Dear CHOPErs,

I present to your attention a draft of my work. Unfortunately, at this stage, I could not offer an elaborate study, and what I have now is rather a speculative opinion essay. I tried to focus on methodology and not discuss deep epistemological issues, so as not to slide into amateur philosophy of science, but I could have failed. I am very interested to hear about your views on the issues discussed in this essay, even if the framework that I am trying to offer does not seem to you a reasonable basis.

Best wishes,

Murat

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HOPE Center Lunch

Why Not Be Data-Driven? Historical Arguments and Their (Ir)relevance to Modern Economics

Murat Bakeev

Centre for the History and Methodology of Economic Science, HSE University

Center for the History of Political Economy, Duke University

mb810@duke.edu, mbbakeev@gmail.com, mbakeev@hse.ru

I

Changes in economics in recent decades, sometimes described as an "empirical" or "applied" turn, have reduced the popularity of theoretical work in the profession. This situation differs markedly from that which was witnessed by economists in the middle of the previous century. A fraction of economists is skeptical of the new data-driven approach and urge empirical economists to be more cautious (see, for example, the opinion of Russ Roberts[1]). How reasonable is their position?

Looking back at the history of economics, one can find arguments against over-reliance on statistics and other sources of empirical data and emphasizing the importance of theoretical analysis. As I argue in this essay, the key to these arguments were two theses, sometimes found together and sometimes apart. The

[1] https://shift.newco.co/2017/03/02/What-Do-Economists-Actually-Know-/#.bulnugqdj

first one is the certainty thesis, according to which deductive construction of theoretical systems using mathematics or logic provides us with certain and secure knowledge. The second one is the complexity thesis, according to which economic phenomena as such are too complex to take insights from them "directly" and must be approached with some idea regarding the general structure or principle underlying the explanatory mechanism.

This essay aims at describing in a sketchy manner the role of these theses in the thought of economists in the 19th-20th centuries. In addition, their place in modern economics is discussed. Particularly, it is argued that although the legacy of the certainty thesis remains influential in some areas of economics, in general it is rightfully losing relevance. As far as the complexity thesis is concerned, I claim that in many respects it remains relevant.

II

The key source of empirical data for economics is economic statistics. Accordingly, relying on a data-driven approach for an economist has generally meant aiming to derive economic knowledge from the analysis of statistical data, to get initial insights from the data. Classical economists made little use of statistics, and in this sense classical political economy was not data-driven. The reasons for doubting the usefulness of statistics could be very different. Obviously, the key problem was the underdevelopment and unreliability of the statistical data available to economists of that epoch. However, along with this practical problem, there were deeper methodological arguments in favor of a theory-driven approach. Among the classical economists, they have been considered in the most detail by three authors,

supporters of deductivism in economic methodology: Nassau William Senior, John Stuart Mill, and John Elliott Cairnes.

In this essay, I would not like to analyze in detail the nuances of the positions of these three authors. However, I want to highlight two key aspects of their positions. First, it is the idea that a few basic mental or physical facts are available to us from which all pure economic knowledge can be logically deduced. The key premise for all these authors was “that every man is desirous to obtain, with as little sacrifice as possible, as much as possible of the articles of wealth,” as Senior put it (Senior, 1826, p. 35; quoted in Bowley, 1936, p. 288). Others could be added to this principle of homo oeconomicus, such as Malthus' law of population, the law of diminishing returns in agriculture, and so on. The nature of these premises, whether they are positive or hypothetical, as well as the extent to which this pure economic knowledge has a claim in forecasting and solving practical questions, remained a subject of debate, but the general understanding of the deductive nature of the construction of the core of economic knowledge was the same.

The second idea, supported in varying forms by all these authors, is the impossibility of inductively deriving economic theories because economic phenomena are “the result of a great variety of influences, all operating simultaneously, reinforcing, counteracting, and in various ways modifying each other” (Cairnes, 1875, p. 63). It was believed that since the experimental method was not available to economists, it is impossible to isolate experimentally the causal relationships that are of interest.

In these two ideas one can see early representations of the certainty thesis and the complexity thesis, as I have put it. However, in this form, they rather rely on each other and lose their power when considered separately. The causal complexity of social phenomena prevents reliance on empirical analysis as the foundation for economic knowledge. As a result, we are left with only the logical derivation of all knowledge from a few principles, since, fortunately, these principles, which are not subject to doubt, are available to us whether through introspection or a broad understanding of the world.

