
Uneasiness with Economics in the Encyclopedia of Unified Science

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Abstract

In 1937 the University of Chicago ordered the publication of a collection of monographs to be included in the International *Encyclopedia of Unified Science*. Economics had to be part of the plan. The task was entrusted to the Austrian econometrician Gerhard Tintner. The resulting volume, *Methodology of Mathematical Economics and Econometrics*, (MME, published in 1968), however, has not found a place at the history of economic thought nor in the field of the methodology of economics. In this paper we analyze the path towards the publication of this volume, review the methodological conceptions presented in the MME, and evaluate the degree of (dis)satisfaction with the finished product. In order to achieve a better understanding we include a description of the conception of unified science and sketch the ideas of Otto Neurath about Economics. We finally argue that the failure was not on the author but on the Economics project itself, which we consider, then and now, as an almost impossible task.

Keywords: Methodology of Economics, Encyclopedia of Unified Science, Vienna Circle.

JEL Classification: B41, B29

El malestar de la Enciclopedia de la Unificación de las Ciencias para incorporar la economía

Resumen

En 1937 la Universidad de Chicago anunció la publicación de una colección de textos monográficos para ser incluidos en la *International Encyclopedia of Unified Science*. La economía debía formar parte del plan. La tarea fue confiada al econométrico austriaco Gerhard Tintner. No obstante, el volumen resultante, *Methodology of Mathematical Economics and Econometrics* (MME, publicado en 1968), no encontró su lugar ni en la historia del pensamiento económico ni en el campo de la metodología de la economía. En este trabajo analizamos el camino que condujo a la publicación del volumen, hacemos un repaso de las concepciones metodológicas presentadas en el MME y evaluamos el grado de (di)satisfacción con el producto terminado. Con el fin de una mejor comprensión. Incluimos una descripción de la concepción de la *ciencia unificada* y esbozamos las ideas de Otto Neurath acerca de la economía. Finalmente, sostenemos que el fracaso no se debía únicamente al autor, sino también al proyecto mismo acerca de la economía, al que consideramos, tanto antes como ahora, una casi imposible de realizar.

Palabras clave: Metodología de la economía, Enciclopedia de la Ciencia Unificada, Círculo de Viena.

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Introduction

In 1937 the University of Chicago commissioned the publication of a collection of medium size monographs to be included in the International *Encyclopedia of Unified Science*, a project publicized as a sort of "science of science". This ambitious project was an outcome of the philosophy of logical empiricism developed by the members of the Vienna Circle since the 1920s. The main aim of the collection was mainly to disseminate the “*The Scientific Conception of the World*” defended by the logical empiricists. Between 1938 and 1969 twenty books of the *Encyclopedia* were published –in two separate volumes– by the University of Chicago Press. The project was never completed, and we can safely claim that their main objectives were not achieved.¹

Economics, perhaps the most relevant social science, had to be part of the plan, and so the Vienna Circle members faced the challenge. However, the economics volume project was plagued with unexpected complications. After decades of uncertainty and discussions, an Austrian econometrician named Gerhard Tintner was chosen as the author. Almost exclusively known for his contributions to the development of econometrics, this economist wrote a volume that included a good description of the mathematical and econometric topics, but a minor and rather disappointing examination of the scientific features of the economic discipline. The title of the volume was *Methodology of Mathematical Economics and Econometrics* (MMEE).

The difficulties experienced with this particular volume were large. The venture triggered strong internal conflicts, suffered from long interruptions, and recorded several modifications, but even so, the result seems to have left no one truly satisfied. Whoever would aim at knowing the basic methodological and philosophical problems of economics, should be disappointed with the content of the book. In the end, the respected Vienna Circle could not safeguard a major place to the MMEE, neither in the history of economic thought nor in the epistemology of economics. Far from blaming the work of Tintner or that of the members of the Vienna Circle, the fate of the MMEE seems to illustrate dramatically the intrinsic difficulties of the discipline to adopt a definitively scientific stance over its epistemic content.

