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Orozco Espinel, Camila

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Setting up a long-term research project for economics at the Cowles Commission: The definition of theory as a mathematically and abstractly driven form of knowledge

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Setting up a long-term research project for economics at the Cowles Commission. The definition of theory as a mathematically and abstractly driven form of knowledge.

Camila Orozco Espinel

Postdoctoral Fellow (CHOPE, Duke University)

Draft

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Note:
This text was presented at 44rd Annual Meetings of the History of Economics Society in Toronto in 2017. Most of it was translated to French for my dissertation. My goal in the forthcoming months is to give to this text the form of a publishable article. I have not yet decided where to publish it, in a history of economics journal or in a journal as Social Studies of Science.

Introduction

This paper offers an analysis of the challenges the researchers associated with the Cowles Commission faced while establishing a mathematically and abstractly driven definition of "theory" for Economics. My objective is twofold. First, I explain the specific responses, in other words the strategies to these challenges, by relating them to the different positions the scholar network associated with Cowles occupied in the US academic system (from peripheral/dominate to central/dominant). Second, I analyze the impact of these strategies in the discipline of economics. I argue that by establishing a mathematically and abstract-driven definition of theory, the scholars associated with the Cowles Commission set up the foundations of a long-term research project for economics. This project, while abstract in character, allowed an important part of the discipline to cohere around. More concretely, it was critical in forging an articulated response to criticisms about their interest in technical and mathematical problems *per se*, and it was central to the process of setting the guidelines for a path of knowledge accumulation.

I follow the network of scholars associated with Cowles from the early 1930s, during the preambles of the constitution of the Econometric Society, before the creation of the Commission in 1932, to the publication in 1957 of Tjalling Koopmans "Three essays on the state of economic science." I frame the analysis of Cowles's mathematically and abstractly driven definition of theory into concrete institutional issues. I explore the *descriptive*, *evaluative*, and *embattled* role of the definition of *theory* while economists claim the authority of science. Section 1 focuses on the debate that peaked during the establishment of the Econometric Society. Section 2 revisits the Measurement Without Theory Controversy. In Section 3 uses the publication of Tjalling Koopmans "Three essays on the state of economic science," as an entry point to the study of the challenges that the network of scholars gravitating around Cowles faced forging an accommodation in the post-war environment.

1. THE CONSTITUTION OF THE ECONOMETRIC SOCIETY

Historians of economics have focused much attention on the foundation of the Econometric Society (ES) [1]. Section I focuses on seminal ES documents, in particular the letters sent by Irving Fisher, Ragnar Frisch and Charles Roos to organize the first meeting and the responses they received. The analysis of these documents offers a telling empirical illustration of the conflicting demands of *conformity* and *differentiation* that newcomers to a field have to address. Through the analysis of the conception of *theory* mobilized by both sender and recipients of the letter, this Section captures the simultaneously *descriptive* and *evaluative* function of the definition of *theory*. The idea is to show how, by mobilizing *abstractly based* conception *theory* (in opposition to an *empirically based* one), the senders of the letter claimed the authority of the science.

1 For a detailed history of the foundation of the Econometric Society and more generally the birth of the econometric project, see Bjerkholt (2014a), Louça (2007), Morgan (1992), Boumans and Dupont-Kieffer (2011), Epstein (1987) and Bjerkholt (2014a). While the ES effectively coordinate the efforts of many disjoint initiative grounded in both sides of the Atlantic, the heterogeneity of the project never disappeared. For a study of the European origins of econometric project and the subsequent articulation of the pieces in the postwar United-States see A. Akhabbar (2010).

1.1 The descriptive and evaluative role of theory

In June 1930 Fisher, Frisch and Roos sent the first letter to a group of 28 scholars from 10 different European and North American countries, to inquire on the viability and best way to carry out the project of organizing an international association "for the advancement of economic theory" [2]. Six months later the organizing meeting of the ES was held. The initial project draft was not substantially modified.

From the very first lines of the June letter, by connecting what they called "genuine Economic Science" to a *theoretical scope*, the founders of the Econometric Society demarcated their approach from what they call a mere *empirical* treatment of economic questions[3]. This is explicit from the first paragraph of the letter:

> The undersigned are writing to ask your opinion as to a project we have been considering, namely the organization of an international association for the advancement of economic theory. As we see it, the chief purpose of such an association would be to help by gradually converting economics into a genuine and recognized science. Such a purpose, we think, can only be realized by giving the association a theoretical scope. Only in this way, we believe, can one make sure that its work will proceed on truly disinterested lines, exempt from national, political and social prejudice. (Cited in Bjerkholt 2014a, 8–9)[4]

