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"A Critical Regard to the History of Econometrics"

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A Critical Regard to the History of Econometrics

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Part II

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Introduction

A History of Econometrics with specific Selection Criteria

Econometrics is one of economists' most popular tools. For decades, economists have amused themselves with it and have used it in every research field of economics: in macroeconomics, microeconomics, finance, developmental economics… or economic history. University classrooms have also constituted an ideal scenario for econometrics to feel at ease. Today almost every undergraduate and graduate programme in economics contains, at least, one course of econometrics. Some programmes even offer econometrics as a track of specialization.

But the scope of econometrics does not stop at the theoretical or academic level. In fact, econometrics has proved its most influential powers in economic policy. A great majority of governmental bureaus, international organizations, think tanks, consultant's offices, etc., base their policy recommendations (and actions) on econometrical studies of different sorts. No economist today would deny the fact of having used econometrics at least one time in her life, or having recommended an economic policy following the results yielded by some kind of econometric regression.

This popularity and this spreading of econometrics are not striking, though, considering that econometrics bombs economists' every-day-life. Many publications in some of the most renowned journals are accompanied by a strong econometric content. Reports authored by international organizations, as well as standard economic books, also contain a great amount of econometric work. In short, as an economist, you will find yourself surrounded by econometrics. Econometrics, thus, has simply become evident, obvious, almost *natural* everyday stuff. It is just an essential part of the economist's life. But, what makes econometrics so popular and so evident?

Econometrics' popularity is perhaps bounded to the seriousness and scientificity that it emanates. Or, at least, to the seriousness it appears to emanate. When introduced to any discipline, numbers and statistics seem to give a touch of scientificity and rigour. Their introduction in economics has not been the exception, and so, the use of mathematics and statistics increases economists' respectability towards policy makers and their fellow scientists. Many economists have, then, given up their literary work of ancient (and classic) economics, putting themselves a hard and serious task: to render economics scientific and rigorous by means of mathematics and statistics. That is, in any case, what the popular belief preaches. Nevertheless, the story is far more complicated than that. And in order to discover the complexities that underlie econometrics there is no other remedy but to study its history.

Where to begin? Would it be wise just to go and have a look at the main works in econometrics? This exercise would be certainly exciting and worthy, but it would take a lifetime study. There are so many possible beginnings, so many authors, so many definitions, so much known (and unknown) material, that the researcher would be forced to make a difficult choice and start at an arbitrary point. Another, perhaps less extensive, possibility would be to directly study the works devoted to the history of econometrics.

Even if it is not as popular as its own subject of study or even if it is not that old, the history of econometrics is quite rich and it grows continuously. But, of course, the authors that have been hitherto writing the history of econometrics have also been confronted, at some point, with the same question: where should the study of the history of econometrics begin? And they have also been obliged to make a choice in this matter. They have been forced to choose a beginning, and a plot, and an ending. This is the object of study of the present work: to make explicit the choice made by historians of econometrics, and to try to understand what where the conditions that made them go for that particular interpretation and not for another one.

For, as it happens quite often, the choice they have made is just one particular within a broader spectrum of possibilities. Certain specific selection criteria exerted a great influence on their decision, which are worth understanding and worth studying. Not because of the intellectual exercise by itself, but especially because this particular interpretation has an important influence over our whole image about econometrics and about economics.

As well as in economics, there is also a sort of "orthodoxy" in the history of econometrics. There is a way of looking at the history of econometrics that is commonly recognized as the "official" story. This vision is what Boumans and Dupont-Kieffer (2012) have called "the Received View of the History of Econometrics", or as I will refer further on, the "Received View".

This version of the story, which will be defined in greater detail throughout *Part II*, is more or less twenty-five years old, and was mainly written in England in the late 1980's and early

1990's. Their main contributors are Mary Morgan and Qin Duo, both Professors of economics, as well as historians, philosophers of economics, and econometricians themselves.