This paper is organized as follows. Section II describes the problem of selecting the right “unified economic science” towards the publication of the MMEE. In Section III we review the methodological conceptions presented in MMEE. Section IV reviews the degree of (dis)satisfaction with the finished product. Section V concludes noting, from a modern perspective, some possible hurdles to create a work that summarizes the scientific content of economics.

¹ For the history of the publication of the Encyclopedia an important source is Morris (1960); see also Hegselman (1987), pp. xiv ff.

Which Economic Science?

The path to the publication of the MMEE volume was stressful. First, members of the Circle would have to solve their personal differences on what economics was and was not, and its scientific status². Once confronted, personal perspectives emerged more diverse than they perhaps expected. To summarize the disagreements in one sentence, in economic issues it was not at all obvious how to harmonize the "Marxist-historicist" tendencies of some members with the "logical positivist" view characterizing the general conception of the Circle. These deep disparities, rooted sometimes in differences in ideological and political orientation, augured complications when choosing the topics that would be discussed in the volume, and of course when selecting the author. It was a real challenge to select a name that would be able to produce a scientifically "objective" work, in accordance with the philosophy of the Vienna Circle.

The Concept of Unified Science

The notion of unified science played an essential role in the program of logical empiricism. Even if understood as a "working hypothesis" (like in Oppenheim & Putnam 1958), it also contained social and political features. As underlined in the famous programmatic work *The Scientific Conception of the World. The Vienna Circle*, the program pursued the collective construction of a "unified science" (*Einheitswissenschaft*). According to the program, "we would establish the 'cross connections' from science to science and thus create a structure that knows no 'philosophy', no 'epistemology' with special propositions - whichever one of these two is applicable has found its place either in the 'logic of science' or in 'behaviouristics': *the program of unified science*." (Neurath 1935, p. 115)

This methodological and philosophical program included, among other things, "the search for a neutral system of formulae, for a symbolism freed from the slag of historical languages; and besides the search for a total system of concepts." (Neurath 1929, p. 306). Thus, unified science was seen as a problem concerning *scientific language*: to have a unique language, that should be the way to formulate and communicate all scientific knowledge, obtained in every science (a kind of "scientific universal language"). Among the members of the Vienna Circle, Rudolf Carnap investigated specifically this idea (see Carnap 1931). He followed the *universalistic* tradition concerning scientific languages, as it was presented by Gottlob Frege and later defended by Bertrand Russell, who was (as it is very well known) extremely influential in the entire movement of logical empiricism. According to this tradition, there is only one language *stricto sensu*, the universal language, so that it is the unique medium for the expression of any scientific knowledge. Carnap proposed the formal language for logic (developed mainly in the *Principia Mathematica*) as a basis for the construction of a unifying language for science. In this sense, the project of the unified science would be an application of symbolic logic to the reconstruction of the factual sciences.

² The original idea of the Encyclopedia was Otto Neurath's. After the planning stage, the detailed work on the Encyclopedia fell largely on Rudolf Carnap and Charles Morris.

The prevailing idea within the Vienna Circle was that the universal language should be understood in a physicalist way, in the sense that every scientific concept could be expressed in physical terms (see Carnap 1931, pp. 443 and 448).³ This idea was a consequence of the way the Vienna Circle conceived (a) the meaning of the basic sentences of the language, and (b) the means to determinate the truth or falsity of sentences. In other words, the physicalist approach should be based on the specific conceptions of meaning, truth and knowledge that were endorsed within the Vienna Circle. To put it briefly, (1) truth can only be predicated of empirical (synthetic) or analytic propositions; (2) authentic knowledge can be gained only by experience. Hence, propositions contained in the unified science must be legitimated with respect to these two principles (see Heggemann 1987, where these principles are called the *meaning thesis* and the *base thesis* respectively). So, the idea of unified science was originally connected with the reductionism underlying the physicalist point of view: the concepts with empirical meaning should be physical concepts, so that the concepts of other fields, if empirical, should be reduced to physical concepts. And this should be the case also of the concepts of the social sciences.