Words are sites of power, as sociologists know well (Bourdieu 2001, 423). In the letter the terms *theory* and *theoretical* do descriptive and evaluative work: so as *empirical* and *empirically*, they are "thick" terms (William 1978, 1985). Hence, by applying them in the letter Fisher, Frisch and Roos were not just saying that society's scope was of a certain nature, but they were also implying an either favourable (in the case of *theory*) or negative (in the case of *empirical*) judgment[5]. Moreover, the term theory was used

[2] For the details of the organization of the first ES's meeting see Bjerkholt (2014a).
[3] It is worth mentioning that during those years, statistics was as well going through a period of redefinition and *theoretical* methods were gaining importance.
[4] No emphasis in the original.
[5] Compared to the economic theory developed at the Cowles Commission in the oncoming decades, Fisher, Frisch and in particular Roos's work does not seem particulary mathematically-driven and strongly based on *a priori* knowledge. What we are suggesting is that at this point they were using these elements to demarcate their project and claim the authority of the science.

as membership criteria. For instance, while the importance of empirical research was underlined, the senders of the letter concluded:

> We believe that the association should not include those who have merely treated economic problems empirically, without reference to fundamental theoretical principles. (Originale letter cited in Bjerkholt, 2014a, p. 10)

As Gabriel Abend (2014) points out, "while there is only one sign, one English word-form 'theory,' there are many people who want to use it in different ways, and who have a practical interest in doing so." (192) The responses of the recipients of the letter, especially for those in a position of compromise, sheds light on the evaluative way the funders of the letter were using the term theory. J.M. Clark's response is particularly insightful.

> If the association is to represent theory in general, and not simply one kind of *theory*[6], it seems to me that it should not select its membership by a test of fitness for the mathematical- statistical type of work alone, nor set up a journal committed to giving this type of work dominant place. At present, I favour giving the society and journal the broader scope, though there is much to be said for a society and a journal of mathematical-statistical economics. (Cited in Bjerkholt, 2014a, p.15)

And later, when replying about his own eligibility:

> I should be glad to be a charter member of such an association if it successfully solves the problem suggested above. I should be reluctant to lend support to the complete capturing of 'theory' by the mathematical method; especially as I expect to do my main work in theory, but not mainly in that field. (Cited in Bjerkholt, 2014a, 21-22)

1.2 Facing the newcomer's dilemma: conformity and differentiation

Fisher, Frisch and Roos were aware that "In practice, the line will be difficult to draw" (Cited in Bjerkholt, 2014a, 10). The heterogeneity of the list of recipients of the seminal letter illustrates this point. Although it comprised mostly scholars whose work embodied both a mathematically driven deductive approach and the use of inductive methods of quantification, from a contemporary point of view it seems remarkably

[6] No emphasis in the original.

diverse. This can be understood as the expression of the intellectual continuum running in economics. Then, in the interwar period it was possible to "hold a number of different economic beliefs and to do economics in many different ways without being out of place or necessarily forfeiting the respect of one's peers." (Morgan and Rutherford 1998, 4)

Furthermore, the heterogeneity of the recipients' list illustrates the position occupied by the international network of scholar gatherings around the Econometric Society. In consistence with Pierre Bourdieu's[7] theory of action, the position agents (or groups) occupy is essential to explain their actions. François Divisia's response to the letter's inquiry on the association journal's name, clearly objectivize their newcomers (dominated) position:

> As to the name of the journal I think that the formula Economic Science is very dangerous. It seems to indicate that we want to monopolize economic science and that we are the only ones who represent the true economic science. This may perhaps be at the bottom of our thoughts but I do not think that the time has yet come to proclaim it. I would even add that it might seem a little ridiculous to adopt so important a name for a periodical that would perhaps in the beginning be rather modest. In this respect, it seems to me that we ought to present ourselves as cultivating a certain method of economic research (or group of methods) because we think they are good, and not because we have the pretention to decide definitely the question of knowing whether other methods may also be interesting. As to this question, we will see later, judging from the results » (Bjerkholt 2014a, 19)

Taking this into account, the reorientation –from the first to the second letter– to a society with two groups, one of regular members and one of fellows, with the power vested in the latters, it can thus be better understood as a response to the conflicting demands of conformity and differentiation that newcomers face at the moment of entering a field[8].

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[7] For a systematic presentation see for example (Bourdieu and Wacquant 1992).

[8] Camic and Xie (1994) define the *newcomers dilemma* as situation where newcomers in a competitive interdisciplinary field face conflicting demands of conformity versus differentiation. They use the concept to propose a sociological approach for understanding the process by which statistical methods were originally incorporated into the social sciences in the United-States.

