THE MAKING OF INDEX NUMBERS IN THE EARLY 1920s: A CLOSER LOOK AT THE FISHER–MITCHELL DEBATE

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ABSTRACT

The systematic emergence of the axiomatic approach to index numbers took place in the early 1920s, a period marked by pluralism in American economics. Promoted by Irving Fisher, the axiomatic approach to index numbers aimed to select a universally valid, ideal formula for index numbers by employing a series of statistical tests. However, from the first presentation of Fisher's approach, at the 1920 Annual Meeting of the American Statistical Association, to the publication of his book, *The Making of Index Numbers*, in 1922, he faced a series of criticisms, not addressed to his ideal formula *per se*, but rather aimed at the very idea that a universal formula for index numbers could be singled out. The main individuals involved in this debate were Wesley Mitchell, Warren Persons, Correa Walsh (who was Fisher's only supporter), and Allyn Young. Among them, the foremost representative of Fisher's antagonists was Mitchell. This study argues that the disagreements between Fisher and Mitchell result from their different backgrounds and their distinct understandings of economics as a "science." Therefore, the objective of this study is to illustrate how Fisher, as a mathematical economist, privileged the universality of economic theories, while Mitchell, as an institutionalist, understood economics as a contextual and historical discipline, and how these preconceptions spilled over to their debates on index numbers. To illustrate their positions, this study explores their archival correspondence.

Keywords: index numbers, Irving Fisher, Wesley Mitchell, axiomatic approach.

JEL Codes: B16, B31.

1. INTRODUCTION

Five months before his death, Francis Ysidro Edgeworth's (1845–1926) last published words on index numbers were a cry for pluralism in that subject. At the terminus of a period marked by intense discussions on index numbers, Edgeworth's (1925: 388) final remarks on the issue were: "It would be too much to ask economists, what Cromwell asked theologians, to think it possible that they might be mistaken. Each maker of index-numbers is free to retain his conviction that his own plan is the very best. I only ask him to think it possible that others may not be entirely mistaken." By the time Edgeworth spoke these words, a long-lasting tradition of theories and methods on index numbers, whose roots may be traced back to Bishop William Fleetwood in 1704, had boiled down to a series of interrelated—and often antagonistic—studies on the matter (Balk 2008; Diewert 1988; Ferger 1946). Such authorities as Arthur L. Bowley,

Irving Fisher, Wesley Clair Mitchell, Warren Milton Persons, Correa M. Walsh, Allyn Abbott Young, George Udny Yule, and Edgeworth himself took part in these discussions, which lasted approximately from 1920 to 1925. The controversy carried out by these individuals did not bring to an end the interest in index numbers, but it certainly marked the subject's liveliest period throughout the history of economics.

The main point in dispute in this controversy concerns the existence of an ideal formula for index numbers. Fisher inaugurated this debate, and, in opposition to Edgeworth's aforementioned plea, sought an ideal formula for index numbers appropriate for all purposes. His first defense of the so-called ideal formula took place nearly one hundred years ago, in a meeting of the American Statistical Association, in December 1920. Fisher's presentation was discussed by Mitchell, Persons, and Walsh, and Fisher himself closed the debate with a rejoinder. These five addresses were published in the *Quarterly Publications of the American Statistical Association* (Dimand 1998c: 131) in 1921, and inaugurated a period in which the axiomatic—or test—approach to index numbers prevailed.

The core idea of the axiomatic approach is that the most appropriate formula for index numbers should be selected from its capacity to satisfy a number of statistical criteria in the form of tests (Banzhaf 2004: 599; Diewert 1988: 9; Dimand 1998c: 128, 2019: 138). In the 1920 debate, Fisher's defense of an ideal formula suited for all purposes through the test approach, which would later be expanded into his 526-page book *The Making of Index Numbers* (henceforth, MIN), published in 1922, was endorsed by Walsh, and Walsh was the only supporter of Fisher's proposal among the authorities at the center of the controversy throughout 1920–1925.[1] Mitchell and Persons, meanwhile, held critical positions regarding Fisher's quest for an ideal formula, questioning its validity and challenging the tests as insufficient for the establishment of a formula's all-purpose superiority over the others. Their opposition to Fisher's recommendation had nothing to do with the formula Fisher regarded as the best one, but focused on the premise that *any* formula could be singled out as the best one for all purposes.

Our claim about the nature of the debates centered on Fisher's ideal formula is in line with Friedman (2014: 130). We argue that the clash concerning the universal validity of a formula for index numbers was rooted in a deeper and more meaningful divergence: the debates contrasted two opposite conceptions of "science." One side of the debate defended economics as a purely mathematical science, capable of unraveling the inner mechanisms of the economy

[1] In fact, Walsh had inspired Fisher's approach in the first place through his 1901 book *The Measurement of General Exchange-Value*. Fisher, however, "carried this test approach to index numbers much further than Walsh had" (Dimand 2019: 152).

through deductive reasoning. The other side favored historical events and empirical observations in economics, with no *a priori* commitment to a theoretical compass—an approach later labeled "measurement without theory" (Koopmans 1947). Fisher, opening the debate, hoisted the colors of the first standpoint, while Mitchell was the foremost representative of the second. Alongside them, to a greater or lesser degree, all other scholars at the center of the controversy espoused one conception of science or the other, which oriented their perspectives on the universal validity of any formula for index numbers.

Accordingly, our objective is to explore the early-1920s—that is, 1920–1922—controversy regarding the existence of an ideal formula for index numbers, and to cast light on the conceptions of science involved in the dispute. To do so, we first present Fisher and Mitchell against the pluralistic background of 1920s American economics. Then, we focus on the construction of Fisher's ideal index number formula from the 1920 Meeting of the American Statistical Association to the MIN. Finally, we discuss how the plurality of intellectual backgrounds served as a catalyst for the different conceptions of "science" and conditioned the debate about the making of index numbers during the early 1920s. Some concluding remarks close our study.

2. FISHER AND MITCHEL: BACKGROUND AND INFLUENCE IN A PLURALISTIC ENVIRONMENT

American economics in the interwar period was marked by pluralism. This is especially true in relation to the 1920s. That "[p]luralism meant variety, and that variety was evident in beliefs, in ideology, in methods, and in policy advice" (Morgan and Rutherford 1998: 4). Many different ways of doing economics coexisted and there was no "correct" way of doing economics that prevailed. "There was no hegemony of method" (Morgan and Rutherford 1998: 6). Influential economists relied on different methods of scientific research and studies based on history, statistics, mathematics, empiricism, and theoretical deduction blossomed.

In this exciting and fruitful environment, neoclassical economists and institutionalists were especially representative of the profession. American neoclassical economists, grounded in price theory and a predilection for statistics and mathematical analysis, found in Fisher, Frank Knight, and Jacob Viner, its most prominent individuals.[2] American institutionalists,

[2] According to Emmett (1999: chapter 5), even though marginalism had some centrality in Frank Knight's thought, his contributions to economics reveal no dominant paradigm. This renders it difficult to place Knight in a school

meanwhile, did not share a homogeneous research program despite their propensity for historical analyses; the most prestigious economists in this approach were John Maurice Clark, John Commons, Walton Hamilton, and Wesley Mitchell (Morgan and Rutherford 1998: 2–3).

