Rigorism and abstraction: Mainstream economics boundaries. Econometric Society and Cowles Commission demarcation from institutionalism (1930-1960)

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Camila Orozco Espinel
Ph.D student EHESS (Paris)
camilaorozcoe@gmail.com

Introduction

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" (K. J. Arrow 1983, 1). Closing here the paraphrase of John Donne's XVII Meditation[1], 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"*[2]. In this paper, I argue that, while never isolated and despite its contributions to economic analysis are inextricably mingled with those of *other currents*, during the 1930s, 1940s and 1950s, economists associated to the Cowles Commission drew on and reinforced *specific* boundaries of what Arrow rightly called in 1983 *mainstream economics*.

Due to the cognitive authority of science in American political culture, its different representations are a key entry point to issues of demarcation of academic territories. The boundaries drew on and reinforced at Cowles are related to what have been call "The New Rigorism in the Human Sciences" (Schorke 1998). Rigorism and abstraction, two characteristics usually associated with science, were mobilized during "boundary disputes" at Cowles Cowles and thenceforth became constitutive trends of *mainstream economics*. Indeed, after the years these paper focuses on, an important part of what was considerer *the most accomplished achievements of economics* matches with what Cowles represented. Demarcation from institutionalist

[1] The Meditation XVII was published in 1624 in *Devotions upon Emergent Occasions*. The original canto is: "No man is an island, entire of itself; every man is a piece of the continent, a part of the main. If a clod be washed away by the sea, Europe is the less, as well as if a promontory were, as well as if a manor of thy friend's or of thine own were: any man's death diminishes me, because I am involved in mankind, and therefore never send to know for whom the bell tolls; it tolls for thee".
[2] No emphases in the original.

approaches, thence back dominant in the United-States, is thus a privilege angle to explore Cowles' *boundary-work*.

This paper refers to *boundary-work* in the sense of Thomas Gieryn (Gieryn 1999; Gieryn 1983; Gieryn 1995), that is to say, as a practical problem for scientists when confronted to: 1) the demarcation of science from other intellectual activities (such as art, religion and folklore); 2) the confrontation between rival claims of (genuine) scientific approaches; and 3) the separation between the production of scientific knowledge and it consumption by non-scientists (the government and industry, but also engineers or technicians)[3].

At the Cowles Commission, during the years studied, different projects were indeed collectively carried up. These successive –though sometimes overlapped- projects were funded and mobilized different –not necessarily coherent- ideas, on the one hand, about on the scientificity and, on the other hand, about economics' role and scope. I argue, first, that a common tread, concretely, an underlining tension between *theoretical* and *empirical* research, runs through these different *orders* -to borrow Phillip Mirowski's expression. Second, both, this common tread, and what made possible the ruptures can be explained by the intertwined of specific institutional conditions and the trajectories of the scholars that were involved. Specifically, my analysis relies on the hypothesis that an ineligible relationship exists between the position and dispositions of those who participated in the demarcation process, on the one hand, and the boundaries they drew, on the other hand (Bourdieu 1968; 1991; 1992; 1994; 1984; 2001).

The aim of this paper is threefold. First, make explicit the characteristics of sciences put forward in the context of the Cowles Commission and how they were mobilized to demarcate *mainstream economics territory.* Second, explain why these characteristics flourished in the particular context of the Cowles Commission. Concretely, what are the social, intellectual and institutional reasons that made possible the specific process of demarcation of *mainstream economics territory* that took place at Cowles? Third, bring out how the characteristics associate sciences in the context of the Cowles were crystalized in the form of institutions (reviews, professional association, research centers, …) and durably transformed economics.

To study these process, this paper focus in three episodes. First, the preambles and constitution of the Econometric Society (1930s). Second, the Measurement Without Theory controversy (1940s). Third, the Three Essays (1950s).

Thomas Gieryn (Gieryn 1999; Gieryn 1983; Gieryn 1995) has shown with remarkable clarity, that science is a space that "acquires its authority from and through episodic

[3] For other uses of the concept of boundary work in the history of economics see for example: (Mirowski 1999, 690–691; Mata 2009; Fourcade 2009, 8–9,77–78,90–93).

negotiations of its flexible and contextually contingent borders and territory"(Gieryn 1995, 405). Different representations of science are mobilized to erect separate disciplinary boundaries in response to *different* challenges and obstacles to scientists' pursuit of authority and recourses. Rather than an anomaly or a lack of consistence, the ambiguity of disciplinary boundaries results from the simultaneous pursuit of specific (but interconnected) disciplinary goals, each one requiring boundaries to be built in different fronts and ways.

The analytical devise presented in this paper aims to locate *mainstream economics* in an encompassing frame and capture the tension between consensus and heterogeneity[4]. Hence, *Mainstream economics* is presented as a heterogeneous territory whose different pieces –in Arrow's terms *currents*- established different boundaries and thus served different functions in the process of monopolization of scientific authority and resources. In other words, this paper goes a stanza further on John Donne's XVII Meditation: *every research institution is a piece of the continent, a part of the main*. The paper studies thus *mainstream economics* as a *continent*: neither a single *thing* (much importance is given to the fact that the boundaries were drawn and redrawn over the time), nor a monolithic island (the emphasis is put in the ambiguous ways in which boundaries changed)[5].

Institutional - Neoclassical: overlapped

Institutionalist economists during in the interwar period were an extremely diverse group. As Morgan and Rutherford (1998) remarked, "Institutionalism consisted of a number of loosely related research programs, one cluster centering on business cycles and unemployment, with a reform agenda involving some notion of overall planning, and another cluster centering on the legal dimension of market, with a reform agenda focusing on labor law and business regulations" (p. 2).[6] Moreover, institutionalism never exited as an articulated, tightly knit theoretical agenda. Yet, its different clusters were all grounded in a shared, distinctive attitude towards economic research.