In any case, within the Vienna Circle there was not a complete agreement about the characterization of the unified science. The most involved member of the Vienna Circle in the project was undoubtedly Neurath. For him, the *Encyclopedia of Unified Science* "does not propose a new doctrine, new dogmas, it must not become a new deity." (Sebestik 2011, p. 53). "Unification" was a key concept, depicting science as a system of claims aimed at making predictions that could only be achieved by overcoming the compartmentalization, and the irreducible specificity of its objects, its methods, and its languages, and both the logicist program and the reductive physicalist program should not be a necessary condition for the unified science.

Neurath was against the use of formal languages as a basis for the unified science. For Neurath, the universal scientific language should be, unlike Carnap's position, the informal ordinary language, enriched by technical terms constituting a specialized jargon or slang (see Neurath 1932/1933). He explicitly rejected explanatory reductionism (the search for ultimate explanations in the laws of particle physics) and did not believe it was irremediable to use the language of physics to study other sciences. In short, Neurath considered this project as an applied one, necessary for common work among disciplines (see Potochnik, 2011). Furthermore, the unified science should not constitute a closed or fixed system. On the contrary, it would be open to the further development of science: "we do not arrive at 'one' system of science that could take the place of the 'real world' so to speak; everything remains ambiguous and in many ways uncertain." (Neurath 1935, p. 116).

Neurath the Economist and the Economic Volume

Neurath had voice in the development of the MMEE because he was not just a philosopher, but also an economist⁴. More specifically, he was an economist with Marxist inclinations, holding a highly critical view of the neoclassical revolution that were developing in the

³ Obviously, mathematical and logical concepts were the exception. In these cases, logicism was the prevailing conception within the Vienna Circle in the 1920s and 1930, according to which mathematics should be reduced to logic, and logical laws were tautologies, that is, analytic sentences without a real content.

⁴ Neurath wrote about economics mainly between 1906 and 1917. A summary of these works and other selected writings can be found in Cohen and Uebel (2004).

discipline since the 1870s, represented first by marginalism, and later by general equilibrium theories. According to Neurath, “the words and phrases favored by economists are essentially a mask, concealing both the lack of clarity and solid empirical grounding” (cited in Turk, 2016 p. 374). Neurath also criticized the tendency of the discipline to impose assumptions of clairvoyance and infallible rationality, a Cartesian approach that he considered unsuitable for a scientific discipline (Uebel, 2004 p. 10).

Neurath devoted much of his work as an economist to criticize the traditional definitions of economic concepts (capital, price, value), their meaning and measurement⁵. He noted, for example, the nuisance of defining the object of economic activity as “economic well-being”, an assumption usually made by marginalists⁶. On the discipline as a whole, Neurath's view was that the nature and scope of economics should be understood as the study of “organization systems” in general, rather than being limited to specific market economies. The notion of economics pursued by Neurath was a combination of “history” and “political economy”, combined with the technical aspects of the production and distribution of the wealth of society (Becchio & Leghissa, 2017 pp 87).

Neurath was not the only member of the Circle to openly challenge what would become later the mainstream economic theory. In 1941 Edgar Zilsel, a Marxist philosopher of science and historian, claimed that the stage of economics at the time was not scientific enough to be part of a unified science, and so must not be included in the project (see De Santillana and Zilsel, 1941). The main obstacle for Zilsel was that political economy was often exposed to “selfish interests, political pressures, and wishful thinking”. Zilsel emphasized that “in political economy scientific agreements could be reached only on comparatively unimportant questions; in fact, there are separate schools which do not even recognize each other”. Some of them, he said, cling to experience without developing theory, while others engaged in constructing large deductive systems to find theories, albeit based on scanty observations (De Santillana and Zilsel, 1941, pp. 832 f.).

