Chapter 2

An Essay on Humble Economics

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The man who claims for economic science much exactitude is a quack.

L. Robbins ([1927](#), 176).

Abstract The purpose of this essay is to show that the departure from the Walrasian branch of neoclassical economics could lead to conceptualization of economics as a humble science, i.e. a science that is aware of its explanatory capabilities, but also convinced that discovering unambiguous regularities is not possible. Humble economics does not claim the power of formulating explanations which will not need further justification. It is based on an ontology of potentialities where one does not find laws, but capacities, tendencies, natures, and Aristotelian *dynamis*. It is based on the recognition that there is an irremovable mystery inherent in the functioning of the economy.

Faculty of Economic Sciences, University of Warsaw, and a member of the Monetary Policy Committee at the National Bank of Poland. This research was financed by a research grant within the framework of The National Programme for the Development of Humanities (NPRH) (grant no 2bH 15 0266 83).

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P. Róna et al. (eds.), *Words, Objects and Events in Economics*, Virtues and Economics 6, https://doi.org/10.1007/978-3-030-52673-3_2

2.1 Introductory Remarks

Economics refers to laws, causes, regularities, mechanisms, or dependencies of a purely probabilistic nature. These issues are, however, the subject of hot disputes among economists themselves. Many of them believe that the explication of economic phenomena through their subsumption under laws does not make sense, while others would like laws in economics to resemble the status of almost universal laws of nature.¹ L. Walras was the first to claim that “[...] pure theory of economics is a science which resembles the physico-mathematical sciences in every respect” (Walras 1874/1984, 71), and then, almost 80 years after the publication of Walras’ *Éléments d’Économie Politique Pure*, K. Arrow and R. Debreu offered a mathematically expressed proof of the existence of an equilibrium for a competitive economy. Their paper was published when the deductive-nomological model of explanation was at its peak of popularity. Therefore, it was only a step away from projecting economics as the physics of the social sciences. However, in the late 1960s economics started to become more and more diverse, and in the philosophy of science itself, the Hempel-Oppenheim model began to falter at its foundations. A discussion has begun on whether explaining empirical phenomena by subsuming them under general laws is appropriate at all.

Many important changes in economics took place in the 1970s. In this exceptional period a cognitive turn impacted economic science, key architects of the general equilibrium framework proved that macrophenomena are not easily reducible to micro ones, and thus the dominant mechanistic perspective has slowly begun to be questioned.² Economists such as H. Simon, D. Kahneman, A. Tversky, and V. Smith, in a sense, accomplished the unfulfilled dream of A. Marshall who wanted to make economics more rooted in the biological sciences.³ K. Arrow himself said many years later:

[...] the very notion of what constitutes an economic theory will have to change. For a century, some economists have maintained that the biological is a more appropriate paradigm for economics than equilibrium models analogous to mechanics. [...] economic theory may well take an analogous course (1995, 1617–1618).

The changes in neoclassical economics at the end of the twentieth century gained such a momentum that D. Colander in his insightful 2000 paper wrote that

¹For the sake of simplicity, I equalize such laws to universal regularities (of the kind: if X , then Y) that are omnitemporally and omnispacially true, being at the same time characterized by a high level of necessity (Hardt 2017). Also, such laws are not equal to moral natural laws.

²Here I refer, among other factors, to the implications of the Sonnenschein–Mantel–Debreu theorem which states that aggregate demand functions that are built on individual preferences and other neoclassical postulates do not meet the weak axiom of revealed preferences (WARP). And since WARP is violated, then on the aggregate level it is impossible to prove the existence of an unique and stable equilibrium. Hahn (1995) saw the SMD theorem as the most devastating attack on micro-founded neoclassical economics.

³Here I refer to well-known words by Marshall: “The Mecca of the economist is economic biology rather than economic dynamics” (1898, 43).

neoclassical economics is dead.⁴ The message of his text was that economics is so diverse and pluralistic that it makes no sense to use the term ‘neoclassical economics’ which is associated with the general equilibrium framework, and it is better to just talk about mainstream economics. The years that followed witnessed a further expansion of economic explanandum and explanans and, as a result, disputes erupted over whether the limits of economics are to be defined by its methods or a set of problems to which economics can be applied. Heated discussions about the status of neuroeconomics are a case in point.⁵

The above provokes the following question – was the Walrasian neoclassical economics only a short-term interlude in the development of economics which otherwise has a humble view of its research capabilities and communicates to the public that it can only acquire knowledge about contingent tendencies, causes, or natures of things, but not about universal and constant laws of nature? Classical economics was to a large extent such a humble science as it is illustrated by the well-known statement of N. Senior that “[The economist’s] conclusions, whatever be their generality and their truth, do not authorize him in adding a single syllable of advice” (1836/1951, 2–3), or J.S. Mill’s claim that economic laws are statements of tendencies only. Classical economists were much more careful in giving their opinions binding force than their neoclassical successors (Colander 2011).

The widespread use of formal models by neoclassical economists meant that the positivist understanding of science became less and less aligned with their research practice. Since modelling has become a *signum specificum* of economics, its methodological appraisal should include references to different ways of studying science, namely the ones subscribing to a model-based ideal of science, rather than to the well-established ideas of science built on laws of nature. This also makes it necessary to move away from Newton’s deterministic mechanics and implies the need to face a multi-layered world in which unobservable factors play a key role.⁶

The purpose of this essay is to show that the departure from the Walrasian branch of neoclassical economics leads to the conceptualization of economics as a humble science, one that is aware of its explanatory capabilities, but also convinced that discovering unambiguous regularities is not possible. This humble economics does not claim the power of formulating explanations which will not need further justification. In other words, it is simply not possible to remove the mystery which is inherent in the functioning of the economy.