[4] Although scholars of interwar and postwar economics have shown the deep heterogeneity that existed between mainstream *currents*, a classificatory rather than explanatory principle has generally led the analysis, and heterogeneity has often been underestimated.

[5] This paper is part of a broad project which tries to articulate the analysis of different boundary-work processes -between 1930 and 1960. The aim is to understand mainstream economics' monopolization of the definition of science through the study of the boundaries that had to be established in order to achieve disciplinary hegemony. Boundary-work is used as an analytical tool and as well as a way to link the specific boundaries under study. The project centers on three institutional locations: the Cowles Commission, the University of Chicago and MIT economic departments.

[6] Institutionalism roots in the United-States extend back to the 1880s, nevertheless, it became a self-identified movement only in 1918 (Rutherford 1997). It included Wesley C. Mitchell's quantitative methods, John R. Commons's documentary histories and interviewing, Walton Hamilton's cases studies of firms and industry, and John M. Clark's applied theorizing. On Institutionalism in the United-States.

Stemming from a strong belief in the usefulness of economic knowledge for human and societal betterment, institutionalist attitude to research was grounded in an inductive, empirical approach to the study of the economy and the conviction of the inadequacy of an unregulated market. Both characteristics rely on a common epistemological frame where objectivity depended on assumptions based either on *unprejudiced empiricism* -based on the systematic collection and analysis of data- or in the detailed inquire of the historical and institutional condition as the only means to address and face economic issues. Nevertheless, in spite of their diversity, in the United-States, institutionalist remained more closely associated with empirical methods (Ross 1979, 417; Camic and Xie 1994). W.C. Mitchell's full engagement with the identification of empirical regularities through close quantitative observation is the maximum expression. His work with Arthur Buns, *Measuring Business Cycles* (1946), is the quintessence of this approach.

The heterogeneity within institutionalist approaches went to its boundaries. Thus, during the years *mainstream economics* was drawing its boundaries, a position of intellectual compromise between the two approaches was current.

Hence, as heterogeneous were the groups demarcating one to the others and ambiguous the boundaries between them, the confrontation was framed by alternative ideals of quantification (Porter 1997). Whether relaying on inductive or deductive arguments, US American economists "scientificity" is closely linked to

The constitution of the Econometric Society: boundary and ground-work

It is important to highlight that the *econometric project* -as originally conceived by the founders of the Econometric Society- did not born with the seminal documents analyzed in this section. These documents made explicit something that was already happening on both sides of the Atlantic Ocean: a hitherto disarticulated movement to bridge theoretical considerations and observation, based on both mathematical and statistical tools[7]. While the Econometric Society effectively coordinate the efforts of many disjoint initiative, the heterogeneity of the group never disappeared. The analysis is concentrated here on Cowles particular appropriation of the econometric project[8].

In June 1930, Irving Fisher Ragnar Frisch and Charles F. Roos sent a first letter to a group of 28 colleagues from 10 different European and North American countries, to inquire on the viability and best way to carry out the international scholarly

[7] 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).

[8] For instance, while Fisher saw most of his previous work as embodying the research orientation that the Society should follow, not all the future member of the society shared Frisch particular approach (Bjerkholt 2014a, 3).

association through which they expected to further the project[9]. Although the diversity of responses is surprising, the 28 recipients of the first letter almost unanimously agreed on the project's general pertinence. The second step was to organize a meeting with the objective of rendering the Society's foundation official. An invitation was sent to 83 recipients with, in attachment, a draft of the Society's project. [10] Six months after the first letter has been sent, during a joint meeting of the American Economic Association, the American Statistical Association and the American Mathematical Society, the organizing meeting of the Econometric Society was held. Sixteen people assisted, of which six had not been officially invited[11]. The initial project draft was not substantially modified. Notably, the subtitle "Econometric Society, An International Society for the Advancement of Economic Theory in its Relation to Statistics and Mathematics", which up to today figured on the cover of Econometrica, went unaltered.

The first steps of the official constitution of the Society make explicit its modest (this should be relativized) beginnings[12]. Modesty, that strongly contrasts with the ambitions of their project. This tension between modest origins and an ambitious project can be explained by the fundamental tension that characterized the beginning of the Econometric Society and the Cowles Commission: a central role in an international network which importance was strengthen and a marginal place in American economics.

In the June letter, Fisher, Frisch and Roos attributed selected characteristics to what they called *genuine Economic Science* to demarcate their approach, from what they considered not to be science: the mere empirical treatment of economic problems, i.e. institutionalist economists' empirical methods and emphasis on data collection. The seminal letter was, certainty, a preliminary and somewhat de-structured *enquête*,

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[9] Amongst the 28 recipients there were eight Americans (T. N. Carver, John B. Clark, John M. Clark, Griffith C. Evans, Mordecai Ezekiel, Henry L. Moore and Warren M. Persons and Henry Schultz), four French (Clément Colson, François Divisia, Jacques Moret and Jacques Rueff), three English (Arthur L. Bowley, A. C. Pigou et John M. Keynes), two Swedes (Gustav Cassel et Bertil Ohlin), one German (Ladislaus von Bortkiewicz) one Russian (Eugen Slutsky), two Austrians (Hans Mayer et Joseph Schumpeter), one Dane (Harald Westergaard), one Pole (Wladislaw Zawadski), five Italians (Luigi Amoroso, Umberto Ricci, Pietri-Tonelli, Gustavo del Vecchio and Corrado Gini).

