

Is economics a science?

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Economists' Haiku

Science or not?

I look out the window:

The answer is no.

Abstract

After more than a century of near-complete hegemony of the neoclassic paradigm in the academy, we take it for granted that economics, as it is nowadays practised, is a science in the same sense we believe other fields of scientific inquiries to be. Although many critiques and dissenting voices can and have been heard questioning various aspects of different theories and models, the overall scientificity of economics as such has mostly been left unquestioned. Economists still present themselves as experts, Nobel and various other prizes in economics are awarded, thousands of papers are published in "scientific journals", and hundreds of thousands of students engage in their studies worldwide to become professional economists. All following a remarkably similar student plan and curriculum all around the world. However, in this paper, I go a step further and ask the question, not without hesitation due to the seriousness of the negative answer I propose to it, whether economics as it is practised nowadays by experts and presented to the public at large, can be considered a science at all. More than the question about whether we have good or bad economic science, it is the whole edifice and the way modern economics has come to be, which is, thus, being questioned.

Introduction

To the question proposed by this paper, to stay in the realm of scientific debate, a somewhat completer answer than the haiku stated above may be needed... For a start, the answer certainly depends on how we define "science" in the first place. If we consider science in empirical and heuristic terms, as a way to grasp and get to terms with actual reality, current economic theories based on abstractions and generalisations aiming at universally valid "models" and "economic laws" have shown to be of little help to understand concrete and specific historically changing economic realities. The idea, as proposed explicitly by the founders of the currently hegemonic neoclassic approach in economics, that the economic reality can be studied and approached scientifically by applying the mathematical, model-based deductive method borrowed from Newtonian physics, did not seem to live up to its promises. It has lead, instead, to a scientific practice which has been increasingly alienated from empirically observed economic reality, dwelling in the creation of sometimes sophisticated mathematical models which, nevertheless, failed to predict important real-world events in the past. Nor has it allowed us to understand better the more profound historical, cultural, political and ecological forces shaping our economic life in the present.

If we take a Popperian falsificationist perspective (Popper, 1959) and evaluate economics' scientific status in terms of the theoretical predictions made by the theory which may be revealed to be false by future observations; economics' practice of basing its models on a series of restrictive, non-empirically observable assumptions and particularly on the all-pervasive *ceteris paribus* assumption leaving crucial variables relevant to the real-world developments out of their models, can certainly not be assumed to be "scientific". By

considering a series of critical variables as unchanging and thus external to the model – as for instances technological change, political and cultural contexts as well as ecological factors; and, as most theoretical models do, by assuming free-market competition (when a series of restrictions and limitations of a political, administrative, cultural and even geographical nature to the functioning of real markets exists); like many other of the standard assumptions as for instances perfect information or at least information symmetry; the 2X2X2 (two products, two countries, two factors of production models on which most standard trade-theories are based); purely rational, individual profit-maximising human behaviour; rational expectations among many others; it merely has become all too easy to fend theories from empirical falsification efforts by attributing the deviation between the actual observed facts and the predictions to all these external factors which have not been considered by the model or which had been considered as “unchanging” by it.

As for the assumptions made by the standard economic models, they are already assumed to be not just unverifiable but directly untrue, unless a very short time – and thus a minimum validity of the model – is assumed. Indeed, the *ceteris paribus* assumptions place the model – and thus theory – out of the flux of time. It assumes an abstract moment in which change of all these “external” variables is supposed not to happen while, miraculously perhaps, the “internal”, endogenous variables of the model, do change. Although many of these variables deemed as external to the model are actually a constituent and changing aspect of reality and do affect the outcome.

It is true, Milton Friedman’s highly influential *Essay on the Methodology of Positive Economics* precisely defends economics’ scientific status by claiming that it does not matter whatever assumptions are made and whether they are empirically observable. As his argument went, “theory is to be judged by its predictive power for the class of phenomena which it is intended to ‘explain’”; thus, arguing that

“the relevant question to ask about the ‘assumptions’ of a theory is not whether they are descriptively ‘realistic’, for they never are, but whether they are sufficiently good approximations for the purpose in hand. And this question can be answered only by seeing whether the theory works, which means whether it yields sufficiently accurate predictions” (Friedman, 1953, pp. 8 and 15, respectively).

From a Popperian perspective, it is undoubtedly a well-argued point. However, we could apply it as well to Friedman’s theory that economics is a positive science comparable to natural sciences and theoretical physics, as he and standard economists alike like to believe. Seen from this perspective, the main prediction of his theory is that economist’s practice follows this method and, thus, that theories whose predictions fail the empirical tests are abandoned in favour of others who are kept as provisional explanations until they as well are eventually abandoned in favour of others who render not-yet falsified predictions. Nevertheless, any even superficial observation of past and current practice by professional economists will show that this has not been the case in the past, nor is it the case today. It predicts as well that the “assumptions” commonly made are at least “sufficiently good approximations for the purpose at hand”. Here too, how can we expect that the assumption of free-markets, no political, cultural and ecological factors affecting the economic process, no-technological change and the absence of changing and new historical contexts like, for instances, at the time I am writing this paper, the global Covid-19 pandemic, can be expected to be a “sufficiently good approximation” to reality? They are not, and it would be only by

chance that leaving out of a model crucial elements, the model would still bare correct prediction.

Nevertheless, despite that, models are not abandoned as predicted by Friedman's theory. As Mark Blaug in his methodological assessment of economics argued,

“analytical elegance, economy of theoretical means, and generality obtained by ever more ‘heroic assumptions’ have always meant more to economists than relevance and predictability. They have in fact rarely practiced the methodology to which they have explicitly subscribed, and that, it seems to me, is one of the neglected keys to the history of economics. The philosophy of science of economists, ever since the days of Senior and Mill, is aptly described as ‘innocuous falsificationism’” (Blaug, 1975, pp. 410-411).

“Economists have always regarded the core of their subject as ‘science’, in the modern sense of the word: the goal was to produce accurate and interesting predictions that were, in principle at least, capable of being empirically falsified. In practice, they frequently lost sight of this scientific objective and the history of economics is certainly replete with tautological definitions and theories so formulated as to defy all efforts of falsification” (Blaug, 1978, p. 697).

Beyond Friedman's wishful thinking, looking at economist's actual practice, what we find is a series of different research lines sharing the same mechanistic, model-based, mathematical and reductionist approach to the economic process which became hegemonic with neoclassic economics. All based on hypothesis and assumptions which are not just non-verifiable, but mostly distant from being even approximations to the actual economic, historical reality. Keynesianism, monetarism, macroeconomics' and microeconomics' approaches, among others, have all been evolving in parallel, often not even touching each other, but all were disregarding both falsificationism and the need of basing their models on accurate and empirically verifiable assumptions. While on a limited level in economics what Imre Lakatos (1978) called “scientific research programs” (SRP), consisting of a series of interconnected theories, may show some kind of progressive developments, some models and authors being replaced or just forgotten in favour of others, they nevertheless show strong inertia and are shielded from external attempts of falsification. They are often fiercely guarded by its practitioners from external critique, despite their lack of predictive power.