Undoubtedly, later these theses tended to go together in deductivist accounts of economic methodology as well. Nevertheless, they can be considered separately, and the arguments that have been put forward in support of them could differ.

III

It seems that the certainty thesis was largely implicitly supported by the establishment in the practice of economic theorizing of the ideal of Euclidean geometry, which Hutchison (1998) associated with David Ricardo and James Mill, the father of J. S. Mill. This happened even though classical economists developed their deductive systems on the basis of verbal logic, not mathematics. The argument was that in case of the method of Euclid's proofs, "the only relevant critique is that the premises are false or inadequate or the chain of reasoning imperfect; failing this, the conclusions are binding" (Letwin, 1963, p. 97; quoted in Redman, 1997, p. 315). In other words, the deductive method allows one to achieve universality of conclusions, impartiality of analysis based only on logic, and not, perhaps, on biased empirical facts, and limiting possible errors to basic principles and the

subsequent chain of logical inferences. As Deborah A. Redman (1997, p. 315) put it, this approach "resorted to became synonymous with scientific theory and objectivity par excellence."

With the discovery of non-Euclidean geometry, the geometric metaphor and the associated idea of the universal and certain knowledge based on self-evident axioms faded somewhat (Weintraub, 2008). Nevertheless, mechanical metaphors such as equilibrium came to the fore instead, which continued to support the idea of a connection between scientific objectivity and the deductive approach. For instance, Claude Ménard (1980) explored the reasons for the resistance to the use of statistics by several well-known 19th-century French economists and pointed to the key role of adopting the rational mechanics metaphors based on the idea of uniform and unchanging laws. The probabilistic nature of statistical data was seen by Antoine Augustin Cournot and Léon Walras as incompatible with the goal of building a precise and exact system, conceived practically as a mechanical device.

Alternative interpretations of the certainty thesis have been developed by economists of the Austrian School. Particularly important for our discussion are the approaches of Friedrich von Wieser and Ludwig von Mises. Wieser's idea of certainty can be related to the view that economic knowledge rests on mental facts that are directly available to us through introspection. In this sense, our introspective understanding is more accurate, since it does not require long chains of inductive inferences, where at each stage there is a risk of running into an error. He framed it in this way: "For all actions which are accompanied by a consciousness of necessity, economic theory need never strive to establish a law in

a long series of inductions. In these cases we, each of us, hear the law pronounced by an unmistakable inner voice" (Wieser, 1929, p. 8; quoted in Scheall 2015a, p. 97).

Wieser's vision can be seen as closely related to the praxeological method of Ludwig von Mises. Without resorting to neo-Kantianism and not seeking to prove the unempirical nature of mental facts, Wieser speaks of universal implications of mental experience that are directly accessible to every person and constitutive of human thinking as such (Caldwell, 2004, p. 112). Within the framework of such reading, Mises develops Wieser's reasoning, radicalizing it and trying to bring it to a more solid philosophical foundation.

Scott Scheall (2017) distinguished two theses that Mises relied on in his reflections. The first one is the Reason without Experience thesis, according to which "pure reason (inner experience, intuition, introspection, etc.) and not contact with the external world is the source of the economist's knowledge of the fundamental principles, axioms, etc., from which economic reasoning proceeds" (Scheall, 2017, p. 228). Mises differs from other iconic deductivists in the history of economic thought, such as Senior, Mill, Cairnes, or his contemporary Lionel Robbins, by his strict adherence to this thesis. The rest were not so radical and could argue that the fundamental principles are confirmed by both reason and experience, or that part of the fundamental principles is based on experience.

The second thesis is partly close in meaning to what I have called the certainty thesis. Scheall (2017) calls it the Greater Certainty thesis, which asserts that in virtue of being introspectable, intuitable, etc., our knowledge of human

action is more secure, more certain, than knowledge acquired through experience of the external world. Although this thesis could be supported by other deductivists (for example, using the Wieser-like argumentation mentioned above), Mises was the most principled among them, as he insisted on the "apodictic certainty" of the knowledge thus obtained. The basis of this knowledge was the action axiom, which states that all humans act purposefully (Mises, [1949] 1999). Mises claimed that this axiom is akin to Kant's categories: "As an a priori category the principle of action is on par with the principle of causality" (Mises, [1960] 1981, p. 14; quoted in Caldwell, 2014, p. 124). Accordingly, any knowledge correctly deduced from the action axiom should have the same universal, indisputable status.