[10] Those who responded positively to the first letter, plus the additions to the initial participants' list made upon demand by Fisher, Frisch and Roos.

[11] Those who were invited: Ragnar Frisch, Harold Hotelling, William F. Ogburn, J. Harvey Rogers, C. F. Roos, Josef Schumpeter, Henry Schultz, W. A. Shewhart, Ingvar Wedervang, and Edwin B. Wilson. Those who were not invited but participated to the meeting: Karl Menger, Frederick C. Mills, Oystein Ore, M. C. Rorty, Carl Snyder, and Norbert Wiener.

[12] As Bjerkholt (2014a, 16) underlines, the first years of the Econometric Society were rather modest. After sending the seminal letter in June and receiving the responses, the organizers quickly moved ahead without any funding for the Society and with no more than an embryonic list of members.

where very general and very specific questions were alternated. Nevertheless, from its very first lines a very ambitious project was explicitly expressed:

> 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 in 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. (Originale letter cited by Bjerkholt, 2014a, pp. 8–9)[13]

An emphasis on theory was the distinguishing feature drawing the line which separated genuine scientific economics from what was not. This line, first and foremost, was the boundary demarcating economics from politics: a barrier protecting economics from politically motivated distortions. This invocation –and instrumental use- of science's legitimacy is not exclusive of the small network of scholars who founded and gathered around the Econometric Society.

The particularity of the *boundary-work* of the small network of scholars who founded and gathered around the Econometric Society relies in the specific meaning accorded to the world theory:

> The word theory in this connection, should, of course, not be interpreted as synonymous with abstract reasoning only, but as including also the analysis of empirical evidence suggesting or verifying theoretical laws. (Originale letter cited in Bjerkholt, 2014a, pp. 8–9)

This definition brings to the fore a delimitation of the repertoire of genuine economic science, and thus an attempt of demarcation from rival approaches to economic knowledge. The separation implies a hierarchy between one approach that combined abstract-rational and empirical methods and pure empirical studies.

This boundary is reinforced several times throughout the letter. For instance, while underlining the importance accorded to the quantitative character of economic theory, Fisher, Frisch and Roos conclude:

> 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)

For the senders of the letter,

[13] No emphasis in the original.

> […] it will be largely through a constant and close *connection*[14] between the abstract-rational and the concrete-empirical points of view that the modern quantitative movement in economics will produce significant and lasting results. (Originale letter cited in Bjerkholt, 2014a, p. 10)

For them, this connection was only possible to achieve through a

> […] constructive and rigorous thinking similar to that which has come to dominate in the natural sciences (Originale letter cited in Bjerkholt, 2014a, p. 32)

The rather unspecific but narrow character of the definition of theory fostered by the Econometric Society organizing group was evident for the recipient of the letter. This was all the more so for those in a position of intellectual compromise between the two approaches. J.M. Clark's response evidenced this, first with regards to the membership requirement:

> If the association is to represent *theory in general*, and not simply *one kind of theory*[15], 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 favor 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-quantitative method; especially as I expect to do my main work in theory, but not mainly in that field. (Cited in Bjerkholt, 2014a, 21-22)

Fisher, Frisch and Roos were aware that "In practice, the line [between economists whose work adjusted to their exclusive definition of theory] will be difficult to draw"(In Bjerkholt, 2014b, p. 10). The extent, to which the separation between eligible and non-eligible candidates was blurry, emerges from the list of recipients of the seminal letter. Although it comprised mostly scholars with links to the great names of the marginal revolution, whose work exemplified the use of mathematics in economics, from a contemporary point of view the list seems remarkably heterogeneous. This heterogeneity can, in the first place, be understood as the expression of -the already mentioned- intellectual continuum running from *mainstream* to institutional economics between the wars.

[14] No emphasis in the original.
[15] No emphasis in the original.

Then, in the interwar period American economist' identity was still under construction and 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). Indeed, the scientistic vein of US American economist allowed them to hold some common standards, argue over matters of method and yet share the same platforms and contribute to the same journals. For instance, the relations between NBER and Cowles were essentially mutually supportive and cordial in the 1930s and 1940s. The analysis of W.C. Mitchell's links with the Econometric Society is particularly revealing of this matter.

During the early 1930 Mitchell was one, if not the most, important economists in the United-States. While absent from the list of recipients of the seminal letter, he was invited to the inaugural meeting of the Econometric Society the 29th December 1930[16]. Mitchell did not attend the meeting, nor was his name part of the Society's first Council. Once the confusion surrounding the equivalence between the terms *quantitative* and *statistics* was cleared up during the organizing meeting, Wesley C. Mitchell name was withdrawn as a candidate for Council Membership (Bjerkholt 2014a, 27–28)[17]. Although its broad character, the Society's definition of theory –at the base of the delimitation of the repertoire of scientific economics- let not room to Mitchell's empirical methods based on detailed statistical data. At this particular, though, juncture institutionalist economist was fundamental to further the econometric project.

Thus, in the second place, the heterogeneity of the recipients' lists can also be understood as the reflection of the non-dominant (new comers) position in the economics field occupied by the international network[18]. François Divisia's response to the letter's inquiry on the association journal's name, illustrates this idea:

> 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 may 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

[16] Mitchell was also in the first advisory council that directed the Cowles Commission during its first years and was the first US American president, after Fisher, of the Econometric Society.
[17] Schumpeter is the responsible for the elucidating speech that clear up the confusion. The articulation element of his argument is the exclusive definition of theory mobilized by the Society.
[18] It is worth to remember that in Bourdieu's sociology the principle of the action is explained by agents' dispositions. Dispositions that in turn are correlated with the position the agents occupied in the field, that is to say the objective relations.