In each of these SRP and neoclassic economics at large, there are a series of lesser assumptions and secondary theories which are open to a certain degree of scrutiny. Some being abandoned in favour of new ones. However, the core of these different research programs is strongly protected from attempts in falsificationism by that which Lakatos termed “protective belts”. Thereby, it may endure decades or, in the case of neoclassic economics, now over a century, without being abandoned nor replaced by its practitioners. Despite its failure to produce “sufficiently accurate predictions”. Thus, while minor aspects may be improved on, the core of given paradigms, to use Kuhn's (1962) perspective, may go on for long periods engaging in what Kuhn termed “normal science”. A standard and commonly shared practice by a scientific community despite external critique and the non-observation of its predictions as supposed by Popper's falsificationism and Friedman's wishful depiction of economics.

Facts, even in natural sciences and even more so in the case of the human complex, historically and context-specific economic practice, are always stochastic, affected by a series of variables and aspects which make them differ from a theoretical model which always means a simplification of reality. Thus, to be accepted as a sufficient reason to abandon given theory, there has to be first of all a consent on whether particular observations do or do not represent a falsification of the core aspects of a theory; or whether they just mean a minor deviation which can be attributed to a conjectural stochastic fluctuation. Thus, in between Popper's hard, normative perspective on science describing how "science ought to be" and Kuhn's descriptive, historical perspective on how "science is carried out", Lakatos proposed what he termed "sophisticated falsificationism". In this view, although its practitioners do not quickly abandon the different research programs, they nevertheless can be distinguished according to their progressive or regressive nature. Some research programs showing a strong internal dynamism, managing to increasingly explain external phenomena as well as abandoning some of its lesser assumptions and explanatory frameworks in favour of others due to empirical observations and falsification efforts. In contrast, others become increasingly defensive and closed in themselves, presenting little progress in their theories and capacity to make "accurate and useful predictions" about reality. The latter being more of a dogmatic, self-referential and commonly agreed on practice by its members than a science in the normative terms set-out by Popper.

In Lakatos' view, eventually, scientists may switch from one research program to the other, and thus, those approaches who have shown little internal progress may eventually be abandoned in favour of others, some research programs gaining more weight and importance within the field, while others decline. Sometimes there is even a generational switch: new practitioners adhering to somehow more progressive research programs, while others simply die out once the interest on them fades and those scientists who happened to follow certain lines leave the academy. Thus, although not as swiftly and pure as expected by Popper, there is falsificationism going on in science and, eventually, progress, once those SRP who show a lesser capacity to provide useful and accurate predictions end-up being replaced by others who do.

Seen from that perspective, what can we observe within economics as an accepted and practised science? Is there an observable "sophisticated falsificationism" happening and does economics, as a whole, presents a progressive character? Has it shown to possess a progressive nature, helping us to better understand and come to terms with reality, although strong falsificationism is absent from its practice? Can there be observed cases of more progressive SRP being adopted and more regressive ones being abandoned along the way? These are somewhat more difficult questions to answer, and it certainly requires a more global perspective.

Nevertheless, here too, it does not seem to be the case. Not least because theories and SRP within economics have shown a remarkable capacity to endure despite their failure to predict and adequately represent real-world events. Indeed, there are new approaches which emerge in specific periods, like for instances when "game theory" was applied to economics or nowadays neurosciences and behavioural economics. However, these have always been limited within the more general development of neoclassic economics as a whole and its near-complete hegemony in the academy. As such, 20th- and 21st-century economics has shown remarkable resistance to abandon its theories in the face of empirical falsification. Nor is it clear that today's theories are better at explaining actual reality than those of the past.

As Persio Arida (1983) showed, although in the hegemonic tradition the history of economic thought is presented in an analogous way to that of physics, newer research and discoveries being presented as possessing a higher heuristic power as regards older ones, in practice, this assumption cannot be upheld in the field of economics. Already the very historical variability of its study object and thus the continuously changing essence of the study object places those insights applicable to some realities and contexts out of touch with others. Natural laws are unchanging, and thus Newton's gravitation theory can be compared to Einstein's, although more than a century separates them. Nevertheless, how to compare the capacity of the classics at the time of Smith and Ricardo explaining the economic reality of their time, to that of current economists explaining ours? Can they even be compared? We cannot judge past theories whose predictions may have been relevant at their time in the face of current events once reality has changed. Thus, the heuristic content of past theories cannot be assessed by looking at today's historical reality, nor can we expect a theory to make the same kind of accurate predictions when applied to different spatial contexts. What may have been observable for the USA in the 1930s is not the same we may expect to happen in China today. Thus, we simply cannot compare adequately Keynes's *General Theory* of the 1930s with that of Keynesian economists trying to understand and trying to give answers to the current economic depression resulting, among others, from the Covid-19 pandemic. Even less can we expect Keynes' answers to be universally valid to any specific geographic and historical context.

Thus, as Arida showed, the history of economic thinking is full of examples of theories which have been abandoned at given times just to be recovered later, while competing and contradicting theories can coexist for long periods. As it happened for instances concerning Say's law or if we look at Keynesian and Monetarist economics coexisting as the two hegemonic views since the 1930s. As further argued by Arida, the reason why economists stick to given theories and SRP within the field of economics often has much more to do with internal, rhetorical aspects, following commonly agreed rules and managing to be accepted by its peers, than to the external empirical accuracy of its predictions. The accepted hegemonic paradigm and even research lines are continually being protected by additional hypotheses and tautological arguments that "explain" dissonances that may arise between predicted and observed facts. Thus, as I will try to further show in these pages, although presenting itself as "scientific" from a Popperian perspective, economics has become a highly closed, self-referential and remarkably detached from reality normal practice in the sense given to this notion by Kuhn. A practice in which the attendance to the commonly shared methodological procedures which have become hegemonic in the field and the often purely rhetorical procedures are much more important to explain the supremacy of one model over another, than their actual heuristic content.

As Georgescu-Roegen argued in the early 1970s, the trouble with economics is methodological: the attempt to explain an essentially qualitative, entropic and irreversible process by applying the Newtonian method which had been developed to study passive objects existing in a purely abstract, reversible time. The attempt to reduce to some simple mathematical equations an essentially complex and multidimensional process. As he, a highly-respected econometrician, argued:

"with the mechanistic epistemology, the mathematical tools were also introduced in the Economist practice, a mutation that ought to have constituted a blessing. However, the mechanistic epistemology by its very nature encouraged an uncontrolled use of that tool. The result has been an

outgrowth of ‘paper-and-pencil’ (PAP) exercises, most of them having no relation with actuality. Unfortunately, the growth is far from benign. It has given standard economics its present tonal mode, so much so that PAP endeavors, even when empty, now represent the highest aspiration of professional performance. The hardest task of any special science – that of coming to grips with facts – has been relegated to secondary level of importance and of professional respect. And so have preoccupations with institutions and qualitative change which are the essence of evolution” (Roegen, 1976, pp. x-xi).