Logical Positivism and the Scientific Smell of the Neoclassical Approach

The views from Neurath were far from the ones Charles Morris and Rudolf Carnap had in mind when they discussed the integration of economics into the project. They were much more interested in the new developments in economic theory, in part because those models made use of the formal tools of mathematics and statistics, and paid particular attention to the rising discipline of econometrics⁷. In 1935 the logical empiricists succeeded in organizing a *Congrès International de Philosophie Scientifique* in Paris, an influential meeting that included a session on “Unité de la Science”. In this session, the French econometrician Robert Gibrat read a paper on “La Science économique. Methodes et philosophie” (Gibrat 1936), where he argued in favor of Econometrics as the proper *scientific* economics, since it could unify economic theory and economic data with a “methodic and rigorous spirit”, by

⁵ One of the most striking ideas was his rejection of the conventional notion of capital, arguing that he saw no basis for treating it as homogeneous. This critical review anticipated the famous “Capital Controversy” that began in the 1960s (see for example Harcourt, 1969).

⁶ Neurath's concern that the economy should capture real-life situations instead of measuring national income is related to the criticism of the traditional economic measures presented by Amartya Sen and the alternative indices to measure the economic well-being of a country (Leßmann, 2007).

⁷ An amusing upshot of these disagreements was the somewhat paradoxical agreement reached by those involved in the project that the definition of economics was “unclear” (Becchio and Leghissa, 2017 pp. 88).

using statistical tools (Gibrat 1936, p. 27)⁸. It must be noted that Carnap had devoted intensive attention to the theory of probability after World War II⁹.

Morris and Carnap could have been persuaded by the endless rise of marginalism, that by the 1920s had converged towards the construction of a unique theoretical system that became dominant in almost every academic circle in Western countries. Alfred Marshall and Arthur Pigou in Great Britain, Carl Menger and Eugen von Böhm-Bawerk in Austria, Vilfredo Pareto in Italy, Knut Wicksell and Gustav Cassel in Sweden, Irving Fisher and John Bates Clark in the United States. All of them developed and popularized a brand new theoretical apparatus. The classical system give way to an orthodoxy claiming to be a single science with its own rules and language. The neoclassical system was born.

How “scientific” looked this system at the time? Inequivocally, neoclassical ideas were presented by their defendants as pure science. Lionel Robbins famously stated: ‘Scarcity of means to satisfy ends of varying importance is an almost ubiquitous condition of human behaviour. Here, then, is the unity of subject of Economic Science, the forms assumed by human behaviour in disposing of scarce means’ (Robbins, 1932, p. 15).

The neoclassical approach also works with a reductionist view, a strategy that many natural sciences also follow. Neoclassics refer to individual decision-making units, such as households and companies, and avoids collective agents as the social classes and political bodies. Economics embraced methodological individualism, meaning that knowledge of the properties of a system comes from the knowledge of the properties of its elements.

Neoclassical economics also presented itself as the only theoretical body able to attain the historicity of economic laws. This was the opportunity to equate economics with natural sciences, physics in particular, by applying its methodology to find absolute and objective laws. The universal validity of economic laws implied getting rid of social relationships and, of course, of ideology. The neoclassical revolution much consisted in developing research through “mere technical relationships”. Also, the tendency to extend the basic model to every branch of economic investigation gave an impression of a truly universal analytical apparatus, an idea that Paul Samuelson pushed further by claiming that all economic problems can be reduced to a mathematical function to maximize under constraints.

The methodological reflections in the neoclassical approach also showed affinities with logical positivism. In 1874 Stanley Jevons published *The Principles of Science*, a treatise on formal logic and scientific method where he presented economics as a science which “besides being logical, is also mathematical” (p. 80). Francis Edgeworth was another passionate supporter of mathematical economics. Carl Menger stated that since pure science is always value-free, freeing economics from value judgements was the right orientation. The most famous argument of the neutrality of economic science was that of Lionel Robbins in his 1932 Essay.

The influence of logical positivism on Anglo-American social science was vivid last century. But with the philosophical setting attained by the Vienna Circle, many economists began to speak in that language. The concept of “observability” as a demarcation criterion between science and non-science, and the neutrality with respect to value judgements as a criterion to discriminate science from ethics.

A case in point in the relationship between neoclassical economics and logical positivism was the development of General-Equilibrium Theory (GET). In order to make

⁸ A discussion of Gibrat’s contribution to the congress can be found in Armatte 2018.