> 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)

New comers dilemma

Taking this into account, the reorientation –from the fist 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, can thus be better interpreted as a response - of a group in a non-dominant position- to the new comers dilemma. Confronted with the conflicting demands of conformity and differentiation the organizer of the Society chose a strategy combining both, conformity and differentiation. Bjerkholt's (2014a) close scrutiny of the membership requirements in fact shows that the criterion stated in the June seminal 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".[19] (p. 24)

The emphasis Clement Colson, Divisia's elder, put in his response to the fist letter on the importance of keeping a low profile, is not less telling of the weak position of the Society's international network in the academic field. Moreover, Colson's response is reveling of the institutional *enjeux* structuring Cowles' boundary-work:

> 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. (Bjerkholt, 2014a, p.16)

Yet, the organization of a small network of like-minded European and US American scholars' -with serious background in mathematics- to join forces in order to promote their common ideas about the future of economics, certainty, does not explain postwar impetus of their project. It is however the watershed. Significantly, the initial design of some of the main pieces of the disciplinary structure that bore mainstream economics throughout the second half of the 20th century can be traced to the seminal letter.

[19] It was Schumpeter, in his response to the June letter, who was at the origin of the two-level membership policy for the society.

There, Fisher, Frisch and Roos mentioned a journal to further their project. Provisionally called Oekonommetrika, the journal, was planned as the platform to publish works bridging the gap between abstract rational and concert empirical. Three complementary functions were presented:

> Besides the publishing of original papers there would, in our opinion, be three main functions for the journal: (1) reviewing and abstracting the more important mathematical economic works, both those currently published in other economic, statistical and mathematical journals, and the outstanding works of the past, past, (2) furnishing biographical notes regarding mathematical economists of the (3) preparing a complete and systematic annotated bibliography of mathematical economic literature. This would require the cooperation of correspondents in many countries. (Originale letter cited in Bjerkholt 2014a, p.11)

Other tasks for the academic society were considered and Fisher, Frisch and Roos announced as epilogue of the first letter:

> Besides the creation and publication of a journal, there might be numerous other possible tasks for the association, as for instance, promoting the establishment of chairs of economic theory including mathematical economics in the universities, helping toward the standardization of the notation and terminology of economic theory, publishing a lexicon of technical terms in economic theory, serving as a bureau of reference for commercial firma who have problems offering theoretical and statistical difficulties, and so forth (Originale letter cited in Bjerkholt, 2014a, pp. 11–12).

A new research center: Cowles

Almost immediately after the official constitution of the Econometric Society, a wealthy Colorado banker, named Alfred Cowles III, gratified the *scientistic scope* mobilized by its organizer providing financial backing[20]. Cowles' underwriting was decisive for both the Society and the construction of the disciplinary structure of *mainstream economics* -A. Cowles supported the publication of Econometrica and the creation of a research center, the Cowles Commission (later Foundation). Under Jacob Marschak direction (1943-1948) at the Commission, the project of connecting abstract and empirical methods took the specific form of providing Walrasian system with empirical content. The influence of the ideas of Frisch first, and Haavelmo latter, was decisive to Cowles' specific appropriation of the econometric project (Bjerkholt 2014b, 14–15).

[20] The Cowles Commission history is today well known. See for example: (Düppe and Weintraub 2014; Mirowski 1999; Christ 1952; Bjerkholt 2014b).

It was precisely this project that Tjalling Koopmans defended in *The Measurement without theory controversy*, epitome of the boundary-work carried out by institutionalist and *mainstream* economists associated with Cowles to demarcate their approaches.

Measurement without theory

The *Measurement Without Theory Controversy* is a series of 4 papers, all of them published in the *Review* of *Economics and Statistics* between August 1947 and May 1949. The controversy publicly took issue with Kopmans' critical review -whose title gave name to the controversy- of Mitchell and Burns' *Measuring Business Cycles* (1946). Koopmans' 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[21]. Koopmans' reply and Vining's rejoinder completed the episode. The two replys and the rejoinder were all published, in 1949, in the second number of the journal.

Without underestimating the methodological relevance of the episode, this section analyzes the controversy as set of mechanisms through which the Commission delimitated the repertoire of scientific economics (*mainstream economics*) and increased the value of its symbolic resources to enlarge its material resources[22].

Concretely, the *Measurement Without Theory Controversy* revolves around three arguments which Koopmans expounds in his review to highlight what he considered as the limits of Mitchell and Buns's "empiricist position"[23]. Throughout the review, Koopmans discredits Mitchell and Burns's work by reducing it to 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, 161). Each argument is symptomatic of a shift toward greater abstraction –the primacy of theory- that economics took on after the Word War II under Cowles' aegis (external factors ES).

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[21] Vining, was at that time research associate (in Mirowski, and visiting fellow in Louça) at the NBER, had graduated five years before from the University of Chicago with a thesis on regional variations of short-time business cycles. Due to the Mitchell's health problems and Burns' political obligations it was Vining who responded to Koopmans' critics => Hypothesis: generational change?

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

[23] Statistical turn in American social sciences => general phenomenon during the turn-of-the-century => movement to quantification 1895-1930 => premise: statistical methods were the touchstone (789)(Camic and Xie 1994)

First Argument

Koopmans' first argument develops the reasons for a *primacy* of theoretical considerations over empirical methods. For Koopmans theory is the *only* means to direct analysis towards what it is essential to study in economics: the behavior of economic agents. "Human responses", in Koopmans' terms, are "the ultimate determinants of the levels of economic variables as well as of their fluctuations" (Koopmans 1947, 164). Since empiricist studies remain at the level of combined "effects" of responses, they cannot *explain* and abide at the level of *description*. Scientific economics explains, asks about motives and determinants factors. Instead empirical studies describe, evading fundamental reasons and underlying laws.