Scheall (2017) argued that the Greater Certainty thesis is a non-sequitur: it does not follow from the convenience of introspection as compared to experimentation or empirical testing that knowledge based on introspection is epistemically more reliable. Indeed, in this formulation, we cannot but agree with this criticism. However, this is only partly related to the certainty thesis that I am talking about. The Misesian vision of certainty elevates it primarily to a certain status of fundamental principles on which economic knowledge is based and which we can justifiably question. At the same time, one can associate certainty not only with the foundations of the system, but also with how it is built. As our previous discussion shows, it is possible to advocate a deductive, theory-driven approach while treating the underlying foundations of economic knowledge as empirical.

Moreover, even assuming the need for empirical testing, we can still consider the deductively inferred elements of knowledge to rest on a firmer basis,

more certain than the basis of elements derived from empirical evidence, within the general structure of knowledge. This approach praises one of the most important values in science, that is, coherence. Especially in a situation where we do not have complete confidence in the reliability of our data and methods of empirical analysis, there is nothing strange in considering more certain knowledge that is not contradictory, and sometimes sacrificing external consistency with data in favor of internal consistency with the larger body of the rest of the knowledge. We may in principle recognize the need for both internal and external consistency, and yet consider internal consistency to be a relatively more important scientific value, and if these two values conflict, tend to prefer internally coherent systems of knowledge.

In the history of economic thought one seems to find episodes when this logic prevailed. The deductivism of the 19th century discussed above, which was based on mathematical metaphors, relied on such logic, and the development of theoretical mathematical approaches in economics in the 20th century, which sought to build and analyze axiomatic systems, seems to have continued this line. Michel De Vroey (2016) describes the triumph of new classical models in macroeconomics at the end of the 20th century as a massive methodological shift from the Marshallian approach that prioritizes external consistency to the Walrasian approach that favors internal consistency.

While the Misesian arguments about certainty stemming from the action axiom now are influential only within the Austrian school, less radical views that do not deny the role of empirical testing as such, but nevertheless require the use

of a certain style of deductive analysis, viewing it as a *sine qua non* certainty condition, have some influence on economics in general. Again, for example, in the new classical and new Keynesian frameworks in macroeconomics, existence of equilibrium became a postulate; it was no longer considered as a characteristic of reality, but the way economists view reality, which anticipates and disciplines their reasoning. Discussing this, De Vroey (2016, p. 185) made an allusion to Kantian epistemology by calling what Robert E. Lucas Jr. did with the equilibrium concept "a Copernican revolution" in macroeconomics. On a pragmatic level, we can see the continued influence of the certainty thesis in that the optimal strategy for entering the mainstream for previously heterodox approaches in the recent decades has been to integrate rational choice theory in some way and formalize their analytical apparatus in accordance with the canons of the mainstream. This can be demonstrated, for instance, in case of the evolution from the "old" (1950s–1970s) to the "new" behavioral economics (1980s–present) (Avtonomov & Avtonomov, 2019).

And yet, it seems that the certainty thesis is losing its relevance in modern economics. First, the last decades in economics have been characterized by increased fragmentation of the discipline. As Alessandro Roncaglia (2005, p. 468) put it, "research has ramified in different directions and its very foundations – methods and techniques of analysis, crucial concepts and simplifying assumptions, central problems – have undergone broad diversification." Apparently, this state of affairs contradicts the ideal of an internally consistent, harmonious axiomatic system that embodies the certainty thesis.

Second, there has been a “models to methods” paradigm shift in economics, as pointed out by Matthew T. Panhans and John D. Singleton (2017): instead of applying rational choice models, economists are increasingly relying on studies of causal relationships using experimental and quasi-experimental tools. Obviously, such a shift blurs the foundations of the coherent deductive system that economists have so far tried to build.