The task of this research will be twofold. First, it will delineate the story told by the Received View, characterising it and trying to comprehend it. Then, this research will focus on the study of the conditions influencing the writing of the Received View. The aim here is to understand what were the background factors that made their interpretation take one particular direction and not another one. In order to accomplish both tasks the present dissertation will rely on the combination of two methodological (historiographical) frameworks, thoroughly discussed in *Part I*. These frameworks, both historically and philosophically "well-informed", are: the body and image of knowledge framework and the styles of scientific reasoning framework.

In regard to the first task the criticism of the present work mostly lies on the internalist approach of the Received View. Their particular notion of progress constitutes important evidence that allows us to consider their narrative as internalist. This notion of progress – discussed at length in *Part II* – is based, among other things, on their differentiation between a creative and a mature stage in the development of econometrics, which coincides with the period before and after Haavelmo's 1944 paper[1]. This allusion to Haavelmo leads us to a second criticism.

The second criticism resides in the optimism expressed by the Received View in regard to the role played by Haavelmo and by the Cowles Commission. I argue that this optimism does not contribute to a deeper understanding of the emergence of econometrics. On the contrary, this optimism shows Haavelmo as the main character of a sort of "creational myth" – the creational myth of econometrics – which actually serves as a "self-authenticating technique", feeding the popular image of econometrics as a neutral, objective and timeless tool[2].

In relation to the second task – the study of the conditions that rendered possible the emergence of the Received View – this research will focus on two different kinds of aspects.

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[1] When refering to Haavelmo's 1944 paper, this research refers to his well-known "The Probability Approach in Econometrics" published in *Econometrica* in 1944.

[2] I am referring here to concepts that will be treated in detail in *Part I* of the dissertation. The notion of a creational myth and that of self-authenticating technique, as well as that of objectivity are related to Ian Hacking's framework of styles of scientific reasoning. The popular image about econometrics alludes to Leo Corry's image and body of knowledge framework.

First, it focuses on the conceptual, theoretical and methodological complementarities common to the authors of the Received View. Second, it takes into account a series of cultural, institutional and social complementarities affecting these authors. These factors are not separated, though. On the contrary, they are considered as being part of the same thing, both interacting with and influencing the historians' minds and practices at the same time.

It is worth noticing that the authors of the Received View found themselves in a particular situation around 1990. On the one hand, there was an increase in methodological work in economics of a rather eclectic, pluralistic but also unclear nature (see Hands, 1990, 2001a, 2001b). On the other hand, the history of econometrics was strongly influenced by econometricians themselves, for historians of econometrics not only worked side by side with them, but also because the writers themselves were econometricians. This influence is not to be understood as being negative or counter-productive. On the contrary, it is interesting to analyse the gains that historians of econometrics could have pull out from this complementary teamwork.

Historians did surely gain a lot in terms of their understanding of technical matters, because of this close collaboration. Nevertheless, this teamwork, which was embedded in a particular institutional, social and relational context, certainly marked the way in which the history of econometrics was written and the interpretation that historians gave to some important events in the evolution of econometrics. This collaboration with econometricians pulled historians to get interested mainly on internal factors. That is, historians of econometrics focused on technical and theoretical aspects of the evolution of the econometric tools and methods.

This particular situation in which historians found themselves raises several questions. Some questions have to do with the context in which the Received View was conceived, whereas others are concerned with the history or the evolution of econometrics itself: (1) What are the social and institutional factors that influenced the writing of the Received View back in 1990? (2) What was the theoretical and methodological existing knowledge of econometrics by that time? (3) What effects do have the precedent questions on the interpretation of particular events of the early history of econometrics, such as Haavelmo's *Probability Approach*? (4) And, finally, can we provide a new interpretation of the history of econometrics today, even if we know that there is a prevailing body as well as a prevailing image of knowledge of our own epoch that biases our own interpretation?

These questions have to be analysed at two levels. The first level will be the object of the present work. It analyses the Received View as such, which means that it studies the writing of the history of econometrics, and is affiliated in a historiographical perspective. The second level analyses the history of econometrics anew, and it will be the subject of a further stage of this work.

Both levels, however, take necessarily into account, not only the theoretical factors already studied by the Received View (Morgan, 1990 and Qin, 1993) or the social factors studied, for example, by Mirowski (1989a and 2002), but also the study of the interaction between these two kinds of factors. And again, this is the aspect on which this work focuses: the interaction between the external and internal factors, or as we will define it later in a more accurate way, the interaction between the image and the body of knowledge.