Within this pluralistic milieu, our interest lies in one economist of each group, that is, Fisher and Mitchell. The former, a Yale University professor, was a pioneer of the mathematical approach to economics in the American academy, modeling the economy according to such areas as mechanics and thermodynamics, but he was also committed to his theory's statistical and historical correspondence with reality (Dimand 1998a: 450; Scott 2018: 525–526; Tobin 1985: 30). The latter, a Columbia professor, unlike most institutionalists of his time, also had a tendency to privilege quantitative methods in economics, with special emphasis on statistics in the pursuit of scientific knowledge and little attention to *a priori* theory (Dimand 1998a: 460, 1998b: 197; Hirsch 1976: 196). Fisher and Mitchell, accordingly, differed in terms of theoretical affiliation, but shared the same esteem for measurement in economics (Breslau 2003).

The contact points between these two economists, however, transcend their admiration for quantitative methods. First, their relative importance to the profession in the 1920s is demonstrated by the number of citations they received from their peers in that decade. Between 1920 and 1930, the two most cited economists in the English language were Fisher (30 citations) and Mitchell (24 citations) (Deutscher 1990: 189).[3] Second, we must acknowledge the—direct or indirect—influence exerted on their formation by Yale professor and social theorist William Graham Sumner. Sumner is credited with suggesting to Fisher that he write his dissertation on mathematical economics and advised him in the study, giving birth to Fisher's 1891 dissertation entitled *Mathematical Investigations in the Theory of Value and Prices*—which Samuelson (1967: 22) once called "the greatest doctoral dissertation in economics ever written" (Cot 2005: 795; Dimand 1998a: 450). By then, however, Sumner had already advised, in 1884, the dissertation of the pioneer of American institutionalism, Thorstein Veblen, entitled *Immanuel Kant's Ethics*, and Veblen, for his turn, was one of the most influential economics professors on Mitchell's formation, at the University of Chicago, as institutionalism is regarded as passed from Veblen through Mitchell—as well as Robert Roxie—to the next generation of American

of thought. The literature, however, highlights the legacy he left in promoting neoclassical economics. In Emmett's (1999: 63) words: "[…] the rise of neoclassical economics during the interwar period is, in no small part, directly attributable to Knight"—the same argument is defended by Kern (1997). Hence, for his legacy, this study classifies Knight as a neoclassical economist.

[3] Deutscher (1990) counted citations in articles included in the *AEA Index of Economic Journals* under the headings "Aggregative and Monetary Theory and Cycles" and "Money, Credit and Banking." His numbers, therefore, do not include discussions referring to index numbers, which likely would have increased even further both Fisher's and Mitchell's position—especially Fisher's (Dimand 1998b: 194).

institutionalists (Banzhaf 2004: 596: Biddle 1998: 45; Breslau 2003: 400–401; Rutherford 2011: 125–128).

Furthermore, both Fisher and Mitchell were directly involved with entities responsible for empirical work in economics. Mitchell worked with the Bureau of Labor Statistics during World War I and later on (Banzhaf 2004: 592; Schumpeter 1950: 142), and was one of the founders of the National Bureau of Economic Research in 1920 (Biddle 1998: 53; Dimand 1998a: 460, 2013: 24). Fisher founded the Index Number Institute, in 1923 (Barber 2005: 51; Dimand 1998c: 140, 2013: 32, 2019: 150), and participated, alongside Ragnar Frisch and Charles Roos, in the establishment of the Econometric Society, in 1930 (Bjerkholt 2017: 175; Dimand 1998b: 194; Scott 2018: 527). Both also served as presidents of the American Economic Association (Fisher in 1918, Mitchell in 1924), the American Statistical Association (Mitchell in 1918, Fisher in 1932), and the Econometric Society (Fisher 1931–34, Mitchell 1942–43) (Dimand 2013: 24).

Finally, it is worthwhile highlighting the formation of Fisher's and Mitchell's intellectual interests, as this may shed light on the paths that led them to their considerations of index numbers.

Fisher was a purebred Yale scholar. He joined the university in 1884 as an undergraduate student, went on to become a doctoral candidate after graduating with a BA in 1888, and graduated with a Ph.D. in mathematics and political economy in 1891. Fisher's Yale pure breeding kept him away from the path followed by most of his contemporaries, whose postgraduate formation usually took place under the direct influence of the German Historical School of Economics. Influenced by Josiah Willard Gibbs, Fisher relied on the methods that had built physics to develop economics into a true mathematical science. Once appointed faculty member, also in 1891, Fisher did not succeed in attracting the devotion of many students, and his classes were usually small. Still, in 1898, he was promoted to a full professor of political economy. Fisher's intellectual interests were diverse and transcended economics, especially after his recovery from tuberculosis in 1901, which convinced him that life as an ordinary academician was not sufficient: he had also to answer a social calling (Barber 2005: 45–46, 49; Breslau 2003: 395–397; Cot 2005: 796; Dimand 1998a: 451, 2013: 33–34).

From that time onwards, Fisher became "an inveterate crusader for causes" (Barber 1998: 31), which led Frisch (1947: 2) to state that he knew of "no man who has such a broad range of interests as Professor Fisher." On his social crusades, Fisher covered such subjects as compulsory health insurance, diet reform, the controversial issue of eugenics, the prohibition

of alcohol, and the creation of the League of Nations. Likewise, his scholarly interests were manifold, but always related to the development of economics as a mathematical science. Accordingly, economists owe Fisher for several theoretical advancements in, among others, the fields of general equilibrium analysis, index number theory, monetary economics, and econometrics (Banzhaf 2004: 596; Barber 2005: 43–46, 49; Cot 2005: 794; Dimand 1998b: 195, 2013: 20, 2019: 2).

Mitchell, in the words of Adolf Berle Jr. (Berle 1953: 169), "can fairly be named the greatest American economic scholar of the twentieth century." Mitchell received his Ph.D. from the University of Chicago in 1899, and he considered the university as the most stimulating for social science at that time (Gruchy 1947: chapter 4). While studying toward his Ph.D., Mitchell was exposed to a large social science perspective, which shaped his view of economics. This perspective encompasses J. Laurence Laughlin's traditional and Veblen's non-traditional views of economics (as previously highlighted), John Dewey's pragmatic philosophy (Ginzberg 1997; Gruchy 1947: chapter 4), and Jacques Loab's behaviorist psychology (Berle 1953; Schumpeter 1950). For Gruchy (1947: chapter 4), Laughlin influenced Mitchell's interest in price analysis and monetary theory, and Veblen influenced his perspective on business cycles. Ginzberg (1997: 375) summarizes the relationship between Mitchell and Veblen, in terms of economics, as an agnostic student being reinforced by a skeptical professor. According to Ginzberg (1997: 375), "Veblen thought that economics had much to learn from biology [Darwinism], but Mitchell desired to emulate physics and chemistry. These sciences depended largely on observation." From Dewey, Mitchell learned a great deal of the logic of human conduct, reinforcing his predilection for empiricism (Gruchy 1947: chapter 4). For Schumpeter (1950), Mitchell's contact with both Dewey and Loab opened his mind to a much broader notion of social science than that of a usual economist of his time.