In general, the ideal of internal consistency, despite strong positions in some areas, is fading. In this sense, in my opinion, the profession really tried to take up the call of Thomas Mayer (1993) to prefer truth to excessive precision, at least at the level of rhetoric. Mayer's text contains a critical argument against the interpretation of the certainty thesis that prevailed in economics: while the formally explicit parts of arguments got main attention, other parts tended to be far worse developed and convincing, and the strength of a chain of arguments was often measured by the strength of the strongest argument. This “principle of the strongest link” demonstrates an inadequate selectivity in the claims to certainty, which, in my opinion, is difficult to overcome. Even in the most formalized studies, there will always be weaker implicit, tendentious, metaphorical, etc. elements that refer us to the scientist's vision and help to identify an explanatory narrative from the abstract system she proposes. I believe that Deirdre McCloskey’s methodological writings depict this aspect of hidden interpretative thinking in scientific texts quite well. For example, she analyzed John F. Muth’s classic paper on rational expectations (Muth, 1961), demonstrating that his arguments involve such things, as the appeal to a uniformity in social nature, unproven analogies, unreasonable from the formal point

of view narrowing of the range of alternative theories that are taken into account, etc. (McCloskey, 1998). Thus, excessive theoretical certainty becomes an unattainable, false ideal, a poor excuse to prefer the theory-driven approach over the data-driven one.

IV

Despite the unsatisfactory character of the certainty thesis, in my opinion, one can be skeptical of the data-driven approach without supporting it. One can rely on the complexity thesis, which I formulated as the idea that the complexity of economic phenomena prevents deriving explanations from them on the basis of pure data analysis. We can say that this is an argument against the so-called "naive empiricism." Accordingly, deductive analysis here does not have to be recognized as certain, secure, and universal, but, nevertheless, we have to rely on it due to a serious imperfection or impossibility of purely empirical research.

Complexity here can be defined in many ways. For example, Friedrich August von Hayek [1964] (1967b) wrote that "the minimum number of elements of which an instance of the pattern must consist in order to exhibit all the characteristic attributes of the class of patterns in question appears to provide an unambiguous criterion." However, I would prefer to leave this concept rather "empty," focusing on the understanding of complexity simply as some kind of epistemological or ontological obstacle to obtaining scientific explanations from the data directly.

The mere recognition of the fact of complexity is not enough to favor the theory-driven approach over the data-driven one. Instead, we can come to an agnostic position, doubting the success of scientific explanations in principle. In order for the data skeptic to use the complexity thesis, additional supporting arguments and interpretive assumptions are needed. In the following, I will try to discuss three such possible interpretations, using examples from the history of economic thought, although there may be more, and I do not claim to be certain and complete here.

The first idea among economists was most often defined as the inevitability of using abstractions in scientific research. This idea, formulated radically, asserts the fundamental impossibility of access to data as such in all their completeness and authenticity. There is always some abstract optics with which we approach data, separating "important" from "unimportant" or "relevant" from "irrelevant." Accordingly, reliance on a theory-driven approach becomes inevitable, and in fact we are faced with a choice between an explicit, thoughtful theory and an implicit, vague theory that is hidden behind a scientific text that wants to appear as a purely empirical study. In this situation, the first approach seems to be more honest and solid. As Mises put it, the choice is between the "universally valid propositions" and "naive theory of pre-scientific thought" (Mises [1960] 1981, p. 101; quoted in Caldwell 2004, p. 121).

One of the first important examples of the use of this argument in the debate of economists was the position of Carl Menger in his famous *Methodenstreit* with Gustav von Schmoller (Caldwell, 2004, pp. 68-69; Hodgson, 2001, pp. 79-81).

Menger stated that any empirical research depends on non-empirical assumptions of identity, continuity, and measurability (Menger, [1883] 1985, pp. 214-215). In any case, economists will rely on abstractions, and therefore it is necessary to choose such abstractions that will set apart superficial appearances from complex empirical reality and leave only essential foundations. It is hard to argue with this. Using the soccer metaphor to describe the Methodenstreit, Geoffrey M. Hodgson even called Menger's argument "the most spectacular" goal scored in the match, even though Menger, according to Hodgson's assessment, lost on aggregate (Hodgson, 2001, p. 94). Nevertheless, Bruce J. Caldwell (2004, p. 69) in this context notes that Menger left more questions than answers, since his discussion of the arguments in favor of choosing those abstractions that were proposed by him, and not by Schmoller, left much to be desired.