Olav Bjerkholt's scrutiny of the membership requirements in fact shows that the criterion stated in the first letter were identical with the requirements for fellowship stated in the draft constitution and quoted in the November invitation letter. The eligibility policy reorientation, Bjerkholt suggests, was a "better proposal, both with the regard to promoting econometrics through a low threshold for joining the society and for keeping the society on the right track and animated by the true econometric spirit through the power exerted by a relatively small group of Fellows". (Bjerkholt 2014a, 42)

The emphasis Clement Colson, Divisia's elder, put in his response to the first letter on the importance of keeping a low profile, is not less telling of the dominated position occupied by the network of scholars gathering around the econometric project. Moreover, Colson's response reveals the institutional tensions structuring the foundation of the ES.

> Above all, [we] would avoid hurting those economists who are interested in facilitating the use of more precise methods in our science without being able to use these methods themselves. It would be very unfortunate to provoke a reaction against our ideas by the people who hold the majority of the chairs and the official executives who have consequently great influence on the youth. (Original letter cited in Bjerkholt, 2014a, p.16)

1.3 The first steep to continuity: the institutional crystallization

The organization of a small network of like-minded scholars, certainly, does not explain the postwar impetus of their project. It is however a watershed. Conspicuously, the institutions through which the abstractly based conceptions of theory, defended by the funder members of the ES, entered *the academic unconscious*[9] of economics are directly linked to the birth of the society. In the first letter, Fisher, Frisch and Roos mentioned a journal to further their project.

[9]For an historical analysis of the concept *academic unconscious* see the preface to « Pour une Histoire de Sciences Sociales. Hommage à Pierre Bourdieu » (Heilbron, Lenoir, and Gisèle Sapiro 2004).

Provisionally called Oekonommetrika, the journal was planned as the platform to promote the advancement of the economic theory on their terms. Moreover, besides the publishing of original work, Econometrica was initially conceived as a welding tool for the international network and thus important to solder their group and secure its continuity. The senders of the letters expected the journal would reinforce a sense of belonging and recall "outstanding works of the past" by the publishing reviews, biographical notes and systematic annotated bibliography of mathematical economic literature. Fisher, Frisch and Roos announced other tasks as an epilogue of the first letter for the academic association: the promotion of the establishment of chairs of economic theory in the universities, the standardization of the notation and terminology of economic theory and publishing a lexicon of technical terms in economic theory.

Alfred Cowles's financial backing was decisive at this point. Concretely, he supported the publication of Econometrica and the creation of a research centre where the founders of the ES grounded and developed their project: the Cowles Commission (later Foundation)[10]. During the thirties and forties, at the Commission different and overlapped projects were collectively carried out. Conspicuously, not all these projects were driven by a single abstractly based conception of theory. Important empirically driven work was developed at the Cowles Commission. The institutional conditions of a research centre -in contrast to a university department- made this coexistence possible. Two elements, that are worth mentioning here are: first, the flow of people that the research centre hosted for short periods during the interwar and post-war years, and second, the dynamics of teamwork between different groups (statistical, mathematical, economic) that characterised the Commission. While a dynamic of collective work existed at Cowles, as their seminars and internal documents testify, during the 1930s to the 1960s scholars' spent an average of one year at the research

[10] The history of the foundation and interwar years of the Cowles Commission is a well-known episode of economics in the United States. See for example Mirowski (2002a) and Düppe and Weintraub (2014a). For an archivally-based history of the Cowles Commission and Foundation commissioned by the Cowles Foundation see (Dimand 2019). For an official history see Christ (1952).

centre. Certainty, the circulation of scholars favoured the coexistence of different projects and the transformation of the balance between *empirical* and *theoretical* in the research centre. Moreover, it invigorated the spread of the ideas and new tools cherished by scholars associated to Cowles.

Cowles position in-between universities and the national (military) laboratories -the two postwar sites of the production of knowledge in the United-States- reinforced both the dynamics of circulation and the coexistence of different projects.[11] Moreover, the heterogeneity of the projects carried-on by scholars associated to the Cowles Commission pair with the disjoint initiatives grounded both sides of the Atlantic- that the ES aimed to coordinate[12].

During the first years of the Commission the influence of ideas coming firstly from Frisch, and then from Haavelmo, were decisive[13]. Under Jacob Marschak[14] direction (1943-1948), the project of the ES took the specific form of providing the Walrasian system with empirical content and to produce *tools* that could be used for very concrete aims such as economic planning.