During his Ph.D. years, Mitchell became more skeptical about theory and was suspicious of established logics, mainly because of his contact with Dewey (Ginzberg 1997). Alternatively, we can affirm that, given Mitchell's exposure to a large social science perspective, he looked for an economic analysis free from theoretical preconceptions (Rutherford 1987). He considered facts to be much more important than theories (Ginzberg 1997). This is the reason why Mitchell contributed largely to the improvement of empirical methods in economics. Berle (1953) affirms that Mitchell guided economics from a speculative development of hypotheses to science because of his devotion to empiricism. Mitchell is nowadays recognized as an outstanding American empirical economist from the first half of the

twentieth century and as the father of business cycle research (Ginzberg 1997; Gruchy 1947: chapter 4; Klein 1983).

3. THE RISE OF THE AXIOMATIC APPROACH: FROM THE 1920 ANNUAL MEETING OF THE AMERICAN STATISTICAL ASSOCIATION TO THE MIN

The interest in something resembling index numbers is usually traced back to Fleetwood, in 1704, who tried to compare the value of money in Oxford between 1460 and 1700 (Balk 2008: 2; Diewert 1988: 4; Fleetwood 1745).[4] In the two centuries between Fleetwood's work and the 1920 Annual Meeting of the American Statistical Association (ASA), several subjects and purposes were covered, and multiple approaches were sketched.[5] Prominent economists and statisticians, such as Gian Rinaldo Carli, Joseph Lowe, William Stanley Jevons, Étienne Laspeyres, Hermann Paasche, Alfred Marshall, Harald Westergaard, and Edmund Ezra Day, helped to develop these approaches.

This diversity of approaches and interests nearly came to a halt in the 1920s, with the rise of the axiomatic—or test—approach to index numbers. By no means was this approach the only one present in the 1920s discussions, as Aldrich (1992) and Frisch (1936) make clear, but it certainly dominated as none had before the debates on index numbers throughout that decade, especially between 1920 and 1925.[6]

The first researcher to work on the axiomatic approach in a more systematic manner was Walsh (1901), but it was Fisher (1921a, 1922) who placed it at the center of the debate (Banzhaf 2004: 599; Frisch 1936: 5).[7] It is curious to note, however, that Fisher's first reaction to Walsh's 1901 book, *The Measurement of General Exchange Value*, was hostile to the axiomatic proposition. Fisher criticized Walsh's work for seeking a single method in the making of index numbers, instead of considering the different purposes involved in such processes. In

[4] According to Kendall (1969: 8), however, it is likely that the first use of the word *index* to denote the efforts to measure and compare the value of money and the level of prices took place only in 1837, in a series of letters published in the *Aberdeen Herald* by W. Cross.

[5] Diewert (1988) identifies five different approaches to index numbers: (i) the fixed basket approach; (ii) the statistical (or stochastic) approach; (iii) the test (or axiomatic) approach; (iv) the Divisia approach, and; (v) the economic approach.

[6] The 1920s also witnessed the rise of the so-called economic and Divisia approaches. The pioneer of the former was A. A. Konüs in 1924 (published in English in 1939), who was strongly influenced by Edgeworth's stochastic approach. The latter was designed by François Divisia, in 1926. For more on these approaches, see Aldrich (1992), Diewert (1988), Divisia (1926), Frisch (1936), and Konüs (1939).

[7] Boumans (2001) offers an alternative account of Fisher's approach to index numbers, labeling it instrumental, instead of axiomatic. In Boumans's account, Fisher's background as a mathematician and a philosopher cannot be ignored. This is an insightful interpretation, but, as much as the label does not make a difference to the argument we present, we restrict ourselves to the usual definition of Fisher's approach as axiomatic.

his review to Walsh's book, published in 1902, in the *Yale Review*, Fisher wrote: "[w]e cannot agree with Mr. Walsh when he maintains that the purpose of an index-number has nothing to do with its measurement" (*apud* Fisher 1924: 91). Walsh (1921b: 120) would later argue that Fisher's diagnosis followed Edgeworth's (1901), who, while recognizing Walsh's merits and "almost Kantian elaborateness" (Edgeworth 1901: 405), raised similar questions.

Fisher eventually changed his position "to (essentially) that of Walsh" (Fisher 1924: 91). This is manifested in *The Purchasing Power of Money* (henceforth, PPM), published in 1911, which would become even more flagrant in the MIN. The evolution of Fisher's approach to index numbers led Vogt and Barta (1997: viii) to identify the PPM as the Old Testament and the MIN as the New Testament of his approach to index numbers. Nonetheless, the key event in the escalation of Fisher's defense of the test approach was the Annual Meeting of the ASA in December 1920. In the event, Fisher engaged in his first systematic defense of the so-called Fisher ideal formula for index numbers, a formula that would supersede all others in its capacity to fulfill the "supreme tests" of factor reversal and time reversal (Fisher 1921a: 534). Since, according to Fisher (1921a: 536), few formulae conformed to both tests, the simplest formula should be singled out as the "best" formula.

Fisher's 1920 presentation inaugurated a period in the history of index numbers in which the discussions were almost entirely concentrated on his proposed ideal formula. His presentation was published in the *Quarterly Publications of the American Statistical Association* in 1921. Fisher (1921a: 533) defined five elements considered in the making of index numbers: the character of data, the data sources, the frequency of calculation, the base chosen, and the mathematical formula used. His only concern was the latter. Accordingly, his aim was to identify the *best* formula for index numbers, and only by testing different formulae, he argued, could one single out the best formula to be used. Among the different tests—Fisher at this point recognized a dozen tests—the two supreme tests were the factor reversal test and the time reversal test, which Fisher (1921a: 534) did not label in this fashion yet. The former stipulated that multiplying a price index and a quantity index for a given year should equal the index number of value—the value itself—for that same period; the latter specified that calculating an index number for, say, 1918 in terms of 1913 should be consistent with calculating an index number for 1913 in terms of 1918.