A different approach to considering the problem of abstractions in economics was proposed by Karl Marx even before Menger. I am not ready to discuss the peculiarities of Marx's methodology in this essay, but I will nevertheless note that Marx, like Menger, believed that the process of cognition begins with abstractions (Ilyenkov, [1960] 2008). The abstractions that are initially available to us, incomplete and distorted, are nevertheless not accidental and are the product of our involvement in social practice. Through dialectical analysis and the removal of contradictions between these abstractions, we are able to begin "the ascent from the abstract to the concrete." Marx's approach seems very ambitious in the sense that, despite the complexity of reality and the inevitability of relying on imperfect abstractions at the first stages of cognition, we can nevertheless come to describe

reality in its entirety at subsequent stages. Thus, Marx did not essentially reject the certainty thesis, although he relied on his own unique dialectical method in the construction of his system.

In the 20th century, many proponents of the theory-driven approach also used this kind of argument. For example, Tjalling Koopmans during the famous "measurement without theory" controversy stated that theoretical assumptions are necessary because they can provide information about the appropriate estimation techniques and ways of measuring key concepts (Koopmans, 1947). But, again, I would say that Koopmans also largely drew on the certainty thesis, supporting the Walrasian approach in macroeconomics and considering deduction from formal structures, based on the premises of neoclassical theory, as a secure way to do economic research (Hoover, 2006; Mirowski, 1989).

In this context, the figure of Hayek seems worthy of attention, since for him the idea of "the primacy of the abstract" was crucial, while the certainty thesis was not particularly appealing. Hayek supported the deductive method, but at the same time he criticized Cartesian rationalism, the expression of which is essentially the certainty thesis. As Norman P. Barry (1979, p. 197) put it, "while, of course, on many issues Mises and Hayek are in agreement, the former displays an almost Cartesian certainty … in the truth of his fundamental economic axioms."

Building on his theory of mind, as well as referring to the ideas of his friend Karl Popper, Hayek ([1968] 1978, p. 40) argued that "an abstraction is primarily such a disposition towards certain ranges of actions, that the various 'qualities' which we attribute to our sensations and perceptions are these dispositions which

they evoke, and that both the specification of a particular experienced event, and the specification of a particular response to it, are the result of a superimposition of many such dispositions to kinds of actions." According to Hayek, the primacy of abstract perception of empirical phenomena is sewn into a person at the physiological level, but this abstraction does not at all guarantee certainty and infallibility. Over time, people accumulate new abstract dispositions and learn to apply them more effectively, which allows them to better explain the patterns of the phenomena they are interested in.

In my view, the idea of the primacy of the abstract is closely related to the idea that the process of collecting, analyzing and interpreting data is determined by our theoretical priors, is theory-laden. This idea was put forward by such figures as, for example, Thomas Kuhn or Paul Feyerabend, and is still generally endorsed by philosophers of science. However, some modern philosophers of science and methodologists are exploring ways to limit the consequences of theory-ladenness, thereby pointing to a greater place for the practice of the data-driven approach in science.

For instance, Wolfgang Pietsch (2015) studied aspects of theory-ladenness in modern data-intensive machine learning methods: classificatory trees and nonparametric regression. According to Pietsch's findings, these methods are theory-laden in "an external sense," with respect to the framing of research questions and selection of parameters and data corresponding to them, but not in "an internal sense," that is, with respect to the causal connections that link parameters and functional dependencies between them.

In general, these findings point to a fact that is difficult to disagree with: even if it is impossible to get rid of theory in principle, it is possible to reduce the degree of reliance on it, and thus move towards a data-driven approach. An obvious example of this in economics is the rise in popularity of VAR (vector-autoregressive) models, which are often characterized as "atheoretical" and assume that every variable influences every other variable in the system. In terms of forecasting performance, VAR models outperform macroeconomic models based on theoretical assumptions, such as DSGE (Dynamic stochastic general equilibrium) models[2].