Up to this point, the research center was able to attract "human capital". Nevertheless, it lacked the social and financial support necessary to increase its influence and secure the continuity of their project. Older, larger, and well-established institutions, such as the National Bureau of Economic Research, still dominated the American economics field. At this point it was thus essential to influence the outside world's perception of what was happening within Cowles's walls. Driven by what had been call the "The New Rigorism in the Social Sciences" (Bender 1997), during the late 1940s conformity requirements lost centrality and differentiation needs gained

[11] Düppe and Weintraub (2014b) present Cowles as a hybrid institution between an university department and a national laboratory. For a presentation of the two post-war sites of research and Cowles in-between position see pages 160-163.
[12] For a study of the European origins of econometric project and the subsequent articulation of the pieces in the postwar United-States see A. Akhabbar (2010).
[13] See for example Bjerkholt (2014b, 14–15).
[14] For details of Marschak's participation during the sunup of the Econometric Society see Bjerkhol (2014b).

importance. The analysis of the Measurement Without Theory Controversy offers an illustration of the incentives for subversion that settled -but still dominated groups- address while the logic of the system changes in their favour. They engaged in direct confrontation, increased the value of the scientific resources and redefine the dominant form of intellectual capital in economics.

2. MEASUREMENT WITHOUT THEORY

The Measurement Without Theory Controversy (MWTC) is a series of 4 papers, all of them published in the *Review of Economics and Statistics* between August 1947 and May 1949[15]. Section 2 focuses on this episode to zoom in on the pivotal moment when *differentiation* concerns replaced the need for *conformity* and explores the *embattle* role of *theory*. The MWTC publicly took issue with Tjalling Koopmans's[16] critical review - whose title gave name to the controversy - of Mitchell and Burns' *Measuring Business Cycles* (1946). Koopmans's review was followed, first, by a reply under the title "Koopmans on the Choice of Variables to be Studied and the Methods of Measurement" written by Rutledge Vining[17]. Koopmans' reply and Vining's rejoinder completed the episode. The two replies and the rejoinder were all published, in 1949, in the second issue of the journal[18].

[15] Mirowski (1989b) analyses the controversy as a confrontation where the "major weapons were the prevalent cultural images of what it means to be "scientific"(p. 69). The analysis of the controversy presented here partially relies on Mirowski's work. Nevertheless, the broad perspective in which this article inscribes the controversy and the emphasis on theory distances to a certain extent the conclusions.

[16] For details of Koopmans's trajectory see for example Mirowski (2002b).

[17] Vining, at that time, an associate researcher at the NBER, had graduated five years before from the University of Chicago with a thesis on regional variations of short-time business cycles. Mitchell and Burns' absence in the controversy has been explained by their respective health problems and political obligations. See for example Mirowski (1989a) and Hendry and Morgan (1995). A generational change at the NBER may be a more general explanation. Today, thanks to resent archival work, we know that Milton Friedman helped Vining draft a replay to Koopmans, but he refuses to be publicly acknowledged (Cherrier 2011, 350).

[18] The controversy can also be framed as a debate between two different approaches to statistics: Cowles's probabilistic approached inspired by Haavelmo work and NBER's line of research based on descriptive statistics used by Mitchell.

2.1 The embattled role of theory

Throughout the controversy, Koopmans, inspired from natural sciences, justified the superiority of Cowles's approach by highlighting, first, the limits of empirically based knowledge and then by introducing the advantages and potentialities of mathematically-driven deduction. Concretely, Mitchell and Burns work is presented as the typification of an "empiricist position", an approach based on observations tied to specific times and places that did not, and could not, involve any *theoretical* framework. For Koopmans, "Measuring business cycles" was no more than an exercise where "a large scale gathering, sifting, and scrutinizing of facts precedes, or proceeded independently of, the formulation of theories and their testing by future facts" (Koopmans 1947, 167). Without an explicit and *a priori* formulated *theory*, Koopmans claims, of the empirical regularities found by any method of inductive quantification, were but "the eruption of a mysterious volcano whose boiling caldron can never be penetrated" (Koopmans 1947, 167). This is to say, "socially irrelevant" due to their "unreliability as instruments for economic policy" (Koopmans 1947, 167).

Consistently throughout the replays Vining presented Mitchell and Burns' work as an exercise implying a *theoretical* framework. For Vining, as any other process of inductive quantification, *Measuring Business Cycles* embodied a theoretical effort. Conversely, the lack of previous accumulation of observation-based knowledge (in the form of series of statistical data, for example) is highlighted as the main limit of Koopmans's abstractly based conception of *theory.* Hence due to the outlets of such an approach, Vining claims, could be but "unaccomplished" pieces of work which interest is reduced to "abstract and technical problems *per se*". Furthermore, Vining's pushed the debate towards their respective institutional records. Mitchell and Burns' work was thus associated to an institution that "will bear comparison with the work of any other research agency from the point of view of social usefulness"(1998).

2.2 Facing the moment of subversion

In sharp contrast with the early 1930s, during the aftermath of World War II the defence of an approach based on an explicit and *a priori* formulated *theory*, was an effective mechanism to enhance one's own scientific legitimacy. The period 1940-1960 witnessed the establishment of the primacy of abstractly oriented methods and mathematically driven deduction over inductive forms of quantification.