Vining replied to Koopmans contesting the exclusive character of his definition of theory and affirming the existence of a theoretical framework in all empirical studies. The controversy over the role of theory in quantitative research, Vining asserts, "might turn upon the nature of the entity the behavior of which is to be accounted for"(Vining 1949a, 79), in other words "business cycles" versus "individual economizing agent". Vining maintains that the aggregate has an existence over and above the existence of individual units: economic variables and their fluctuations are not deductible from the behavior of their components. He finally highlights the dated nature of the controversy claiming that the only novelty of Koopmans' criticism is the introduction of the Walrasian system as his theoretical frame.

Second Argument

The second argument connects the primacy of theory to what Koopmans presents in the review as criteria of social usefulness, i.e. economics relevance to the guidance of policy. The anticipation of the effects of economic policy, Koopmans claims, are not detached from the *explanation* of economic variables. Empirical regularities based on observation are thus unreliable instruments of economic policy. Koopmans goes so far as to present empirical regularities as the result of "the eruption of a mysterious volcano whose boiling caldron can never be penetrated" (Koopmans 1947, 167).

Vining's counterargument relies on institutional track record. While Mitchell and Burns' work is associated to an institution that "will bear comparison with the work of any other research agency from the point of view of social usefulness"(Vining 1949a, 83), Koopman's approach is characterised as unaccomplished and lacking results

Third Argument

Through the analysis of random variability, the third argument introduces a technical element to reinforce the two previous points. For Koopmans, in absence of an *explicit* theory of economic variables' formation, random disturbances –a constitutive element of economic phenomena- can neither be explained nor incorporated to the process of statistical estimations. In the absence of such a procedure, Koopmans claims, no reliable guidance for economic policies can be secured.

Vining's reply assert the primacy of empirical methods: comprehensive mathematization and the formulation of assumptions about economic relations are only possible when the exploratory stage of observation (such as that of Burns and Mitchell) is sufficiently advanced. This was not, Vining claims, the case of economics in the late 1940s. Applications are, at this stage, counterproductive to the quest for scientific legitimacy -"is fortune telling too hard an expression for much of what we do? " (Vining 1949a, 83).

The controversy

Demarcation

The Measurement Without Theory Controversy boils down to the establishment of a hierarchical relation between empirical theoretical methods. Whether relying on methodological individualism or in holism (Camic and Xie 1994, 796), both Koopmans and Vining were trying to delimit the repertoire of scientific economics. Concretely, they opposed conflicting notions of theory and competing views of the role of theoretical and empirical methods.

Koopmans' opposition between *description* and *explanation*, *fundamental laws* and *empirical regularities*, *theory* and *observation* is the bedrock of his justification for the primacy of theory. Through the definition of theory -as the Econometric Society did seventeen years before-, Koopmans links abstract and empirical methods to delimitate the repertoire of scientific economics. Theory is equated to Cowles' appropriation of the econometric project –i.e., provide the Walrasian system with empirical content. If the definition of theory during the preambles of the constitution of the international society was at once broad and focused on the *combination* of abstract and empirical methods, in 1947 Koopmans' rather exclusive definition tips the balance by affirming the *fundamental* character of theory and individual behavior as the determining factor of economic variables and fluctuations.

Vining questions Koopmans' exclusive definition of theory and highlights the necessity of previous accumulation of observation-based knowledge (in the form of series of statistical data) to support theoretical work.

Capital

To examine the arguments mobilized during the controversy as mechanisms through which both Koopmans and Vining aimed to increase the value of their respective symbolic resources –specifically, their scientific capital- can prove particularly insightful. As Bourdieu stated "Uns des enjeux des lute épistemologiques est est toujours la valorization d'une espace de capital scientifique, theoricien ou experimentateur par example" (Bourdieu 2001, 126).

Koopmans

From the very beginning of the controversy, Koopmans introduces an analogy between economics and physics. He specifically applies physics' stages of cumulative development (i.e. Kepler's and Newton's stages) to economics. In so doing, Koopmans uses physics' position in the disciplinary hierarchy as the archetype of science, in order to draw the boundaries of the repertoire of scientific economics. Moreover, the introduction of physics in the controversy is the mechanism through which Koopmans –by training physicist - valorizes his scientific capital. After his arrival to the Commission, Koopmans rapidly became its main spokesman on matters of philosophy and methodology, not because "[…] he had any empathy for philosophy or the history of doctrinal disputes in economics [as he readily admitted]; but rather, because he, more than any figure in the period, spoke with the authority of a self assured physicist about science" (Mirowski 1989a, 76).

Confronted with Vining's charge of lack of results, Koopmans could therefore only admit the unaccomplished character of the project he represented and rely on his scientific capital:

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

Vining

It is noteworthy that there is no explicit reference to Walras' General Equilibrium Theory in Koopmans' review. Vining, in his reply, made the reference explicit. By doing so, he on the one hand associated the project Koopmans represented, with an out-of-date approach and, on the other hand, with a line of development where only mathematical and computational problems were addressed. After presenting Koopmans' main arguments, in his reply, Vining states:

> All [Koopmans] has to insist upon is the mathematical form, and from his discussion it appears not unfair to regard the formal economic theory underlying his approach as being the main available from works *no later*[24] than those of Walras. (Vining 1949a, 80)

[24] No emphasis in the original

Regarding the analysis of random variability, Vining charges Koopmans with emphasizing "somewhat heavily, the estimation aspects of the problem" (Vining 1949a, 85). On the other hand, Vining also accuses Koopmans of being interested in abstract and technical problems *per se*. In the review and the rejoinder, Vining stresses the concrete character of the approach Mitchell and Burns' work represented, for being based on observation and endorsing actual results.

