In Defense of Useless Economics

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Abstract: Although the assertion that economics should be as relevant as possible to real-world problems is uncontroversial when it comes to applied work, it is less persuasive when extended to economic theory. This paper contends that economic theorists ought to be exempt from the demand for immediate practical relevance for two reasons. First, research that appears useless in the short run can prove highly valuable in the long run. Second, the ultimate aim of economic theory is to help create a coherent picture of the world we inhabit, a goal independent of direct applicability. Moreover, because the activities of theorists and applied economists differ in important ways, economic theory and applied economics should retain their relative autonomy.

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1. INTRODUCTION

According to Adam Smith (1967, pp. 22-23), it is not practical considerations what marks the beginning of a science but an excitement of 'wonder' at something new or uncommon.[1] Although in general his observation is probably correct, the science, of which he is generally considered to be the founding-father, seems to be an exception. It has been usually dissatisfaction with the world around them that led people to study social phenomena; and they have done so expecting to find remedies to social problems, particularly, to that of poverty (Senior 1831; Hayek 1933; Stigler 1959; Robbins 1963; Blaug 2002; Bowles 2006). Indeed, Smith himself held the view that the "first object of Political Economy is to provide subsistence for the people" (Smith 2008, p. 395). This focus on practical issues is only natural, since for a long time a potential subject-matter for a science—something which could possibly excite purely theoretical interest (or 'wonder')—did not seem to exist at all.[2]

It was long believed that all social problems were caused by the ignorance or perhaps moral failures on the part of rulers. The *art* of Political Economy was then to provide instructions to the government that would be instrumental in ensuring prosperity of the nations; ruling a nation was thus considered analogous to the management of a household.[3] Nevertheless, the failures of social reformers revealed that there were some social 'laws' (or a 'natural order') at least to

some extent independent of human will (Mises 1996; Hayek 1933). This finding, together with the conviction of non-supernatural origin of the social 'laws' (Schumpeter 1954), opened the door to the science of Political Economy, which was to study these 'laws.'

It is not to say that with the emergence of the science of Political Economy the practical branch of it began to decline: quite the opposite. Instead of disinterested enquiry of the workings of the 'invisible hand' the theoretical study continued to be pursued above all (but not exclusively) to supplement the basis for practical decisions (Keynes 1999, p. 26; Marshall 1982, p. 2ff).[4] Economists desired to discover the laws of production and distribution of wealth to be able to make a use of them or perhaps even to control them—quite in line with Comte's dictum 'savoir pour prévior, prévoir pour pouvoir'.[5] As the result, the border between the pure and applied science remained blurred. Some economists wanted to separate the two by giving the name 'economics' to the pure science (Keynes 1999, p. 179fn); yet, the attempt was unsuccessful.

The demand for the practical relevance of economics remained high until today, without recognizing that the theoretical part need not (or perhaps even must not) be pursued with immediate applications in sight (Hayek 1933). Thus for instance Bowles (2006) claims that economics has always been about changing the way the world works and himself continues the tradition; for Varian (1994)—although he admits that "one could argue a reasonable case for economic theory on purely aesthetic grounds"—is economics primarily a 'policy science' analogous to engineering or medicine; Klein (1999) asks, whether economists are "led to promote ends of human betterment"; and according to Šťastný (2010) the failure of economists to be socially useful at least as garbagemen should be a source of embarrassment. There is only a handful of economists, who do not consider 'uselessness' (that is, inapplicability to practical problems) of economic theories a vice (Rubinstein (2007) would be an example at case).

Why is this the case? One explanation is that practically minded individuals tend to self-select into economics, while those who are more theoretically or philosophically inclined—unaware that economics can also pursue purely theoretical questions—gravitate toward disciplines such as mathematics, philosophy, biology, or physics. In addition, the broader academic trend over the past several decades has favored a shift from non-applied topics to more practical ones.[6]

First of all, there is no doubt that individuals disappointed with worldly affairs often seek refuge in the idealized world of pure theory (Romero and Klein 2007) provide an extensive list of various motives for escapism; however, this holds equally for economists as well as other scientists: for instance, English mathematician G. H. Hardy's turn to pure mathematics was presumably partly the result of his disgust that mathematics was applied in the warfare (G. H. Hardy 1992). In spite of the fact that the discoveries in natural sciences are often ill-applied, we do not observe, I believe, a massive shift towards non-applied issues. In fact, in line with my own claim, the contrary seems to be the case: as early as in the 1950s, Karl Popper complained about young overspecialized physicist, who ignored unpractical philosophical issues as unimportant; this attitude, to Popper's regret, "may easily lead to the end of science and its replacement by technology" (Popper 1992, p. 100).