⁴It was even P. Samuelson, the so-called *Mr. Neoclassical Economics*, who claimed in 1976 that “To talk about neoclassical theories has certain implications. It is more appropriate to talk about mainstream economics” (in: Pizano 2009, 117). So, even he was somehow influenced by a changing spirit of economics in the 70’s.

⁵See, e.g., a special issue of *Journal of Economic Methodology* (2010, no 2) dealing exclusively with the methodological status of neuroeconomics and its explanatory potential.

⁶We can even treat the unobservable as an anathema to many empiricist conceptions of science. Also, allowing the entry of unobservables into science makes metaphysics necessary in philosophical accounts of science (Chakravartty 2007, 16).

The essay is organized as follows. First, some preliminary insights concerning the way I understand a humble science are offered. Next, some reflections on the classical roots of modern economics are presented. It is shown that economists such as A. Smith, R. Ricardo, J.S. Mill, and A. Marshall were against dogmatism in economics. What follows is an analysis of metaphysics making its way back into philosophical reflection on economics. Next, I comment on D. Colander's essay on *Creating Humble Economists* (2016) and I propose that the lack of humility in economics cannot be helped by trying to persuade economists to behave like engineers (as it is suggested in Colander's paper) but rather by sticking with N. Cartwright's metaphysically rich statement that "Our most wide-ranging scientific knowledge [in this case knowledge about the economy] is not knowledge of laws but knowledge of the natures of things" (1999, 4). Conclusions follow.

2.2 Disentangling the Idea of a Humble Science

Science is about knowing the world. It is important, however, to realise that it is not an unlimited knowledge. Sticking to a position that science alone can explain every aspect of existence leads one inevitably to scientism which is a form of scientific dogmatism. Its proponents often believe that such a position can be made from within science. They are wrong. Statements about science are not scientific statements but rather meta-scientific ones and thus when reflecting on the scope of science one should benefit from insights from the philosophy of science.⁷ And here, for instance, we can refer to K. Popper's claim that "It is important to realize that science does not make assertions about ultimate questions — about the riddles of existence, or about man's task in this world" (1978, 342). So, first of all, a humble science does not claim monopoly in providing us with knowledge on how the world works: "there is more to the physical world than has met the scientific eye" (Polkinghorne 1990, 88). Also, as L. Kołakowski wisely claims, "But although we cannot pierce the mystery and convert it into knowledge, our awareness that there is mystery is in itself important; although we cannot tear the veil from ultimate reality, we should know that such a veil exists" (2001, 10). Therefore, I disagree with people like P. Atkins (1994) who states that "There is nothing that cannot be understood" (1). But I shall not focus too much on studying the nature of general statements about science. My aim is rather to discuss the character of knowledge about the world that science (here economics) as such gives us.

⁷J. Lenox puts emphasis also on the fact that scientism contradicts itself: "The statement that only science can lead to truth is not itself deduced from science. It is not a scientific statement but rather a statement about science, that is, it is a meta-scientific statement. Therefore, if scientism's basic principle is true, the statement expressing scientism must be false. Scientism refutes itself. Hence it is incoherent" (2009, 43).

If our aim is to analyse the nature of scientific knowledge, then we should move towards epistemology.⁸ Here questions of how we know things and how we can know that we know them emerge and they can be studied from various perspectives offered by numerous theories of knowledge. The traditional dilemma of knowing also applies: are we able to know the world for certain (absolutism) or can we just know it from a mere perspective (relativism) and, therefore, nothing is known with absoluteness or certainty (scepticism)? Simply speaking there is a long tradition in Western thought to see this very dilemma as a matter of all or nothing, as a choice between dogmatism and scepticism. However, there is also a short history of thought that searches for some middle ground in this debate. My take on a humble science subscribes to this latter tradition.

Since the subject of this paper is economics, there is a particular justification to refer to Hume while studying the issue of epistemological humility: apart from being one of the most important philosophers studying the nature of human understanding, he was also a crucial source of inspiration for Adam Smith. At the very end of *An Enquiry Concerning Human Understanding* we can find the following description of human beings as inherently epistemologically humble: “[People] must act and reason and believe; though they are not able, be their most diligent enquiry, to satisfy themselves concerning the foundation of these operations” (Hume 1748/2004, 104). In Hume’s case his epistemological humility is strongly intertwined with his ontological humility: “Nothing in the world is perpetual. Everything, however, seemingly firm, is in continual flux and change. The world itself gives symptoms of frailty and dissolution” (1777/2006, 603). As Holland (2013, 31) writes, “[...] no one in the European philosophical tradition has a keener sense of the limits of human knowledge than David Hume”. Therefore, knowing something for certain is simply impossible for him and the only thing one can achieve is to have “a degree of belief, which is sufficient for our purpose” (1740/2000, 122). However, a belief which would be free from any doubts is hardly possible: “Belief, being a lively conception, can never be entire, where it is not founded on something natural and easy” (ibid.). Interestingly, Hume claims that beliefs are somewhere in-between knowing something for certain and knowing something with a given degree of probability only. However, Hume’s beliefs are subjective, and they are based on impressions about the causal structure of the world. Therefore, they are always subject to error, since we only *feel* a certain necessity in the causal connection between empirical states.