In short, I am interested in the study of the history of econometrics, because I want to understand what makes econometrics, at the same time, so popular and so obvious for economists. I will argue that the specific interpretation of the history of econometrics given by the Received View has contributed to the perpetuation of the image of econometrics as an objective, neutral and timeless tool. Many specific factors worth studying have shaped this interpretation. They have established specific selection and interpretation criteria and have yielded a fascinating, but somehow, biased interpretation of the evolution of econometrics.

These factors are multifaceted: they are theoretical, methodological, philosophical, cultural, social, and even political in their nature. And it is this multifaceted nature, which makes them a rich, but difficult object of study, worth of a special and specific methodology. This methodology, which is a joint version of the body/image framework and the styles of scientific reasoning, seems to be accurate, because it takes into account the interaction between all these factors, while it considers science as a domain completely embedded in culture.

Before passing to the presentation and explanation of this specific methodological framework in *Part I*, let me first make a brief sketch of my own process of reflection about this subject.

Fine-tuning Hypotheses

At the beginning of this investigation I was concerned with the following question: to what extent did a positivist view of the world influence the writing of the history of econometrics in the late 1980's and early 1990's? This was a quite plausible question to ask, since the history of econometrics to which I am referring to (hereafter, the Received View) has been mainly written at the London School of Economics (LSE). As is commonly known, Sir Karl Popper has occupied an important position at the LSE from the late 1940's until the late 1960's. Then, Imre Lakatos took up Popper's chair until the tragic accident that took his life in 1974. The presence of the two paramount philosophers of science could, presumably, have had an effect over an important part of the academic community in London and, particularly, at the LSE.

This influence, however, turned out to be quite difficult to reveal. Even if it is true that the "Received View" of the history of econometrics presents some elements that can be associated to a positivist approach – like a certain notion of progress (see Morgan 1990 and Qin 1993) or the formation of a certain "hard core" and a "protective belt"(see Qin 1993) – it is also true that this history does not completely fit to the model proposed by Popper or by Lakatos. Furthermore, the Received View turned out to be a far more interesting and complicated approach, than it first appeared (a further and more detailed characterization of this view will be provided in the next sections). Whence I realized that I might have been asking the wrong questions.

As McCloskey (1988) argues, asking whether a given discourse or research programme actually fits to a Lakatosian or Popperian model can result in a rather "thin" analysis. For there are many other aspects to be discovered than the mere exercise of comparing a rigid rationally constructed theoretical structure with the continuously moving, non-linear, unclear, accidental, conversational, etc., social activity of doing science. On the contrary, if one could get rid of such a rigid methodology of mere comparison, a "thicker" analysis could be attainted, which could unravel many other captivating facts, from which new questions would arise. Some of these novel questions could be suggested, for example, by focusing on the way historians of the Received View actually undertook their conversations and tried to persuade their readers and colleagues of their particular view. These questions could also take the form of the study of the historical and institutional conditions under which the Received View was written.

But I was still thinking about Popper's and Lakatos's influence over the writing of the Received View. Further reading of De Marchi's (1988a) edition of the conference held in 1985 at the occasion of Joop Klant's retirement was revealing too. My optimism about the influence that such a positivistic approach could have exerted over the Received View was quickly lessened by just going over the preliminary conclusion of the conference presented at the very introduction of the book:

> "Although a minority of the symposiasts might disagree, it seems fair to conclude that for most, and for a variety of good reasons, there is no very substantial Popperian legacy in economics" (De Marchi, 1988a, p. 12).

This work, of course, did not include an analysis of the Received View of econometrics, which at that point was just in its gestation form. Still, the idea that followed was that if there were no substantial Popperian legacy in economics, the history of econometrics would be also detached from that view. This assertion, certainly, exerted a discouraging effect on the first ideas that I had been spinning in my mind.