Does this mean that we have to reduce the degree of abstraction as much as possible? Apparently, when we are interested in the accuracy of forecasts and we have the appropriate methods, this strategy is quite reasonable. And yet, from my point of view, the theory-driven approach remains relevant. This can be demonstrated using the same example of VAR and DSGE models. In this context, I would like to quote the words of Charles L. Evans, spoken at a meeting of the Federal Open Market Committee of the US Federal Reserve in 2011: "What I like so much about the DSGE models … is that they tell a story about how things are playing out in the economy. There's really a beginning and a middle and an end to this story. <…> I think it's very valuable to understand some of the stories that these models can tell, and I agree with Jim that it may not generate the best forecast, but it helps us with better policy thinking" (p. 22[3]). It turns out that predictive

[2] https://mainlymacro.blogspot.com/2013/05/data-theory-and-central-bank-models.html
[3] FOMC meeting transcript, June 21-22, 2011.

efficiency is not the only thing we want from science. Understanding is also wanted, which requires purely human modes of argument and persuasion, such as stories.

A theory-driven approach is better for establishing understanding, as it leaves us with more control and comprehension of how the model works. At the same time, the data-driven approach can increase the risk of misunderstanding of the outputs. This risk is especially serious when we start working with complicated, philosophically loaded concepts.

Here, in my opinion, an example of using self-reports to measure such concepts as, for example, happiness, which has been gaining popularity in applied economics, will be appropriate. As Erik Angner (2013) notes, a careless interpretation of self-reports risks replacing complex abstract concepts with a questionnaire-generated constructs whose nature is not entirely clear. He calls this substitution “reification,” that is, the erroneous perception of an abstract idea in the form of a real or concrete object. The question is how legitimate it is to talk about the similarity between happiness in the sense of the word that interests philosophers and which has a moral meaning, and the construct that is formed on the basis of subjective data about one's own well-being. Similarly, the psychometric approach to measuring well-being as a latent variable constructed using factor analysis based on self-report data is criticized by Anna Alexandrova and Daniel Haybron (2016): questionnaire questions on the basis of which the constructs are formed that reflect the level of happiness are in fact thoughtlessly selected using statistical tests, while theoretical and philosophical considerations regarding the nature of the objects measured are largely ignored.

V

A somewhat different approach to the complexity thesis focuses on the fundamental incompleteness of the empirical evidence available to us. Foremost, this concerns subjective data, such as needs, desires, intentions, expectations, etc., which are recognized as fundamentally non-measurable. Furthermore, important data may exist in a latent, tacit form, as Hayek (1945) convincingly describes. In this situation, the theory-driven approach seems to have an advantage: even if it does not grant us with absolutely certain knowledge, with the help of theoretical constructions we can try to comprehend the phenomena of interest fully, without losing sight of the non-measurable elements.

The idea of the non-measurability of subjective data has been used to justify the use of the deductive method in economics by many well-known figures – for example, this argument was key to Frank Knight (Gonce, 1972). Knight claimed that "if we accept the aphorism, 'science is measurement,' as a definition of science, then there is no such thing as 'economic' science" (Knight 1934, p. 236; quoted in Gonce, 1972, p. 548). Economic phenomena can be deductively comprehended by the mind due to its possession of interests and ethical values, which allow, by analogy, to reflect on the motives of other people's actions, to place them in a value-laden conceptual framework that gives them the meaning that interests us. Pure economic knowledge, i.e., the theory of rational choice and its implications, receives from Knight the same exclusively subjective foundation as from Mises, but nevertheless appears more as a flexible interpretive value-driven framework than as a certain, universal system. As Richard Gonce (1972, p. 549) put it, for

Knight, “the mind cannot grasp such confounding reality in any ultimate sense, … it can only create instruments called 'knowledge' that may help to achieve its purposes.”

In addition, the argument about the non-measurability of some important economic parameters, which implies the fundamental incompleteness of econometric models in macroeconomics, was one of the key points in Keynes' criticism of the method proposed by Jan Tinbergen (Keynes, 1939). Keynes doubted the ability to adequately quantify and measure such things as expectations, political and psychological factors; he also did not consider it correct to measure uncertainty using probabilistic risk measures.