Hume’s introduction of beliefs into epistemology was followed by various thinkers offering this category a legitimate place in modern philosophy of science. Importantly, for instance, Kant in his *Critique of Pure Reason* offers three categories of human understanding, namely opinion, knowledge, and belief, and thus writes:

⁸ It does not, however, mean that we are to commit a kind of ‘epistemic fallacy’, namely a reduction of ontology to epistemology.

Holding for true, or the subjective validity of a judgment in relation to conviction (which is, at the same time, objectively valid), has the three following degrees: opinion, belief, and knowledge. Opinion is a consciously insufficient judgment, subjectively as well as objectively. Belief is subjectively sufficient, but is recognized as being objectively insufficient. Knowledge is both subjectively and objectively sufficient (1781/1996, 749).

Here, as in Hume, belief is subjective. Nevertheless, Kant is a little less pessimistic than Hume since what we know about a given thing is a result of an interaction between our experience of this very object and its “thing-in-itself”. Instead of delving further into Kant, I shall move to Charles Peirce who (together with W. James) founded pragmatism - a philosophical movement claiming that “the rational justification of scientific beliefs ultimately depends on whether the method generating the beliefs is the best available for advancing our cognitive goals of explanation and precise prediction” (Almeder 2014, 103). Here is Peirce’s famous passage about the role of beliefs:

Doubt is an uneasy and dissatisfied state from which we struggle to free ourselves and pass into the state of belief; while the latter is a calm and satisfactory state which we do not wish to avoid, or to change to a belief in anything else. [...] the irritation of doubt causes a struggle to attain a state of belief. I shall term this struggle *inquiry* (1877, 66–67).

Such world view is often criticized for rejecting the very notion of truth and focusing solely on the utility of beliefs (see, e.g., Rorty 1995). I think that we may let pragmatists defend so strongly the utility of beliefs but at the same time claim that at least some of these beliefs are true. Also, true beliefs do not have the status of Humean regularities, e.g., believing that lower interest rates should lead to higher investments can still be true even if in a given situation (say in Poland in 2019) this is not the case.⁹ What is very important in pragmatists’ stance on the character of knowledge is that the way beliefs are formed matters for their justification.

I hope that it is now clear that insights given by Hume, Kant, and Peirce can be seen as supporting the ideal of a humble science, one that accepts fallibility of some of its claims. Or, to be more precise, a humble science does not reject the possibility that some of its statements may just be wrong. The character of humble science can also be illustrated by contrasting it with its opposite, namely a science characterized by vices, including arrogance, vanity, conceit, egotism, grandiosity, pretentiousness, snobbishness, impertinence (presumption), haughtiness, self-righteousness, domination, selfish ambition, and self-complacency (Roberts and Wood 2003, 258). If the way we practice science matters for its success (as pragmatists claim), one may ask whether humble scientists are always to opt for a humble science. Here we are approaching virtue epistemology where many authors claim that intellectual humility serves as a prerequisite for a humble science. Hazlett (2012, 220) states that intellectual humility is “the disposition not to adopt epistemically improper higher order epistemic attitudes”. Also, but in a slightly different context, Whitcomb

⁹This can be so because inference can be of a purely probabilistic nature, disturbing causes can be at place, etc.

et al. (2015) claim that being a humble scientist means to know one's self-limitations and thus being open to criticism. Roberts and Wood (2003, 272) nicely describe a humble researcher by portraying her antithesis:

The intellectually vain person is overly concerned with how he 'looks' to the people who count: he wants to impress, and is very concerned not to look silly at conferences and in front of his bright students. This concern may incline him not to admit, and maybe not even to notice, when someone has raised a good objection to his views. It may also incline him to fudge arguments when he thinks he can make them look good enough to get away with. The intellectually vain person may be genuinely concerned to accomplish intrinsic epistemic ends: to figure out what's what and to give his students a good education. But he also has the extrinsic concern to look good intellectually, and we are saying that this is in general an epistemic liability. By contrast, the lack of concern to look good frees the intellectually humble person to pursue intellectual goods simply and undistractedly (think of G. E. Moore).

Saying that „everyone nowadays is, I take it, a fallibilist about scientific theories” (Worrall 1989, 268) does not mean that we cannot approach the truth about the world and that there are no places where Humean regularities can always be true. They can hold, for instance, in theoretical models. What I claim is that vices like pride, haughtiness, and conceit can block any scientific progress since one can just conclude that he knows everything about the world and no further scientific inquiry is necessary. In other words, pride leads to intellectual laziness. But humility, on the contrary, makes people curious about the way the world works and therefore “it is humility that makes men as angels” (St. Augustine), and hence as McCloskey (2006) stresses when citing T. Merton: “Humility is a virtue, not a neurosis. A humility that freezes our being and frustrates all healthy activity is not humility at all, but a disguised form of pride” (1956, 55). One can also refer to Saint John Paul II's lecture at the Jagiellonian University in Cracow in 1997: “Reason should act and be active in the spiritual climate of moral virtues, like honesty, courage, humbleness, and genuine concern for human beings” (St. John Paul II 1997/2006, 988). And in *Fides et Ratio* he adds: “Human wisdom refuses to see in its own weakness the possibility of its strength; yet Saint Paul is quick to affirm: ‘When I am weak, then I am strong’ (2 Cor 12:10)” (no 23).