Fisher dismissed the arithmetic and the harmonic means for their inadequacy in front of these tests, and pointed out that simple weighting methods would introduce biases. Laspeyres's index (formula 1), an arithmetic mean, has an upward bias, while Paasche's index (formula 2),

a harmonic mean, has a downward bias. Fisher then worked around these issues by suggesting a geometric mean between Laspeyres's and Paasche's index, which would offset such biases.[8] This formula, which Walsh (1921a: 539) named Fisher's index (formula 3), was the simplest, and therefore, the *best*—to satisfy Fisher's supreme tests. This formula had already been briefly suggested by Bowley (1899), Walsh (1901), and Pigou (1920), but no extensive defense of its validity had been made before Fisher.[9]

The debates initiated in the 1920 annual meeting of the ASA led Fisher to expand his initial idea for a paper into a larger work, giving birth to the 526-page book MIN, in 1922. From its publication onwards, the MIN and its propositions would serve for several years as the barycenter for discussions on index numbers.[10] As a product of its context, Fisher's book followed the controversy of purpose-guided index numbers versus the idea of a universal, ideal one. Accordingly, in the first chapter, Fisher (1922: chapter 1) established that different forms of calculating index numbers would imply different results. The key issue for Fisher (1922: 1–2) was: "[h]ow different are the results, and can we find reasons for accepting some and rejecting others?" That is the reason that Fisher (1922: 2) focused the MIN on both the investigation and the test of "all the formulae for index numbers which have been or could reasonably be constructed." Fisher was worried about the uncritical use of index numbers. Another goal of the book was "to help make the calculation of index numbers rapid and easy" (Fisher 1922: 2). Looking at the MIN as an evolution of Fisher (1921a), we can affirm that he went even further in testing index numbers to reaffirm his ideal formula introduced during the 1920 Annual Meeting of the ASA.

Fisher (1922: Chapter 2) relies on the price movements of thirty-six commodities, from 1913 to 1918, as the raw material for the calculation of the index numbers he would like to analyze. Fisher used 134 kinds of formulae and extracted the data collected by Mitchell for the War Industries Board. Even though 134 kinds of index numbers were calculated by Fisher

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[8] Fisher (1922: 59) credits Walsh (1901: 558) with the creation of the names Laspeyres's index number and Paasche's index number.

[9] $I_{01}^{L} = \frac{\sum p_1 q_0}{\sum p_0 q_0}$ (1)

$I_{01}^{P} = \frac{\sum p_1 q_1}{\sum p_0 q_1}$ (2)

$I_{01}^{F} = \sqrt{\frac{\sum p_1 q_0}{\sum p_0 q_0} \cdot \frac{\sum p_1 q_1}{\sum p_0 q_1}}$ (3)

[10] The MIN is dedicated to Edgeworth and Walsh, who Fisher (1922: dedication) called "pioneers in the exploration of index numbers" (in the dedication, no page).

(1922), they all followed six types: (1) arithmetic average, (2) harmonic average, (3) geometric average, (4) median, (5) mode, and (6) aggregative formula. According to Fisher (1922: chapter 2), these were the types of average considered, or likely to be considered, for index numbers during that time.[11] Fisher (1922: 43) affirms that "[…] the purpose of any index number is to strike a 'fair average' of the price movements." However, Fisher (1922: 43) points out that there are enormous differences in importance. Hence, for Fisher (1922: chapter 3), it is essential for an index number to take the weighting into consideration. A key question for Fisher (1922: chapter 3) is how to provide fair weighting. Fisher (1922: chapter 3) analyzes the constant weighting and what he called the system of base value weights ("Weighting I" in MIN) and the system of given value weights ("Weighting IV" in MIN).[12]

For Fisher (1922: 62), for an index number to be fair, it has to work for both prices and quantities—any two commodities to be averaged, two times to be compared, or two sets of associated elements for which index numbers may be calculated. The rule of changing places must be applicable to different magnitudes: commodities, time, and the two factors—price and quantity. The rule of changing places means three separate things: interchanging any two commodities, interchanging any two times, and interchanging prices and quantities. Therefore, the rule of changing places must treat alike (1) any two commodities; (2) any two times; and (3) the two factors. Fisher (1922: Chapter 4) proposes three tests of fairness: (1) a preliminary test: the commodity reversal test, (2) Test 1: the time reversal test, and (3) Test 2: the factor reversal test. In Fisher's (1922: 63) words: "[a]ny formula to be fair should satisfy all three tests."[13] For Fisher (1922: 118), not only Test 1 and Test 2 reveal errors in index number

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[11] For Fisher (1922: chapter 2), just one of them, (5) mode, had never been seriously proposed for actual use for index numbers. The mode, nonetheless, is also referred to in connection with the index numbers debate.

[12] For Fisher (1922: 45–46), constant weighting is never correct as values of the weight process widely change. Fisher (1922: 53–56) introduces two other systems of weighting. Fisher (1922: 53) names the system of base value weights "weighting I" and the system of given value weights "weighting IV." The reason for giving the names "I" and "IV" is that the others, for Fisher (1922: 53–54, emphasis in the original), fall between these extremes.

> "In Systems *II* and *III* each commodity is weighted by a hybrid value, relating not to the base year alone nor the given year alone but partly to one and partly to the other. In system *II* the value is made by multiplying the *price* of each commodity in the *base* year by the *quantity* of that commodity in the *given* year. In system III each commodity is weighted by the other hybrid value formed by multiplying its *price* in the *given* year by its *quantity* in the *base* year."

In Fisher's (1922: 55–56) numerical illustration, weight systems II and I give almost identical results, and the same applies to systems III and IV. However, the gap between the results of the latter pair is bigger than that between the former. The same results—I and II versus III and IV—are found in the weighted medians (less pronounced) and the weighted modes (the contrasts are present but less perceptible).

[13] The commodity reversal test means that the order of the commodities must make no difference. Commodities could be interchanged and their order reversed without affecting the resulting index number (Fisher 1922: chapter 4). For Fisher (1922: 63), the commodity reversal test "[…] is merely taken for granted and observed instinctively." Regarding the time reversal test, Test 1, for Fisher (1922: 64), an index number is associated with a set of commodities, but it is implied in only two times (or places). The times (or places) chosen cannot make a difference;

formulae, but they also allow rectification. To rectify the formulae, Fisher (1922: 118) proposes calculating their antithesis, naming them time antithesis and factor antithesis, respectively.[14] Furthermore, Fisher (1922: 183, emphasis in the original) claims that "[i]nstead of crossing the formulae themselves, previous students of index numbers have crossed their *weights* […]." However, Fisher (1922: chapter IX) is critical of weighting, and his criticism relies on testing. For Fisher (1922: 196), formula crossing is a universal method of compromising between two formulas. Weighting cannot rectify any formula through Test 2 and just some formulae through Test 1.[15]

Therefore, Fisher (1922: chapter X) compares numerous formulae to select the theoretically best formula. By best formula, he means the most accurate one. Fisher (1922: chapter X), looking for the best index number, segments the evaluation of accuracy into two kinds: (1) regarding no weights, and hence, the use of simple averages, and (2) concerning the opposite, working with weights.[16] After ruling out several formulae by testing their accuracy,

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Test 1 relies on that (Fisher 1922: 64). Test 1 is about getting the same result despite the time taken as the base. "[T]he index number reckoned forward should be the reciprocal of that reckoned backward" (Fisher 1922: 64). For Fisher (1922: 65), most index numbers in use during the 1920s failed the reversal test. The factor reversal test, Test 2, means that the index number formula ought to allow interchanging prices and quantities without giving inconsistent results (Fisher 1922: 72). Consistency means that "[…] the two results multiplied together should give the true value ratio" (Fisher 1922: 72).