By focusing more on the commonly shared methodology and language that became hegemonic within the discourse of economics than on the actual explanatory power of the different theories and the accuracy of its predictions, economics has increasingly become what a group of disaffected French students of economics and economists called an “autistic science”.¹ That is, a science closed in its inner world and detached from reality, unable to engage in a fruitful dialogue with its surroundings. This can be seen if we look at the repeated failures of the models to predict concrete events like past financial crisis, downturns of economic activities or how different IMF imposed “structural adjustment programs” have affected countries who have had to undergo these plans in significantly different ways than predicted by the plan. We just need to look at the failure of the profession at large to predict the 2008 financial crisis or to predict or even understand the current crisis we are entering now. It is all too easy to dismiss the current downturn as invalidating past theories by pointing to the fact that the Covid-19, as well as the social and political answers to pandemics, have all being assumed by the different theoretical models as being external to the models under the *ceteris paribus* assumption. However, this is of little help in order to better understand the actual economic reality emerging from these events and factors.

We may as well look at one example which may be taken as symptomatic: the fate of the LTCM (Long-Term Capital Management) in the 1990s. The LTCM was an ambitious speculative investment fund set-up by John Meriwether, a famed Salomon Brothers bond trader, who assembled an all-star team of traders and academics in an attempt to create a fund that would profit from “the combination of the academics’ quantitative models and the traders’ market judgement and execution capabilities” as stated by the promoters at the time. Some of the biggest banks in the world, from Citibank to Deutsche Bank, invested \$1.3 billion at the inception of this new fund. It was based on a sophisticated formula predicting the best combination of assets and investments to minimise risks and maximise earnings of financial, speculative assets, derived from the so-called Black-Scholes Formula and which goes like this:

¹ This term became known due to an open letter advocating for a deep reform and plurality in the teaching of economics published in *Le Monde* on June 21, 2000. *Lettre ouverte des étudiants en économie aux professeurs responsables de l’enseignement de cette discipline* (Open letter of economics students to economics teachers). The letter, presenting itself as a wake-up call, 1) affirmed the desire for escaping imaginary worlds; 2) opposed the uncontrolled use of mathematics and; 3) asked for a plurality of approaches in economics. Nevertheless, the term “autism” being a medical term, the use of it to depict economists’ practice has been deemed to lack empathy with those who actually suffer the disease. Thus, the term “post-autistic economics” was later replaced by the more neutral “Real-World Economics” instead and gave rise to the current World Economics Association. See Fullbrook, Edward (Ed.) (2006) *Real World Economics - A Post-Autistic Economics Reader*. London: Anthem Press. ISBN-13: 978 1 84331 247 5.

$$C(S_t, t) = N(d_1)S_t - N(d_2)PV(K) = N\left[\frac{1}{\sigma\sqrt{T-t}} \left[\ln\left(\frac{S_t}{K}\right) + (r + \frac{\sigma^2}{2})(T-t)\right]\right]S_t - N\left[\frac{1}{\sigma\sqrt{T-t}} \left[\ln\left(\frac{S_t}{K}\right) + (r + \frac{\sigma^2}{2})(T-t) - \sigma\sqrt{T-t}\right]\right]Ke^{-r(T-t)^2}$$

This formula was derived by economists Myron Scholes, Robert Merton, and the late Fischer Black. It predicts how much a call option is worth at any given time and earned Scholes and Merton the 1997 Nobel Prize in Economics once

“their innovative work in the early 1970s, which solved a longstanding problem in financial economics, has provided us with completely new ways of dealing with financial risk, both in theory and in practice. Their method has contributed substantially to the rapid growth of markets for derivatives in the last two decades.”³

The formula had a significant impact on economics, as well as the financial markets. Even before being published, options traders simply programmed the Black-Scholes formula into their calculators or bought ready-made software and, by pressing a few buttons, they could find the exact expected price of any option at any time as well as determinate the ideal “risk-free” investment portfolio. Zvi Bodie (Bodie and Merton, 2000), in his manual about *Finance*, likens the impact of its discovery to that of the structure of DNA. Both gave birth to new fields of immense practical importance: genetic engineering on the one hand and, on the other, financial engineering.

In 1994, both Merton and Scholes were hired by John Meriwether to give the scientific expertise of the academy complementing the professional brokers “market judgement” and expertise. In the early phases, everything seemed to go fine: Scholes and Merton got their Nobel prize and early 1998 the highly-leveraged portfolio under LTCM’s control amounted to well over US\$100 billion, although their net asset value was only about some US\$4 billion. It had become a major supplier of index volatility to investment banks, was active in mortgage-backed securities and was dabbling in emerging markets such as Russia. Nevertheless, then came the Russian financial crisis and devaluation of the Russian Rouble in August, a variable not included in any point of the used formulas, and the fund received a severe blow. In early September, its equity dropped to US\$2.3 billion. The behaviour of the markets ceased to be “normal” and strange results appeared...

“Although their models told them that they shouldn’t expect to lose more than 50 million or so on any given day, they began to lose 100 million and more day after day after day till finally there was one day, 4 days after Russia defaulted, when they dropped half a billion dollars, 500 million in a single day” (Lowenstein, in BBC2, 1999).⁴

² Where $N(\cdot)$ is the cumulative distribution function of the standard normal distribution, $T-t$ is the time to maturity expressed in years (final time minus initial time), S_t is the spot price of the underlying asset, K is the strike price, r is the risk-free interest rate (which is the rate of return of a hypothetical investment with scheduled payment(s) over a fixed rate that is assumed to meet all payment obligations), and σ is the volatility of returns of the underlying asset.

³ The Royal Swedish Academy of Sciences – The Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel 1997. <https://www.nobelprize.org/prizes/economic-sciences/1997/advanced-information/?print=1>

⁴ BBC2 (1999) “The Midas Formula.” In *Horizon*, opening minutes, Broadcast by BBC2, Thursday, December 2 1999, 9:30 pm.

On September 22, LTCM's equity had dropped to US\$ 600 million. At the end of the month, after having lost substantial amounts of the investors' equity capital and teetering on the brink of default, it had to be rescued by a US\$3.5 billion rescue package from leading investment and commercial banks, orchestrated by the USA Federal Reserve, to avoid a major crisis in the world financial system.

As Maurice Ash notes, this crisis is not unrelated to the "secretiveness and sheer incomprehensibility to all but a tiny group of initiates" of the formula that gave birth to this financial hubris.

"It is now forty years since (...) Peter Winch showed in *The Idea of Progress* that an inquiry into society can be pursued only in the language in which that society itself is conducted (or which could be intelligibly derived therefrom). Manifestly, this precludes the imposition of any explanation of social behaviour derived from logic outside it, and above all the imposition of any universal theory or ideology or any formula. (...) Nobody, of course, paid any attention to such ideas, and certainly not the economists. Their newfound powers of measurement were making them the arbiters of society – just as physics (pre-quantum mechanics) was the arbiter of the natural world. (...)

The board of LTCM thought it could play God with the market. This is what happens when you try to speak a private language. The truth of whatever you say is not verifiable except by reason of your own assertion of it. In practice, however, even if one holds a conversation with oneself – as we all constantly do – one must use public language to do so. Should we nevertheless persist in the construction of an inner world, the outcome can only be, at worst, madness of one kind or another, or sheer folly" (Ash, 1999, pp. 36 and 37).