At the very beginning of this section I wrote that a humble science is a kind of a middle ground between dogmatism and scepticism. Now, I can add to the above that intellectual humility can be treated as an Aristotelian mean state between absolutism of certitude and timidity of incertitude. Therefore, humility is not only a moral virtue but also an epistemic one. In any case, they are intertwined since being a humble person makes it easier to be a humble scientist who can practice a humble science. In the following section I will look at classical economists and show that economics they practiced can be treated as an example of a humble science.

2.3 Classical Economics as a Humble Science

When focusing on classical economics, one usually starts from the analysis of Adam Smith's works. However, in order to understand Smith, we should take note of D. Hume and his reflection on the character of knowledge economics offers us.¹⁰ We did it partially in the previous section, but now we would like to be more precise and focus solely on his economics.¹¹ Hume used a lot of thought experiments in his economic research. As Massey (1991) comments "[...] perhaps no other philosopher has conducted his thought experiments with the degree of care and sophistication that Hume bestowed on his" (293). Also, Schabas concludes her paper on models in Hume's thought by writing that "I have not yet found such a concentration of examples [of thought experiments] since the ones devised by Hume" (2008, 168). What then do Hume's thought experiments look like? Here I would like to refer to one of his most important experiments, namely the analysis he presented in the *Essay of Interest* (1758/1993, 181):

For suppose, that, by miracle, every man in Britain should have five pounds slip into his pocket in one night; this would much more than double the whole money that is at present in the kingdom; and yet there would not next day, nor for some time, be any more lenders, not any variation on the interest (emphasis added).

Referring to the "miracle" means that we are dealing here with an imaginary situation, a kind of thought experiment, and not a precisely defined and isolated model.¹² Moreover, this magical appearance of additional money in the pockets of British subjects indicates that Hume analyses solely the influence of monetary variables on the economy – only they have meaning, and other factors are either fixed or absent (*ceteris paribus*). This is important because it served him to show the neutrality of money. Explaining empirical facts through thought experiments presupposes that the mechanisms described in them have their counterparts in the economy. It is difficult to find in Hume's thought anything that better supports this thesis than the following words:

¹⁰There is a huge amount of literature on Hume-Smith connection and presenting it in details is not necessary here, however, I would like just to cite Smith from his letter written to W. Strahan shortly after Hume's death: "I have always considered him, both in his lifetime and since his death, as approaching as nearly to the idea of a perfectly wise and virtuous man, as perhaps the nature human frailty will permit" (Smith 1776, cited in: Rasmussen 2018, 51).

¹¹Writing about Hume's economics does not mean that he can be undoubtedly called an economist. He was definitely a moral philosopher and only later started to be described as an economist (Schumpeter 1954/2006, 120).

¹²Although models and thought experiments are similar in many respects, one should notice also important differences. First, models are more decoupled from their targets than thought experiments. Second, such experiments (at least in Hume's interpretation) should refer to empirical targets that are at least potentially possible; models, on the other hand, can describe fairy-tale worlds. Third, thought experiments' insights are often illustrated by some references to empirical domains, but the ones of models are usually discussed in purely theoretical schemes or pictures (Schabas 2008).

Now it is evident that the same causes which would correct these exorbitant inequalities, were they to happen miraculously, must prevent their happening in the common course of nature, and must forever in all neighboring nations, preserve money nearly proportional to the art and industry of each nation (Hume 1758/1993, 191).

The first fragment of the above paragraph from Hume's work refers to the situation of the thought experiment, whereas the second part points out to empirical reality, hence the use of the phrase "nearly proportional", not simply "proportional". The above shows that Hume willingly went from analysis in terms of a thought experiment to the study of the surrounding empirical phenomena. In his text *On Public Credit* (1758/1993) we can find an interesting reference to the then Great Britain: "[the situation] to which Great Britain is visibly tending" (211).¹³ What is always true in the thought experiment, because it happens "miraculously", namely in isolation from other potentially disturbing causes, in reality takes place in a tendency only. To be more precise, a given cause produces a tendency to a particular result and not necessarily the very result itself. Certainty can only be in place in thought constructs, strictly axiomatized and defined. It is in these model worlds that there are universally true relationships and laws. In his *Treatise*, while accounting for the nature of human knowledge, he writes:

Here remain, therefore, algebra and arithmetic as the only sciences, in which we can carry on a chain of reasoning to any degree of intricacy, and yet preserve a perfect exactness and certainty. It is impossible for the eye to determine the angles of a chiliagon to be equal to 1996 right angles, or make any conjecture, that approaches this proportion (Hume 1740/2000, 51).

Before commenting on this passage, I would like to quote below the following statement from Mill's 1836 essay *On the Definition of Political Economy*:

The conclusions of geometry are not strictly true of such lines, angles, and figures, as human hands can construct. But no one, therefore, contends that the conclusions of geometry are of no utility, or that it would be better to shut up Euclid's *Elements*, and content ourselves with 'practice' and 'experience' (Mill 1836/2008, 46).

Both Hume and Mill state that in isolation, i.e., in a given model (Mill) or in a particular thought experiment (Hume), certainty is possible and that true statements in these theoretical worlds become approximations of truth if they are related to empirical facts – a triangle drawn by the author of this essay is to have only approximately 180°, and this is even more striking in a hypothetical case of a chiliagon. So, we will never have universally binding and precise laws that are also context independent.