[14] According to Fisher (1922: 118), the time antithesis is found by applying Test 1 to that formula. By interchanging the two times, we obtain the index number reversed in time. Then, dividing the last found expression into unity, the result should be the original formula. In this case, Test 1 is fulfilled. If Test 1 is not fulfilled, the resulting formula is the time antithesis. As stated by Fisher (1922: 125), the factor antithesis for a price index is found through Test 2. It is by interchanging the prices and quantities that we may unravel the index number of quantities. Through the division of that expression into the value ratio, the original formula would be the result in order to fulfill Test 2. If the result is not the original formula, it is its factor antithesis.

[15] Fisher (1922) also addresses a circular test for his ideal formula. Fisher (1922: 270) points out that some index numbers might not work consistently between two times or places. Test 1 requires such consistency; the circular test, however, requires consistency between all times and places. For Fisher (1922: 270), students of index numbers have not been taking the consistency of all times and places into account. They simply assume consistency between two times or places. "The only formulae which conform perfectly to the circular test are index numbers which have *constant weights*, *i.e.* weights which are the same for all sides of the 'triangle' or segments of the 'circle,' *i.e.* for every pair of times or places compared" (Fisher 1922: 274, emphasis in the original). Fisher (1922: 295) affirms that the circular test could be considered a triangular test. Of course, Fisher's ideal index number fulfills the circular test (Fisher 1922: 280). It incurs only in small deviations, which, for Fisher (1922: 284), cannot be understood as errors. For Fisher (1922: 297), students of index numbers would be inclined to accept the theoretical fulfillment of the circular test, but not its practice. According to Fisher (1922: 299), it is not difficult to calculate index numbers that fit better each pair of years. However, such procedure deals with the problem of tracing price movements through a series of years. Fisher (1922: 299) relies on his ideal formula to calculate for each separate year as the base. The result is a remarkably small difference between the bases taken into consideration, and thus, it would be a waste of time to calculate all possible inter-year indexes (Fisher 1992: 299). Hence, the practical inconsistencies are so slight that they can be neglected (Fisher 1992: 299).

[16] Fisher (1922: 206–207) eliminates formulae that possess upward or downward biases, as proved by Test 1. He also evaluates each index number formula in relation to its freakishness—the quality of being erratic (assuming that there are no nonerratic index numbers). Fisher (1922: 211) concludes that his geometric formula is the best one. However, he establishes that simple weighting should be considered proper weighting for that assumption to be correct—which Fisher does not consider to be the case. For him, the simple weight is freakish. Hence, Fisher

Fisher tests the remaining forty-seven formulae and determines that no more than thirteen satisfied both Test 1 and Test 2; the others are classified as biased (Fisher 1922: 219–220). Moreover, Fisher (1922: chapter XI) relies on other criteria to finally assume that his ideal formula is the best.[17] In the following chapter, Fisher (1922: chapter XII) compares his ideal formula to the twelve others he casts as nonbiased and nonfreakish, but, remaining faithful to his previous assessments, he concludes that his formula is the best one, and, therefore, is the ideal formula for index numbers.

4. THE DEBATE ON INDEX NUMBERS IN THE EARLY 1920S AND CATALYST BAKGROUND PLURALITY

This section relies on the nontechnical reactions to Fisher's ideal formula, that is, those that are not related to his statistical technique, but to his underlying theoretical compass. As previously addressed, Mitchell—alongside Fisher—was the key figure in American economics during the 1920s. Hence, as Mitchell was an active figure in the construction of index numbers (Mitchell 1915, 1921a) and a critic of Fisher's ideal formula (see Mitchell 1921a, 1921b), his perspective on Fisher's ideal formula was especially important for the debate. Nonetheless, the debate also involved other figures. Fisher's 1920 presentation during the ASA annual meeting, for instance, was published (Fisher 1921a) alongside three replies, by Mitchell (1921b), Walsh

(1922: 211–212) points out that it is not possible to close an argument that the geometric formula is better than the median. Accordingly, he closes chapter X affirming that the simple geometric average and the simple median gave closer approximations to the correct result and, if simple weighting does not happen to be too erratic, the geometric average is the best formula. According to Fisher (1922: chapter XI), simple index numbers and their derivatives should be excluded—those derivatives are mongrels that are almost contradictions in terms. Fisher (1922: chapter XI) also rectifies simple index numbers by Test 2. This test, however, presupposes a knowledge of weights that is not possible for simple index numbers. Fisher (1922: 214) stipulates then that rectifications of simple index numbers are mere curiosities. Simple index numbers, their antitheses, and their rectifications are thus ruled out by Fisher (1922). In addition, all modes and medians and their derivatives are discarded because they are freakish (Fisher 1922: chapter XI). That freakishness is true to weighted modes and medians and simple modes and medians (Fisher 1922: 214)—the freakishness of the median is less than that of the mode (Fisher 1922: 217). For Fisher (1922: 216), the freakishness of the modes and medians could be, obviously, mitigated—as any other index number could be improved by a larger number of commodities. Unfortunately, nonetheless, Fisher (1922: 216) affirms the lack of availability of data for such correction.

[17] For Fisher (1922: 220–221), "crossed formulae" can be preferred to "cross weight formulae" and their derivatives, because the latter ones "[…] fail to insure a middle course between the original formulae whose weights are crossed. They seem slightly erratic as compared with the rest." Those criteria reduce the thirteen remaining formulas to four. Fisher (1922: chapter XI) compares the remaining four formulas looking for the most accurate and algebraically simple one. For Fisher (1922: 228–229), the probable error of formula 353 is smaller than the probable error of the other formulas: it is smaller than 1 percent and it is at least eight times more precise than it needs to be. Fisher (1922: 224) points out—as, according to him, so does Pierson—that index numbers can never be absolutely precise: it is a question of degree of precision.