Seen from that perspective, the troubles of economics as a science is not just a question relative to some aspects of given theories or particular authors, provisional understandings and explanations which may eventually be replaced by others with a better explanatory and predictive power. At a much deeper and fundamental level, it is something which affects the whole of the profession and the way economics is professed nowadays. It is not just a matter of the wrong use of a given scientific tool or a result of incomplete information; it is the consequence of using the wrong tool in the first place. Of a misplaced methodology and approach to the phenomena or, as we saw Ash arguing by following Winch's insights into Wittgenstein's studies of language, the imposition of a "universal theory or ideology" to the social, complex and ever-changing phenomena (Stahel, 2006 and Stahel, 2020, Part I, pp. 34-141). It is the consequence of modern economic science using a private, a secrete language, instead of engaging in a dialogue with reality.

Whence it all began

At the time when Adam Smith wrote his seminal *An Inquiry into the Causes and Origins of the Wealth of Nations* in 1776, Newton's mechanics was firmly established and seen as the epitome of science and genius. As can be seen from N. W. Chittenden (Chittenden, 1846, pp. vi, 30 and 32) on his description of Newton's life introducing the Principia's first publication in America,

“To the teacher and the taught, the scholar and the student, the devotee of science and the worshiper of Truth, the PRINCIPIA must ever continue to be of inestimable value (...).

The law of falling bodies at the earth’s surface was at length identified with that which guided the moon in orbit. And so his GREAT THOUGHT, that had for sixteen years loomed up in dim gigantic outline, amid the first dawn of a plausible hypothesis, now stood forth, radiant and not less grand, in the mid-day light of demonstrated truth. (...)

And thus was ushered into existence The PRINCIPIA – a work so which pre-eminence above all the production of the human intellect has been awarded – a work that must be esteemed of priceless worth so long as science has a votary, or a single worshipper be left to kneel at the altar of Truth.”

It is in this cultural context that modern economics was born. With Newton and his mechanics, a new religion and a new god to be worshipped was then created. As the poet, Alexander Pope, proposed as an epitaph to Newton, “Nature and Nature’s laws lay hid in night: God said, Let Newton be! and all was light”. As Prigogine’s and Stengers’ depicted it, “Newton is the ‘new Moses’ to whom the Tables of the Law were shown. (...) A nation comes together to celebrate the event: a man discovered the language of nature and to which it obeys” (Prigogine and Stengers, p. 57). A prestige which, as we can see in Chittenden’s praise of Newton’s achievement, only grew in the 19th century.

With Newton’s as well simple, elegant and far-reaching mechanical laws of movement and gravity, the dream of perfect human control and forecast in front of a universe earlier seen as hostile, unpredictable and subjected to God’s unfathomable will, could start to take shape. A universe which, as a favoured image, was depicted as an immense clockwork, being God relegated to the position of the supreme watchmaker who, once his work of creation was done, retires to the background and leaves it clocking through eternity. As an immense mechanical clockwork, it was subjected to linear mechanical causality laws which once understood by the human mind could be controlled and directed by human industry, technology and mastery of the potent energies mined and harvested like coal, petrol, electricity and later nuclear fuels and renewables.

Such was the faith in the newfound powers and understandings of its mechanical nature, that in the late 19th century the French physicist and noblemen, Pierre Simon, marquis of Laplace, stated that

“we ought then to regard the present state of the universe as the effect of its anterior state and as the cause of the one which is to follow. Given for one instant an intelligence which could comprehend all the forces by which nature is animated and the respective situation of the being who compose it – an intelligence sufficiently vast to submit these data to analysis – it would embrace in the same formula the movements of the greatest bodies of the universe and those of the lightest atom; for it, nothing would be uncertain and the future, as the past, would be present to its eyes. The human mind offers, in the perfection which it has been able to give astronomy, a feeble idea of this intelligence. Its discoveries in mechanics and geometry, added to that of universal gravity, have enabled it to comprehend in the same analytical

expressions the past and future states of the system of the world” (Simon, 1814/1902, pp. 4-5)

It was this idea of mechanical physics and of a growing power of prediction of science which came to dominate science. Not just in the natural sciences, but in the social and historical sciences as well. It is there that economists would seek for inspiration.

Although Adam Smith in his writings makes plenty of factual, descriptive observations, as well as historical digression about actual historical economic facts, when it comes to stating his theories, he bases them on hypothetical realities, not on the described facts. General observations are taken for rhetorical reasons, not as the starting point of his theories. From his ideas about the “invisible hand” up to his trade theory and his assertion that the origin of all value has to be found in “human productive labour”, he bases them all on some imagined hypothetical examples which are assumed to have universal validity. Thus, for instances, his famous needle manufacture example illustrating the higher productivity resulting from the social division of labour and how it supposedly results from the “invisible hand” and not from an altruistic impulse, is not the result of empirical observation, but an imagined abstraction. As is his hunter taking supposedly twice as long to hunt a deer than it takes him to hunt a beaver, example on which he grounded his labour-value theory. Nor are his butcher and bakers, providing good food out of their self-interest, actual observed professional, but abstract imaginary characters. As are his trade models applied to two imaginary countries benefiting from trade between them according to some simple hypothetical numerical examples.

As for physics, he sets out to establish “universal laws” and “theories” by getting rid of the historical, concrete and unique realities, eliminating from them all “disturbing” forces and frictions. However, by doing so, he was highlighting those aspects which prove his point while ignoring those who do not. Thus, he quotes some potentially positive outcomes resulting from free-market competition like the need to attend the consumer’s preferences and needs; but ignores other potentially harmful outcomes like for example over-exploiting natural resources, labourers or, at the consumption side, how there can as well be observed various situations in which producers try to cheat or mislead consumers. Something which was certainly as well part of his historical reality, had he taken the time to observe the actual practice and growing capitalist activity around him. As for Newton stating his laws of movement and formula for frictionless bodies following inertial movements, Smith too aimed to understand the economic process by mentally eliminating from his observations all frictions and complexities affecting the concrete situations. Thus, an imaginary, frictionless, ahistorical reality from which he removed all kind of emotionally, culturally or politically driven behaviour was imagined, then logically deducing some laws which were assumed to be of universal scope.

With David Ricardo, this tendency to look for abstract, supposedly universal laws governing the economic process was increase even more. Refining Smith’s theories and models, his examples far from resulting from actual historical observations, were directly abstract imaginary constructs build-up to sustain his theories. Thus, for instances, going beyond Smith’s trade theory pointing to the absolute advantages countries have in concentrating their productive activities in those sectors they are more productive than others, he created his model of the benefits of free-trade on an imaginary depiction of the reality of his time. Although quoting Portugal and England’s trade in textile and wine as an example, ignoring the reality of his time, he just imagined the following numerical example (Ricardo, p. 82):

“England may be so circumscribed that to produce the cloth may require the labour of 100 men for one year; and if she attempted to make wine, it might require the labour of 120 men for the same time. England would therefore find it her interest to import wine, and to purchase it by the exportation of cloth.

To produce the wine in Portugal might require only the labour of 80 men for one year, and to produce the cloth in the same country might require the labour of 90 men for the same time. It would, therefore, be advantageous for her to export wine in exchange for cloth.”