In economics, the shift from fundamental to practical concerns can be traced through leading economists' attitudes toward the discipline's philosophical problems across historical periods. The classics—Senior (1831, 1854), Say (1836, 1840), Mill (1844, 1869), and Cairnes (1857)—devoted serious attention to supposedly 'useless' topics such as definition, scope, fundamental assumptions, and method. In their work, they valued explanation and understanding of social phenomena at least as highly as immediate practical application. Cournot (1971, p. 171) showed no frustration when he likened economists' influence on society to that of grammarians on language, and Gossen (1983, p. cxlvii) aspired to be the Copernicus, not the Galen, of social science. Accordingly, early Austrian economists concentrated on advancing theory, although the school later developed a distinct perspective on economic policy (Andersson and Hudik 2022; Leeson and Rouanet 2023).

More than a century later, Hausman (1984) complained that economics professed scorn for philosophizing, which at the same time he found unsurprising, given that very few economists practice it. And even though interest in philosophy and methodology of economic got some new vigor by Blaug's (1992)

well-known book, pursuing this subject remains to be a ticket to obscurity. Writing at about the same time as Hausman, Samuelson (paraphrasing G. B. Shaw) expressed the attitude of many economists when stating: "Those who can, do science; those who can't, prattle about its methodology" (Samuelson 1992, p. 240). In his Nobel lecture, the same author considered important to emphasize usefulness of economics for businessmen and bureaucrats, which was according to him achieved only in the second half of the 20th century (Samuelson 1972). This evidence, although admittedly scant and anecdotal, suggests no shift towards pure theory and fundamental issues of the science at the expense of applicability in the last two hundred years; on the contrary, it may be—together with Popper—feared that scientist's (and philosopher's) 'wonder' will eventually be replaced by the want for usefulness of a practical individual.

It seems non-controversial to say that those engaged in applied economics should strive for maximum relevance; it is arguably more difficult to defend the view that there is nothing wrong with economic theorists pursuing research with no direct or indirect applications. The purpose of this paper is precisely to make such a case. One argument has been suggested by Hayek, according to whom "too deliberate striving for immediate usefulness" is "likely to corrupt the intellectual integrity of the economist" because "immediate usefulness depends almost entirely on influence, and influence is gained most easily by concessions to popular prejudice and adherence to existing political groups" (Hayek 1933, p. 40).

Hayek's argument, although perhaps valid, applies to social sciences only; I shall pursue a different course here and focus on more general arguments in favor of 'useless' theorizing. Specifically, I argue that research that appears to be useless in the short run can turn out to be useful in the long run. Hence, to limit research only to fields with direct and obvious applicability can be short-sighted since uselessness is only apparent. Furthermore, I suggest that the ultimate aim of economic theory is to help us to understand the world around us and that this aim has nothing to do with practical applicability. Improved understanding is achieved through the process of explanatory unification. However, as theory becomes more general and 'abstract', it is necessarily less 'relevant' to everyday experience. Real-world relevance is the primary goal of applied economists and only an indirect concern for economic theorists. I therefore distinguish the activities of theorists and applied researchers and argue that economic theory and applied economics should remain separate spheres.

2. A PRACTICAL CASE FOR USELESSNESS

The first argument in favor of 'useless' research is presumably easy to accept even for highly practical individuals: what seems useless at one point in time may later prove to be extremely useful—sometimes after a period so long that it far exceeds the average human lifespan. Whitehead (1911) provides several examples: first one is Faraday's law of induction discovered in the early 1830s. When asked what the use of his discovery was, Faraday answered: 'What is the use of a child—it grows to be a man.' And as Whitehead (1911, pp. 34-35) adds: "Faraday's child has grown to be a man and is now the basis of all the modern applications of electricity."

His next example is even more striking:

> No more impressive warning can be given to those who would confine knowledge and research to what is apparently useful, than the reflection that conic sections were studied for eighteen hundred years merely as an abstract science, without a thought of any utility other than to satisfy the craving for knowledge on the part of mathematicians, and that then at the end of this long period of abstract study, they were found to be the necessary key with which to attain the knowledge of one of the most important laws of the nature (Whitehead 1911, pp. 136-137).