(1921a), and Persons (1921b), and a rejoinder by Fisher (1921b).[18] In the MIN's preface, Fisher himself recognized that his ideal formula had been highly criticized; Walsh was an exception. Despite that, Fisher (1922: preface) declared that only a "partial disagreement" existed, as several writings seemed to support his perspective at some level, like those of Persons (1921b) and Mitchell (1921b)—both replies to Fisher (1921a)—as well as Young (1921). We respectfully disagree with Fisher that these scholars disagreed only partially with his proposal; their disagreement was complete. The early-1920s debate regarding Fisher's ideal formula, accordingly, opposed Fisher (1921a, 1921b) and Walsh (1921a, 1921b), on one hand, and then Mitchell (1921a, 1921b), Persons (1921a, 1921b), and Young (1921), on the other hand.[19]

Mitchell (1921a) stresses that Fisher and Walsh held the position that an index number is itself a purpose.[20] For Mitchell (1921a: 24), the making of index numbers faced its experimental stage during the 1920s. However, his understanding of the evolution of the construction of index numbers differed from Fisher's (1921a, 1921b, 1922). According to Mitchell (1921a: 24), progress in the making of index numbers would differentiate many types of formulas, each with their clearly defined uses. Consequently, Mitchell (1921a: 24) affirms that the most systematic form to deal with the making of index numbers "[…] would be to begin with the different uses of index numbers and to consider the methods appropriate to each."[21]

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[18] In our analysis, we do not observe any criticism by Walsh of Fisher. The reason for that is our focus on a nontechnical debate. Walsh's reactions to Fisher rely on statistical issues. As aforementioned, Walsh (1921a, 1921b) echoes Fisher's diagnosis, which had actually been inspired by Walsh himself. Walsh (1921a: 539, emphasis in the original) recognizes that Fisher's index number is "perhaps *the* best," even though he has some reservations regarding Fisher's assessment of the subject. Walsh regards Fisher's use of the time reversal test as insufficient, as Fisher adapts it only to two periods, instead of adapting it to an indefinite number of periods. He also regrets the approach to index numbers Fisher adopts in the PPM, and remarks that the method Fisher uses to reach the best formula for index numbers is very different from his own. Ultimately, nevertheless, Walsh (1921a: 544) concludes his discussion by stating that: "for the past [the compiler should always] use Lowe's index number, and for the present and the future, [the compiler should always] use Fisher's." Walsh presents Lowe's index number in the form $\sum qp_1 \Big/ \sum qp_0$, in which the quantities are estimates of past quantities to which the researcher does not have precise data.

[19] After the MIN's publication, other individuals would engage in a new phase of this debate, such as Bowley (1923), Edgeworth (1923a, 1923b), and Yule (1923).

[20] Mitchell (1921a) is the Bureau of Labor Statistics Bulletin 284, an update of Bulletin 173 (Mitchell 1915). The differences are located in section "IV. Varieties of methods used in making index numbers." In this update, Mitchell receives contributions from Fisher, Young, Royal Meeker, and C. H. Verrill, who read and criticize drafts of Bulletin 284 (Mitchell 1921a: 7).

[21] In his assessment to the MIN, Mitchell had allies from other sciences. The great British statistician George Udny Yule, for instance, in his review of the MIN, introduced technical criticisms on Fisher's elaboration (Yule 1923). Even from his statistical perspective, however, Yule (1923) addresses nontechnical criticisms to Fisher's ideal index number as well. Yule's (1923) key point is that students of index numbers must take the purpose of a particular index number as the starting point and therefrom deduce the formula, and the MIN, he argues, seems to ignore it; by ignoring it, the purpose of an index number is not taken into account. Yule (1923: 427, emphasis in the original) highlights that the MIN stressed that "[…] accuracy is based solely on the agreement *inter se* of 13 different formulae, no one of which may be right for any clearly specified purpose; the so-called 'probable errors' on p. 227 are simply the quartile deviations of the 13 numbers from the general average."

For Mitchell (1921a: 24), students of index numbers should work under an inverse methodology. “Instead of studying methods in the light of uses, we must study uses in the light of methods. In other words, we must analyze the effect of the different methods followed in practice and so determine what the resulting figures mean and the uses to which they may properly be put” (Mitchell 1921a: 25). For Mitchell (1921a), Fisher’s perspective on index numbers exclusively focuses on prices. If we consider the MIN, we can affirm that Fisher himself would agree with Mitchell, as Fisher (1922: 43) affirms that “[…] the purpose of any index number is to strike a ‘fair average’ of the price movements—or movements of the other groups of magnitudes”—as previously addressed.

Fisher reacts to Mitchell (1921a) in the MIN. In the MIN, Fisher (1922: chapter XI) rejects the necessity for various index numbers in a very technical manner. According to Fisher (1922: 232), “[i]t is clear that a considerable part of the disagreement is more apparent than real and due to misunderstandings.” As Fisher states (1922: 232–233), Mitchell (1921a: 76–78) addresses seven purposes for using different index numbers for different situations. For Fisher (1922: 233), “[o]ne of these ‘purposes’ is the comparison with an existing series of index numbers, in which case the formula used should be identical with that used in the existing series.” Furthermore, Fisher (1922: 233) believes that Mitchell (1921a) did something different from him in his book, as Fisher (1922) compares different formulae with each other. Another issue that Fisher (1922: 233) addresses is Mitchell’s interest in making index numbers understandable by the common person. Fisher (1922: 233), alternatively, points out that his purpose was to secure the most accurate index number, his ideal formula. A reason for Fisher standing for an ideal formula is his belief that “[…] an index number formula is merely a statistical mechanism like a coefficient of correlation. It is as absurd to vary the mechanism with the subject matter to which it is applied, as it would be to vary the method of calculating the coefficient of correlation” (Fisher 1922: 234).

Mitchell’s (1921a, 1921b) reactions to Fisher’s proposal, however, were not the only ones. As Fisher made extensive use of his personal contacts with prominent students of index numbers in the composition of the MIN, he invited Mitchell, during the summer of 1921, for a weekend meeting on index numbers, to which Walsh was allegedly one of the names confirmed (Fisher to Mitchell, June 24, 1921, Wesley Mitchell Papers). Two months later, in a follow-up letter to Mitchell, Fisher writes that he had held a conference—possibly the same one mentioned in the previous correspondence—with several members of the Committee on Index Numbers of the Economic Association, but regretted Mitchell’s absence. The objective of the conference

was to discuss Fisher's ongoing work on index numbers, "[…] a manuscript containing the comparisons, by diagrams and otherwise, of index numbers calculated by different formulae" (Fisher to Mitchell, August 20, 1921, Wesley Mitchell Papers). That manuscript would become the MIN.

Despite Fisher's effort to gather comments on his manuscript from as many students of index numbers as possible, it is quite clear that he was especially eager to hear from Mitchell: "[…] I am anxious to secure your help and advice also. Would you be willing to look over the preliminary manuscript if I should send it to you in a few weeks?" (Fisher to Mitchell, August 20, 1921, Wesley Mitchell Papers). In November, Fisher finally sent the MIN's draft—then provisionally named *Methods of Constructing Index Numbers*—to Mitchell, hoping that the Columbia professor might offer some insightful comments on the manuscript (Fisher to Mitchell, November 10, 1921, Wesley Mitchell Papers).