Comparing four disciplines in the social science and the humanities, Schorske (Ellis 1949) synthetized the general shift of the post war years as "The New Rigorism in the Social Sciences".

The general reconfiguration of the legitimate methods to claim the authority of science is intimately linked to the intensive government investments in national defence that started, but did not stop, with the II World War. Whilst developed to solve concrete-practical problems, the tools developed throughout these collaborations were heavily based on deductive abstract procedures that satisfied the new standards of scientificity[19]. Research contracts with the Air Force's Research and Development, an initially private corporation to be known by its acronym RAND, were overriding during those crucial years[20].

As Pierre Bourdieu[21] suggested, groups in a dominated position engage in "subversion strategies" to reinforce the logic of the transformation of the system to their own benefit. The self-confidence and virulence displayed by Koopmans in both, the review and his replay could be read through this frame. In a context of social and intellectual reorganization of scientific activity, subversion (and thus direct conformation), rather than conformity, was important to increase the possibilities of support and

[19] For an analysis of this apparent paradox see (Akhabbar 2010, 54).

[20] For instance, the 1949 Conference on Activity Analysis of Production and Allocation -funded by the RAND research contracts "Theory of Resource Allocation"-, as Düppe and Weintraub (2014b) point out, "defined, more than any other single event, the emergence of a new kind of economic theory growing from game theory, operations research, and linear programming and the related mathematical techniques of convex sets, separating hyper planes, and fixed-point theory." (454)

[21] See for example (see Bourdieu 1975).

recognition outside academia. Indeed, dominance principles are dualistic, they are the product of a tension between strictly scientific resources and the financial resources necessary to buy and build the institutional structure on which scientific authority relies on. During the constitution of the Econometric Society, the support of institutionalist economist for the "advancement of economic theory", as discussed in Section 1, was necessary. After World War II Mitchell's good reputation amongst philanthropic organizations became an obstruction to Cowles' ambitions. Since the beginning of Marschak's directorship, the Commission started looking to widen its institutional support. As Mirowski has argued, "even with the continuing support of Alfred Cowles, they still were not a match for the army of researches at the NBER, with their extensive sources of support from the SSRC, the Carnegie Corporation, the Rockefeller Foundation, the U.S. Government and private business" (Mirowski 1989b, 73-74)[22].

Craving for the necessary institutional and personal status to raced funds and the lack of the actual results necessary to prove Vining's accusation of being *per se* interested on abstract and technical problems wrong, Koopmans relies first, on the promises emanated from an abstractly based and mathematically-driven conception of *theory*, and then, on his "scientific capital":

> In view of the insufficiency and inconclusiveness of the "results" reached so far, the only remaining criteria of choice are partly formal (logical clarity and consistency), partly empirical (analogies from other and older sciences that have attained more satisfactory results). (Koopmans 1949, 86)

At this point what was stake at was nothing less than the redefinition of the dominant form of intellectual capital in economics. Certainly, Marschak's top-down approach to econometrics, as (Christ 1952, 47) suggest, allows to anticipate the mathematically-driven and abstractly-oriented research that was carried out at Cowles during Tjalling Koopmans directorship (1948 – 1954). However, this turn could not be yet taken for

[22] For Morgan and Hendry (1995) the Measurement without theory controversy "was an intellectual argument between the Cowles Commission and the NBE, against a backdrop seeking funding for their work –the Cowles in theoretical econometrics and the NBER in applied economics" (69).

granted. In 1952, in an official account an internal observer, presented the latter 1940s-early 1950s Cowles's situation "as a relative shift toward *theoretical* work to obtain better models preparatory to another phase of *empirical work*" (Clark 1947, 75)[23]. Moreover, during the early 1950s at least half of the Econometrical Society, and certainly most members of the American Economic Association, were poorly endowed with intellectual (mathematical) capital required to understand the work undertaken at Cowles under Koopmans' directorship.

For instance, in a note published in *Econometrica* in 1947, John Maurice Clark denounced "mathematical economists" of remaining "a growing and able sect, using an esoteric method and a special language, which makes their results increasingly inaccessible to the rest of us" (Clark 1947, 75)[24]. The note is a clear illustration of the changes in the balance of forces. Indeed, Clark starts by characterizing his "standpoint" by the "distinctly rudimentary" level of his mathematical equipment measured by the "present standards". Furthermore, he insists on his "tremendous respect for the accomplishments of the mathematical students" and expressed his desire to make "as much use of them as possible" or, at least, "to see them utilized as fast as necessary" (Clark 1947, 75). Clark was not alone, as Robert Solow expressed it in 1957: "Next to the desire for salary increases, the desire most frequently expressed by economists is for a translation of some of the more recondite results of recent mathematical economics for the use of the profession at large" (Solow 1958, 178).