Now, and before moving to Smith, it is quite noteworthy that some interesting reflections on humility and pride can be found in Hume's *Treatise*. For instance, in the section titled *Of Greatness of Mind* Hume starts his analysis by clearly stating that "An excessive pride or overweening conceit of ourselves is always esteemed vicious, and is universally hated; as modesty, or a just sense of our weakness, is esteemed virtuous, and procures the good-will of every-one" (Hume 1740/2000,

¹³This sentence did not appear in the first edition of Hume's essay.

378). Earlier in the text he claims that “Humility exalts; but pride mortifies us” (ibid., 193). Although these words are put forward in the sections of his work which deal with morals and not human understanding as such, one can apply them to the issue of the nature of human knowledge in view of Hume’s analysis presented in his *Enquiry Concerning Human Understanding*. In Hume’s *Enquiry* we can find a clear statement denying the possibility of reaching ultimate knowledge, while at the same claiming that it is modesty which eases researchers to be humble in assessing the character of knowledge they possess about the world: “Hence we may discover the reason why no philosopher, who is rational and modest, has ever pretended to assign the ultimate cause of any natural operation, or to show distinctly the action of that power, which produces any single effect in the universe” (Hume 1748/2004, 17). Here reference is made to philosophers but nowadays we can also apply it to economists. Hume’s vision of human knowledge then is very close to the one we described earlier as a humble science.

As far as Adam Smith’s ideas concerning the nature of laws in economics are concerned, one may say that they are somehow contradictory. However, they are such only on the surface and do not give ground to another *Das Adam Smith Problem*. Since Smith was hugely influenced by Newton, he shared with him a mechanistic world view in which one has structures, mechanisms, and never changing relationships. In other words, both conceived of a reality in which omnispatially and omnitemporally constant conjunctions are possible. Take, for instance, the following insight from *The Theory of Moral Sentiments*: “Human society, when we contemplate it in a certain abstract and philosophical light, appears like a great, an immense machine, whose regular and harmonious movements produce a thousand agreeable effects. (1759/2013, 280). In Smith’s *Essays on Philosophical Subjects* there is another, clearly stated observation: “The universe was regarded as a complete machine, as a coherent system, governed by general laws, and directed to general ends” (1795/1980, 113). On the other hand, we can also find Smith’s statements that seem to be totally antinomous to the above ones. In *The Theory of Moral Sentiments*, he writes: “In the great chess-board of human society, every single piece has a principle of motion of its own” (1759/2013, 204). It seems therefore that, we do not have universal rules of human behaviour but each human subject has its own principles of conduct. How then can we reconcile the two seemingly contradictory views on the nature of knowledge about the economy? Here Smith himself can help us with the following statement:

Systems in many respects resemble machines. A machine is a little system, created to perform, as well as to connect together, in reality, those different movements and effects which the artist has occasion for. A system is an imaginary machine invented to connect together in the fancy those different movements and effects which are already in reality performed. 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 effect may be more easily produced. The first 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 1795/1980, 66).

Is not Smith just writing about modelling economic phenomena? His “imaginary machines” are models that are used to “discover principles”, including those governing economic systems. In his interpretation of the above cited excerpt Condorcet concluded that “in all the arts, the truths of the theory are necessarily modified in practice” (Condorcet 1804, 292).¹⁴ So, in models or thought experiments one can have perfect and unchangeable regularities, but once we refer them to empirical domains we only have some beliefs or “some degrees of certainty to which we may hope to attain” (ibid., 138). Referring to a more recent example, one can say that in the model of perfect competition price is always equal to the marginal cost of production, but outside the model, when we appeal to the real economy, $p = MC$ does not, in fact, obtain. Economic models do not give us certain knowledge about the world. Thus, the most we can do is to humbly make inferences from models to their targets.

J. S. Mill was another nineteenth-century economist who questioned the possibility of formulating precise and universally binding laws of economics. Instead, he proposed the following definition of economic laws: “All laws of causation, in consequence of their liability to be counteracted, require to be stated in words affirmative of tendencies only, and not of actual results” (Mill 1843, 523). Mill also wrote about the idea of “abstract truth”, namely the truth of theoretical claims insofar as they are placed in abstract places (models). However, “the conclusions correctly deduced from these assumptions [models], would be as true in the abstract as those of mathematics; and would be as near an approximation as abstract truth can ever be, to truth in the concrete” (Mill 1836/2008, 49). Interestingly, Mill’s tendencies are acting powers that make things happen (ibid., 56). I shall elaborate on this point in the following section.

