Another look at Wesley C. Mitchell's Economic Methodology

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Abstract

In our view, Schumpeter's assessment that Mitchell's work has been "repeatedly and even recently" discussed in unsatisfatory ways is still valid. He is treated as a naïve empiricist. This judgement is used even by institutionalist economists. In this paper we take the "measurement without theory" controversy raging in the pages of the *Review of Economics and Statistics* and at conference halls in New York and Chicago, invoking the names of Tjalling Koopmans and Rutledge Vinning (1947-49) as a pivotal episode in recent history of economic thought. It is one of the moments that showed the demise of institutionalism as a mainstream school of thought, in exchange for the "formalist revolution". This paper aims to contribute to a better understanding of Mitchell's "methodological position". To do so, this paper is organized in the following: Section 2 deals with the Mitchell's original institutional economics, his influences, and his theoretical project for economics; Section 3 gives a detailed survey of "measurement without theory" controversy, where some aspects of Vining reply are highlighted for their ontological implications (bringing forth issues such as emergent properties and units of analysis to the fore); Section 4 discuses the controversy, pondering about its legacy, which lingered in economics well after its end. Section 5 brings some concluding remarks.

Keywords:

history of economic thought; economic methodology, original institutional economics.

JEL Codes: B20, B23, B25, B41

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> "Scientific," like "democracy" and "American," is becoming a shibbolethic password which the naive think they have only to pronounce with sufficient glibness to be welcomed among the respectable and recognized elect. (Albert Wolfe 1924, quoted in Yuval Yonay 1998, 77)

> Almost forgotten today are the acrid debates following the publication of Arthur F. Burns and Wesley Mitchell's *Measuring Business Cycles* concerning whether that approach concentrated on "measurement without theory". Mitchell is today primarily associated with empirical research rather than economic theory; that he was moreover a leading U.S. institutionalist is rarely mentioned. An exception is Paul Samuelson, who recently [1980] wrote, "Although Veblen and institutionalists had some followers in American economic life – as, for example, Wesley Clair Mitchell, the founder of the National Bureau of Economic Research and theory-eschewing taxonomist of business cycles-forty years ago institutionalism seemed to wither away as an effective counterforce in economics" (Philip Klein 1983, 869-70).

1. Introduction

Just two weeks before his death, Joseph Schumpeter had completed a memoir on Wesley Clair Mitchell (1874-1948), to be published at the *Quartely Journal of Economics* in February 1950. It is a tribute to Mitchell's contributions to economics which delved in his works on business cycles, and interesting enough, claimed that "Mitchell's own methodological position can and must be scrutinized more closely both because of the outstanding importance of his work and because it has repeatedly, and even recently, been discussed in a manner that seems to me not entirely satisfactory" (Schumpeter [1950] 1997, 243). Later, however, Mitchell would be rated as a non-theoretical, a (naive) empiricist and even, as Samuelson would say, as a theory-eschewing taxonomist. It is not difficult to find similar descriptions even in texts by institutionalist authors. Not surprisingly, Mitchell is virtually forgotten today, except for scant and short chapters and papers. What happened to turn Mitchell's reputation upside-down less than a decade after his passing?

In our view, Schumpeter's memoir has some clues: at his time, Mitchell's work has been "repeatedly and even recently" discussed in unsatisfatory ways, his fingers are pointing to the "measurement without theory controversy", at that very moment raging in the pages of the *Review of Economics and Statistics* and at conference halls in New York and Chicago, invoking the names of Tjalling Koopmans and Rutledge Vinning. This paper aims to contribute to a better understanding of Mitchell's "methodological position". In our view, this controversy is at the juncture of two important and related episodes in the recent history of economic thought: the demise of institutionalism as a mainstream school of thought (Yonay 1998, chapter 9) and the formalist revolution in economics (Blaug 1999; Kesting and Vilks 2004).

Beyond the outstanding (and prejudice-ridden) claim that empirical methods as practiced at the NBER – where Mitchell was the leading figure – were naive fact-gathering, of little use to advance economics – the controversy is seldom discussed in economic literature. Even the four papers that comprise his main contributions are read in a cursory and careless way, assuming they are read at all. For a good part of the later 20th century, Mitchell's contributions were almost treated like folk tales, from a distant time when economists were infant scientists, adhering to an outdated and defective methodology.

Fortunately, recent research (Biddle and Hamermesh 2017; Orozco-Espinel 2019; Dimand and Rivot 2021; O'Sullivan 2021) has shed new light on this topic. From these studies, and from older accounts of the "measurement without theory" controversy (Klein 1983; Rutherford 1987; Mirowski 1989; Yonay 1998), we can tell a different story. We claim the bone of contention was not on the relative merits of different quantitative methods, which was its explicit topic; rather, the controversy belongs to the moment that contemporary mainstream economics was being defined. Two conceptions of what is "proper" economic science (or theory) were in dispute. New mathematical approaches were fighting for the high ground of "true" economics[1] – a fight waged, in this case, against original institutional economics. As it is usual in any scientific community, the controversy was also a dispute for financial support: the more "scientific" one approach appears to be (according to the prevalent cultural images of what "science" means), the more funding it will probably get. And last but not least, scientists are not angelical beings: their political leanings also did matter, as we shall see.

We claim that the "measurement without theory" controversy was methodological in its nature; economists discussed the differentiation and demarcation of "proper" boundaries of economic science, in which persuasion had a pivotal role. As a result, mathematical economics emerged as an expanding, well-funded and socially accepted research program. The present notions of optimizing rationality, systemic equilibrium and axiomatic theorizing as reference points[2] of mainstream economics were all consolidated in the aftermath of the controversy. Its rival, the original institutional economics and the quantitative methods associated with them were rapidly displaced to "the underworlds of heretics", as Keynes famously put it. Since it was a battle over scientific credentials (as Wolfe's epigraph implies), our *leitmotiv* is to evaluate the meanings of "science", "theory" and "scientific endeavour" to critically evaluate the pervasive conception of original institutionalism as not, anti- or unsufficiently theoretical.

The first questions this paper attempts to answer was "how would Mitchell have reply to Koopmans?"[3] As we immersed in this topic, we realized that there are more

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[1] This adjective was cautiously used by François Divisia in his answer to Fisher, Frish and Roos letter probing about the constitution of The Econometric Society, "to promote mathematical economics" (quoted in Orozco-Espinel 2019, 5).

[2] It is beyond the intentions of this paper to discuss whether the "changing face of mainstream" such as behavioral and experimental economics, evolutionary game theory, etc. are contesting or reinforcing those tenets, but all of them take those notions as reference-points.

[3] Arthur Burns had helped elderly Mitchell to complete *Measuring Business Cycles* (published in January 1946). Mitchell did not reply himself as he "suffered a series of heart attacks in 1947-1948 and died in October of 1948". The Koopmans-Vining exchange occurred in August 1947 and May 1949. The skirmishes continued in conferences on business cycles sponsored by the NBER (25-27 November 1949) and by the

interesting questions to historians and methodologists of economic thought. We argue the controvery put on display matters such the justification for empirical studies using quantitative methods other than sophisticated econometrics, and the steps of the formalist revolution itself. Aiming to put this episode in the light of the recent history of economics, and its institutional, methodological and sociological dimensons, this paper is organized as the following: Section 2 deals with the Mitchell's original institutional economics, his influences, and his theoretical project for economics; Section 3 gives a detailed survey of "measurement without theory" controversy, where some aspects of Vining reply are highlighted for its ontological implications (bringing forth issues such as emergent properties and units of analysis to the fore); Section 4 discuses the controversy, pondering about its legacy, which lingered in economics well after its end. Section 5 brings some concluding remarks.