Although simple and straightforward in its pure mathematical logic,⁵ it is hardly noticed that this is not the way empirical historical reality could be observed at Ricardo’s time. From a historical and even in a static perspective at the time Ricardo wrote it, wine and cloths’ production in Portugal, England or anywhere else simply did not happen to be so. He simply ignored the real way they were being produced in these countries and the actual trade of wine and cloths going on between Portugal and Britain. He ignored and said nothing about how, effectively, it was a trade which had been firmly established by the Methuen treaty signed between these two countries in 1703, a commercial agreement as well as a military alliance, which put Portugal in a clear historical dependence *vis-à-vis* Britain. A trade relation which had a profound effect on both countries’ economic development, particularly of Portugal who struggled to industrialise and catch-up with other European countries having remained an essentially primary goods exporting and manufactures importing country. (In Stahel, 2020, pp. 61-64 the example and its historical repercussions are discussed more in length.)

Implicit to Ricardo’s imaginary model are, of course, a series of assumptions which neither Ricardo nor his followers bothered to make explicit. Like for instances, that goods are assumed homogeneous (i.e., identical) across firms and countries (we are not talking of qualitatively different wines or cloths); production functions are linear, and thus the marginal productivity of additional labour units does not increase or decrease once more or less labour is employed to produce wine or cloths; labour is homogeneous within a country but heterogeneous (non-identical) across countries; goods can be transported without cost between countries; labour can swiftly be reallocated between industries within a country (that is, you can move workers from wine to clothing without losing productivity and need for training or adaptation within each country); British labourer cannot move to Portugal and produce there were productivity, according to Ricardo’s example, is higher in all sectors, and thus the highest productivity would be gained if not just wine, but textile as well were all produced in Portugal – as to a certain extent can be observed nowadays when we see how the bulk of industrial-goods production is delocalised to China; labour is always fully employed (otherwise, global production could just be increased by employing more labour, and a rise in unemployment due to specialisation could undermine the gains in productivity); production technology differences exist across industries and countries, being reflected in labour productivity parameters, and they do not change in time; labour and goods markets are

⁵ The mathematics of the model are easily explained to graduate economists (usually without any further consideration about all the implicit assumptions of the model, as if the rigour of the algebra is already proof enough of its validity): considering that labourers in Portugal are relatively more productive at producing wine than cloth, while in England it is the other way around, if each country dedicates its total labour force to that which labourers are relatively better at internally, the combined output of wine and cloths would improve and both countries would benefit from trade in which the exchange-value of wine in terms of cloths is $8/9 \leq \text{Wine/Cloths} \leq 12/10$.

assumed to be perfectly competitive in both countries and firms are assumed to maximise profit while consumers (workers) are assumed to maximise utility. These and other implicit assumptions can be summarised by saying that historical changes and development of both countries are simply wiped out of the model, and Ricardo's reasoning applies to a no-place, no-time, abstract reality instead. As if the effects of free-trade should not be assessed according to the effects they have on the historical developments of both countries as a whole. Not just at a given moment, but on their future developments as well. In any case, hardly a good ground to forecast developments in the real world in which policies based on these models were implemented and where Portugal was running persistent trade deficits with England, thereby resulting on a significant part of its colonial riches ending-up in Britain. Thus, financing British and not the Portuguese industrial revolution instead.

How it continued

It is this same abstract approach to an imaginary world and mathematical logic which is behind the still prevalent neoclassic trade models based on what has become known as the Heckscher-Ohlin trade model. Bertil Ohlin first proposed it in his *Interregional and International Trade* published in 1933; a time in which, as is well known, world-trade had collapsed due to the great depression and the world was heading towards World War II. A reality far-removed from the general-equilibrium model presented by Ohlin based as well on the work of Heckscher (who has been his PhD adviser and whom he succeeded in the Stockholm School of Economics in 1930).⁶

It is nowadays firmly assumed and even expected that economists base their models and theories not on real-world historical realities, but imaginary models. However, it has not always been so. At the time the neoclassic school was being established, Wilhelm Dilthey (1883/1989) strongly rejected the application of a methodology formed exclusively from the natural sciences (*Naturwissenschaften*) to the human sciences (*Geisteswissenschaften* or "spiritual sciences"). While the former was centred on the **explanation** of natural phenomena subjected to unvarying natural laws, the latter had to deal with life's creative manifestation and historical change. While within the natural sciences we seek to **explain** phenomena in terms of observable cause and effect which repeats itself universally in space and time, in the human sciences we seek to **understand** them in terms of the relations of the part and the whole, in their specific and unique contexts, as living, changing realities. Thus, according to Dilthey, a distinctively hermeneutic and phenomenological approach had to be applied to understand the *Geisteswissenschaften*. Thus, according to this view, the hermeneutical understanding and interpretation of ancient texts, religious works, law, history, aesthetic theory, among others, were needed not just to understand and translate to the present past

⁶ We will not enter into the formal description of the model, which can easily be found in most economic textbooks. It runs basically in the same line of Ricardo's model by mathematically showing that in the face of different factors of productions endowments, under free-trade, countries tend to specialise in those productions they are relatively more endowed and thus cost-efficient (although, as for Ricardo's model, not necessarily in absolute terms), while benefiting from those they are relatively less efficient through international trade. As for Ricardo, it is mathematically proven that free international trade is of the benefit of all, while basing the argument on a purely static-comparative analysis and not on the real historical effects of trade on different countries. Although standard economists struggle to find strong empirical evidence for these theories and even less for their normative claims about the long-term benefits of free-trade, it may be noted that Bertil Ohlin was jointly awarded the Nobel Memorial Prize in Economic Sciences in 1977 – together with the British economist James Meade - due to "their path-breaking contribution to the theory of international trade and international capital movements." http://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1977/.

realities by taking into account their context, but was seen by Dilthey as being a fundamental tool to understand complex, historically-changing and context-dependent processes. Like, for instances, the economic process.⁷

At this time as well, within the German universities, the so-called historical school of economics was the hegemonic and preferred approach. The historical school largely controlled the appointments to chairs of economics in German universities, particularly once many of the advisors of Friedrich Althoff, head of the university department in the Prussian Ministry of Education 1882-1907, had studied under members of the school. Moreover, Prussia being the intellectual powerhouse of Germany, and given its global reach beyond the German area, it managed to dominate academia not only in central Europe but also in the United States until about 1900, insofar as holders of German PhDs led most of the American economics profession.

Nevertheless, its dismissal and the academic hegemony of the neoclassical school meant not only a reversal of power within the academy, but the wholesale exclusion of these authors from the economics tradition as such. Thus, authors like Karl Knies, Bruno Hildebrand, Gustav von Schmoller, Etienne Laspeyres, Karl Bücher, Adolph Wagner, Georg Friedrich Knapp, although highly relevant and known in their day, are virtually unknown nowadays and ignored by contemporary economic science. Others, like Werner Sombart and Max Weber, although still known, are regarded not as economists but as sociologists or historians instead. The same may be said of Karl Polanyi, with his careful analysis of the evolution of the free-market system, or the monumental *Study of History* by Arnold J. Toynbee with his analysis of the evolution, rise and fall of civilisations, including the Western one. All these authors, who would fit perfectly into the historical tradition and who aimed to study the actual economic and historical reality, are virtually unknown by professional economists nowadays. Nor are they mentioned in contemporary economics textbooks.