Now, before closing our study on classical economists, I shall recall A. Marshall who is rightly treated as one of the founding fathers of neoclassical economics but whose economics did not lose its classical roots (Colander 2011). One can even argue that the Marshallian branch of neoclassical economics is closer to the ideas of Smith, Ricardo, and Mill than to the Walrasian branch of neoclassicism. This is particularly true of Marshall’s insights concerning the nature of economic laws. Suffice is to quote some select views of his here: “If the subject matter of a science passes through different phases of development, the laws of the science must have a development corresponding to the things of which they treat” (Marshall 1890, 65); “Every cause has a tendency to produce some definite result if nothing occurs to hinder it. Thus gravitation tends to make things fall to the ground: but when a balloon is full of gas lighter than air, the pressure of the air will make it rise in spite of the tendency of gravitation to make it fall” (Marshall 1920/2013, 26); “there are no economic tendencies which act as steadily and can be measured as exactly as gravitation can: and consequently there are no laws of economics which can be compared

¹⁴M. de Condorcet was one of the most important interpreters of Smith’s ideas (Rothschild 2001). Although he did a lot of research in theory of probability, he was conscious that economic systems are so complex that interplays between various events are not of a probabilistic nature, and thus he often referred to beliefs as being somehow in-between knowledge and probability.

for precision with the law of gravitation” (ibid.). So, simplifying a bit, not only does each economy have its own set of laws but these laws are statements about tendencies only. Therefore, not being humble in assessing our abilities of understanding economic systems would be just wrong.

In what follows, I will come back to Mill’s tendencies but in a metaphysically richer way. In doing so I will claim that while assessing the nature of our knowledge about the economy we cannot abstract from the very character of the economic realm. In other words, ontological fundamentals of economic systems matter for the scope and certitude of our knowledge about them. Or, as Lawson 2015 claims “[...] method must fit with the nature of its object” (13), and thus, for instance, Róna states that “clarity on the ontology of the subject matter of the social sciences seems urgently needed” (2018a, 5).

2.4 Metaphysics of Economic Systems

If we would like to describe an economic reality, we should ask how its organizing parts come to be. More fundamentally, a study into the nature of specific elements of this reality is necessary. It is claimed here that “all features of reality can be viewed under the aspect of their being” (Lawson 2014, 19). And scientific ontology can be understood as primarily interested in investigating the natures of particular economic objects. It differs from philosophical ontology which deals with general aspects of being.¹⁵ As I show in my recent book on *Economics without Laws* (2017), and what I have just indicated with respect to classical economics, the economic world is not governed by universal laws such as *whenever A, then (always) B*, but rather by such entities as powers, mechanisms, tendencies, and structures. Such a world is a world of potentialities only. Also, speaking metaphorically, such a world is a world where its constituting parts interact “chemically” and not “mechanistically”.¹⁶ In such a realm, macro phenomena emerge from their micro

¹⁵M. Bunge, for instance, describes this difference as follows: “Ontology can be classed into general and special (or regional). General ontology studies all existents, whereas each special ontology studies one genus of thing or process physical, chemical, biological, social, etc. Thus, whereas general ontology studies the concepts of space, time, and event, the ontology of the social investigates such general sociological concepts as those of social system, social structure, and social change” (Bunge 1999, 200). Our scientific ontology, and economic ontology in particular, is an example of Bunge’s special ontology.

¹⁶Here I refer to Mill’s analysis presented in his chapter *On the Composition of Causes* in *A System of Logic*. According to Mill, when causes interact chemically then we cannot use the method of isolation and thus the very ontological foundations of classical (and neoclassical) economics need to be questioned. In Mäki’s words, “This is the question of whether the causes of economic phenomena combined ‘mechanically’ or ‘chemically’, to use Mill’s phrases. When causes combine ‘mechanically’, their effects can be ‘added up’ like vectors [...]. On the other hand, when causes are combined ‘chemically’, some qualitative novel, emergent outcomes ensue” (1992, 349).

foundations rather than being reducible to their constituting parts. So, as Lawson puts it:

[...] these or related notions [law-like statements in economics] must be conceived in terms of potentials; as potentials that may or may not be expressed, and if expressed that may or may not be actualized because of countervailing tendencies [...] (Lawson 1997, 106).

And before he also writes that:

[...] science aims at uncovering causal factors, that is, it is concerned with identifying structures, mechanisms and the tendencies they ground, which produce, govern or facilitate phenomena at a different level. And if the aim of science is to illuminate structures that govern surface phenomena then *laws* or *law-statements* are neither empirical statements (statements about experiences) nor statements about events or their regularities (whether unqualified or subject to *ceteris paribus* restrictions), but precisely statements elucidating structures and their characteristic modes of activity (ibid., 24; italics in original).

Three ideas from the above statements by Lawson are important for our purpose, namely those that treat about tendencies, characteristic modes of activities, and *ceteris paribus* clause. So, let me begin with tendencies. They can be defined by referring to N. Cartwright's philosophy of potentialities, i.e.: "Substituting the word 'capacity' for Mill's word 'tendency', his claim is exactly what I aim to establish in this book [...]. I suggest that the reader take my 'capacity' and Mill's 'tendency' to be synonymous" (Cartwright 1989, 170). The following may serve as an example: Mill would claim that lower interest rates produce a tendency of investments to rise. Cartwright would rather say that lower interest rates carry the capacity to cause higher investments. Or, to use Cartwright's favourite example, saying that aspirin relieves headaches should be read as claiming that aspirin has the capacity to relieve headaches.¹⁷ Now, we come closer to the second idea underlined above by Lawson, namely things' characteristic modes of activity. In Cartwright's terms one should just say that the very nature of *A* is to produce *B*. For instance, the nature of lower interest rates is to make investments higher. But, as in the case of aspirin, a particular capacity may be dormant, or its functioning may be offset by some disturbing factors. So, one may have low interest rates without a corresponding shift in investment. No universal laws please, but only natures are real, and they make economic events happen: "Aristotle's notion of nature is far more suitable than the concept of *law*, *regularity* and *occurrent property* to describe the kind of knowledge we have" (Cartwright 1999, 78; original italics). Claiming the contrary would lead one to fundamentalism which we should oppose.