To stay within mathematics, we may cite G. H. Hardy who had, in his own words, "never done anything 'useful'" (G. H. Hardy 1992, p. 151); yet, he contributed to (relatively) applied field of population genetics with what is now known as Hardy-Weinberg law.[7]

These examples suggest that when science is pressured to focus solely on directly applicable ideas, it tends to underperform relative to its true potential. Such a decline in intellectual productivity appears to have been evident in the past, particularly, when the Greek culture was replaced by Roman, as again beautifully documented by Whitehead, this staunch advocate of 'useless' theorizing:

> The death of Archimedes by the hands of a Roman soldier is symbolical of a world-change of the first magnitude: the theoretical Greeks, with their love of abstract science, were superseded in the leadership of the European world by the practical Romans. Lord Beaconsfield, in one of his novels, has defined a practical man as a man who practices the errors of his forefathers. The Romans were a great race, but they were cursed with the sterility which waits upon practicality. They did not improve upon the knowledge of their forefathers, and all their advances were confined to the minor technical details of engineering. They were not dreamers enough to arrive at new points of view, which could give a more fundamental control over the forces of nature. No Roman lost his life because he was absorbed in the contemplation of a mathematical diagram" (Whitehead 1911, 40-41).

All the examples provided above concern mathematics and natural sciences; are there examples from economics as well? One candidate is the auction theory that was elaborated as an application of the game theory. Game theory is a descendant of Lasker's and Zermelo's investigations into chess and although already in the 1940s RAND supported game theoretical research with a hope of warfare applications (Leonard 2010), this hope eventually remained largely unfulfilled: game theory continues to be for the most part conceptual framework for the study of decisions that affect each other (Rubinstein 2007; Leonard 2010). Yet, auction theory, developed from the game theoretical foundations, was successfully applied, for example, in the sale of 3G mobile phone licenses in Great Britain (Binmore and Klemperer 2002) or licenses to use electromagnetic spectrum for personal communication services (PCS) in the USA (McMillan 1994; McAfee and McMillan 1996; Cramton 1995, 1997, 1998). It thus appears that even in economics can sometimes 'useless' theorizing turn out to be useful in the long run.[8]

3. A PHILOSOPHICAL CASE FOR USELESSNESS

I now turn to my second argument: I suggest that economics helps to explain and understand the world around us and together with other sciences is part of cosmology—an endeavor to "paint a coherent and understandable picture of the Universe" (Popper 1992, p. 1). What I mean is that economics should attempt not only to explain *concrete* phenomena but more importantly to show, how seemingly unconnected explanations can be considered manifestations of the same underlying principles. This process may be called explanatory unification (Mäki 2001) or (scientific) reduction (Popper and Eccles 1983).[9] In his *History of Astronomy*, Adam Smith, comparing a scientific or philosophical system to a machine, described the explanatory unification as follows:

> The machines that are first invented to perform any particular movement are always the most complex, and succeeding artists generally discover that, with fewer wheels, with fewer principles of motion, than had originally been employed, the same effects may be more easily produced. The first [philosophic/scientific] systems, in the same manner, are always the most complex, and a particular connecting chain, or principle, is generally thought necessary to unite every two seemingly disjointed appearances: but it often happens, that one great connecting principle is afterwards found to be sufficient to bind together all the discordant phenomena that occur in a whole species of things (Smith 1982, p. 66).

I now present two examples of this unification in economics. First example is the development theory of price formation from the classics to the present. Ricardo's (2005) cost-theory of price focused on the expla-

nation of long-run prices of reproducible goods, although he leaves the impression that he would be able to provide more comprehensive theory (Marshall 1982, p. 670ff). Nevertheless, he failed to do so and the task had to be taken up by Marshall (1982), who was already equipped with marginal utility theory that Ricardo lacked. He added more emphasis on the demand side, which opened the door to the analysis of the short run. Yet, he still let the costs of production co-determine the price.

Böhm-Bawerk (2006) and Wicksteed (1957), building on earlier contributions of Jevons (1965) and Menger (1950), argued that ultimately, it was only valuations of market participants what determined the price. They thus established the most general model of price formation that could account not only for the prices of goods but for *all* prices, including those of non-reproducible goods and of factors of production. Later it was shown by von Neumann and Morgenstern (1953) that the model of price formation can be thought of as an instance of even more general theory of coalitional games, which has much wider applicability in economics (Telser 1994; Moulin 1995), as well as in other social sciences (e.g., in political science (Austen-Smith and Banks 2000, 2005).