⁹ See for example Carnap (1950).

progress on the topic, it was needed to find economists well trained in mathematics. Karl Menger, son of Carl and a member of the Vienna Circle, was active in pursuing the axiomatization and consolidation of the scientific work on GET with formal tools. In the 1930s Menger organized a series of seminars, the *Mathematisches Kolloquium*, attended by many important mathematicians and logicians at the time. The participants of the *Kolloquium* judged traditional economic theory as not enough founded in mathematical terms. Oskar Morgenstern, a logical positivist, was considering the task of creating a whole mathematical language to formulate every economic problem. At the end of the 1930s, many “mathematical economists” were working in the United States, including von Neumann, Morgenstern, Leontief and Tintner. They all helped explicitly to push the GET agenda further, but the main impulse was given by the works of Paul Samuelson and its *Foundations of Economic Analysis*. In the 1950s Kenneth Arrow and Gérard Debreu wrote the Walrasian final word in GET.

The developments in GET are a good example of the growing relationship between logical positivism, mathematics and neoclassical economics. These relationships contributed to the scientific smell of the whole neoclassical endeavor, a smell that could have influenced the final choice of Tintner to write the MMEE. The neoclassical approach was selling itself at the time as the only hope to make economics a pure science.

After Neurath's death in 1945, Morris and Carnap took command of the final publication of the volume. It took almost twenty years to start what would be the definitive volume. The later Nobel Prize awarded Jan Tinbergen was the first candidate to write a manuscript entitled "Mathematical Tools in Economics" in 1960, but he refused. It was not until the mid-1960s that Morris wrote to Carnap to hasten the author's choice, and Morris proposed the volume on economics to be assigned to Tintner, a former student of Carnap in Vienna. After three revisions, Tintner's volume was finally published in 1968 as the *Methodology of Mathematical Economy and Econometrics* (MMEE).

Tintner's Methodology of Economics

The methodological core in the MMEE volume is depicted in the Introduction, which consists only of ten pages. Tintner presents the discipline borrowing the standard Robbins' definition of economics: "the science that studies human behavior as a relationship between ends and scarce means which have alternative uses" (Robbins 1945, 16). But to block possible criticisms, Tintner cites also Lange's version, according to which economics is "the science of the administration of scarce resources in human society" (Lange, 1953). This is the first indication that MMEE volume is being written in the middle of the cold war, where opposite economic systems (market and central planning) were still shown as plausible alternatives.

Tintner recognizes that using mathematics in economic theory had become increasingly common, and endorses this trend as being the most appropriate tool to develop "theoretical economics". While admitting that "the bulk of the results of theoretical economics has been achieved without mathematical means" (pp. 1), he insists that "mathematical economics and econometrics are the *only* methods for the study of problems in economics." (pp. 2, emphasis in the original). Tintner also stresses the need to close the gap between theoretical concepts and empirical observations, emphasizing the goal of economics to "construct fundamental models which we try to apply to concrete economic

problems" (pp. 1). Despite his confidence in the power of formalization, Tintner acknowledges (pp. 2) that economics did not have as many scientific achievements as physics or genetics, and agrees with Georgescu-Roegen (1965) that this state of affairs is partially related to the "envy of physics", the process by which economics emulates physics by assuming that economic relationships are always measurable and linear.

The author cites in the introduction the famous criticism by Popper of Marx and Hegelian dialectics. Tintner agrees with Popper that many Marxist and Hegelian concepts are "empty" and motivated by ideology, and it is not clear that Neurath would have agree with these raw statements. Even so, the text points out –although without further detail- to several Marxist ideas as a foundation to understand the processes of economic development. Tintner also agrees with Oskar Lange when he acknowledges that the existence of several schools of economic thought do not undermine the potential objectivity of economics as a science. Another potential methodological antagonism with Neurath appears when the author decides to analyze specifically the problems of aggregation and its solutions, which he considers an example of the unity of the scientific method in the natural and social sciences. Quoting Carnap and Popper, Tintner evokes the neo-positivist philosophical approach that seeks the unity of the scientific method in natural and social sciences and declares that economics could be included in this project only because it uses mathematics and applies them to real cases with econometric tools.