For modern economics, this problem remains relevant, although, perhaps, in general, the claim about the fundamental non-measurability of subjective data now seems too strong. Since the marginal revolution, economists have made various attempts to measure utility, the key subjective parameter for economics, directly and indirectly (Moscati, 2019). In recent decades, applied economists started to use novel methods of neuroeconomics and self-reports to measure subjective variables. The development of new econometric techniques, such as, say, latent variable modeling or Hidden Markov models, has expanded the possibilities of indirect measurement of various parameters. This does not resolve all issues: in particular, applied economists continue to have a great deal of debate about how reliable self-reported data are (see, e. g., Bond & Lang, 2019; Kaiser & Oswald, 2022). Nevertheless, the dispute is shifting to the practical dimension, and it becomes more and more difficult to talk about the fundamental non-measurability.

In addition, it is worth saying that the hope that the theory is able to illuminate all important aspects, to reflect all phenomena, may not be justified. In this context, Scheall (2015b, p. 42) draws attention to the fact that Hayek's methodology, along with — what Scheall calls — "a data problem, i.e., the hopelessness of populating theoretical models with data adequate to full explanations and precise predictions," implies "a theory problem: theoretical models of complex phenomena may be underspecified so that, even when all data is available, a full explanation could not be inferred from the model." Theory may have its own gaps and dark spots, and this reduces the strength of the non-measurability argument.

VI

The final deductivist implication of the complexity thesis that I would like to discuss is the impossibility of conducting crucial experiments that would determine a better theory. Hayek wrote the following about this: "But if it is true that in subjects of great complexity we must rely on a large extent on such mere explanations of the principle, we must not overlook some disadvantages connected with this technique. Because such theories are difficult to disprove, the elimination of inferior rival theories will be a slow affair, bound up closely with the argumentative skill and persuasiveness of those who employ them. There can be no crucial experiments which decide between them" (Hayek [1955] 1967a, p. 19; quoted in Caldwell 2004, p. 370). Also note in this context Keynes's words to Tinbergen: "Prof. Tinbergen begins with a very important and necessary admission. "The part which the statistician can play in this process of analysis," he explains on

p. 12, “must not be misunderstood. The theories which he submits to examination are handed over to him by the economist, and with the economist the responsibility for them must remain; for no statistical test can prove a theory to be correct.” Can the statistical test, nevertheless, prove a theory to be incorrect? Here also Prof. Tinbergen qualifies his claims, but he does go so far as to say: “It can, indeed, prove that theory to be incorrect, or at least incomplete, by showing that it does not cover a particular set of facts.” But is not this going too far?” (Keynes 1939, pp. 559-560; Keynes’s emphasis).

There has long been a discussion in the philosophy of science about how unsuccessful theories are rejected, what role experimental evidence can play in this, etc. In this essay, I am not ready to enter this discussion. However, I would like to note that the positivist view that we can unambiguously reject a theory by conducting an experiment or a series of experiments of a certain type does not seem very convincing. And recent advances in the development of data processing methods, as well as the introduction of experimental and quasi-experimental methods into economics, in my opinion, can hardly be a reason for the transition to such a positivist approach.

Although experimental results are often more valid than the results of statistical analysis of observational data, they are still not immune to the problem of “fragility,” as Edward Leamer (1983) wrote about it. In particular, experiments may lack external validity, which prevents generalization and potential extension of experimental findings to situations similar to the experimental setting (Schram, 2005). For instance, there is the issue of potential or actual non-replication of data,

which can limit the experimental setting to narrow context-specific conditions: what is found in one circumstance, such as in the population of one village or country, may not be replicated with the same results in other circumstances (Hamermesh, 2007).

In this context, from my point of view, reliance on the data-driven approach may lead to theory stagnation — as opposed to the idea that exploratory empirical research may now be the leading way to discover new theories, which was expressed, for example, by Rob Kitchin (2014). Kitchin identified two trends within the new paradigm that is bringing empirical research to the forefront: "new empiricism" and "data-driven science." They differ from each other in the degree of radicalism: while the new empiricism involves a rigid opposition of theory and data, the belief that the latter is quite enough for the reproduction of knowledge, that data can speak about themselves without any theory, the data-driven science, leaving room for theory, gives data a wider role than what was previously accepted in the practice of scientific research. Particularly, it involves the inductive development of hypotheses, which are then conceptualized within the existing theory.