Still at the end of the 19th century, at a time in which the neoclassic school was being established, Arnold Toynbee, namesake and uncle of the author of *Study of History*, at the time a well-known British economist and representative of the historical approach to the economic reality, was convinced about the imposture and dismissal of the abstract, model-based approach to the understanding of the economic process.⁸ From today's perspective, I cannot avoid wishing that he had been proven right instead of utterly wrong. In any case, he was firmly convinced that

“the bitter argument between economists and human beings has ended in the conversion of the economists. But it was not by the fierce denunciation of moralists, nor by the mute visible suffering of degrade men, that this

⁷ This point is made in more detail in my recently published book (Stahel, 2020) in which an alternative phenomenological approach to the understanding of the economic process is proposed and presented. Applying a different methodology, aiming to look to a living reality in living terms, renders not just a completely different understanding of how the economy works; it led as well to a different understanding of what the economy is all about. Thus, I propose a different term, *oikonomy*, to recover the original broader meaning of the term, instead of the conventional “economy” dealing solely with the quantitative, chrematistics or the market-related aspects of the economic life.

⁸ Although highly respected at his time, being among others responsible for popularising the term “industrial revolution” in Britain, a definition coined by the French who had gone through their revolution, mesmerised by the extraordinary historical changes they were observing at the other side of the channel, he is nowadays virtually unknown by the economist profession. I only discovered his references by pure chance and the help of Wikipedia, looking for information about his better-known namesake nephew.

conversion was effected. What the passionate protests of *Past and Present* and the grave official revelations of government reports could not do, the chill breath of intellectual criticism has done. Assailed for two generations as an insult to the simple natural piety of human affections, the Political Economy of Ricardo is at last rejected as an intellectual imposture. The obstinate, blind repulsion of the labourer is approved by the professor.

Yet very few people even now understand the nature of that system. I have called it the Political Economy of Ricardo, because it was he, more than any one, who gave to the science that peculiar form which, on the one hand, excited such intense antagonism, and, on the other, procured it the extraordinary influence which it has exercised over English thought and English politics. (...)

At first sight nothing appears more strange than this antipathy to, and this adoration of Ricardo. (...) Why should a treatise so remote, so abstract, so neutral, not filled with passion, like the *Wealth of Nations*, not eloquent in denunciation and exhortation, stating conclusions without eagerness, suggesting applications almost without design, why should such a treatise as this excite an uncompromising moral repugnance? Because it was remote, abstract, neutral, because, while excluding from its considerations every aspect of human life but the economic, and dealing with that in isolation, it came, nevertheless, though not with conscious intentions of its author, to be looked upon and quoted as a complete philosophy of social and industrial life. (...)

But again, we may ask why should a treatise so destitute of sympathy, observation, imagination, even literary style – a great part of it is nothing more than bald disjointed criticism of other books – dealing as it did with the most interesting, the most vital of human affairs; why should such a treatise as this dominate the minds of nearly all distinguished men of a distinguished time? Because, I answer – and not one answer will serve as a complete explanation – of its marvellous logical power, the almost faultless sequence of the arguments. Systems are strong not in proportion to the accuracy of their premises, but to the perfection of their reasoning; and it was this logical invulnerability that gave to the *Principles of Political Economy* its instantaneous influence. Ricardo has been recently compared to Spinoza; and what was said of Spinoza may be said of him: grant his premises and you must grant all. The contrast in the case of Ricardo, between the looseness and unreality of the premises and the closeness and vigour of the argument, is a most curious one.

For a complete explanation, we must push our investigation further. (...) Undoubtedly, the influence of his book was increased by the fact that in method and spirit it coincided completely with the mental habits of the most vigorous and active thinkers of that age. (...)

Besides the influence of the school of Bentham on political thought, and Ricardo's presence in Parliament, we may find still another reason for the magical effect of his treatise in the circumstances of the time. He lived in an

age of economic revolution and anarchy. The complications of industrial phenomena were such as to bewilder the strongest mind. No light had been thrown by Adam Smith on those vital questions, discussed before every Parliamentary committee on industrial distress, as to the relations between rent, profits, wages, and prices. (...) Not one of those who pored over piles of blue-books, or spent years in minute industrious observation of the actual world, had offered one single suggestion for the solution of these problems. The ordinary business man was simply dazed and helpless. (...)

To people groping in this darkness, Ricardo's treatise, with its clear-cut answers to their chronic difficulties, was a revelation indeed. But Ricardo's solution of the problem, *i.e.* that the prices of freely produced commodities depend upon costs of production, measured in labour, and that wages, profits and rent are not the causes but the result of price; this solution was only reached by making certain audacious assumptions (...).

Ricardo himself never realised how great were the postulates he was assuming. It is a strange but indubitable and most important fact that he was unconscious of the character of his own logical method. He thought, as has been recently pointed out, that he was talking of actual men and things when he was in fact dealing with abstractions. He makes but one allusion to the great assumption of pure competition. Of his assumptions, such as private property, perfect mobility of labour, perfect knowledge of wages and profits at all times and in all places, there is no trace of recognition from beginning to end of his treatise. And just as Ricardo remained unconscious of the nature of his method, so he never seems to have realised the scope and effect of his work. His intention was to investigate certain concrete problems which bewildered his contemporaries. His achievement was to create an intensely abstract science – Deductive Political Economy. (...)

His powerful mind, concentrated upon the argument, never stopped to consider the world which the argument implied (...) that world less real than the island of Lilliput, which never has had and never can have any existence.

A logical artifice became the accepted picture of the real world. Not that Ricardo himself, a benevolent and kind-hearted man, could have wished or supposed, had he asked himself the question, that the world of his treatise actually was the world he lived in; but he unconsciously fell into the habit of regarding laws, which were true only on that society which he had created in his study for purpose of analysis, as applicable to the complex society really existing around him. And this confusion was aggravated by some of his followers, and intensified in ignorant popular versions of his doctrines. His hard, clear delineation, with its audacious solutions of hitherto insoluble problems, asserted itself in spite of protest. It was laid as a mask over the living world, and hid its face. (...)

If Ricardo himself was unconscious of the logical character of his method, the same cannot be said of his chief disciples of the next generation. Both Mill and Senior state with the utmost plainness the exact character of their abstract science, and the assumptions upon which its conclusions are true.

Mill in his *Logic*, published in 1843, and in his essay on the *Method of Political Economy*, written much earlier (...), explains the nature of Ricardo's method with a clearness which leaves nothing to be desired. But what both Mill and Senior ought to have done was not merely to point out what the assumptions were which Ricardo made, but to ascertain from actual observation of the industrial world they lived in how far these assumptions were facts, and from knowledge thus acquired, to state the laws of prices, profits, wages, rent, in the actual world.

This work they never attempted" (Toynbee, 1894, pp. 1-7 and 9).

Nor would the neoclassic economists do so. Quite the opposite: they would firmly embrace the mechanistic, model-based mathematical method and consider it the unique way of approaching the economic reality. Thus, although affirming to depart from Ricardo and the classic economists, they embraced his method of establishing their science on "logical artifice" alone. And since then, economists "never stopped to consider the world which the argument implied (...) that world less real than the island of Lilliput, which never has had and never can have any existence", as if it is the actual world we are living in.