The MMEE volume explicitly repudiates the use of value judgments in the discipline. Tintner considers "deplorable" the influence of ideology of both "left and right" economists who "have been very much influenced by the ideological struggles of their time, and have sometimes illegitimately presented value judgments as scientific truth" (pp. 4). He stands against ideological motivations even when economics studies human action as many other social sciences do. Again, in order to moderate these intentions and to turn economics into real science, mathematical and statistical methods must be used. Tintner also criticizes the concept of Natural Law (including the "invisible hand" of Adam Smith), since they were historically used to justify political agendas.

The MMEE asks why the discipline has not shown tangible progress. Tintner made reference to the scarce availability of economic statistics as an obvious limitation but also complains about the state of the theory, based on static and unrealistic models, such as perfect competition. These abstractions, he points out, do not allow us to fully understand all development processes, such as the analysis of pre-capitalist structures. Tintner abhors oligopolies, which he considers the main disruption of modern capitalism, and criticizes the models that ignore them. He also stands against what he calls the "Ricardian vice" (a concept he somewhat strangely extends to Keynes), which consists of saturating analytical theories with assumptions just to obtain purely tautological results.

Overall, the methodological section of the MMEE seems lacking, even if compared with the state of the philosophy of economics at that time. The brief methodological extension does not even implicitly refer to Milton Friedman's seminal work (1953) on positive economics, an essay that generated a host of interpretations and discussions by most philosophers of economics.

The main body of the MMEE describes the econometrics and mathematics of the mainstream, the only one considered as "really scientific". Dedicated to explain in some detail the techniques of the time, this part does not involve a discussion about the merit of

economic theories or schools of thought, nor does it dispel any doubt over the "scientific" character of the discipline.

Tintner was mostly a mathematician and econometrician, so his skill allowed him just to describe formal methods (a branch that today starts to be taught in undergraduate courses as Mathematics for Economists), plus the new developments of an embryonic branch of econometrics, but lacking the calculation power of modern computers. It is difficult to assess how much in MMEE is economics and how much is mathematics; how much is econometric theory and how much is statistics; how much is methodology and how much is a mere description of methods.

Unfulfilled Expectations

It can be said that the MMEE did not meet the expectations of almost anyone. Morris and Carnap, as "responsible editors", were not particularly enthusiastic about the results. It is evident that the book does not discuss the place of Economics in the program of the Unified Science, as sketched in section II.1. Furthermore, Tintner neither uses the notions underlying the program nor adopts a critical position about them. More important, perhaps, the work failed to establish any (more general) debate on the philosophy or methodology of Economics. Only a handful of MMEE reviews have been written, all of them with a descriptive tone. Although several copies were sold, the volume influence was so scarce that there was no place for new editions or subsequent revisions. The most renowned economists completely ignored the work (then and now), while the econometricians remember Tintner only for his specific contributions to their field (see Fox, 1969). The scarce historical traceability of the figure of Tintner reveals its small relevance in the history of the profession. His name is absent from the *Stanford Encyclopedia of Philosophy* and the corresponding entry to his biography on *Wikipedia* has only half a page (only in English) that does not even include his role as author of the MEME.¹⁰

Would Otto Neurath have also been disenchanted with the economics module of the Encyclopedia? Perhaps. Becchio and Leghissa (2017, p. 89) conclude that the MMEE ended up being a symbol of the transformation of the discipline from "political economy" to just "economics", which meant the success of the neoclassical approach of the University of Chicago, a view that Neurath rejected as the suitable method to develop economics.

The project did not even get support by mainstream economists, who could have thought that the MMEE was a good opportunity to amend formalization in neoclassical economics by presenting a "scientific" module of the discipline. In fact, this quest was already pointless, because by 1968 -the publication year- Chicago and its economists no longer needed an advertising project. Neoclassical economics was by then considered the queen of social sciences, and it had even launched to colonize other disciplines. In this state of affairs, the contribution of a mere volume of the extinct Vienna Circle to the leadership or renovation of mainstream economics would have been insignificant.