2. Wesley Mitchell, Institutionalism and Business Cycles

2.1. Mitchell and original institutional economics

Almost twenty years younger than Veblen, Mitchell grew up in the American Gilded Age (1870-1896), marked by industrialization, urbanization and rapid economic growth after the Civil War. Being part of these great transformations helped to built Mitchell's worldview, who was children of New England farmers before going to the University of Chicago in 1892. At Chicago, his main influences were John Dewey's pragmatism and Veblen's heterodox economics. Mitchell was advised by James Laurence Laughlin[4] (1850-1933), first department-head of political economy in Chicago, a conservative economist that harbored serious doubts on the empirical soundness of the quantity theory of money. Mitchell defended his PhD. thesis on the emission of greenbacks during the Civil War in 1899 and, after a brief period as a journalist at the *Chicago Tribune*, he became instructor of economics at the University of Chicago (1901-1902)[5].

Dewey's pragmatism had an important influence on Mitchell and other original institutionalists (Hirsch 167, 1988; Mirowski 1988, chapter 7). Growing up in an

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Cowles Commission (early December 1949). See Mirowski (1989), 79-82. One can find echoes of the controversy in Vining (1950), Friedman (1953, 11-13) and Koopmans (1957, second essay).

[4] Although Laughlin was an accomplished professor and beloved by his pupils, his influence on Mitchell was small. "Professor Laughlin... accepted classical economists as systematized by Mill, but with the variations recommended by Cairnes... He was sure that the quantity theory of the value of money was unsound empirically and theoretically. Unfortunately, his statistical knowledge was of the most elementary character, and he saw no reason why anybody interested in economics should bother with mathematical refinements... Despite his limitations, Laughlin was a most effective teacher. He owed this success to his firm faith in the laws of economics, to the zeal with which he sought to make his students accept the truth as he saw it, to his genuine interest in our personal fortunes, and most of all to the fact that we could not accept his hard and fast doctrine" (Mitchell 1945, quoted in L. S. Mitchell 1953, 85). On Laughlin, see McCann and Kapuria-Foreman (2022), and his biography, *J. Laurence Laughlin: chapters in the carrier of an economist,* by Alfred Bornemann (1940).

[5] His career as a full professor included University of Berkeley (1903-1912), Columbia (1913-1919, 1922-1944), visiting professor at Harvard (1908-1909) and helped to found the New School of Social Research (1919-1922). Besides, he was the founder and first director of the National Bureau of Economic Research (1920-1945). See Schumpeter ([1950] 1997), Mills (1949), Rutherford (2011, 129) and Ken (2021).

academic culture that Hirsch calls "common sense philosophy", scholars such as Pierce, McDougall, Dewey and Veblen could not accept its complacency. Its traits could be summarized in two foundations: (i) the pre-ordered harmony of facts, both in the natural world as in the human world, planned by a beneficial a superhuman entity, God, for the lack of a better term, and was surely identified with Him by its more religious supporters. Therefore, the world needed to be interpreted in this framework of the "Natural Order", which served a greater and beneficial objective – which puts the emphasis of the explanation at the final causes, being taxonomical and teleological; (ii) this philosophy provided support to laissez-faire, once it saw the capitalist economy as a manifestation of this Order and intervention as heresy. Herbert Spencer's social Darwinism popularized this concept both among the general and the academic public, with the jurist William Graham Sumner, who was Veblen's professor, being one of its greatest defenders, in using it to guide juridical decisions[6]. The common-sense philosophy was a trinitarian faith in progress, market and Divine Providence; "so, in a sense, was Spencer's system" (Hirsch 967, 79). Spencer provided its rationalization and "dynamism".

Against this view, Dewey developed his pragmatic logic, based on his philosophy of science, education, and ethics. For Dewey, the learning process only happens through solving concrete problems. Human knowledge would be schematically divided between problematic and non-problematic situations. In the latter, there is no challenge to human knowledge, which conforms to the events. In the former, critical thinking, creativity and collective effort are needed to overcome problems and, thus, set knowledge forward. Mere knowledge memorization or reproduction is not really learning and this was Dewey's most important critique to the education of his time; likewise, conforming to the canon of traditional knowledge will not help to solve problematic situations.

We can summarize Dewey's pragmatism through its proposals: human knowledge is always directed towards objectives, solving problems. In the solution, *theoretical suppositions must be avoided*. This is not due to some Baconian empiricism lurking in the backstage. If the situation is problematic, the available knowledge either does not conform to the facts or cannot solve the problems. The search for knowledge must be, in some way, creative and directed towards solving concrete problems. The validity of knowledge (epistemology) is given by its problem-solving capacity. New theories and hypotheses are verified, reformulated or discarded according to empirical evidence, in a constant process of give-and-take between theory-crafting, empirical research and application[7]. His theory is also ethical, in the sense that considers education and citizen participation important to problem-solving, which are always life problems in a democratic industrial society. These traits, we might add, do not promote the taste for

[6] Hirsch (1967, 78) claimed that "whereas conservative Spencereans like William Graham Sumner could use Spencer's notions to bolster up the laissez-faire conclusions of political economy, Veblen used these notions to question the very significance of much of the work that economists had done." We argue that Veblen did not recur to Spencer, but to Darwin directly.

[7] On the realisticness of premises, Hirsch (1988, 4) wrote that for Dewey, "one did not have to be directly concerned with showing that premises were invariably and unconditionally true (...) since a theory with false premises or assumptions would necessarily have some implications which are not consistent with the facts, if in the process of inquiry the attempt is made to shape theory so that its implications accord more and more closely with the facts, the premises of the theory will at the same time be brought closer and closer to empirical reality". That is to say, research is a process of improving theories getting them consistent with not only with facts, but also with workable theories of problem-solving.

general theories: focused studies, in time and space, on concrete problems are preferred to general ones. As Schumpeter ([1950] 1997, 242-3) lamented, American economics teaching in the 1890s, in spite of "Uncle Larry" Loughlin's efforts, was not able to attract their restless minds to the classical or neoclassical theories, driving them into "institutionalist revolt".