In the rejoinder, by breaking down the central issue of the delimitation in particularly enlighten terms, Vining explicitly mentions the capital he was reclaiming:

> Human, structural and functional characteristics of evolving societal forms [are] not a matter of *logic*, but rather a matter of *facts*[25].(Vining 1949b, 92)

While Koopmans omits all institutional references and bases his legitimacy on the authority of physics, Vining projects the *Measurement Without Theory Controversy* onto an institutional background in order to claim legitimacy.

3. Positions and Strategies

Both Koopmans and Vining's strategies can be explained by their dispositions and respective position in the late 1940's field of American economics. While after the Second World War Koopmans –the senior research, soon to became director, physicist and spokesman of the Cowles Commission- was in the position to challenge the supremacy of empirical methods used at the NBER and claim the primacy of a theoretical approach, Vining –a junior member of the NBER staff[26]- without explicitly denying the potential of abstract research, defended the advocated the need for empirical research as a support to theoretical work. Significantly, Vining describes his reply as a "Defense of empiricism as a fundamental *part*[27] of scientific procedure" (Vining 1949a, 79).

Compared to the organizer of the Econometric Society, during the Measurement Without Theory Controversy, Koopmans was writing from a position of dominance. While 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, in the aftermath of World War II Mitchell's good reputation amongst philanthropic organizations was an obstruction to Cowles' ambitions. Indeed, 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 match for the army of researches at the NBER, with their extensive sources of support from the SSRC, the

[25] No emphasis in the original.
[26] Generational change.
[27] No emphasis in the original.

Carnegie Corporation, the Rockefeller Foundation, the U.S. Government and private business"(Mirowski 1989a, 73–74). The next step was to broaden the resource base and thus to challenge the supremacy of Mitchell's NBER. It was under these circumstances that hostilities between Cowles and Mitchell's NBER escalated. During the "Measurement without theory" controversy, this tension began to surface.

4. Scientific resources vs. financial resources

Indeed, intellectual changes depend on the possibilities of support and recognition outside academia. As Bourdieu argued (Bourdieu 2001, 115), dominance principles are dualistic, 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.

Whether Cowles' based its structural equations on individual behavior or not - actually they did not- is not central to our question. As Thomas Gieryn states:

> To reduce ideologies of science to illusions concocted only to serve professional interest assumes an unrealistically gullible public and a cynical and *merely*[28] instrumentalist scientific community. But to reduce the ideologies to reflections of strains forgets that scientist too struggle for authority, power and resources. Neither strain nor interest are themselves sufficient to explain the successful ideologies of science. (Gieryn 1983, 792)

World War II

The arguments mobilized and the *result* of the controversy show that, in 1947, the defense of a scientific approach to economics could rely in the primacy of theory. During the aftermath of World War II –in contrast to the early 1930s- the deference of an approach where theory came first was an effective mechanism to enhance one's own scientific legitimacy. Indeed, after World War II, in a context of social and intellectual reorganization of scientific activity, statistical methods were no longer the standard of legitimate science. Henceforth, compliance with acceptable scientific models – in which transformation economists actively participated- became compatible with an increasingly theoretical approach.

The technical tools economists developed during the war were not based on NBER's statistics; they were the outcome of mathematical models (Akhabbar 2010, 54). These tools => different kind of action capacity + action capacity that econometrics gave to mathematical economics stated to pay off

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Koopmans' second argument in the review can be better understood by taking into account the duality described by Bourdieu. After World War II, "speculations" based

[28] Emphasis in the original.

on "logic" rather than "facts" –using Vining's own terms- constituted a legitimate support to claim "a scientific basis for public policy" and thus a solid argument in the funding race. Koopmans certainly knew this:

> Political economy has traditionally sought justification for its speculations in the search of scientific basis for public policy in economic matters. (Koopmans 1949, 89)

The end of the controversy

At the end of the controversy, Koopmans was successful in demarcating the repertoire of scientific economics. The boundary dispute resulted thus in NBER's a loss of authority and resources -scientific economics' assumed boundaries left little room for it. While the NBER began a period of decline -which extended until to the capture of Institutionalist by *mainstream economics* in the 1960s-, Cowles' vision of empirical economic inquiry predominated the American economics' profession. It is well known that at this juncture Cowles' approach of simultaneous equations estimation of large econometric models was the object of strong criticism within the mainstream.

Just as the practical and methodological limitations of the Cowles approach started to surface, Cowles interest in empirical economic inquiry began to disappear of it main lines of research.[29] Yet, during the early 1950s Cowles was in a situation of tension: while the approach vehemently defended over the last 20 years was coming to predominance, its methodological and practical limitations were coming to the surface. This tension coincided with the turn in Cowles' disengagement with empirical inquiries.

In 1952, an internal observer in an official account presented the latte 1940s-early 1950s Cowles' situation "as a relative shift toward theoretical work to obtain better models preparatory to another phase of empirical work" (Christ 1952, 47). No "fundamental changes in philosophy" relative to the lines laid out by Marschak, just a "changes in emphasis".

Towards a new Order

By the late forties, after the "Measurement without theory" controversy was over, the path for further abstraction was open (*Gieryn, 1983 p. 789). Henceforth, the standards and practices of *mainstream* economics changed (Mirowski 2002, 166). Walras system was reanimated under Von Neumann's paradigm of game theory, bringing *mainstream economics à la Cowles* into line with the developments in 20th century science. The abandonment of the project to improve the empirical estimations

[29] L. Klein cf. Pinzón Fuchs

of Walras's system of equations by means of new statistical techniques thus accomplished the inversion of hierarchy between inductive and deductive approaches in economics.