My second example concerns the development of the demand theory, specifically, the explanation of downward sloping demand. The stage was set by Marshall (1982), who explained the negative slope of demand with diminishing marginal utility (Hudik 2020). This explanation, however, required the assumption that consumers are able to rank differences of utilities; Hicks and Allen (1934a, 1934b) and Hicks (1946, 1986), elaborating on the insights of Pareto (1971) and Slutsky (1998), have shown that we can dispose with this assumption and still obtain all important results of the demand theory. Finally, Samuelson (1974) proved that downward sloping demand is the implication of consumer making consistent choices. Samuelson's theory was then transformed into a general choice theory now used both, within and outside economics.

Note that the more unification advances, the less relevant to the world of our immediate experience (more 'abstract') theory appears. Whereas Böhm-Bawerk's investigations of marginal pairs or Marshall's discussion of marginal utility are easily comprehensible and clearly related to the problem at hand (i.e., formation of price and downward sloping demand, respectively), Samuelson's and Houthakker's examination of integrability can seem remote, and only specialists can trace the route from such high-level principles to the concrete models used by practitioners.

4. THEORY AND HISTORY

Morrison (2000) and Hodgson (2001) point out that there is a trade-off between unification and the 'informative content' of a theory, and they claim that this unification has limits. This trade-off implies that 'useless' theoretical research must be complemented by 'useful' historical research. Whereas a theorist's goal is explanatory unification—focused on universal statements—a historian seeks explanations of concrete events—focused on singular statements—such as the Great Depression or U.S. unemployment in 2009. Theory and applied work should not be conflated: as Popper (1979) has already shown, a historian, unlike a theorist, takes theories as given (he does not test them) and instead examines the presence or absence of initial and boundary conditions. Hayek (1955) argues that this is the typical approach not only in economics but in all disciplines that study complex phenomena.

Consider, for example, Milgrom et al.'s (1990) attempt to explain the success of the Champagne Fairs that began in the twelfth century. The authors observed the absence of strong political authorities in France and, assuming the validity of a game-theoretical model, showed that a system of private judges could sustain markets without public enforcement. Their task was to identify the initial conditions and apply an appropriate model; they did not test the underlying theory.

Their explanation would be false if either a) the conditions they posited never existed—precisely the point raised by Edwards and Ogilvie (2011), who argue that political authorities were in fact present and actively enforcing property rights, providing security, and building infrastructure—or b) it were shown that people generally do not play subgame-perfect Nash equilibria in indefinitely repeated games, which

would invalidate the model's theoretical foundation. Demonstrating the latter would amount to an unintended test of theory, but conducting such a test is not the historian's aim.

If this account of what theorists and historians do is correct, then McCloskey's claim that economics is a "proper subset of history" (1986, p. 64) does not hold; theorists and historians address fundamentally different questions. Both groups should work on clearly defined problems, yet a theorist typically tackles conflicts among universal statements or mismatches between theory and evidence, whereas a historian confronts problems that arise directly from concrete events in the real world. Although history may seem more useful because it engages directly with concrete problems, it cannot be adequately pursued without theory, which helps connect the dots (Mises 1985; Leeson and Boettke 2006).

5. THEORY AND POLICY

In the previous section, I argued that the work of a theorist differs from that of a historian. Explaining historical events with theory is only one form of applied research; another is policy design. Accordingly, this section examines the difference between the roles of theorists and policy advisors. In fact, a policy advisor's role closely resembles that of a historian: both take existing theories as given and look for the presence or absence of specific initial conditions (Popper 1979).

To give an example, suppose a policy advisor is designing a tax system. His goal may be to achieve a particular income distribution or to maximize tax revenue subject to specified constraints. Before he adopts a specific tax scheme, he must examine factors such as the possibility of migration, possible differences in individual preferences, the nature of the economy's returns to scale, market structure, and demand- and supply-elasticities. The appropriate scheme—whose theoretical foundations the advisor does not question—is chosen on the basis of empirical investigation of these initial conditions. Some factors (for example, the nature of individual preferences) are often very costly to determine, and the choice of an appropriate system sometimes depends on which conditions can actually be measured. If the chosen scheme fails to achieve its intended goals, the fault lies either with the initial conditions or with the theory—that is, the theory was misapplied or it is incorrect. Nonetheless, as with historical explanation, it must be emphasized that improving theories is not the primary concern of economic policy: for both the policy advisor and the historian, theories are instruments to be applied more or less appropriately.

