be true of another. [...] This reliance on multiple models does not reflect the inadequacy of our models; it reflects the contingency of social life. Knowledge accumulates in economics not vertically, with better models replacing worse ones, but horizontally, with newer models explaining aspects of social outcomes that were unaddressed earlier. Fresh models don’t really replace older ones. They bring in a new dimension that may be more relevant in some settings (67).

Although Rodrik’s thesis may well be open to methodological questions in several respects, it can be easily predicted that it is likely to become a sort of “mainstream” methodological view within economics in the next couple of decades. This because it provides explicit represen-

²⁶ See BLAUG (1976: 160 and 174) and BLAUG (1992: 111 and n. 34), where he acknowledged to have borrowed “this happy phrase [from] CODDINGTON (1975: 542)”.

tation of a point of view which may well fit generally shared feelings among applied economists. In fact they are no more interested neither in theoretical models pursuing generality first of all, even at the cost of highly abstract – and therefore unavoidably unrealistic – assumptions, nor in “caricature” models aiming nonetheless at identifying pervasive important mechanisms.²⁷

In order to support judgments of this kind a systematic survey of the relevant literature in the various subfields would be obviously needed, but this would evidently exceed the scope of this paper. For now, I can simply offer to the reader a few examples of applied research that I find totally irrelevant, in the sense that they do not add anything to what we might have known even before (and therefore without) doing such pieces of “applied research”. By this I mean that, if someone asked me – possibly at the pub – what should be expected as a result, I would give the right answer simply relying on common sense, possibly coupled with some understanding of basic economic theory. I quote from the abstracts, which are the places where authors usually try to show why their results may be of some interest for the prospective readers.

The first one deals with a traditional issue in international economics (*i.e.* intra-industry trade). The conclusions appear to be more than obvious if we assume that: *i*) consumers with higher disposable income prefer higher quality goods, and *ii*) the production of higher quality goods requires higher quality inputs, and that is all.

This paper examines the extent to which the destination of exports matters for the input prices paid by firms, using detailed customs and firm-product-level data from Portugal. We use exchange-rate movements as a source of variation in export destinations and find that exporting to richer countries leads firms to charge more for outputs and pay higher prices for inputs, other things equal. *The*

²⁷ As pointed up by AYDINONAT (2018), however, to provide singular explanations is a complex process that cannot be easily summarized. Anyway, “... provided that there is already a set of established theoretical models concerning a given explanatory task, we can summarize the process of moving from models to singular explanations in the following steps.

- (i) Determine the set of models that are relevant for the explanatory task,
- (ii) Assemble a list of possible explanations from the menu of possible explanatory factors which are suggested by this set of models,
- (iii) *Empirically verify* which of these factors are actually causing the fact or event to be explained,
- (iv) If available models fail to lead to a satisfactory explanation, look for other relevant models, or build new models to expand the menu of possible explanations, and
- (v) Repeat the preceding steps until a satisfactory explanation is found.” (p. 248, italics added).

On the necessity of a process of verification in this context see also GRÜNE-YANOFF and MARCHIONNI (2018).

results are supportive of the hypothesis that an exogenous increase in average destination income leads firms to raise the average quality of goods they produce and to purchase higher-quality inputs (Bastos et al. 2014: italics added).

A second example is provided by a study (through a survey of experiments conducted in six countries: Germany, France, Italy, Sweden, U.K., and U.S.A.) on how the perception of immigrants influence natives' preference for redistribution. Can somebody tell me what the following findings would add to our prior knowledge of the occurrence dealt with?

[...] We find strikingly large biases in natives' perceptions of the number and characteristics of immigrants: in all countries, respondents greatly overestimate the total number of immigrants, think immigrants are culturally and religiously more distant from them, and are economically weaker – less educated, more unemployed, poorer, and more reliant on government transfers – than is the case. While all respondents have misperceptions, those with the largest ones are systematically the right-wing, the non-college educated, and the low-skilled working in immigration-intensive sectors. Support for redistribution is strongly correlated with the perceived composition of immigrants – their origin and economic contribution – rather than with the perceived share of immigrants per se. [...]. We also experimentally show respondents information about the true i) number, ii) origin, and iii) “hard work” of immigrants in their country. On its own, information on the “hard work” of immigrants generates more support for redistribution. However, if people are also prompted to think in detail about immigrants' characteristics, then none of these favorable information treatments manages to counteract their negative priors that generate lower support for redistribution (Alesina et al. 2018, italics added).

A third case in point is about a topic that at first sight might appear as a typical product of economic imperialism. In any case it reaches a conclusion about “some complementarity between student ability and college quality”, which is plain common sense for everybody involved in education.