Three essays

1.

Published in 1957, "Three Essays on the State of Economics Science" is a rather eclectic book. Systematic presentation of Cowles work during the war, methodological plea and proscriptive comment on the current and future of economic research, the book is the output of "an important opportunity to spent more than a year in reading and reflection about economics in the present phase of its developments" (Koopmans 1957, vii).

Far from the attacks of the Measurement Without Theory controversy, in the Three Essays Koopmans attempts to communicate, with whom he calls *the generalist economist*. Concretely, the book is a response to J.M. Clark's plea for communicability (in 1947 at the AEA meeting). For Clark "mathematical economists remain a growing and able sect, using an esoteric method and a special language, which make their results in- creasingly inaccessible to the rest of us" (Clark 1947, 75)

Each essays is independent from the others and they do not require a specific reading order. While, Koopmans claims, the three essays are concern with questions of "substance", of "method" and of "tools", respectively, they are all, with different emphasis, an opened defense of what Koopmans calls "the explicit formal model construction,[30] both in theory and in empirical research" (Koopmans 1957, viii–ix). Though present and interlace in each essay, particular….

1. Systematic version
2. Commutation method of the construction of knowledge
3. Incorporation into the tradition

2.

Koopmans starts the Fist Essay, "Allocation of Recourses and the Price System", with a parallel between what he is aiming and Paul Samuelson's work. If in "Foundation of Economic Analysis" (1947) Samuelson resembled a variety of problems arising in diverse parts of economic theory as matters of maximization under constraints, in the Fist Essay Koopmans aims to "pursue Samuelson's purpose *a step further*[31] into the realm of tools" (Koopmans 1957, 5). Indeed, introduced as "an attempt to communicate [with the general economist] the logical content, and some of the

[30] Different ideas of what model construction => Solow review of the volume.
[31] No emphasis on the original.

underlying reasoning, of certain recent developments in the mathematical economics" (Koopmans 1957, vii), the First Essay seeks to presents as "offshoots from the same mathematical stem" (viii) –namely the theory of linear spaces: 1) the model of competitive equilibrium (Wald; von Neumann; McKenzie 1954; K. Arrow and Debreu 1954); 2) theory of the use of prices for the efficient allocation of resources (Debreu 1954; K. Arrow 1951), the models of activity analysis (Dantzing 1951a; Dantzing 1951b; Koopmans 1951) and input-output (Leontief 1941).

Throughout the 125 pages of the Fist Essay, while justifying, "for the mathematical tools it make available" (Koopmans 1957, 15), the difficulties to find empirical meaning, Koopmans repeatedly attempts to help the *general economist* to navigate his presentation. The "reader who mistrust abstract formulation" (Koopmans 1957, 13) is serve with "somewhat artificial applications that bring out the nature of the contributions [of the theorems introduced] in isolation and impart a sense of its obviousness" (Koopmans 1957, 13). Clarifications are never too obvious when striving for great rigor and precision. For instance, after introducing the concept of close set, Koopmans adds: "Since closeness is not a *practical* [32] issue, the boundedness of production sets is the controlling consideration for the applicability of this theorem" (Koopmans 1957, 15–16). Likewise, directly addressing the *general economist*, Koopmans insisted on the "highly elementary and mathematically trivial character of the reasoning employed" (Koopmans 1957, 22) which, a few lines further he highlights, not "conceal their central importance to economic theory" (Koopmans 1957, 22). Koopmans reassured the not-mathematical trained reader: when the explanation concern the mathematical level and not the economic interpretations or applications, passages (typed in smaller type) can be passed "without losing the main threads of the reasoning" (Koopmans 1957, 55)[33].

Whether or not the Fist Essay accomplished its original objective of responding to Clark's appeal for communication and effectively transmitted to the *generalist economics* is not very clear.

Yet Koopmans did *bring together* substantives parts of the work developed in the context of the Cowles Commission -or by scholars associated with the research center- during the war. Through what Koopmans repeatedly calls *more fundamental mathematical tool*, the Fist Essay brings out the "basic unity" of most of the research advanced at Cowles under his direction. Indeed, for the first time, a systematic version of these *developments* was wrapped up in a single structure. The importance of this *accomplishment* lies on the edifice to be constructed within the structure and its incorporation into the "traditional economics".

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[32] No emphasis on the original.

[33] It is worth mentioning how passages typed in smaller type start. For example, "By giving sufficiently free rein to our imagination we can still visualized the condition (a) of Proposition" (Koopmans 1957, 111).

Moreover, the Fist Essay –as Koopmans explicitly motioned - illustrates a method for the construction and, significantly, the accumulation of knowledge in economics: *the postulational method*. Without reference neither to its history none to its uses in mathematics[34], the postulational method is explicitly introduced in the Second Essay, "The Construction of Economic Knowledge", a plea for the recognition of the potentialities of the tools discussed in the Fist Essay. First and foremost, the method is presented as a solution to the compromise (trade-off) between rigor and realism convoked when the limits of the introduction in economic reasoning of "mathematical concepts, theories, and theorems" comes to the fore:

> […] we look upon economic theory as a sequence of conceptional models that seek to express in 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. Each model is defined by a set of postulates, of which the implications are developed to the extent deemed worthwhile in relation to the aspects of reality expressed by the postulates. 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. The card file of successfully completed pieces of reasoning represented by these models can then be looked upon as the logical core of economics, as the depository of available economic theory (Koopmans 1957, 142–143).