Another illuminating reading was De Marchi's (1988b) own paper presented at that conference, concerning *Popper and the LSE Economists*. In this paper De Marchi studies Popper's influence over the economists working at the LSE. De Marchi asserts that Popper did in fact exert an influence over a very selected group of economists at the LSE, but that this influence was of a rather short duration. This event actually dates back to a brief period from 1957 to 1964, which precedes the writing of the Received View by over twenty-five years. De Marchi's analysis focuses on the attack over Robbins' *a priorism*, directed by a "group of unusually talented young members of the economics staff at the (…) LSE, led by Richard Lipsey and Chris Archibald" (De Marchi, 1988b, p. 141).

Their main purpose, so De Marchi says, was to make of economics a quantitative science, which could be expressed in falsifiable terms. It was to render economics more rigorous and to take out of it the abstract *a priori* method inherited from Robbins and the Austrian School. They wanted to recast the old idea "in which they had been schooled, that quantification is not only difficult but unnecessary" (ibid.). So they created a seminar parallel to that of Robbins (who was the leading figure at the LSE's economics department), which name suggested their critical posture: "LSE Staff Seminar in Methodology, Measurement and Testing" or "M^2T".

Lipsey and Archibald were worried about the question of "how economics advanced" (ibid. p. 143). And, in this regard, they believed that economics should rely on empirical work rather than on the *a priori* investigations promoted by Robbins and many others, in order to unravel its own evolution. Hence, two elements made of the Popperian model a very seductive one for this group: (1) the critical nature of Popper's approach; and (2) its emphasis on the harsh and rigorous testing of theories, which provides a criterion to refute these theories. The main objective of the "M^2T" seminar was, indeed, to criticise the *a priori* method, but also to found a new economic theory that could be subject to harsh testing and that would reject refuted theories.

Nevertheless, and despite the great enthusiasm with which they embarrassed Popper at first, this coalition between the "M^2T" group and Popper did not last long. At least, it did not last in the strict way in which the economists had intended to follow the philosopher at the beginning. Lipsey and Archibald realized that there were major difficulties in reconciling Popper and economics. Not only was it difficult to hold Popper as an accurate guide to practical testing of economic theory, but the criterion of refutability also turned out to be insufficient to completely throw away the *a priori* theory they were attacking. Their refutability arguments based on Popperian criteria were just not strong enough to fully reject *a priori* theories.

Consequently, Popper did not leave important traces of his influence at the LSE. Or, at least, these traces are not directly tractable or easily recognizable. Despite that first feeling of discouragement –bordering on frustration – further reading and analysis of the Received View revealed, as I already mentioned, a far more interesting and complicated dimension of analysis. A reassuring and stimulating feeling of challenge then followed, because now the Received View appeared to be a much greater source of knowledge, stories and information than I had first imagined.

After this first attempt of approaching the Received View I abandoned the idea of trying to establish whether the approach of the Received View fitted into a Popperian or Lakatosian framework. I identified that there were some other interesting things happening in the late 1980's and early 1990's in economic methodology, to which I should definitely direct my attention. Why was there suddenly so much interest in the history of econometrics? Why were econometricians themselves trying to understand the history of their own discipline and how is it that people that were not "purely" econometricians (like Morgan) got to work on the subject? How was the relationship between the non-econometricians and the econometricians that still appeared to have exerted an important influence in the way the history of econometrics was written?

These questions generated a necessity of asking about several contextual, methodological, social and theoretical aspects. On the one hand, trying to determine what was the view that historians of econometrics, economic methodologists and econometricians themselves, had about econometrics and its history around 1990 appeared to be important. On the other hand, the relationships between the researchers themselves, and their relations towards the main institutions, also seemed important and had to be examined. The main goal of this examination would be to understand how the history of econometrics had actually been written. Furthermore, I could not forget about econometrics itself and the evolution of its theoretical nucleus and its methodological practice. So, the evolution of the econometric tools and models could not be forgotten either.

There are, certainly, many ways of bringing this analysis into light. One that turns up to be quite accurate seems to be that proposed first by Yehuda Elkana (1981), which analyses science as a cultural system, later popularised by Leo Corry (1989) when analysing the history of mathematics, and more recently used for the first time in economic history and methodology by Roy Weintraub (2002). Their contribution is better known as the body/image of knowledge framework and its discussion constitutes chapters one and two of the present work.