Veblen's influence on Mitchell, however, is much more extensive and deeper. Mitchell wrote several papers that we could call theoretical, in which they develop themes of original institutional economics. Veblen's influence is evident in these papers, especially with his distinction between the industrious and pecuniary aspects of economic life. Mitchell ([1909] 1995) adopted the same critical tone, the same terms, the same cultural development phases used by Veblen to explain modern civilization. Following contemporary social psychology, he criticized the rationality of neoclassical economics (Mitchell 1910), considering it inadequate to study actual economic activities (Mitchell 1914). Following the Veblenian distinction between industrial activities – which aim to improve subsistence production – and pecuniary activities, Mitchell (1912, 1922) noted the implications of a monetary economy and calling the attention to its business cycles[8]. Mitchell adhered completely to original institutional economics, as shown by Rutherford (1987, 69, our italics):

> Mitchell argued that quantitative workers, unlike orthodox theorists, would be unable to evade the Veblenian distinction between making money and making goods because statistical series are expressed either in physical or in monetary units. Thus, "out of this technical characteristic of the statistical data we may expect to come a close scrutiny of the relations between our pecuniary institutions and our efficiency in producing and distributing goods, and, further, that investigations of this type will broaden out into a constructive criticism of that dominant complex of institutions known as the money economy" (Mitchell, 1950, p. 30). In this way Mitchell quite explicitly joined his quantitative methodology to the Veblenian dichotomy between pecuniary institutions and technological means. While Mitchell did not seek a Veblenian solution to the problems he attributed to pecuniary institutions, he argued that the problem of "how to make production for profit turn out a larger supply of useful goods under conditions more conducive to welfare" was a problem that was particularly amenable to attack through quantitative methods (Mitchell, 1950, pp. 137-148). *Mitchell's methodology, then, was closely connected with his views on the importance of institutions*. Because of this he could place his quantitative research within a conceptual and theoretical context, although admittedly a very broad one. *Mitchell's approach involved the close examination of data and working hypothesis in the light of an overall framework of investigation which emphasized the role of institutions, profit seeking, and the distinction between making money and making goods*.

[8] Speaking with Allan Gruchy, Mitchell explained: "Of course Mr. Laughlin knew nothing about business cycles. Nor did I until I had been several years in Berkeley and found the study of these cycles a necessary preliminary to a book I projected on the workings of the Money Economy." (Gruchy 1947, 251 footnote 5).

Mitchell made a vast theoretical contribution to original institutionalism, from which we single out his rejection of traditional theory. In the text "The rationality of economic activity", Mitchell claimed economists made an "intellectual fallacy" by incorporating non-realist psychological theories. For him, the issue was that the adoption of a hedonist psychology did not consider crucial aspects of human behavior, such as their anti-intellectual and irrational actions. Even in face of this limitation, Mitchell argued that the adoption of such explanations of the human behavior, founded in a particular idea of rationality, happened because a few observed experiences showed its effectiveness. According to Mitchell (1910, 198), "these assumptions, it is commonly believed, are not artificial; on the contrary they are held to substantially verified by observation. And they contain the measure of 'rationality' really imputed to men by economic theory".

The idea of an imputed human rationality is accepted through a "established routine proved by experience to be effective" (Mitchel, 1910, 199). This experience is verified against industrial progress and continuous growth of commercial techniques. This way, economic theory can extend such rational behavior to any activity, treating them as bargain between two consumption goods, using diagrams for marginal analysis and other tools that could allow calculation, to observe a utilitarian determinism in economic activities. For Mitchell (1910), however, a historical (and ethnographic) observation would allow us to conclude humanity moves in a non-reasonable way.

Mitchell called the importance of "conspicuous psychological facts" (Mitchell, 1910, 200) – the facts that standard economic rationality would be unable to explain, such as "habit, suggestibility, and the instincts of emulation and imitation must be brought in, if we are to account for our own subservience to fashion, our conspicuous waste, and our slovenly dependence on the advertiser" (Mitchell, 1910, 200). According to Mitchell, economic rationality would be an "acquired aptitude" and could not be considered an innate trait in human behavior.

This approach to traits that economic rationality could not explain was in line with British psychologist William McDougall's social psychology. This approach Mitchell considered the most appropriate to study economic human behavior (Ken 2021). McDougall wrote that, if humans are the product of biological evolution, there would be a great number of behaviors that would be innate (Ken 2021). To McDougall, the social human behavior would be explained from a set of "instincts" and "associate emotions", which would encompass both personal behaviors as well as social ones: refusal, fight, curiosity, sex, sympathy and imitation. On the other hand, at the same time, behavioral psychology emerged, represented by John B. Watson, who emphasized learning and the objectively observable environments. McDougall's theory, however, started to be seen as exceedingly subjective and non-scientific, losing its relevance after the 1920s (Ken 2021).

In spite of that, Mitchell still grounded his critique to rational economic theory in social psychology. Mitchell (1910, 197), following McDougall, wrote that "mankind is only a little bit reasonable and to a great extent very unintelligently moved in quite unreasonable ways", concluding that "economists have committed 'the intellectual fallacy'".

Mitchell was also interested in McDougall's discussion of instincts and associated emotions. Particularly, his interest was in the habit, "the emotion that controls our instinctive behavior" (Ken 2021, 33). Opposing Bentham's utilitarian philosophy and inspired by McDougall, Mitchell argued that pleasure and pain would not be driving forces of human behavior, only influences in the formation of habits. According to this view, behaviors associated with pleasure are promoted and become habits, while behaviors associated with pain are suppressed and decline (Ken 2021, 34). For Mitchell (1910, 201), therefore, according original institutional economics, habits and instincts are the starting point of the study of human behavior.

Also, Mitchell considered that the economic rationality of his time was constructed through a resemblance between the concepts of monetary life and psychology; the interpretation of self-interest as the maximization of pleasure could be accounted as monetary units. In Mitchell's (1910, 213) words, "Every interest of his life should be reducible to terms of one common denominator – the dollar". He argued that this mental accounting of an "ideally perfect money-maker" could be converted into Bentham's hedonist psychology terms.

Mitchell saw an alignment between economic behavioral norms and norms that developed along the financial history (Ken 2021, 38). This, however, was an artificial way to represent mental processes of economic life (Mitchell 1910, 205). According to him, social concepts are the core of social institutions, as they provide the foundation for rationality. These concepts, which are part of social institutions, reach certain independent of their makers and create unintended consequences (Mitchell 1910, 203-4). Using this, Mitchell argued the error of economic theory is taking, in a deterministic way, the facts of its time as universal. It ignores that people did not always act according to rational economic theory. He admitted that the rational economic approach has both advantages and disadvantages. The advantage is that it reduces the number of problems an economist must deal with, from an evolutionary perspective, which was considered "modern science" in his time. This, however, would deny the economist from reaching the true nature of economic phenomena.

2.2. The influence of original institutionalism on Mitchell's ideas on business cycles

Mitchell belonged to his time. His evolutionary ideas are easily noticed in his approach to human behavior and were in line with other similar authors. It is easy to see Veblen's influence in his work on cycles. In his presidential address before the American Economic Association (1924), Mitchell (1925, 7) wrote:

> Of the content of this quantitative economics we can form but uncertain surmises. One topic, however, is fairly sure to receive much attention – the topic defined twenty-four years ago at the thirteenth annual meeting of the American Economic Association by Dr. Veblen [i.e, Veblen's "Industrial and Pecuniary Employments", 1901]. This is the relation between business and industry, between making money and making goods, between the pecuniary and the technological phases of economic life.

This influence, however, was filtered by the young Mitchell's empirical inclination. He wanted to produce quantitative theories, i.e., peruse the data and propose hypotheses that could resist available empirical tests. When he started to think of project related to cycles, Mitchell wanted a monetary theory with strong institutional and historical content, writing drafts between 1908 and 1910. He abandoned this idea after 1910 because it was too ample, redirecting his efforts to study business cycles[9]. Veblen's ideas, in Mitchell's opinion, were too broad; facts should be studied more particularly. In his opinion, "Veblen's work was 'not accurate in detail,' he 'paid too little attention to checking his conclusions by patient observation,' and failed to establish the relative importance of 'the factors he dealt with and the factors he scamped" (Rutherford 1987, 66).