We consider the effects of student ability, college quality, and the interaction between the two on academic outcomes and future earnings using data on two cohorts of college enrollees drawn from the NLSY-79 and the NLSY-97. We find that student sorting has increased modestly between cohorts, and that student ability and college quality strongly improve degree completion and earnings. These patterns imply that, on average, students benefit from “overmatch” of the sort generated by affirmative action in admissions. We find little evidence of match effects on degree completion at eight years or on STEM degree completion, *but suggestive evidence of some complementarity between student ability and college quality in degree completion at four years and long-term earnings. Such complementarity implies a trade-off between equity and efficiency for policies that move lower ability students to higher quality colleges (Dillon and Smith 2017: italics added).*

The following two are taken from the field of experimental economics, one of the emerging subfield commonly praised by the advocates of “mainstream pluralism”. The reported findings are so trivial that I leave the reader to judge from herself:

The effect of expert opinion on demand for experience goods is difficult to quantify, as the relationship between purchases and reviews may be driven by product quality. Further, it is unclear whether a review-based demand effect is due to providing quality or existence information. Using a retail field experiment to overcome these obstacles, we find a significant positive average consumer response to expert opinion labels for wine. Demand decreases for low-scoring wines and increases for wines scoring average or higher. Results indicate that expert opinion labels transmit quality information as opposed to solely shelf visibility (Hilger et al. 2011, italics added).

We conduct an experiment with a representative sample of the Dutch population to study whether trustworthiness depends on the ethnicity of the interaction partner. Native Dutch trustees play with an anonymous trustor, who is either another native Dutch or a non-Western immigrant. We find that trustees reciprocate trust up to 13% less if the trustor is a non-Western immigrant than if he/she is native Dutch. This percentage increases up to 23% for trustees who report disliking ethnic diversity in an independent survey. Since the decision to reciprocate does not involve behavioral risk, our results provide evidence of taste-based discrimination (Cettolin and Suetens 2019, italics added).

Finally, this last one is added merely to confess that I myself am not, sometimes, innocent in this respect:

Many different carriers operating on the same route is usually regarded as a signal of a competitive setting and, therefore, as a situation potentially beneficial for customers in terms of lower prices. [...]. Across different routes, however, the number of carriers depends also on the level of demand for each particular pair of destinations, so that we cannot assume a priori that fares per kilometre on “monopolistic” routes are higher than on more “competitive” ones. We study the price policy during 2008 of the two main European low cost carriers, Ryanair and easyJet, with reference to one hundred of the least, and one hundred of the most, dense routes among those operated by the two carriers respectively. The systematic occurrence of higher (for Ryanair), or at least no lower (for easyJet), average prices on competitive routes if compared with prices on routes with a single carrier by the same airline, ..., supports the conclusion that a low level of demand is sufficient to impose low fares to some extent irrespective of the degree of competition (Malighetti et al. 2014, italics added).

FINAL REMARKS

Within recent methodological literature on economics a number of contributions have pointed at the rising in the last four decades or so of a so called “mainstream pluralism”, implying that mainstream economics is no more to be regarded as monolithic, and in particular monolithically “neoclassical”. At the same time it has been rightly observed that research in economics is becoming more and more “applied research”.²⁸

If taken as describing recent trends in economics, these contributions are surely interesting and not particularly open to question. Their authors cannot but be thanked for having shed some light on a number of issues, including – among other things – the future perspectives of heterodox approaches within economics.

Here and there, however, it seems to emerge a favourable appreciation of such trends, regarded as potentially suited to improve our knowledge of economic mechanisms actually at work. In this respect this paper is meant to express some caution, pointing out a couple of issues ostensibly in need of further reflections. The first comes from noticing, with reference the recent literature in microeconomics, a widespread reliance on partial equilibrium analyses, without its legitimacy being explicitly discussed. The problem of the legitimacy of partial equilibrium analysis, of course, is by no means new. However, traditional discussions aimed at finding *exact* (sufficient) conditions to be satisfied in order to assure its absolute consistency²⁹ do not help very much for the intended purpose in this context.

A second reason for caution is about the real worth of many pieces of applied research we may currently find in top journals or in working papers series of esteemed research organizations. Even without tarring all this kind of research with the same brush, it is undeniable that it is all too easy to find examples of applied works that attain very poor results in terms of really informative content. While twenty years ago the main target of (methodological) criticism could still be the sterile formalism of much of economic theory (see, for instance, Blaug 1999 and 2002), now the main sin seems to have become the irrelevance of much of “applied research” currently flourishing within economics. Just how to do economics during the formalist era mathematical expertise was more (or at least as) important than economic intuition, now econometric competence (favoured by the availability of ready-to-use software packages such as Stata

²⁸ Relevant references concerning both issues have been previously provided all along the paper.

²⁹ See footnotes 16 and 17 before together with the corresponding text.

or a bit more demanding ones such as Matlab) seems to be held in higher esteem, on the average, than the ability to identify really relevant economic issues.

To sum up: mainstream pluralism is probably doomed to become in any case a new chapter in the history of economic thought. As repeatedly happened in the past, however, progress in economics may nonetheless leave old questions unanswered and/or raise entirely new ones.

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