What now about *ceteris paribus* clause? Would it not suffice as a basis for a humble economics? It would not as long as it is understood merely as 'other things being constant' restriction. Also, such laws are either empirically false (because disturbing factors occur) or they are trivially true (if they are understood as purely analytical statements). Countless papers have been written on how such laws can be

¹⁷As the author of *Metaphysics* put it: "Something is said to be a capacity [potentiality, power] when it is a starting-point of movement or change either in another thing or in a thing insofar as it other" (Aristotle 2016, 83).

defended. Here I would refer to just two such defences that interpret *ceteris paribus* laws as humble statements about the world. First, just before studying *ceteris paribus* clause in the third chapter of his *Principles* A. Marshall wrote the following: “And following our definition of an economic law, we may say that the course of action which may be expected under certain conditions from the members of an industrial group is the normal action of the members of that group relatively to those conditions” (Marshall 1920/2013, 28). Marshall talks here about a *ceteris normalibus* clause which says that a particular statement is only true when conditions are normal. For instance, a rise in unemployment *ceteris normalibus* lowers inflation. Such a statement is only true in a particular New Keynesian model where one can have a non-flat Philips curve. Outside such a model we can only believe that such a relation can hold imperfectly. And here we are somehow coming back to Cartwright’s understanding of economic realm mentioned above where *ceteris normalibus* laws simply become statements of the following kind: *ceteris normalibus* *A* leads to *B*, and it means that the nature of *A* is to produce *B*. So, and again, we have a humble way of interpreting laws of economics. As Cartwright nicely describes it:

There is a tendency to think that all facts must belong to one grand scheme, and, moreover, that this is a scheme in which the facts in the first category have a special and privileged status. They are exemplary of the way nature is supposed to work. The others must be made to conform to them. This is the kind of fundamentalist doctrine that I think we must resist (1994, 316).

We live in a dappled world where we do not have all encompassing theories that give us a sense of certitude. Here I agree with Rodrik (2015, 17), who humbly says, “The correct answer to almost any question in economics is: it depends”. But even if we have an appropriate set of theoretical statements crafted for a given empirical domain, we will then be able to describe merely capacities, natures, and powers of the objects under investigation. The above reflection is of key importance for my thesis about the necessity for humility in building economic explanations. Since models produce beliefs that economists have about the world and if they are credible when the structure of a given model is close to its empirical target, it turns out that, for instance, economic laws (even if understood as statements about tendencies only) which give an accurate description of highly developed countries do not have to be adequate in explaining phenomena in less developed countries. Thus, it is not only the case that context is everything but even in a given context we are unable to guarantee that if *A*, then (always) *B* rules, and hence capacities are (nearly) everything, too.

2.5 D. Colander’s Plea for Creating Humble Economists

In an important paper on the current state of economic profession D. Colander (2016) claims that the crucial problem economics is facing is “lack of humility” (737). Therefore, he writes that “we economists have a tendency to convey more

scientific certainty in our policy positions than the theory and evidence objectively would allow” (ibid.). Later he proposes the way for having more humble economists. His solution is to treat economists not as applied scientists but rather as engineers. Colander’s argument for such a solution rests on the assumption that the goal of science “is finding the truth” (738) while that of engineering is “by nature applied” (739). And he cites Koen’s statement that the engineering method is “The strategy for causing the best change in a poorly understood or uncertain situation within the available resources” (ibid.). What matters is also the fact that Colander’s understanding of truth is rather straightforward: “a scientific truth is timeless” (ibid.). Next, he claims that most economists are actually engineers and that they are by nature more humble than pure scientists.

Colander’s arguments are roughly similar to the ones of B. Bernanke, who, while heading the FED, in his Princeton 2010 speech claims that in fact we have three areas we should focus on, namely economic science, economic engineering, and economic management. In his opinion we have more problems with the two latter concepts than with the former one. I do not entirely agree with either Colander or Bernanke, but I find the former FED’s chair arguments a little more appealing since I also think that we do not have problems with economics as such but rather we have some problems with the correct understanding of the claims it makes. However, the very act of correct understanding of economics’ insights does not belong to economic engineering either in Bernanke’s or Colander’s sense. In other words, Bernanke is wrong in claiming that economic science offers theoretical and empirical generalizations, and Colander is similarly wrong in stating that economics should just move more towards engineering and not science. What I would like to stress, and what should hopefully be clear from the insights suggested in previous sections, is that there can be a science, also economic science, which does not offer us eternal and unchangeable strict generalizations but rather informs us about capacities of potentially causative factors. It can inform us about some strictly defined behaviours only insofar as they appear in closed theoretical models. Such rules of behaviour usually do not perfectly hold once we move from models towards their targets, instead of generalizations we rather have some beliefs about real economies. Moreover, these beliefs can be treated more like *ceteris normalibus* laws or, to be even more precise, as mixed cn-cp laws, e.g., *ceteris normalibus* (for ideal markets, i.e., in some model conditions) and *ceteris paribus* (having other variables unchanged), a decrease in interest rate leads to an increase in investments. Also, a particular *ceteris normalibus* clause can be understood not only as being synonymous to a given model condition but rather as a metaphysically rich conditioner, namely *the nature of A is to produce B*. Here we are close to new Aristotelianism. It is, therefore, not only the case that science without laws is possible. Science which does not offer timeless truths is possible as well. And so, paraphrasing Giere (2000, 523), what has traditionally been interpreted as laws of nature describing various economies, turn out to be merely statements describing the behaviour of theoretical economic models. One who realizes this, is very close to a humble economics.