This concern with testing theories against facts is also present in his "scientific creed":

> My criticisms of economics as commonly practiced were valid; but when it came to accomplishing something constructive I had to come down to more concrete and more definite issues. Granted that men did not commonly behave in the calculating manner posited by economic theory, how did they really behave? To find out how men behave in making their livings, I had to observe for myself, or use the records of observation by others. And these observations always referred to specific instances of real people doing definite things, in a given place, at a given time. Using such materials I found it possible at times to generalize, though not in the sweeping, rapid fashion in which I could generalize if I started by imputing certain motives to all men and then reasoned about what they would do. But what knowledge I could get from observations rested on evidence. Others could test my conclusions and tell whether they were good approximations. Progress along that line is slow, but it seemed to me to yield knowledge that is substantially valid and tolerably accurate. Similarly, the realization that institutions play an important role in economic life did not take me very far. I needed to know what this role is. Again, the way to find out was by close study. Once more I had to observe, and observation was limited to actual instances that others had recorded or that I could record. Slow as such work is, it seems to hold out some promise of leading to results that one can call scientific, in the sense that they can be tested and made the basis for further researches along the same lines" (Mitchell 1939, quoted in L. S. Mitchell 1953, 563)[10].

This influence (and growing distancing) from Veblen is captured by Rutherford (2011, 137), who summarize Mitchell's methodology:

> Later in 1910, Mitchell abandoned the project and turned his attention to the problem of business cycles that was both more specific and more amenable to quantitative analysis. As he put it in a later piece on Veblen: "Problems of

[9] Money and business cycles were the main "macroeconomic" themes before Keynes. Their study method followed Say's Law, with disturbances having monetary, not real origins.

[10] In a letter Mitchell adds the following: "If I can demonstrate (and I think I can) that we learn a great deal more reliable and useful knowledge concerning the failures of an economic organization based on the making and spending of money by taking this line than by speculation, I may contribute something useful on one set of problems and encourage others to adopt the methods of inquiry that seem to me most promising." (Wesley Mitchell to Robert Lynd, 31 May 1944, quoted in L. S. Mitchell 1953, 553). Lynd's criticisms will be dealt with later in this paper.

> cumulative change in "life history" are extremely difficult to treat by any method of measurement. Each change is by hypothesis a unique event, begotten by an indefinite number of causes. To disentangle the tangled skein is impossible. Without the aid of elaborate technique it is hard to do more with such problems than what Darwin and Veblen have done – that is, to study the evidence and select for particular attention what seem to be the salient factors... It is only when he comes to recent changes that an investigator has tolerably accurate data. These materials Veblen did not reject; but he made no great effort to exploit them. In this respect, at least, his practice resembled that of most orthodox economists. (Mitchell 1936a, p. 298)". Mitchell came to the view Veblen's work as too speculative, and in place of such speculation, Mitchell proposed a program of studying a particular problem open to empirical examination with the institutional component made explicit but taken as a given. In this manner, Mitchell turned his attention to the statistical examination of the problem of business cycles conceived of as an undesired and unintended consequence of the existing pecuniary habits of mind and institutions: the profit-seeking nature of business enterprise, the functioning of other pecuniary institutions such as banks, and the operation, interrelations, and leads and lags in the movements of prices in various s markets. What he produced was a close empirical "analytical description" of the business cycle consisting of four phases – prosperity, crisis, depression, and revival – with each phase creating the conditions for the next (Mitchell 1913). Business cycles, then, were a microcosm of the kind of cumulative causation discussed by Veblen. Mitchell also explicitly linked institutions with his work on business cycles via his argument that it is institutions that create the patterns of aggregate behavior that result in cycles".

Mitchell dedicated almost his entire studying cycles. Besides his 1913 book – which first part surveys nine business cycle theories[11] –, he wrote published further books in 1927 and 1951. He promoted a dynamic view of cycles:

> Such was his interest in theory that in 1941 Mitchell allowed the theoretical portion (Part III) of his 1913 volume, *Business Cycles*, to be re-published under the title *Business Cycles and Their Causes* (1941). This early effort to construct a dynamic theory, indeed, serves well as an interpretation of Mitchell's last work, What Happens During Business Cycles (1951). But the self-generating theory of business cycles, which was the hallmark of Mitchell's ideas on the subject, remained and still remains to be written (Moore [1987]2009, V, p. 628).

As noted before, he also developed the institutionalist approach in a series of essays written parallelly to his business cycle research. *Measuring business cycles* discussed methods to identify and describe business cycles' shape (valleys and peaks, periodic but non-regular occurrences, leads and lags, etc.). It is not clear to a superficial observer how such an unassuming book started such acid controversy, starting with Koopmans (1947). But the seeds were planted in the 1930s, following Mirowski (1989) and Orozco-Spinel (2019). Ever since then, the movement for mathematical economics,

[11] See Walter Friedman (2018, 296).

gathering American and European economists and statisticians, created the Econometric Society, and its journal *Econometrica*, in 1933.

At the same time, there is a dispute for academic space at the University of Chicago, between the Marshallian professors (Frank Knight, Jacob Viner and Milton Friedman) and the supporters of the quantitative methods developed at the Cowles Commission. The Cowles approach searched for stable foundations on which they could build a sistem of simultaneous equations (expressing its "dynamic" features) that was to be a representation of the economy. Its equations were then submitted to regression analysis by the new method os ordinary least squares. But, through this method, the parameters of the equation describing the economy must have, by assumption, normal/Gaussian distribution of probabilities. This approach was defended by Haavelmo (1944) and opens great possibilities for Walrasian general equilibrium theory analysis (with dynamic and stochastic features!). The stable foundations for the equations were in the "structural parameters" of the economy, namely, equations describing consumer's preferences, equations describing the state of technology (transformation functions) under a given environment of laws and rules.

The quantitative research at the NBER – founded by Mitchell, in 1920, and had illustrious workers such as Arthur Burns, Rutledge Vinning, Simon Kuznets and Friedman himself – used descriptive statistics and consciously avoided *a priori* presupposition from neoclassical economic theory in search for evidences to promote evenhandedness and avoid ideological commitments. The NBER approach reached an enormous success at the time of the controversy, to the point of being funded by the Social Science Research Council (SSRC), Carnegie Corporation, Rockefeller Foundation, the American government and many private businesses. With disputes concerning statistical methods happening since 1940 between NBER and the Cowles Commission, Burns and Mitchell (1946) seemed a great opportunity for the Cowles Commission attack NBER's traditional statistical methods and present itself as "scientific" for the standards of a new science.

3. Measurement without theory controversy

The controversy consisted of four texts published at *Review of Economics and Statistics*: Koopmans (August 1947, 161-172), writing a review of Burns and Mitchell (1946), gave the debate its title. Vining (May 1949a, 77-86) struck back in "Koopmans on the choice of variables to be studied and the methods of measurement", followed by Koopmans's (May 1949, 86-91) "Reply" and Vining's (May 1949b, 91-94) "Rejoinder". We shall overview the controversy, in order to highlight important methodological statements in both sides, that are not duly examined in the literature.