In his reply to Fisher, issued only a month before Fisher submitted the final version of the book to his publisher (Dimand 1998c: 133), Mitchell declared that Fisher's book would "[…] certainly mark a new stage in the discussion of this subject [index numbers]." That said, Mitchell addressed three comments on Fisher's manuscript: (1) Mitchell remarks that Pigou (1920), in *Economics of Welfare*, had developed an index number identical to Fisher's ideal formula; (2) he recommends that Fisher check Fredrick R. Macaulay's (1916) paper for technical remarks regarding chain relatives, and; (3) Mitchell restates that "[…] I wish that you would set yourself to think about the different uses to which index numbers even of wholesale prices are applied" and "[…] I judge from the present manuscript that you have modified the position which you maintained at the meeting of the American Statistical Association two years ago, you still probably remain indisposed to go as far as I do in thinking that many uses for index numbers are likely to be found which may well call for the use of dissimilar formulae" (Mitchell to Fisher, February 16, 1922, Wesley Mitchell Papers). Fisher replies to Mitchell: he had noticed both Pigou's (1920) and Macaulay's (1920) contributions, and his book embraced both.[22] Regarding Mitchell's latter comment, Fisher simply affirms that "I [Fisher] have not modified the position which I maintained at the meeting of the American Statistical Association two years ago,—but you misunderstood me then" (Fisher to Mitchell, February 18, 1922, Wesley Mitchell Papers). In these remarks, it is important to stress that Fisher had no comments on Mitchell's recommendation for him to consider the different applications of index numbers.

[22] Fisher's Appendix III, "Selected Bibliography," mentions Macaulay (1916) and Pigou (1920). His list of selected bibliography is a rather short one, with only twenty entries.

Our argument is that the differences between Fisher's perspective on index numbers—the test of formulas to find the ideal one—and Mitchell's reaction to it are associated with both their backgrounds and their views on economics as a science. In this regard, an exchange of letters carried out between them a few years after the MIN's publication is illuminating. In 1926, Fisher sent to Mitchell a carbon copy of a letter of his to Willford King—Secretary-treasurer of the National Bureau of Economics Research (Fisher to King, November 9, 1926; Wesley Mitchell Papers; Mitchell to Fisher, November 13, 1926). In his letter to King, Fisher affirms that Mitchell was mistaken if he saw a conflict between his—King's—theory and Fisher's statistical findings (Fisher to King, November 9, 1926, Wesley Mitchell Papers). The point of dispute referred to the idea of normal prices and Fisher's distinction between transitional periods and periods of equilibrium, which Mitchell had hitherto deemed an "obscurity" in Fisher's treatment of causal relations. In Mitchell's words:

> "[…] I can interpret the term 'normal' as referring strictly to a condition complying with certain supposition used in developing an economic theory. Since these suppositions are not complied with in the real world, I take it that no historical period is normal. When dealing with statistical materials, are we not always dealing with transitional periods? […] And when you speak of periods of equilibrium, are you referring again to imaginary conditions instead of the historical conditions which our statistics reflect?"

Mitchell stresses that what Fisher considers "normal" is rarely, if ever, historically true, and that a reader could obtain the idea that "normality" is the usual feature, so much so that a historical series would rely on transition periods and normal periods (Mitchell to Fisher, November 13, 1926, Wesley Mitchell Papers).

In his reply, Fisher associates their disagreement with "[…] the fundamental distinction between scientific and historical truth and their interrelations." For Fisher, scientific truth is conditional, and historical truth is absolute. Historical facts give us the means we need to understand the conditional connection constituting science, while scientific truth allows for both the explanation and the forecast of historical facts. A historical fact comes from many scientific relationships, but when one or a few relationships predominate, the historical fact approaches scientific law. As an illustration, Fisher addresses the law of elliptic motion of planets, which does not dictate that planets move in ellipses, but that they would indeed move in ellipses if nothing but the force of the sun acted upon them (Fisher to Mitchell, November 26, 1926, Wesley Mitchell Papers). For Fisher, a theory represents what would occur under

certain ideal conditions, which are never fulfilled absolutely. In many cases, however, there is an approximate fulfillment of such conditions. The more transitional the period is, the less are the theoretical conditions fulfilled, because of the existing perturbations. Normal means freedom from perturbations. In both normal and transitional cases, Fisher argues, theories merely depict tendencies, and equilibrium is ultimately an imaginary feature (Fisher to Mitchell, November 26, 1926, Wesley Mitchell Papers).[23]

As previously stated, the 1920s in the US were marked by pluralism in economics, so much so that no hegemonic perspective ruled over Fisher and Mitchell's disagreements on index numbers. As a Yale student, Fisher studied under Josiah Willard Gibbs, who motivated him to bring mathematics and mechanical and Newtonian metaphors into economics (Banzhaf 2004; Boumans 2001). In addition, Fisher had close contact with William Graham Sumner, the chair of political economy at Yale, who strongly criticized Social Darwinism and advocated for free competition and laissez-faire (Bannister 1973). Hence, Fisher developed no admiration for reading the social sciences through Darwinist lenses. As previously highlighted, as a purebred Yale scholar, Fisher had no intimate contact with any historical school. If we consider Fisher's background, accordingly, it is not difficult to understand the reason why he relied on the law of elliptic motion of planets—a Newtonian perspective—as an accurate illustration for his view of science: such concepts as normal prices and equilibrium are also based on a Newtonian approach to economics, as in Fisher's explanation of transitional periods, periods of equilibrium, and levels of fulfillment.

The same mechanical approach to economics can be found in Fisher's view of history, which, he argues, should be used to build theoretical approaches capable of explaining and forecasting future facts. The theoretical approach that endures becomes a law. Fisher's views on economics are similar to several assumptions that would compose the new mainstream in economics, to emerge in the 1930s. This new mainstream would focus on price theory and rational decision making. The analysis of the formation of prices under different market structures, grounded on rational decisions made by consumers and firms, formed the basis of what most economists would be taught as economic theory from the middle of the twentieth century onwards. One of our arguments is that this association, that is, the contact points between Fisher's mindset and what would become the new mainstream in economics, is an important issue in understanding Fisher's approach to index numbers.

[23] In that letter exchange (Fisher to Mitchell, November 26, 1926, Wesley Mitchell Papers; Mitchell to Fisher, November 13, 1926 and Fisher to King, November 9, 1926), mentions are made of the PPM. As analyzing it would be outside the scope of this paper, we do not take that content into consideration.

Fisher was criticized because he focused his approach to index numbers strictly on prices—this criticism is present, for instance, in the works of the "partial disagreement trio," Mitchell (1921a, 1921b), Persons (1921a, 1921b), and Young (1921). Fisher was looking for index numbers from an economics perspective to which the key variable is prices; in such a perspective, resources are allocated through a system of prices. Moreover, Fisher looked for an approach that could be so general as to be applied to all cases. It was part of Fisher's efforts toward building a scientific approach. Hence, proposing an index number that could be applied to all cases, exclusively focused on prices, is in line with what Fisher had learned as a student and taught as a professor at Yale. Given his background in mathematics (Banzhaf 2004; Boumans 2001), finding an ideal and single index number through mathematical and statistical tests—the core of the axiomatic approach—also makes sense. For the same reason, Fisher debates the making of index numbers in a very technical way. Fisher, as every great thinker, reflected his environment.