Indeed, the postulational method reanimates, up-dates the *clivage* between theoretical and empirical work running through Cowles's history. Conspicuously, while urging for such a clear separation, the postulational method offers a procedure through which theoretical and empirical economics *move closer to each other*. For Koopmans, "The postulational structure of mathematical tool parallels that of the substantive theory to be constructed. The welcome result is that "mathematical" and "literary" economics are moving closer to each other. They meet on the ground of a common requirement for good hard thought from explicit basic problems " (Koopmans 1957, 176).

The postulates adopted set up a universe of logical discourse in which the only criterion of validity is that of the implication by the postulates. Outside of -and separate from- this process of deduction, two process, interpretation and application, give economic contend to the set of postulates used to represent the phenomena studied.

4.

The Third Essay, "The Interaction of Tools and Problems", examines four "recent and current tool developments" (viii): 1) the use of more fundamental mathematics; 2) the

[34] The postulational or axiomatic method is the process of formally deducing theorems from axioms in some system that includes deduction rules. For a history and explanation of the Postulational Method in Mathematics see (Huntington 1934). I am grateful with Quinn Culver for his help and … on the uses and sense of the postulational method in mathematics.

increase in the capabilities and computing and data-processing equipment; 3) the application of methods of statistical inference; 4) the application of simple survey methods of observation[35]. By setting up the distance between the "mathematical" and "literary" economist as explained primarily by the use of different tools, in the Third Essay Koopmans reduces to a matter of communication the tension between the two groups:

> "The present phase of economics is indeed one of turbulence and transition in the domain of tools, more than in the domain of problems and suggested solutions" (Koopmans 1957, 170–171).

The changes in tools brought together in the Fist Essay, Koopmans claims, engendered serious problems of communication within the economics profession. There are, he continues, "risks and frustrations in a situation where separate esoteric languages seem to spring up, among mathematical economists" (Koopmans 1957, 171). The frustrations and risks are to be of temporary character:

> When new activity springs up around a tool development, the need most urgently felt by those engaged in the activity is for communication among themselves. The choice of language is strongly influenced by the immediate needs of this communication, and shows little regard for informing "outsiders". The suspicion of outsiders is the aroused, and may also be fanned by evidence of overestimation of the potential contribution of the new tools on the part of their developers –evidence that somehow seeps through the barriers to communication. This is bound to lead to ultimately to a better appraisal of the usefulness of the tools in question. If specializations remains after this process has run its course, it is likely to be one that is accepted by the profession (Koopmans 1957, 171)

Indeed, the quest for continuity and incorporation –acceptance from the rest of the profession- evident from the Fist Essay is developed in the Third Essay. If for the elaboration of the synthesis Koopmans adopts "traditional economic terminology" rather than mathematical terminology to approach "more classical portions of economic theory" (59)[36], in the Third Essay the sense of continuity and connection between the "mathematical" and "literary" economist is reinforced. The distance from A.P. Lerner's *The Economy of Control* and the mathematical propositions of well fare economics reported in the fist essay, is, Koopmans claims, not large: "If there is a difference, it is one of succinctness of expression rather than of content, concepts, or objective" (Koopmans 1957, 176).

Yet from the "tools" discussed on the Third Essay just the "use of more fundamental mathematics" is part of the synthesis elaborated in the Fist Essay. Copiously, the

[35] It is worth to note how extensive is the label "tool".

[36] For example, the Fist Essay privileges the use of "*price system* rather than set of prices, although price vector would correspond more closely to the mathematical terminology" (Koopmans 1957, 45)

application of methods of statistical inference was not included. While discussing this point, Koopmans classifies approaches to "empirical economics" in three categories. In the first one progress depends on taking more observations, in the second from exercising grater control over the conditions under with the observations are obtained, and on the third, from making more use of plausible a priori knowledge. In just six and a half lines the work of Mitchell and Burns is associated to the fist "avenue of progress". Without any further comment –the Measurement Without Theory Controversy was far behind-, Koopmans passes to the approach which "emphasizes the combination of a priori knowledge with observation" (199), "the econometric approach". After presenting Lawrence Klein's work developed at Cowles and the results of Carl Christ tests of the models in question, Koopmans translates the funding battle to this camp.

The models of economy-wide coverage such as Klein's are opposed to "quantitative research devoted to individual markets " (notas 1 y 2 p. 200). More "successful and cumulative" (208) compared to models of economy-wide coverage, studies of specific types of economic behavior are, to the "the prevailing current of professional opinion", the best place to channeled research resources (209-210). His critical remarks of the models of economy-wide coverage are not intended as criticism of the work of Klein. Their main intent is "to argue that in future empirical work we should seek to use the power of the new tools to achieve an increased concentration on more highly disaggregative studies" (Koopmans 1957, 215).

Koopmans voluntary omits all explicit reference to his institutional affiliation. The "Threes Essays" is presented as "one *man's*[37] explanations of some recent developments in economic theory, his comments and perplexities about the character and basis of economic knowledge, and his intuitions about possible directions of future work in theory and in empirical investigation" (Koopmans 1957, vii). Yet the *developments* in mathematical economics reviewed took all place at Cowles Commission.

Conclusion

The journey of institutionalism from legitimate science to sideshow legerdemain is a consequence of the *boundary-work* by mainstream economist associated to the Cowles Commission, a debate that we trace trough three episodes. Cowles offered a description of scientific economics that effectively pushed research based on the primacy of empirical methods outside its boundaries.

Formerly inextricably mingled, during the second half of the 20th century the differed currents of mainstream economics drew and reinforced specific boundaries. By looking at these crucial years we can advance our understanding of both the

[37] No emphasis in the original.

specificity of each current and the *success* of the general project. This paper analyzed the *current* initiated by the Econometric Society and developed at the Cowles Commission.

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