3.1. Koopmans and the NBER strawmen

Koopmans opens his review in a sarcastic tone, comparing Burns and Mitchell to Brahe and Kepler. He says that the economic work done by Burns and Mitchell is in the "Kepler stage" and waiting for the "Newton stage", "in due course, by further development of theory". But, "this reviewer believes that in research in economic dynamics the Kepler stage and the Newton stage of inquiry need to be more intimately combined and to be pursued simultaneously. Fuller utilization of the concepts and hypotheses of economic theory (in a sense described below) *as a part of the processes of observation and measurement* promises to be a shorter road, perhaps even the only possible road, to the understanding of cyclical fluctuations" (162, original italics). There we have, after a long comparison between Brahe-Kepler's empiricism and Galileo-Newton's "science", the real issue in Koopmans's view: Burns and Mitchell not have not used concepts and hypotheses of economic theory *in a sense described as "better"*, namely, in the sense of Cowles Commission's research.

After listing Burns and Mitchell's objectives, Koopmans argue that there are "various choices as to what to "look for," what economic phenomena to observe, and what measures to define and compute, are made with a minimum of assistance from theoretical conceptions or hypotheses regarding the nature of the economic processes by which the variables studied are generated." (161). And again "It is concerned exclusively with cyclical fluctuations. Its hypotheses are concerned with the character of such fluctuations, rather than *with the underlying economic behavior of man*" (162). In other words, Burns and Mitchell's effort is about the morphology of business cycles, not their underlying causality. Koopmans next argue for the use of the neoclassical approach, as they have properties of "fundamental laws" like the physical ones to guide empirical work (162).

Koopmans's first argument against the book, sumarized in the statement:

(K1) [E]ven for the purpose of systematic and large-scale observation of such a complex phenomenon, theoretical preconceptions about its nature cannot be dispensed with, and the authors do so only to the detriment of the analysis (164).

The next two pages pick out instances of lack of theory. For example, Koopmans says that the criteria for variable relevance depends on their power of mapping the most important aspects of the subjet and on its social impact. Now, Koopmans says, both criteria are only given by a theory. At this point, we must consider this criticism as unjust, as Burns and Mitchell selected their variables after studying no less than nine theories of business cycles, matching the variables in the literature with ones which records are available. One should note that Burns and Mitchell are not anti-theoretical or avoiding theory (even Schumpeter claims that). They have taken original institutional theory as their starting point; and they have searched in the literature for relevant variables; they only were not making the theory explicit in his exercise of data-gathering and hypothesis formulation. This is a subtle point of Mitchell's methodology: theories are servants of empirical work, not their masters. They are held back, but not absent. Moreover, from a Deweyan point of view, this is the way of discovering *novel facts* (we shall return to this later).

Next, Koopmans deals with the implications of economic research for policy making:

(K2) [W]ithout resort to theory, in the sense indicated, conclusions relevant to the guidance of economic policies cannot be drawn (167).

This second criticism claims that Burns and Mitchell cannot offer a "genuine" explanation of business cycles. Genuine explanations, Koopmans urges, is "an explanation in which only extra-economic phenomena are accepted as 'data' without further inquiry, all relevant economic phenomena being subject to explanation in terms of assumed behavior patterns of men in a given institutional and technological environment" (166). A theory must have predictive powers. To do so, its parameters must be constant or fairly stable. In economics (at the time), experimental settings were not available. Having multiple, maybe infinite possibilities of causal factors, we have the *identification problem*. A second-best solution for lack of experimental conditions (or to circunvent identification problems) is to seek a theory that affirms stability in behavioral, regulatory and technological patterns that inform its "structural equations". If one chooses to pass this theory by, there is no garantee of parameter stability, and prediction – that determines the social usefulness of theories – is unreliable.

Moreover, attempts at empirical work without theory are bound to fail, Koopmans warns; we observe the effects, but not their causes – we are still wondering about the origin mechanisms of phenomena. "Measurable effects of economic actions are scrutinized, to all appearance, in almost complete detachment from any knowledge we may have of the motives of such actions. The movements of economic variables are studied as if they were the eruptions of a mysterious volcano whose boiling cauldron can never be penetrated" (167).

Koopmans moves then for problems of statistical estimation. When writing about the irregularity of business cycles, he admits that Burns and Mitchell bring interesting findings, but these irregularities can be an effect of accumulated random errors. Burns and Mitchell do not deal with random errors, as they are submerged in cycles variability. There are procedures to deal with errors, but, by not applying them, Burns and Mitchell were losing information which discriminate structural factors and accidental perturbations in the processes described. In other words, Burns and Mitchell should replace non-parametric methods for parametric ones, and take normal distribution of variables on board.

In short, Koopmans claims that "the extraction of more information from the data requires that, in addition to the hypotheses subject to test, certain basic economic hypotheses are needed concerning distributional assumptions, which often are not themselves subject to statistical testing from the same data. Of course, the validity of information so obtained is logically conditional upon the validity of the statistically unverifiable aspects of these basic hypotheses" (170). And that is his third argument against "purely empirical methods": the neoclassical method gives

(K3) greater wealth, definiteness, rigor, and relevance to specific questions of such conditional information, as compared with any information extractable without hypotheses of the kind indicated (170).

We should note, "hypotheses" here are in the statistical sense, about the probability distribution of stochastic variables. They are "not subject to statistical testing from the same data" and "unverifiable". But they condition "logically" the "validity of information"! Besides ironic moves and veiled accusations of arbitrary data to Burns and Mitchell's book, that is all. The "Conclusion" only abstracts the three criticisms, wrapping in a highly technical language. For economists who are habituated in thinking in hypothesis testing in *his terms*, he is quite persuasive.

3.2. Vining's reaction: Koopmans's "measurement whithout theory"

Daniel Rutledge Vining (1908-1999) was a junior member of the NBER research staff, and he was appointed to face Koopmans's criticisms. We make a bold hypothesis by arguing that his response and rejoinder were in full accordance with Mitchell's "credo" and methodology[12]. Vining acknowledges in his PhD dissertation (his supervisor was not named) professors Lange, Marschak, Mints and Knight. Moreover, he had published a paper on Keynes and Veblen (Vining 1939), and on Knight and quantitative methods (Vining 1950). That is to say, he was fully aware of disputes on quantitative methods going on in Chicago and (original) institutional economics.

Vining's response brings his main arguments at the forefront, as Koopmans did. He says that Koopmans proposed method states only the mathematical form that a theory should be important, not really its economic content. Second, the book under review is about hypothesis-seeking and discovery, and not about estimation and testing. To say that empirical research has to adopt one and only one approach is to put an unnecessary and unduly straight-jacket (77-8).

One should note, however, that a significant point is made about the "scientific endeavour" adopted at NBER, and on the ontology of the subject matter.

> It would seem that we need not bother over whether or not a really discerning observer of phenomena approaches his materials with a theoretical or hypothetical framework in mind. We may take for granted that he does. The controversy might turn not so much upon assertions of the existence or absence of a hyp*othetical framework as upon the nature of the entity the behavior of which is to be accounted for* (79, our italics).