Practices that can be classified as new empiricism, such as machine learning, have gained some currency among economists, but are most often used by them outside of academia, for example, being hired by the private sector to participate in business research projects. In this sense, the image of data-driven science seems to describe better the "empirical" or "applied" turn in modern economics. And if we look at what happens in practice, it often turns out that

exploratory empirical research is able to generate explanations only at the local level or in relation to specific applied problems. Insights in the area of studying what Hayek [1964] (1967b) called “general patterns” (meaning here, for example, Darwin's theory of evolution), seem to be thinning out.

This is largely a speculative claim, but nonetheless, I believe that we can link the slowdown in the field of theoretical exploration of general patterns in economics and the greater reliance on the data-driven approach. For example, Rostislav Kapeliushnikov (2018, pp. 119-120) wrote the following in relation to this (my translation from Russian):

“I came to this (I admit that it can be erroneous) conclusion when I began to analyze the lists of references for papers on labor economics that that fell into my field of vision. Works on econometric estimation techniques included in such lists are usually the most recent publications. Works devoted to the empirical analysis of certain specific problems, too. At the same time, the works that set the theoretical framework for research are almost always dated no later than the early 1990s. Indeed, over the course of about three decades, from 1960 to 1990, there was a real breakthrough in labor economics and theories of human capital, discrimination, the internal labor market, signals, search and match, efficient wages, deferred reward, tournaments and more came out. But since the first half of the 1990s, the flow of major new theoretical ideas seems to have begun to wane.

<....>

Finally, it should be noted that, by chance or not, the attenuation of the flow of major theoretical innovations chronologically coincides with the so-called "empirical turn" in economics that took place at the turn of the 1980s and 1990s, which was enthusiastically welcomed by the most authoritative experts in the methodology of economics."

Of course, in many fields, theoretical development continues quite actively (we can, for example, think about game theory here), whereas this alleged slowdown may be due to reasons rather internal to theoretical economics. Caldwell (2004, p. 372) wrote that "we probably reached the point of diminishing returns" for the ability of the rational choice framework, which Popper called "situational analysis," to explain and predict complex social phenomena. Nevertheless, we can still speculate that without relying on the theory-driven approach, we will not be able to come up with new innovative ways to shed light on the general patterns and mechanisms inherent in social phenomena, because attempts to generalize empirical results at the high level of abstraction seem to be rather unsuccessful and these novel results often end up being built into already existing conceptual theoretical frameworks, which leads to the conservation of those frameworks.

VII

Thus, in my view, there are good arguments that the data-driven approach should not be the only methodological ideal in modern economics. More abstract theories retain their importance as generators of analytical narratives and frameworks that can be used to understand economic processes. I agree with Caldwell's (2004, p. 397) opinion that "the most significant theoretical

developments have been those that have added to the storehouse of basic economic reasoning," and I think that while not all theoretical research is able to make such a contribution, perhaps only theoretical research has such a potential. These analytical narratives and frameworks become part of our daily reflections on how the world works. In addition, theory helps us to keep meanings under control, to clearly understand what we are talking about, keeping us from replacing abstract concepts with empirical constructs, the foundations of which may not be so clear.

That being said, I am not opposed to the data-driven approach per se. This approach to economics has shown its effectiveness in solving many applied problems, forecasting problems, in establishing robust causal relationships at the local level and in local applications to certain problems. But it faces the complexity that limits the scope of empirical inference to specific practical applications or particular causal relationships placed within specific space-time frames. As long as these limitations are recognized by economists, there can be no problem with this approach; at the same time, over-generalizing and providing universal answers about causal chains based on such evidence may not be justified.

If we reject the certainty thesis, then we cannot be sure that the theory-driven approach will give us universal answers as well. Russ Roberts puts it well: *"An American president considering a trade war with China would be wise to consult an economist. I think economists understand a lot about the benefits and costs of trade and who those benefits and costs fall on. An economist would remind a president considering a trade war that the short term and long term impacts might be very different. An economist would suggest that there is evidence that reducing*

trade will make the nation as a whole somewhat poorer. An economist would explain how trade is like innovation and we can learn something about how trade affects labor markets from what we have seen happen in the past. <…> But such claims would not be ironclad or precise. They would be nothing like an Oval Office conversation with a mathematician or an engineer considering the potential for NASA to send someone to Mars." Nevertheless, the very ability to think about these issues in general categories is largely determined by our ability to imagine various abstract patterns and mechanisms – using Hayek's terms, by our ability to offer "explanations of the principle" and "pattern predictions."

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