To summarize the difference between a policy advisor and a theorist, the former takes both the problem to be solved (the policy goals) and the relevant theories as given, and then seeks to determine the initial conditions. The latter likewise accepts the problem itself as given; nonetheless, he does not accept theories uncritically. On the contrary, his task is to replace them with more adequate ones—often by manipulating the initial conditions to delineate the theory's domain of applicability (Popper 1979).

Whereas a policy advisor must engage in empirical research to discover which particular theory applies in a given case, much theoretical work is armchair work: deriving the explanandum from an ever-smaller set of assumptions. Although cooperation between theorists and policy advisors can clearly benefit both sides (the theorist supplies the instruments, and the advisor indicates where those instruments may fail), they are distinct roles and should be recognized as such.

6. CONCLUSION

This paper set out to defend the space for work that seems 'useless' today yet advances our understanding of economic phenomena. I first showed that several discoveries in mathematics and economics were born as curiosities and only later revealed their practical power. Compressing inquiry into projects with an obvious, near-term payoff risks repeating the Roman preference for engineering tweaks over Archimedean breakthroughs, and thus throttling science's generative capacity.

Second, I argued that economic theory plays an indispensable integrative role among the sciences. By stripping away context-specific detail, theorists seek explanatory unification, binding disparate phenomena

under ever more general principles. Inevitably, this abstract work feels remote from everyday experience, but its purpose is not immediate utility; it is to forge the conceptual links that make the world intelligible to us.

Third, I distinguished the tasks of theorists and applied researchers, such as historians, and policy advisers. Applied researchers accept existing theories as tools, investigating whether the initial and boundary conditions required for those tools to work are actually present; theorists, by contrast, question and refine the tools themselves. Confusing these distinct roles should be avoided. In particular, theorists should be spared demands practical relevance: their task is to clarify first principles and they should be allowed to pursue it as effectively as possible.

NOTES

1 See also Hayek (1967, pp. 22-23). Veblen (1919), when addressing this classical ideal of disinterested pursuit of truth, so dramatically expressed by Democritus ("I would rather find a single causal law than be the king of Persia!"), would talk about 'idle curiosity'.

2 However, Whately (1847, p. 97) reports how this sentiment was excited in Moyhanger, the first New Zealander to visit England, who was "struck with especial wonder, in his visit to London, at the mystery, as it appeared to him, how such an immense population could be fed; as he saw neither cattle nor crops. Many of the Londoners, who would perhaps have laughed at the savage's admiration, would probably have been found never to have even thought of the mechanism which is here at work." Europeans experiencing the expansion of markets in the nineteenth-century possibly also wondered about its operation, as documented by Thomas Hardy through a character in his novel *Tess of the d'Urbevilles* (T. Hardy 2007, p. 240). See also note 4 below.

3 Writing already in 1821, James Mill still maintained that "Political Economy is to the State, what domestic economy is to the family" (J. Mill 1844, p. 1).

4 Newcomb (1966, p. 9), according to whom there is "nothing in the wonders of the heavens or the mysteries of chemical combination better fitted to kindle our curiosity, and to gratify our desire to understand what is going on around us, than the study of the social organism," seems to have been an exception.

5 Notwithstanding this, economists have done little of actual 'preaching' (read: policy-recommending) in their professional works—at least if one finds evidence provided by Stigler (1980) sufficient to substantiate this claim.

6 The progressive 'imperialism' of the utilitarian reason over the personality of a modern man has been observed long ago. The classic works here are Schumpeter (2006), Ortega y Gasset (1950) and – from belles-lettres literature—Huxley (2007). Nonetheless, it has also been suggested that in fact, the opposite tendency was observable in economics: that there had been a shift away from policy relevant issues towards pure theory; this shift is said to be caused by the frustration of economists stemming from their limited influence on practical policy (Šťastný 2010, p. 66).

7 Ortega y Gasset (1950, p. 60) even claims that technical requirements are only "useful, practical precipitate of superfluous, unpractical activities."

8 It was suggested to me by Pavel Pelikán that there may be an evolutionary advantage to 'idle curiosity', which can explain the existence of this sentiment. The logic behind it is that some piece of information can gain new function in a new context. If collecting of information is not too costly and if the probability that a piece of temporarily useless information will increase fitness in the future is sufficiently high, it is advantageous to possess a capacity to collect information in excess to what is directly useful.

9 Mäki (2001) distinguishes between two versions of explanatory unification: derivational and ontological. Roughly speaking, the former can be denoted as Kantian or impositionist, the latter as Aristotelian or realist. For the purposes of the present article it is immaterial which of the two versions is adopted.

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