Vining defends that the unit of analysis is macro – in other words, aggregate regularities[13]. If this is the case, (i) valid assumptions on the individual behavior and for welfare analysis are, by themselves, valid to study systemic phenomena; (ii) such phenomena are related to population behavior, not individual behavior. We believe Vining had an important ontological argument:

> I believe that in our discussions of trade fluctuations, national and international, we deal with the behavior of (an entity that is not a simple aggregate of the economizing units of traditional theoretical economics. I think that we need not

[12] We have, as of this writing, very little information on Vining's biography. He completed his PhD dissertation in 1944 at the University of Chicago. His dissertation was on regional clusters of economic behavior and he used this to criticize Koopmans on the units of economic analysis (see Vining 1949a, 81). Begining in 1945 he spent most of his working life at University of Virginia, and it is a little surprise had a role in the origins of Virginia tradition of Public Choice economics, influencing James Buchanan – though he had remained, it appears, a heterodox economist (see Boettke, Marciano and Stein 2021; Wagner 2024).

[13] Hodgson (2000, 68-9) implied that Mitchell was one of the first economists to use the notion of emergent phenomena: "Mitchell tried to break from this individualist foundation. In his 1924 Presidential Address to the American Economic Association, Mitchell (1937: 26) argued that economists need not begin with a theory of individual behavior but with the statistical observation of "mass phenomena." Mitchell (1937: 30) explained that this was possible "because institutions standardize behavior, and thereby facilitate statistical procedure." Mitchell thus hinted at a process of social conformism that stabilized behavior in an institutional context. To rephrase this in the language of emergence and complexity: Mitchell and others saw complex systems involving positive feedback effects that led to relatively stable emergent phenomena at the macroeconomic level".

> take for granted that the behavior and functioning of this entity can be exhaustively explained in terms of the motivated behavior of individuals who are particles within the whole. It is conceivable – and it would hardly be doubted in other fields of study – that the aggregate has an existence apart from its constituent particles and behavior characteristics of its own not deducible from the behavior characteristics of the particles. We should work toward an explicit delineation of the entity itself – its structure and functioning – and the role that hypothesis and formal theory play in the earlier stages of this growth of understanding is subtle and irregular (79).

In other words, Vining is complying with a critical realist desideratum: ontology comes first, epistemology next and only after that we should devise research methods suitable to the nature of the subject matter (Lawson 1994, 282-3). Moreover, Vining is complying with Mitchell's cautious orientation to formal methods: in the "early stages" and "subtle".

Vining summarizes his response, criticizing the main points of Koopmans's review:

(V1) the review is unduly focused in hypothesis testing;

(V2) It is doubtful if relevance to policy-making is a good or single parameter to evaluate a study;

(V3) the whole battery of modern econometric methods is overkill in the stage of hypothesis seeking (79).

To substantiate his first point, Vining repeats that Koopmans recommendations are mathematical form without economic content. What Koopmans promoted is Cowles Commission's preference for Walrasian general equilibrium, but neither this theory was submitted to test, nor any evidence of it was presented by the reviewer:

> Now a formal theoretical model based upon postulated and fixed individual motives and transformation functions might be just the conception that we need for accounting for and analyzing the uniformities discoverable among human individual and population phenomena. But such has not been demonstrated and until evidence of the adequacy of this model is made available, it is an unnecessary restriction upon economic research to insist that the method used shall be essentially that adopted and developed by Koopmans and his associates (80).

Vining attaches a long footnote to this statement to show that there is not one, but several conceptions of the proper way of thinking about "economic man" and economic system: besides Walrasian general equilibrium, we have old style neoclassicals (Knight) and institutionalists (Veblen). This implies that Koopmans is committing a displaced generalization.

Next, Vining turns Haavelmo against Koopmans. Haavelmo (1944) had listed four problems of empirical research: (1) building a model with invariant and autonomous relationships in relation to the changing environment; (2) testing of theories; (3) estimation; (4) prediction. Concerning to the first problems, Koopmans urges that economics should emulate physics (individual preferences, law and rules and technology as ‘data’ and invariable); Vining does not agree:

> In statements of this sort there is no doubt but that these men are considering the vast and rich field of economic variation, where systematic classification and description has hardly begun, but where flows and movements of things may be observed – flows of objects within and between economic or social organisms. The functioning of the great supraorganisms which we see as population agglomerations, their growth and decline, their interdependencies, give rise to quantities having both a spatial and a temporal dimension. Are we ready to accept the particular list of economic relations given in the above quotation as the fundamental autonomous relations, sufficient for our purposes and not dependent on other relations transitory in character? (82)

So, Vining argues, we are still in the first problem, and Koopmans skipped to the last two problems. And because of this, Vining declines to comment (V2). He remarks on the usefulness of observation (given an example from genetics) and the role of diletants in growth of knowledge. But here Vining is missing the point, in our opinion. When Koopmans remarks on public policy utility of research he is demanding theories with predictive power, not claiming that theories must only be acceptable if they have policy relevance.

Finally, Vining criticizes (V3) saying that there is more in statistics than only estimation and prediction. Cowles Comission methods are a hasty and restrained ruler to evaluate researches when we know so little about our object. And he points out where estimation methods are suitable:

> No less do such workers as Burns and Mitchell have hypotheses in mind, and workers in neither field at this classificatory and descriptive level need be burdened down with the emphasis on an advanced and fascinating theory of estimation. There are numerous problems where modern statistics comes into full use – where quantities have been defined, where units of study or "organisms" have been tentatively delineated and their functioning described, where hypotheses regarding numerical aspects of relations have been formulated (84).

In other words, “empiricists” do have a theory. And they do not discard estimation methods – they understand they are not suitable for this stage of research. He quoted two statisticians to support his claims that even experts doubt estimation methods.

Vining closes his response satirizing Koopmans’s position as if he was a Comissar of Research (85-6). This is not gratuitous, or intending to be funny. He is comparing Koopmans’s strictures to a totalitarian court, like Stalin’s purges. The aim of this biased court is known: not only to convict, but also to humiliate political enemies. Thrillingly enough (or in a move of polite disdain) Koopmans did not reject this role in his reply (91).

3.3. Koopmans’s reply and Vining’s rejoinder

Both Koopmans’s reply and Vining’s rejoinder denote their incompatible methodological beliefs. Koopmans reinforces his opinion that empirical research must conform to Walrasian foundations: “While it was long possible and sometimes tempting for physicists to deny the usefulness of the molecular hypothesis, we economists have the good luck of being some of the "molecules" of economic life ourselves, and of having the possibility through human contacts to study the behavior of other "molecules." Besides introspection, the interview or questionnaire, and even small scale experiments, are available as means of acquiring or extending knowledge about individual behavior. Thus we, indeed, have direct access to information already recognized as essential" (87).

He also reinforces the necessity of testing theories. Hypothesis-making can be formalized in order to tested against clearly expressed alternatives. If Burns and Mitchell's hypotheses are not testable against alternative ones, it is not possible to know if they are better than others (91). Koopmans claims to not be dogmatic concerning the form of mathematical models, and that there is a lot to learn from other social sciences. However, he reiterates the need to start from structural equations of consumers' preferences, a set of laws and rules and the state of technology.