Stanley Jevons, one of the principal architects of this new school of thoughts, explicitly aimed,

"to treat Economy as a Calculus of Pleasure and Pain, and [as I] have sketched out, almost irrespective of previous opinions, the form which the science, as it seems to me, must ultimately take. I have long thought that as it deals throughout with quantities, it must be a mathematical science in matter if not in language. I have endeavoured to arrive at accurate quantitative notions concerning Utility, Value, Labour, Capital, &c., and I have often been surprised to find how clearly some of the most difficult notions, especially that most puzzling of notions *Value*, admit of mathematical analysis and expression. The theory of Economy thus treated presents a close analogy to the science of Statical Mechanics, and the Laws of Exchange are found to resemble the Laws of Equilibrium of a lever as determined by the principle of virtual velocities" (Jevons, 1871/1879, p. vii).

Jevons even went on to substitute "the name Political Economy for the single convenient term *Economics*" (Ibid., p. xxii), since he could not help "thinking that it would be well to discard, as quickly as possible, the old troublesome double-worded name of our Science" (Ibid., p. xiv). Further, he argued that he would gladly hand the subject of economics over to skilful mathematicians:

"I do not write for mathematicians, nor as a mathematician, but as an economist wishing to convince other economists that their science can only be satisfactory treated on an explicit mathematical basis. When mathematicians recognise the subject as one with which they may usefully deal, I shall gladly resign it into their hands" (Ibid., p. xiii).

Thus, as he further insisted "I contend that all economic writers must be mathematical so far as they are scientific at all, because they treat of economic quantities, and the relations of such quantities" (Ibid., p. xxii).

Jevons even went so far, in collaboration with Léon Walras, another of the founding fathers of neoclassic economics, as to establish a list of all authors who may fit into this endeavour. Thus, separating those who should be “accepted as economic writers” and those who should not. By defining economics through its method, that is the adherence to the mechanistic mathematical method borrowed from Newtonian physics, instead of by the content and empirical relevance of the different arguments, neoclassic economics excluded all those who took or would take a historical or a phenomenological approach to the economic field. Notably Marxist economics and the historical school we just saw. Furthermore, he argued for specialisation and subdivisions of the field, in line with what he observed in physics:

“as all the physical sciences have their basis more or less obviously in the general principles of mechanics, so all branches and divisions of economic science must be pervaded by certain general principles. It is to the investigation of such principles – to the tracing out of the mechanics of self-interest and utility, that this essay has been devoted. The establishment of such a theory is a necessary preliminary to any definitive drafting of the superstructure of the aggregate science” (Ibid., pp. xvii-xviii).

In an appendix of his book, Jevons presents a ten-page-long list of “Mathematico-Economic Books, Memoirs, and other published writings” dating back to 1720 (Francis Hutchinson’s “An Inquiry into the Original of our Ideas of Beauty and Virtue”). By doing so, he tries to reinforce this approach since, as he argued,

“the fact that some four or more independent writers such as Dupuit, Gossen, Walras, and myself, should in such different ways have reached substantially the same views of the fundamental ideas of economic science, cannot but lend great probability, not to say approximate certainty, to those views” (Ibid., p. xliii).⁹

By establishing a mathematical tradition in economics (in which, curiously, Ricardo was left out) and by placing himself and other like-minded in this tradition, at a rhetorical level the boundaries of this new orthodoxy were established, thus separating “bad” from “good” science... A separation which has been consolidated, leading to a near hegemony of the mechanistic approach to economics since. All those not applying a mathematical approach to economics or those trying to understand the economic process in actual, historically changing terms, being labelled as “non-scientific”. In some cases, as for Ricardo and the Marxist labour-value theory which is as well stated in purely quantitative, abstract terms, leaving them out for any apparent reason besides ideological differences.¹⁰

In any case, as stated by Shackle

⁹ While ten pages in Appendix I are devoted to other authors, nearly five pages are devoted in Appendix II to his writings upon “economical subjects”, followed by references to his four books on logic and his works on Political Economy.

¹⁰ While acknowledging (Ibid., p. xxiii) “that even the father of the science, as he is often considered, is thoroughly mathematical”, he nevertheless excludes him and all other authors from the Classical Political Economy from his list, including Ricardo whom he strongly rejects. However, as we saw, Ricardo’s was a purely logical-deductive and ultimately quantitative approach to the economic process. The ideological and historical reasons behind this wholesale rejection are further explored in Stahel, 2020, part 1.4, pp. 74-141.

“the 40 years from 1870 saw the creation of a Great Theory or Grand System of Economics, in one sense complete and self-sufficient, able, on its own terms, to answer all questions which those terms allowed ... in its arresting beauty and completeness this theory ... seemed to derive from these aesthetic qualities its own stamp of authentication and an independent ascendancy over men’s minds” (Shackle, 1967, pp. 4-5, quoted in Dobb, p. 167).

Thus, although at least until the early 20th century a plurality of approaches still could be found in the academy, progressively the mechanistic approach swept the profession and economics came to be seen and practised in analogous ways as theoretical physics. Through this process, not just all alternative approaches to the interpretation of facts based on other methodological approaches have been ignored, but the historical empirical reality as such ceased to be were economists were supposed to direct their gaze to. Unlike the historical school, all cultural, political, institutional and even ecological qualitative dimensions of the economic process were, thus, ignored.

The result has been that, unless in other social sciences where a plurality of methods and interpretations coexist, economics has been dominated by a single, hegemonic approach. By taking any even superficial look at standard economics’ textbooks, microeconomics’ or macroeconomics’ manuals and teachings, we can see how the theory is presented as a unified body of knowledge; students are directed to memorise it, while exercises at the end of the chapters have to be solved by the students to see whether they learned it right or not... As it happens with standard physics school-books. By establishing economics in quantitative terms and in terms of abstract models of reality, its learning has been equated with learning how to solve mathematically different equations; not a matter of learning to interpret reality. A science which, paradoxically maybe, deals precisely with qualitative transformation and subjectivity, namely “the use and transformation of scarce resources to satisfy human needs” and how this is done in culturally and historically specific ways by different societies and groups. Thus, even though wealth has been already defined in qualitative and relational terms by Adam Smith when he stated that “every man is rich or poor according to the degree in which he can afford to enjoy the necessaries, conveniences, and amusements of human life” (Smith, pp. xvii-xviii), wealth is considered in quantitative, exchange-value terms or directly in terms of monetary prices by modern economics. (For a critique, see Stahel, 2005).

Conclusion

It has been and still is the use of the mathematical method and the establishment of modern economics at the image and resemblance of Newtonian physics which gives it the appearance of “objectivity”, “neutrality” and “scientificity”. It allowed creating, as Toynbee depicted Ricardo’s approach, a science “so remote, so abstract, so neutral, not filled with passion, not eloquent in denunciation and exhortation, stating conclusions without eagerness, suggesting applications almost without design.” A science in which, unless other social sciences, ideology and personal political preferences are assumed not to affect theory. Economists, like astrophysicists, are portrayed as if studying distant stars and not being personally affected whether their conclusions sustain one political and ideological view or another. Ignoring, as Blaug (1975, p. 430) put it, that “when certain theories become the ruling scientific idea of their times for “good” internalist reasons, there are frequently also ideological reasons that make the theory palatable to vested interests and appealing to the man in the

street.” Ignoring, as Maurice Dobb (1973, p. 22) showed, “whatever one may be led to expect *a priori*, the history of political economy from its inception makes abundantly clear how closely (and even consciously) the formation of economic theory was linked with the formation and advocacy of policy. Although the doctrines of the classical school were very abstract, especially in the form given to them by Ricardo (whom Bagehot called “the true founder of abstract Political Economy”), they were related very closely to practical issues of their day, indeed surprisingly closely.” Forgetting, as Dobb put it, that it is in the act of defining and designing the model, by deciding which variables to include, which ones to exclude and how they relate one to the other, that a pre-existing ideological framework guiding our reflections enters the equation. Ignoring that our perception is not passive, but active: we actively, although not necessarily consciously, decide where to look at, what we perceive and what we do not. Then, we actively give meaning to that which we perceive which, without it, would amount just to a series of unrelated sensorial perceptions. Thus, despite the apparent remoteness and objectivity of abstract economic models, the choice of a methodological framework and of what we observe or not is not neutral and objectively given by the reality to be observed as such, but a consciously or subconsciously made decision by the observer.