3. THREE ESSAYS

Published in 1957, "Three essays" is a rather eclectic book whose role and influence in economics is still to be determined. The interest aroused by the book evidences Tjalling Koopmans's legitimacy at this point. For instance, well-established scholars

[23] No emphasis in the original.

[24] It is worth mentioning that Clark's note was an expansion of one of the sections of his address on "Some Cleavages among Economists" given at the dinner meeting of the American Economic Association at Atlantic City in 1947.

reviewed the book in 4 top journals[25]. In this Section we use the demands to which the book replays, Koopmans's methodological prescriptions and their application in the book as an entry point to explore the complementary process of *communication* and *establishment of a sense of belonging* to an (not yet) articulated research project. We address in particular the consequences in disciplinary terms for economics of separation between *theory* and *empirical* work operated by the methodological prescriptions developed by Koopmans and synthetized on what he calls the postulational method.

3.1 Facing the challenges of new dominants: communicability and fusing a group

In sharp contrast with the Econometric Society *newcomers dominated position* and the concomitant strategy of combining conformity and differentiation (discussed in Section 2), Clark's plea for "communicability" states the new position of the scholars committed to an abstractly based conception of *theory* deeply reliant on mathematics. Subversion was something of the past. The time for communication and articulation of a research project had arrived.

3.1.1 Overcoming disciplinary divisions: the time of communication

Overcoming disciplinary divisions was fundamental to forge an accommodation in the postwar environment. As Clark mentioned in his note, "The results [achieved by "mathematical economists"] would be far more widely useful if they became part of the common equipment of economists in general" (Clark 1947, 75). In 1948 in a confidential memorandum written upon the demand of J.H. Willits at the Rockefeller Foundation Milton Friedman suggested communication as key element to increase the value of the research done at the Cowles Commission:

[25] Robert Solow reviewed the book in *Econometrica*, Andreas G. Papaderus in *The American Economic Review*, G. C. Archibald in *The British Journal for the Philosophy of Science,* Marc Nerlove in the *The Journal of Business.*

> It may be in point to mention two steps that might greatly increase the value of the Cowles Commission experiment. First, the Cowles Commission should be urged to provide translations of their basic work from their increasingly specialized jargon into a form in which it will be accessible to, and capable of judgment by, the great bulk of economist (?)[26]

Tjalling Koopmans "Three Essays on the State of Economic Science" is a step in this direction. The first essay is an explicit response to Clark's plea for "communicability", as mentioned in the preface. For Koopmans, the objective was "to communicate the logical content, and some of the underlining reasoning, of recent developments in mathematical economics" to the "general economist" (Koopmans 1957, vii). Far from the attacks of the *Measurement without theory controversy*, Koopmans' work was described by Robert Solow (Solow 1958, 178) in his review as "done with sympathy, skill and care". Theorems demonstrations are set-aside in footnotes and diagrams are profusely used to simplify the understanding of "basic principles" and "applications". Furthermore, the essay is typed in two different sizes to facilitate the access to the "no mathematical reader". Smaller type signals the more "mathematically complicated passages" that can be passed by those with "insufficient mathematical background" without losing the main threads of the reasoning (Koopmans 1957, 55).

Communication with the "general economist" was not quite accomplished, as all the book reviewers noticed. As Andreas Papandreou put it, Koopmans was "only moderately successful" reducing "the isolation of the mathematical economist for his non mathematical colleague". For Papandreou,

> [D]espite the great efforts of the author to simplify the argument and present it in terms as nontechnical as is consistent with the nature of the subject matter, the going is hard. The mythical "average economist" may expect to get the flavour of the argument, but should not really expect to understand it fully, unless he is prepared to acquaint himself with the mathematical tools needed (Papandreou 1958, 668).

[26] I am thankful to Marcel Boumans for bringing to my attention this memorandum and for generously sharing it with me.

Certainly, to acquaint the "average economist" with the necessary mathematical tools to understand the "First Essay", was not a process in which a single piece of work could get done overnight. In his review Solow captured this idea with a very insightful analogy: "for a while people will feel the way they used to feel about modern architecture: it may work, but somehow it does not seem like home" (Solow 1958, 178). Generalizing the approach defended by Koopmans (discussed in Section 2) to the "average economist" was a long-term institutional process that included, first and foremost, the restructuring of economics education. During the 1950s, "mathematical economists" in the United States were aware of this. Important individual pieces of reflection in the role of mathematics on economics, in general and in education in particular, were produced[27]. These individual reflections were all grounded in a broad trans-disciplinary dynamic of change in the social sciences. This process was encouraged by both national and professional agencies and academic associations. For instance, the Social Sciences Scientific Council (SSSC) sponsored in 1940 a study on the role of mathematics on social scientist education. During the same period, the American Economic Association (AEA) sponsored several studies and hosted numerous commissions in the subject of mathematics on economics instruction.[28]