> The basic assumption is that the numerically measurable effects of the implementation of individual decisions have a relatively persistent relationship to the principal numerically measurable aspects of the information that has gone into the making of these decisions – persistent, if not for a given individual, then in some average or aggregate sense for a group of individuals. Since the relationship is between numerical entities, it is necessarily mathematical; since not all pieces of information relevant to the decision can be statistically traced and perhaps also because of basic erratic elements in all human behavior, the relationship has the form of a probability distribution of decision effects which depends on the decision data (some of which are exogenous, i.e., non-economic) (88)[14].

Besides claiming that "structural equations" come from "introspection, interviews and low-scale experiments", there is an aprioristic character in the proposal: the estimation methods, as already mentioned, depend on hypotheses on probability distributions that are not testable by themselves (90)! Koopmans points to the gains with this procedure, in terms of predictive power, and warns that we cannot discard basic theoretical notions before they conflict with real-world experience. Koopmans closes his participation in the controversy accepting his role of Comissar and proclaims his desire to transform economics in a completely mathematized activity, capable of inducing social engineering (92).

Vining's rejoinder also shows how opposed his methodological beliefs were compared to Koopmans's. He notes (i) Koopmans "composition fallacy"; (ii) his wishful thinking about the completeness of formalized Walrasian economics to explain economic phenomena; and (iii) requires, again, evidence of sucess of this approach (92, 93).

The most important issue, however, is their ontological disparities. Koopmans is using physical analogies, while Vining uses biological analogies; the first shows a tendency to interpret the economy in a mechanical way, while the second shows a tendency to see it as a biological organism. He argues that we need to study the functional

[14] It is an historical irony that the demise of macroeconometric models of Cowles Comission is due, in fact of assuming "economic policy equations" as (weakly) exogenous, when prediction requires strong exogenity. The Lucas critique is precisely about this point (see Favero 2007, section 2).

traits and properties of aggregate structures (92). Thus, Vining makes a series of questions: why must economic behavior be first studied using structural equations? Are the alternatives (non-parametric methods) unstable? Where is the evidence of stability of structural equations? Are they really foundational and autonomous, thus adequate for predictions? (93)

In a further strike against Koopmans's authoritarian proposal, Vining refers to the desire to use statistical methods to, indirectly, change individual behavior (alluding to Comissar Koopmans's objective to completely formalize the economy). In summary, from Vining's point of view, the econometrists are not making predictions nor interventions; they are discussing logical possibilities and using traditional economic doctrine as the underlying structure of their analysis – in other words, they are using the Walrasian model as an accurate enough representation of how a real-life economy works (94).

He ends the rejoinder and their controversy in a way that was as surprising as Koopmans: appealing to well-known integrity of Burns and Mitchell (94). But this is according to pre-World War II standards of economic objectivity: based on fairness of evidence interpretation and in the character of the researchers. This standard would soon be replaced with objectivity concerning the use of techniques (Morgan and Rutherford 1998, 8-9).

4. Reactions and reflections concerning the controversy

As pointed by Mirowski (1989), the controversy did not end in May 1949, with Vining's "Rejoinder". The dispute between NBER and Cowles continued in two conferences, respectively in November (NBER) and December (Cowles) of that same year. According to reports, tempers remained high. Burns and Mitchell (1946) was attacked again by Jacob Marschak, who called it a product of "naïve empiricism". Burns reacted to this criticism, but his defense was too measured and not incisive, compared to Vining's. Worse, Mitchell was compared to Marx (Mirowski 1989, 82-3). This mention is not casual or innocent, given Mitchell's inclination for social control and the original institutionalists position against laissez-faire. There is also the poisoned political climate or the early years of Cold War and McCarthyism. Scientific rhetoric is now reinforced by political rhetoric.

Besides, the controversy was a step into the consolidation of modern mainstream economics, along with: (i) the controversy over price theory, with the institutionalist Lester and the neoclassicals Machlup and Stigler (1946-1947); (ii) Friedman's (1953) methodological essay, a defense of traditional Marshallian microeconomics; (iii) the proof of the general equilibrium theorems by Arrow and Debreu, in the early 1950s; and (iv) the translation of Keynesian macroeconomics into mathematical economics made by Hicks, Modigliani, Patinkin, Friedman, among others, culminating with the neoclassical synthesis and Samuelson's *Foundations* (1948) – in the end, the accepted view was that both micro and macroeconomics must be thought of a set of optimization problems under constraints. As Yonay (1998, 184-5, 193) affirmed, there is no continuity between pre-war and post-war neoclassical economics. Not only the original institutionalist lost, but also the old neoclassicals, such as Viner, Stigler and Knight.

However, can we really call it a victory? From the point of view of prestige and academic capital (following Bourdieu), there is no contest. Retrospectively, the debate is usually considered a victory of the econometric approach of Cowles Commission. After that, the Cowles Commission ensured more research funds and became a financially autonomous institution. Institutionalism quickly declined after the controversy:

> At the Annual Meeting of the AEA in December 1956, Kenneth Boulding (1957, 1) did not hesitate to argue that "there is not today anything which would be called either an institutionalist 'movement' in economics nor even an institutionalist group." He admitted that "there are a few economists today who would call themselves institutionalists," but these, he claimed, "tend to be isolated individuals" (ibid., 1). In 1958, the institutionalists established a separate organization (Gambs 1980), a step that signaled the demise of institutionalism as a significant power within mainstream economics (Yonay 1998, 189).

The formalist revolution would consolidate the new notions of "science" and "theory". The vision of the Cowles Commission on the nature of the empirical research would prevail in the profession (Mirowski 1989, 83). But it does not mean that, when the merits are judged by themselves and the conceptions of "science" and "economics" at the time, that the winners' victory is beyond reproach. We argue this for three reasons.

First, we need to observe closer the philosophical influences of the characters of this story. Without it, all we can say is that (i) none of them seemed to have a deeper knowledge of philosophy of science, besides some links with the Vienna Circle and physics-emulating scientism; (ii) from the epistemological point of view, there was nothing inherently superior in the search for advanced mathematical tools. As mentioned before, "empiricists" enjoyed great prestige as scientists[15] .

This prestige, nonetheless, did not prevent even institutionalists from misunderstanding Mitchell's methodology. A case in point is the folowing statement by Rutherford (1987, 68):

> The shortcomings of this method are not difficult to locate. Although Mitchell is not guilty of naive empiricism, he viewed theories primarily as attempts to systematize the known facts (Mitchell, 1950, p. 409). This position militates against the making of bold theoretical conjectures or predictions of 'novel facts,' and easily leads to *ad hoc* theorising as new information is found, or, alternatively, to the continual delaying of the tasks of appraising and re-formulating theories on the grounds that the search for new information should first be allowed to proceed further [a footnote is attached: The latter point is also made by Robert S. Lynd (1967, pp. 120-121). Mitchell's response to Lynd's criticisms can be found in Lucy Sprague Mitchell (1953, pp. 553-568)].

[15] And they did not lose their prestige because of that. Hendry e Morgan (1995, 69-71) reviewed the controversy, taking sides with the NBER. Walter Friedman (2018) recalled Mitchell's contributions to research on business cycles, as Schumpeter ([1950] 1997) once did. Rizvi (1997, 279-80) claimed that the statistical literature vindicates Vining position on probability distribution of variables and on aggregate level properties.