Paradoxically, thus, it is this same choice of applying the mechanistic method to the analysis of the economy which rendered modern economic science unscientific if not in its form, in its practice and results. Human beings, indeed living beings in general even when behaving through instinct, do not behave like passive objects, nor do they follow inertial trajectories until affected by an external force which makes them change their movement like billiard balls or cosmic bodies may do. Even biologists do not aim to predict the behaviour of animals applying mathematical formulas as economists do when studying the behaviour of *homo economicus*. Nor do historians aim to predict future historical events and developments by applying a mathematical formula as done in the field of economics. At best, in the realm of complexity theory and the understanding of self-organised systems, tentative approximations and simulations about potential patterns and future scenarios may be made, as for the case of climatologists and weather forecast. However, this is always done in the understanding of the inherent indeterminacy and chaotic behaviour underlying real-world phenomena and, as in the case of the weather forecast, only for a concise period and a rapidly growing degree of uncertainty. Although, even for the weather forecast, it is a phenomenon with far less critical and stochastic variables affecting it than any economic development. Nevertheless, although we are quite aware of the limits of weather forecasts, certainly not daring to make reliable, detailed forecasts for more than a week or so away, economists do not hesitate to make yearly forecasts for the economy based on their models.

Even for the case of classical physics, as Prigogine showed, simple closed systems may show nonlinear behaviour and indeterminacy once we introduce the notion of entropy’s fundamental irreversibility. Once we consider time’s irreversible character instead of assuming, as economist and classical physics do, that time is a purely quantitative, abstract, external and reversible reality. Thus, following the arguments already put forward by Henry Poincaré at the end of the 19th century, we can see that even in a three-body astronomical system the general solution cannot be expressed in terms of algebraic and universally valid linear functions assumed to be eternally valid as Newton’s laws assumed. They are themselves subjected to resonance and indeterminacy, and their long-term stability cannot be asserted. As Prigogine argued

“there exists in nature systems that behave reversibly and that may be fully described by the laws of classical or quantum mechanics. But most systems

of interest to us, including all chemical systems and therefore biological systems, are time-oriented on the macroscopic level. Far from being an 'illusion', this expresses a broken time-symmetry on the microscopic level. Irreversibility is either true on *all* levels or on none. (At all levels, be it the level of macroscopic physics, the level of fluctuations, or the microscopic level, *nonequilibrium is the source of order. Nonequilibrium brings 'order out of chaos'*" (Prigogine and Stengers, pp. 285 and 287).

"Resonance occurs in all systems involving continual interactions. The phenomena described by Newtonian mechanics, by contrast, are simple idealisations examples involving transitory interactions, such as collision of billiard balls without friction, which are always idealisation. In the real world, interactions are persistent, and the existence of 'Poincaré resonance' is the norm" (Capra, p. 12, describing Prigogine's insights).

Notwithstanding, economists seem to be undeterred by these new understandings brought to physics or even by the blow brought to Newton's mechanics by 20th-century quantum physics and Einstein's relativity. They continue to stick to classical mechanics as if nothing had changed in the way physician understand the physical world. Nor are they bothered by the way other social and political sciences came to understand the social world; or how in neurosciences and psychology, the understanding of the working of the human brain is undoubtedly far-removed from the assumption of "rational behaviour" economists like to base their models on. As have neoclassic economists never seriously asked themselves whether the mathematical equations resulting from leaving aside all potential friction and nuisances while establishing models of the economic reality bear any meaning describing actual reality and real-world events.

Physicists know that to calculate the time it takes a feather to fall from the heights of the Pisa tower to the ground, air friction has to be taken into account. Furthermore, they know that, in the presence of changing wind patterns, it becomes impossible to predict with a minimum of accuracy the time it will take and the location this feather may touch it. Nevertheless, a reasonably accurate prediction could be made for a billiard ball being thrown from the high of the tower, although ignoring these factors. Thus, for predicting the movement of billiard balls or cosmic bodies moving in empty spaces, external friction can be left out of the equations, and they may still hold reasonably accurate predictions. In some cases, some variables, like atmospheric conditions and pressure, assuming them to be homogeneous, may be included to get better accuracy by calculating the trajectory of a cannon-ball. Notwithstanding, physicians would not dare to consider these functions valid for calculating the trajectory of real feathers in the real-world. Indeed, already for a precise Global Positioning System (GPS) based on differently orbiting satellites, Einstein's and not Newton's equations have to be considered. However, are economists not simply sticking to their reductionist practice despite growing complexity and speed of change? Merely ignoring crucial factors in their models and equations; even in times of fast and strong political, technological, social and environmental change?

Indeed, how can we expect any kind of predictive accuracy from a model of the economic reality in which political, cultural, technological and social factors and changes have deliberately being left out of the model? Of course, we cannot. Nevertheless, it is, paradoxically this known incapacity which economists all too frequently and eagerly use to precisely shield their theories and models from any attempt in falsification. Each time

observed real-life events contradict the prediction of a model, theories and models use to be rescued by arguing that it is due to the external factors which have been left outside the model that the deviation occurred. Thereby, in a strange, if not perverse reversal of scientific logic, in economics it goes the other way around: once the model is based on the assumption of non-political interference, free-markets, rational behaviour, among others; and, furthermore, it predicts a general economic equilibrium and full-employment; the deviation is seen as a proof that reality has to be changed and fixed in order to attain these desirable realities on whose assumptions the model is based. It is argued that once political interference is removed, free-market policies implemented, rational profit-maximising economic behaviour stimulated; full-employment, general economic equilibrium and efficiency will be attained... Thus, instead of following Popper's idealised normative behaviour of abandoning a theory once its predictions fail to be observed, economists use their theories as ideological weapons to promote and defend given economic policies. Not searching theory to conform to reality, but the "messy reality" to conform to the theoretical models instead.

Furthermore, it is this ideological function, the way specific theories and models serve given political agendas, as well as economic interests; the way certain theories are "palatable to vested interests and appealing to the man-in-the-street" – or at least the interest of those most benefiting from the existing economic structures and practices – that may explain why modern economic science, although far-removed from being able to give an adequate representation of reality or make even approximatively accurate prediction about the future, is still considered to be a science. Nobel prizes being awarded to cleverly and sophisticatedly designed models and imaginary worlds; professional economists advising and recommending public policies; while, paraphrasing John Lennon, "the reality is what happens while economists are busy thinking about something else".

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