3.1.2 Building a sense of belonging: the time of cohesion

While the mere publication of the "Three Essays" did not make it *seem like home,* the results Koopmans's intended to communicate, their synthetic presentation and furthermore the methodological prescriptions presented in the book had a relevant impact on the constitution of the "mathematical economist's group and thus for the future of the discipline. These ideas were indeed central to establish a *sense of belonging* to an (not yet) articulated research project. Indeed, with the "First Essay" Koopmans did *bring out* what he called the "basic unity" of substantive parts of the work developed in the context of the Cowles Commission from the pre-war to the post World War II years. Koopmans presents as "offshoots from the same mathematical

[27] See for example (Marschak 1947; Stigler 1949; Samuelson 1952; Leontief 1954; Allais 1954; Novick 1954; Bodenhorn 1956).
[28] See for example (Bowen 1953). For an analysis of the Bowen Report see (Orozco Espinel, forthcomming)

stem" –i.e. the theory of linear spaces: i) the model of competitive equilibrium (Debreu 1954; Arrow 1951); ii) the theory of the use of prices for the efficient allocation of resources –modern welfare economics- (Dantzing 1951a, 1951b; Koopmans 1951), iii) the models of activity analysis (Leontief 1941); and iv) input-output analysis (2014b). For the first time this set of dissimilar and separated contributions were brought together within a single conceptual structure.

This *unified system* was fundamental to establish and settle a *sense of belonging* to an (not yet) articulated research project. This was particularly important taking into account the, in average, brief stays of scholars at Cowles already mentioned.

3.1.3 The separation of theoretical and empirical work: the postulational-method

The *unified system* presented in the "First Essay" exemplifies what Koopmans calls the "postulational method". Explicitly presented in the Second essay, the method is a key element to apprehend, on the one hand, the link between the *cohesion challenges* that the new dominants have to address, and, on the other hand, the separation between *theory* and *empirical* work operated by the methodological prescriptions developed by Koopmans.

The postulational or axiomatic method is the process of formally deducing theorems from axioms in some system that includes deduction rules[29]. First, a set of postulates is adopted, and then reasoning develops by following the rules (of logic). At this stage what Koopmans calls "empirical truth" is not a concern: "the only concerns are questions of the *logical truth*[30] and clarity, the correct tracing of the implications of giving postulates, and [the] efficient arranging and recording of conditional, tautological but useful, truths so found." (143) For Koopmans, the method secures the separation of the reasoning from the discussion of its relation to reality, and thus a

[29] For a history and explanation of the postulational method in mathematics see (Huntington 1934). I am grateful with Quinn Culver for his help regarding the uses and sense of the postulational method.
[30] Not emphases in the original.

“clear separation, in the construction of economics knowledge, between reasoning and recognition of facts” (Koopmans 1957, 176). Moreover, Koopmans presents the postulational method as a device to break away the logical (theoretical) sources of knowledge from the factual (empirical) ones. Indeed, the postulates set up a “universe of logical discourse in which the only criterion of validity is that of implication by the postulates” (43) and thus *theories* “can very well stand by themselves as an impressive and highly valuable systems of deductive thought.” (142)

Two independent processes, interpretation and application, give *economic contend* to the set of postulates representing the phenomena studied. Interpretations lend relevance and economic meaning to the postulates by explicitly establishing definitions, statements or descriptions connecting the terms with observable phenomena. For Koopmans the mere process of interpretation brings relevance and economic meaning to the set of postulates. And thus, while urging for a clear separation, the postulational method offers, Koopmans claims, a procedure through which *theoretical* and *empirical* economics *move closer to each other*. They meet on the ground of a common requirement for good hard thought from explicit basic problems ” (Koopmans 1957, 176).

This separation implies and encourages a radical division of labour within the discipline that Koopmans linked to the advances of physical sciences:

> In some of the physical sciences a considerable degree of differentiation has developed between experimental work, devoted to the observation, and theoretical work, devoted to reasoning and to the construction of premises from which to reason. (130-131)

While *theory* and *empirical work* stay inexorably intertwined in economics – maybe with the remarkable exception of Gerald Debreu work –, for economics as discipline Koopmans’s separation had forth different consequences. First, it was important to forge an articulated response to the critics pointing out interest in technical and mathematical problems *per se*. Second, it gave a cumulative character to the research

project. Third, it reinforced the pertinence of mathematical tools for economic reasoning. Finally, gave economics a universal character.