Now, although Rutherford is an expert in the history of institutionalism, we argue this evaluation is plainly wrong. It applies a Lakatosian framework to Mitchell. Isn't it anachronistic? Does any economist comply with Lakatosian criteria for the growth of knowlege (it is easy to remember of McCloskey's critique of Blaug)? One could ask: what about the identification problem? We might reply: is not it pervasive? Have economists found the bedrock of invariant structures on which to build economic theories?

Moreover, in the same page, when Rutherford quoted (Mitchell [1931] 1995, 409), we read the foundation of Mitchell's methodology, instead – the interactive process between theory and facts, as a way of discovering (precisely) new facts! Besides, in the address pointed out in the footnote, Robert Lynd's critique of Mitchell and Michell's response are not about validity of empirical methods, but rather, referencing Feuerbach's 11th thesis, how Mitchell analyses the world as it is, whereas Lynd is unpatient to change it.

Yonay (1998, 185-6) points that the desire to mathematize economics came from young students, encouraged by their professors, who also had interest in mathematics, without institutional support. Changes in academic appointments, such as Robbins being conducted to LSE after Allyn Young's death in 1929, also are important to understand this change. Pointing to many historical factors, the "character of transformation", for Morgan and Rutherford (1998, 14-17), can be explained by (i) the success of optimization techniques and operational research during World War II, suggesting it could extended to other areas; (ii) a change in the meaning of "scientific objectivity" – from honesty and fairness in data analysis to "technical rigor"; (iii) the very Cold War itself, which was also a "war of economic ideologies", favoring more liberal approaches, where new techniques worked better in an "abstract", free-market environment; and (iv) McCarthyism, which gave another impulse to reduce pluralism, in order to keep economics neutral and technical in order to avoid censorship[16].

Second, the controversy was, above all, an opportunity taken advantaged by Koopmans to promote the Cowles Commission's statistical approach and the Walrasian general equilibrium approach – at that moment, still in process of axiomatization – as the foundation of the stability of structural equations of their models. Thus, they are dealing with different conceptions of "science", "objectivity" and "rigor".

Third, as many observers noted, agreeing with Vining, Koopmans's promotion of Cowles was "short-selling", since the Cowles Commission did not currently have works that proved the superiority of the statistical approach[17]. Concerning this issue, we can point to impressions from two economists active during the controversy, but that did not participate in it. The first is Milton Friedman, who cannot be considered fully impartial, since he was part of the dispute with Cowles Commission for the Department of Economics of the University of Chicago. Answering Joseph Willis, director of the

[16] This view is contested by Weintraub (2017), who argued that McCarthyism did not have the influence some scholars claim it had in pushing economics towards a more value-free approach.

[17] And neither they would later. Hendry and Morgan (1995, 71) pointed that the Monograph 10, where the Cowles Commission accounts for its results, was overly shy, aware of the limitations in supporting Haavelmo's program. Mirowski (1989, 83) argued that, after the controversy, Cowles researchers lost interest in empirical works and focused on axiomatic theories for econometrics and economics.

Division of Social Sciences of the Rockefeller Foundation, concerning his expectations on Cowles Commission's research:

> [T]here are certain special characteristics of the group of people listed that lead me to retain considerable condence that their experiment will fail. Almost without exception, the people listed are primarily mathematicians or statisticians rather than economists and have had no occasion to do careful scientic quantitative work on a limited segment of the economy. Koopmans, who strikes me as perhaps the ablest and most promising of the Cowles staff and who probably plays a crucial role in determining the direction their works takes, has fundamentally a theoretical mind and inclination, and came to economics relatively late from mathematics and statistics. He has taught himself a great deal of economics, but his economics is not really part of him. He is likely to do good work in elaborating the mathematical implications of stated premises, in developing statistical techniques, and in expounding complicated mathematical and statistical techniques. But I have no great condence in his judgement about realistic economic problems or about techniques for attaining sound knowledge of economic processes. (Friedman to Willits, September 26, 1947; quoted in Boumans 2016, 598).

The second is Eduard Malinvaud, an unsuspecting econometrist, analyzing the controversy in retrospective:

> Malinvaud ([1972] in his review of the complete works of Koopmans revisited the Koopmans-Vining controversy. After evoking the context of the controversy in the late 1940s, he began by recalling how everyone had found Koopmans' assaults virulent and pretentious, all the more that the probabilistic approach had not yet yielded any tangible results. He went on to affirm that Vining was perfectly well informed of the advances in structural econometrics and that his defence of the empirical approach of the NBER was entirely well founded, writing: "Economic knowledge (notably in the study of fluctuations) was then still insufficient to avoid the search for empirical regularities between macroeconomic indicators or variables and to be based on a behavioural model specifying *a priori* what their relationships could be". He [Malinvaud] finally considered that the most interesting part of the exchanges were those concerning the respective limits of the two approaches. He thus stressed Vining's objection that it was, at the time, still too early to focus on the logically secondary problems of testing, estimation and forecasting writing: "Vining insisted that: "[…] modern theories of statistical estimation and of testing of hypotheses […] are almost beside the point in the exploratory stage that characterizes a great part of the work in all developing fields of knowledge"; "Koopmans finally agreed with this point when he wrote: […] there remains scope for doubt whether all hypothesis-seeking activity can be described and formalized as a choice from a preassigned range of alternatives" (Armatte et al. 2017, 91).

5. Concluding remarks

This paper aimed to correct a pervasive notion by which Mitchell was an adamant empiricist and theory-eschewing taxonomist. He was a scientist and was considered a scientist by his peers. This relates to the meaning we can attribute to the term "theory" in

Mitchell's methodology. Did Mitchell *use* some theory as a point of departure for his investigations? Yes, we hope to have shown so. Or, did Mitchell *develop* any theory of his own? Yes, several economists acknowledge that he did. But, if science is coextensive to a *formal model*, then no, Mitchell did not have a theory.

The validity of inductive methods is an exceedingly complex question to be decided by peremptory epistemological statements, especially if they are formulated in a framework of methodological monism. The "measurement without theory" controversy was used here to understand (i) the inflow of new mathematical devices in the "toolkit" of economists and their efforts to "homogeneíze" the discipline (Orozco-Espinel 2017); and (ii) Mitchell's methodology as applied at the NBER. As we have seen, Mitchell's methodology did bring new contributions to economics and, although deductive methods can bring more psychic safety to the researcher, it is not clear that they are the best path to knowledge. What do we say about the many case studies in different areas of economics? Are they second-class economics? More importantly, Vining's reply shows an ontological concern among the economists of his time, but attuned with the realist perspective of original institutionalism.

The controversy covers an important episode in the history of recent economic thought, the emergence of modern mainstream. But we remain with the impression that the winner side, mathematical economics, had an illusionary victory. Many types of arguments were used, not just cold facts or logic. And the controversy was not only about theory or method. It also involved academic skirmishes over positions and prestige. It was fought with pencils and blackboards, but also with malice and lobbying. We think that our paper contributes to cast doubts on the supposed superior methods and victors in methodological debates. Those among us that are old enough to remember of capital controversy between MIT and Cambridge are suspicious of those claims of superiority. For them, seems more apropriate to evocate the old saying: "If all you have is a hammer, everything looks like a nail".

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