Mitchell also reflected his environment, the pluralistic 1920s University of Chicago. Unlike Fisher, Mitchell was exposed to Darwinism and historical perspectives, as he had contact with Veblen, Dewey, and Loab. Hence, Mitchell understood that different societies and different times would imply different economic structures. From this perspective, understanding different economic realities might demand different economic data. Therefore, Mitchell's background can be highlighted as what conditioned his criticisms of Fisher's ideal formula. Mitchell's perspective that "suppositions are not complied with in the real world" and "of studying methods in the light of uses" is in line with the American institutionalists' criticism of the neoclassical approach, soon to rise as the mainstream in the 1930s (see Ayres 1935; Veblen 1900). Our point here is that the 1920s American pluralism in economics, relying on both American institutionalism and the bases of the forthcoming 1930s mainstream in economics, is an important element in understanding the differences in the nontechnical or methodological perspectives on index numbers during the 1920s.

To reinforce our argument here, we scrutinize the others that Fisher (1922) evoked as being in "partial disagreement" with him, that is, Persons and Young. Persons (1921a: 572) called Fisher's ideal formula "Fisher's Index Number."[24] Such a name could sound as an

[24] For Fisher (1922: 24), the name of the formula should also take Walsh and Pigou into account. His perspective follows Mitchell's. As Mitchell (1921a) points out, Walsh (1901) mentions Fisher's ideal formula in a footnote, but does not then exploit its merits. For Mitchell (1921a), Pigou (1912: 46) introduces the same formula but fails to note that the square root of the product should be extracted—that is remedied in Pigou (1920: 78). Mitchell previously addressed the similarity between Fisher's ideal formula and Pigou's, in a letter to Fisher (Mitchell to Fisher, February 16, 1922, Wesley Mitchell Papers).

agreement with Fisher's construction, as Fisher (1922: preface) himself pointed out. On the contrary, Persons (1921a) was critical of Fisher's perspective. Fisher claims that his perspective on index numbers has no specific purpose; however, for Persons (1921a), it is not possible to average, weight, and select data without purpose, because how to average, weight, and select data depends on a given purpose. Furthermore, Persons (1921a) stresses that it is evident that Fisher's perspective on index numbers has a specific purpose: Fisher's ideal formula fits into his equation of exchange, according to which the product between a price index, for any year, and the corresponding quantity index must equal the relative aggregate value for that year. Persons (1921a) agrees that this is a legitimate criterion for an index number (to be used in the equation of exchange), but remarks that this is not sufficient to assure its universal validity. Young (1921) shares a position similar to that of Persons (1921a) with regard to Fisher's ideal formula. For Young (1921), Fisher's ideal formula is an acceptable solution for a particular problem, and, to the best of his knowledge, Fisher's formula was "[…] the best single index number of the general level of prices. […] But I fear that no single index number will afford a sufficient answer to all such questions" (Young 1921: 573).

Neither Persons nor Young are frequently studied figures, so much so that Young is sometimes wrongly qualified as an institutionalist (Rutherford 2015). In his career, Persons worked as an assistant professor of economics at Dartmouth College and at Colorado College, until he became a professor at Harvard, where he edited the newly created *Review of Economic Statistics* and led the statistical compilation of the Harvard Economic Service (Foster 1939; Friedman 2014). His was known especially by his ability to deal with economic issues through mathematics and statistics (Friedman 2009). Similarly, Young's academic career was also marked by his presence in several universities, including Stanford, Harvard, and Cornell. For this reason, Blitch (1995) characterizes his career as peripatetic. Young's theorizing was notoriously driven by an "institutionalist sensibility" concerning the urgency for greater realism in economics (Morgan and Rutherford 1998: 3). More importantly, both Persons and Young obtained their Ph.Ds. from the University of Wisconsin, Young in 1902 (Blitch 1995), and Persons in 1916 (Foster 1939; Friedman 2009). This means that they received their economics instruction in an intellectual environment shaped by Richard T. Ely, who dealt with historical and institutional studies. In addition, from 1904 to 1932, the University of Wisconsin was the affiliation of John Commons, who is considered a founding father of American institutionalism (Rutherford 2006). Therefore, we can affirm that Young and Persons were influenced by a school organized around Ely's ideas at Wisconsin. We believe that it is then reasonable to

suppose that such contact with historical and institutional perspectives instilled in Persons and Young the notion that universality is not a feature of economics, and, as such, this notion conditioned their criticism of Fisher's general application of the ideal formula for index numbers. They did not share with Fisher the perspective of an abstract and general approach to economics. This placed them next to Mitchell in the index number debate of the 1920s.

5. CONCLUDING REMARKS

In the early 1920s, American economics enjoyed a pluralistic environment, so much so that individuals with opposing theoretical orientations shared the highest ranks of the profession. Fisher and Mitchell impersonate this situation by carrying their dissonant perspectives on economics as a science into the controversy regarding index numbers. Fisher, given his mathematical training, privileged a deductive approach, searching for the best formula for index numbers through a series of statistical tests; Mitchell, alternatively, given his institutionalist training privileging history and induction—as Persons and Young—was contrary to Fisher's theoretical construction: if no universal truth was applicable to all times and places, then no formula could aim to answer each and every question involving index numbers.

The debate this study analyzes is nearly one hundred years old, marked by the 1920 Annual Meeting of the ASA. It involved Fisher, Mitchell, Persons, Walsh, and Young. Nonetheless, the debate, by no means closed the controversy regarding the axiomatic approach to index numbers; it only marked its first phase. A sequel, also centered on Fisher's ideal formula, would follow the MIN's publication. Examples of this second phase of the debate are the disputes between Fisher (1924) and Yule (1923), those between Fisher (Fisher and Bowley 1923) and Bowley (1923), and the spin-off discussion opposing Edgeworth (1923a, 1923b) and Walsh (1924). The evaluation of these disputes may also be interesting both to form a picture of the controversy about index numbers in the 1920s and to indicate how economists' backgrounds condition their perspectives on economics as a science; however, that is another history, for another time.

In conclusion, the heyday of the debates surrounding the axiomatic approach to index numbers took place between 1920 and 1925, and counted with two phases: one pre- and another post-MIN. The echoes of this controversy persisted for some time, as Frisch (1930: 397) claims that Fisher's tests became somewhat elemental in the years following the publication of the

MIN. However, Frisch, (1930), in the same year he cofounded the Econometric Society alongside Fisher and Roos, offered the criticism that ended up causing the prestige of the axiomatic approach to index numbers to wane: no formula is capable of satisfying all of Fisher's tests simultaneously. Frisch's criticism then contributed to the rise of the economic approach to index numbers, inaugurated by Konüs (1939), in 1924, which relies on the optimizing behavior of economic agents. The nature of the economic approach renders its rise comprehensible: it does not matter that Fisher's axiomatic approach came close to the kind of reasoning that would dominate the rising 1930s mainstream in economics; Konüs's economic approach embodied its very soul. As such, it may be true that the axiomatic approach enjoyed a brief spring back to life in the 1970s (Diewert 1988: 14–16), but it would never again experience the prestige it had in the first half of the 1920s, when pluralism was the defining feature of American economics and the making of index numbers was, in Mitchell's (1921a: 24) words, undergoing its experimental stage.

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