Colander’s insistence on the fact that the most important problem economics is facing is lack of humility in economists’ behaviour is definitely true. Also, my claim

that we need a humble economics, namely economics conscious of the kind of knowledge it produces (as it was discussed earlier) necessarily implies that we need economists who understand economics in such a way. My disagreement with Colander's proposal is that we do not have to transform economists into engineers to achieve such a change. We should not abandon science in general, and economics in particular, and the way forward is towards a humble science, and in our case a humble economics.

2.6 Conclusions

There is an ongoing debate concerning the state of modern economics. Some accuse economics of "mistaking beauty, clad in impressive-looking mathematics, for truth" (Krugman 2009); others point to the fact that we do not have any problems with the science of economics but rather with economic engineering and management (e.g., Bernanke 2010); some emphasise the fact that we have problems not with economics as such but with economists and their lack of humility (Colander 2016). In the reflections presented in this paper I also focused on the lack of humility but in a somewhat different sense. The focus here was on the problem that flies in the face of a great number of economists in many areas of economics, namely their dream of finding universal laws governing economic systems. However, these laws do not exist. As Róna (2018b, 189) claims, for instance: "The proper subject of economics, therefore, cannot be the search for nature based law-like generalities with reliable predictive and explanatory power, because there are no such generalities". Although many economists would agree that laws they discover do not have the status of laws of nature, they still have many problems in rejecting the worldview which sees the world as being governed by such laws. However, and again, laws of nature do not rule the world and thus searching for scientific laws that do is futile. What we can aim for is to identify capacities, causes, mechanisms, tendencies, and some imperfect empirical regularities. Unchangeable and omni-spatio-temporally binding laws can only apply inside theoretical models. Claiming that they are to hold in the same way beyond models is mistaken. In other words, taking the model mistakenly for its empirical target may easily lead one to a form of scientific dogmatism. As Rodrik nicely puts it: "there is no such thing as *the* model, but always it is *a* model" (2015, 43). So, explaining the workings of economic systems by using models is inherently imperfect. Said differently, inferences from models to their targets should be made with due humility.

Although we still face the problem of economics' failure to adequately understand the status of its claims and of assigning too much certitude to them (as discussed above), there are many signs which indicate that the economic profession is now turning (though slowly) in the right direction. One example is what is often called an empirical turn in economics which means that more and more papers are applied and focused on data rather than on theory only. Therefore, one can claim that there is a diminishing risk of taking models' claims outside their proper

domains. But still, such a turn in economics does not make economic science less prone to criticism but rather changes what is criticized and how. Studies based on randomized control trials can serve as an example. And there we have a problem of external validity. If you want RCTs results to apply elsewhere in any simple way, you commit the very same error as authors of theoretical models in economics: they want their insights to work beyond their proper domains. Since experiments (including RCTs) are models, their results must be interpreted with due humility. Writing about RCTs Deaton and Cartwright (2017) claim the following: “We often learn much from coming to understand why replication failed and can use that knowledge, in looking for how the factors that caused the original result might operate differently in different settings”. In other words, causes of various economic phenomena identified in empirical work should be given careful (humble) interpretation, e.g., they are only INUS conditions for particular results.¹⁸ Let me explain. Imagine that a lower interest rate causes investment to rise. What should be accounted for with regard to this statement is the presence of firms that would like to invest but need cheap capital, and some regulatory rules that allow for firms’ investments. Together these factors are unnecessary but sufficient to the increase of investment rate (since many other events certainly could lead to higher investment, e.g., a given firm may receive a subsidy from the government, etc.). Within this very set of three events, the lowering of interest rate is an insufficient (since lowering of the interest rate only, without firms willing to invest, for instance, would not cause higher investment) but non-redundant (because higher investment would not occur without it, *ceteris paribus*) part of a condition which is itself unnecessary but sufficient for the occurrence of the result. Therefore, a lower interest rate is an INUS condition for higher investment. So, we are again facing the issue of proper interpretation of knowledge about the world the empirical experiment gives us. Here again a correct way to tackle it is to treat it humbly. With regard to the above example the most one can claim is that the nature of lower interest rate is to stimulate investment. This is the way Cartwright in her writings seems to interpret INUS causality, including the one identified via RCTs.

As we have just seen, although empirical turn is a step in the right direction for economics, it does not alone guarantee a move towards a humbler economics. One may say that it can be treated only as an INUS condition for making economics humbler. Other developments such as making economics more pluralistic and hence context sensitive may also (potentially) make it humbler. Yet, even in thus modified economics some researchers may still give too much certitude to the insights they produce. Therefore, for a genuine movement towards a humble economics one needs economics based on an ontology of potentialities where one finds not laws, but capacities, tendencies, natures, and Aristotelian *dynameis*. In the very least I hope that philosophy of economics will contribute a bit to the occurrence of such a change by making economics more self-critical.

¹⁸ INUS causality is due to Mackie (1974), and INUS clause stands for Insufficient but Non-redundant parts of a condition that is itself Unnecessary but Sufficient for a contribution to the outcome.

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