a. Response to the critics

In the Second essay, Koopmans explicitly addresses the puzzle John Hicks so nicely put in 1939 in "Value and Capital". For Hicks, "Pure economics has a remarkable way of producing rabbits out of a hat- apparently *a priori* propositions which apparently refer to reality" (In Koopmans 1957, 132). As during the *Measurement without theory controversy*, mathematically and abstractly driven definition of "theory" is defended more in its promises than in its achievements: the distance between theory and *reality* is justified by the current (and provisional) state of the former:

> If we look at economic theory as a sequence of conceptional models that seek to express in a simplified form different aspects of an always more complicated reality. At first these aspects are formalized as much as feasible in isolation, then in combination of increasing realism […] The study of the simpler models is protected from the reproach of unreality by the considerations that these models may be prototypes of more realistic, but also more complicated, subsequent models. (Koopmans 1957, 142–43)

b. Accumulation of knowledge

Beyond the defencing effects of an articulated response to critics pointing out the unworldly character of the project, Koopmans's version of the postulational method sets up the guidelines for an effective accumulation of knowledge. Indeed, by applying this method a large body of economic spread from Koopmans' "creative synthesis". As Koopmans expected, "dissatisfaction with the relevance of available models [provided] stimulus for cumulative refinement of models to take into account more and more relevant aspects of reality." (147)

The incorporation of information on economics illustrates how the postulational method effectively functioned as a long-term research project. Confronted by the critics who pointed out the unrealistic (and epistemically implausible) character of the perfect knowledge (of desires, prices and possibilities premises) the researchers

associated to the Cowles Commission introduced *information* as, Mirowski and Nik-Khan (Mirowski and Nik-Khah 2016, 64) show, that the formal incorporation of *information* into economics took place by "build into" Koopmans's "creative synthesis" and followed the guidelines of the postulational method. Indeed,

c. Introduction of mathematics

The postulational method also worked as a strategy to justify the pertinence of mathematical tools for economic reasoning. This is particularly true for convexity techniques, a set of mathematical tools that, during the 1950s, reached a new level of authority in economics[31]. For instance, Koopmans presents the convexity conditions, necessary to keep all decisions makers reconciled with Pareto optimal, as a suitable first step in a process where "analytical difficulties must be taken one by one." (Koopmans 1957, 144) This was central to open the channels of disciplinary reconversion to economics. For Koopmans, a "more explicit use of the postulational approach is favoured also by considerations of the communication between the sciences" (Koopmans 1957, 144). More precisely, "By reducing the body of facts to be recognized to a set of postulates, logical and mathematical skills can be more effectively brought to bear on the deductive aspects of economic theorizing." (145)

d. Universal science

Finally, the postulational method reinforced the universal character of economics a characteristic associated to science that the founders of the ES put forward in their quest of scientific legitimacy. For Koopmans:

> Sometimes quite different economic phenomena are expressible by set of postulates of which the logical content is similar or even identical. As already observed, the reasoning itself depends only on that logical content, and not on the interpretation of the terms used. An economy of effort is thus achieved and insight into logical unity under substantive diversity is gained, if the piece of reasoning is allowed to stand in the record as such, detached to the substantive context, which may have led to its original construction. (144)

[31] See (Düppe and Weintraub 2014b).

This point is fundamental to understand the development of the professional dimension of economics during the 20th century. As Marion Fourcade (Fourcade 2006) has shown, economists are considered as international professionals. See also (Coats 1996)

CONCLUSION

Kenneth Arrow, in 1983, started his presentation for The Cowles Fiftieth anniversary celebration by asking: "In what sense can we isolate the contribution of any individual or institution in the development of the economic analysis?" Arrow's answer: "no research institution is an island entire of itself" (1995). Closing here the paraphrase of John Donne's XVII Meditation, Arrow continued: "Cowles is not and was not a group isolated from the mainstream economics, and its contributions are today inextricably mingled with other currents". Yet, the work of the scholars associated to the research centre was fundamental to define economics in a very particular way.

As we saw, mathematically and abstractly driven definition of "theory" developed and defended by economists associated to the Cowles Commission was especially successful to claim scientific authority.

If for their critics Cowles' project was based on procedures to side-step problems –or as Solow puts it a method to "make it easier to assume the problems away" (Solow 1958, 178)–, for the articulation of the discipline its consequences are fundamental. During the 1930s, 1940s and 1950s, in the context of the research centre the discipline laid the foundations of the long-term research project that allowed an important part of the discipline to cohere around. As we saw 1) it was important to forge an articulated response to the critics questioning the interest in technical and mathematical problems *per se*; 2) it was central to the process of setting the guidelines of a cumulative research project; 3) it reinforced the justification of mathematical tools for economic reasoning; and 4) it reinforced the universal and thus international character of economics.

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