In many ways, Tintner's volume did not stand the test of time. The theoretical topics considered as the most important in the book were not the most studied in subsequent years. The modern oblivion of the theories associated with centralized planning left half of the

¹⁰ See https://en.wikipedia.org/wiki/Gerhard_Tintner

MMEE almost useless, and Tintner's insistence on perfect competition as the main flaw of mainstream theory was not considered key in subsequent developments that still use that benchmark as the cornerstone of macroeconomic analysis. The econometric methods described in the volume were mere historical antecedents, and only a few constituted a point of departure for subsequent development. The dramatic changes brought later by the new digital technologies helped the econometric analysis and wounded several statistical contributions present in the MMEE.

Finally, the methodological contents of MMEE anticipated modern debates only partially. On the one hand, Tintner considered the problem of personal interests and objectivity in economics and they arguably still persist. But on the other hand, modern debates on the epistemological properties of modeling, of the adequacy of assumptions, and of the role of mathematics in the discipline has taken directions that the volume did not foretell.

Summarizing, Tintner's volume was a failure as a component of the *Encyclopedia of unified science*. Undoubtedly, from the current perspective the entire program of Unified Science was not workable at all. The problem is, then, why the failure in the case of economics (the election of the author, the delays in the publication, etc.) is so evident and striking compared to other disciplines.

In this sense, some questions concerning the place of economics in a hypothetical unified science remain open. For example, on one side, it could be argued that there are special features of economics that distinguishes it from the other fields covered by the program and could explain this particular failure. On the other side, it could be argued that similar features can be found in other fields of scientific research, but they were largely overlooked because of an underlying agreement about the nature of these fields in most of the members of logical empiricism (at least in its early stage). These are questions that deserve further exploration.

Conclusion: A Modern MMEE?

The aim of this paper is not to blame Tintner for the failure of the MMEE project. On the contrary, we consider that today a comparable endeavor aimed to impregnate the social disciplines with a scientific spirit would be seen as an intrepid act. As such, the experience turned out to be fruitful for what it failed to do. The project worked as an "experiment" that demonstrated that identifying the scientific aspects of economics was a cyclopean task, even half a century ago.

Moreover, nowadays this quest seems almost impossible, in many respects. One problem hindering the achievement of a modern MMEE has to do with length. Specializations in economics have virtually exploded, so today it is impossible for the single economist to know even superficially the theoretical and empirical underpinnings of each of them. According to the JEL categories, the number of topics has already taken every alphabet letter, and each one is further divided into five to ten relevant branches, making a total of no less than 150 categories, some of them with its own analytical bases. The number continues to

grow and it is difficult to determine which branch will develop faster or generate more interest in the future¹¹.

A second sense why this task may well be impossible has to do with our scarce knowledge (and therefore the lack of consensus) of key issues in economic analysis. To cite just a few, economists continue to struggle to unveil the secrets for solid and inclusive economic development, the sources of the economic cycle, or the prevention of crises. They also disagree strongly about the consequences of trade and financial globalization, the distribution of income, and the role of the state. The permanence of schools of economic thought suggests that debates about many issues are still ongoing, and it remains difficult to isolate objective features from value judgments.

Making a modern MMEE also seems a daring task today. While since the 80s many economists began to declare a growing (neoliberal) consensus in the profession, with claims reaching a peak in the early 2000s, the global crisis of 2007-2009 reverberated discussions about the scientific nature of the discipline, and many analysts had to admit that some of their models were “a giant with feet of clay”. For the first time in decades, some renowned economists wrote books on the epistemology of economics, a clear sign that certain methods and principles of the discipline have to be reconsidered (see for example Rodrik, 2015 and Rubinstein, 2011).

The frustration with the MMEE provides us with a lesson: the endeavor of summing up in a single volume the methodological features of economics is very hard, perhaps too hard. Including economics into an Encyclopedia of Unified Science was necessary, but the discipline was not prepared then, and may not be prepared now, to do so.

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¹¹ Consider, for example, the appearance of Gender Economics, or the economics related to artificial intelligence and its consequences.

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