Chapter three studies Hacking's methodological framework of styles of reasoning, which complements Corry's framework, adding extra notions that allow the undertaking of a historiographical study. That is how *Part I* ends: with the presentation of two methodological frameworks, suitable to make a historiographical and a historical analysis. The main argument of this section is that these frameworks allow the researcher to focus on the practices of the historians of econometrics – for historiographical purposes – but also on the practices of econometricians themselves – for historical purposes in a further stage of this work. This will, in turn, allow the researcher to concentrate, not only on the theoretical and methodological aspects of the history of econometrics, but also on its cultural and social aspects, as well as on the interaction between both kinds of aspects.

Part II focuses on the study of the Received View. Chapter four presents a general characterisation of the main writers, Mary Morgan and Qin Duo, while chapter five gives an introduction of their vision about the history of econometrics, what I call the "funnel vision". Chapter six discusses whether econometrics can be considered a style of reasoning and what are the implications of doing so. Last, but not least, this dissertation finishes with a discussion about some of the concepts retained by the Received View, which would characterise the evolution of econometrics. Some of these concepts are: the notion of progress in the evolution of econometrics, the differentiation between a creative and a mature stage in the development of econometrics, and the optimism about Haavelmo's (1944) contribution and of the role played by the Cowles Commission.

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Part I

Part I

A Methodological Framework

Since the blooming of the "Naturalistic Turn" (Hands, 2001b, Weintraub, 2002), academic work in Economic Methodology has focused on the *practices* of the economists. These works have changed the questions that methodologists used to ask; rather than asking questions more closely related to classical epistemology of the sort – "*how should economics be done?*" – economic methodologists changed their focus and now ask questions of another nature: *how are economics actually done?*

This broader approach of formulating methodological problems has enriched the analysis, for it includes a greater variety of factors in it. Economic methodology and history are not anymore about telling the evolution of the discipline as a "success story" or as following a "linear progress". Economic methodologists and historians have become "philosophically well-informed academicals" (Hacking, 2002), who take into account, not only the history and methodology of a given theory, but also the social and political factors that could have affected the researchers at a given period. To a certain extent, our work is closely related to that of the Received View of the History of Econometrics in this aspect. For, as the reader will see along the present study, both the Received View and our own view, will give prominence to the practices of the researchers and theorists.

The Naturalistic Turn not only affected historians of science, however. Even philosophers of science, today, generally attribute much more importance to the social dynamics that characterize the process of doing science than they did a few decades ago.

Nowadays, some philosophers and many economic methodologists describe science as a *communal activity* or as a *conversation* between the members of a scientific community. Hence, researchers emphasize on the social and collective character of science. Hacking (2002), for instance, labels his "style for historians and philosophers" as "styles of *reasoning*" rather than as "styles of *thinking*". "Reasoning", he says, "is done in public as well as in private: by thinking, yes, but also by talking and arguing and showing" (Hacking, 2002, p. 180). McCloskey (1988) as well, stresses the importance of the social aspect of her "scientific conversations". "The scientific conversation (…) is a matter of listening, really listening, to what our fellows say; then answering, really answering" (McCloskey, 1988, p. 256).

Hence, one can say that two aspects are common to the majority of today's methodological works: (1) the emphasis on the social and communal dimension of science and (2) the importance attributed to the study of the scientists' practices. It is worth noticing, even if we anticipate an argument that will follow in the next section, that these two aspects might be understood as being central to our own Image of Knowledge today[3]. That is to say, that today's methodological analysis is in some way biased by this particular image of science that we have right now, even if we might not be completely conscious of it.

There is nothing wrong about a particular image generating a bias, though. In fact, every scientific community at a given time partakes of a given image of knowledge. We cannot be the exception and we do not escape our own epoch's image of knowledge. It is just a matter of recognizing the sources of our own biases and understanding that both, today's body and image of knowledge, also affect our own approach[4]. Furthermore, it is important to recognize that the existing styles of reasoning make it possible for us to think about econometrics and its history in a particular way (we will come back to this point later, when we talk about Hacking's framework).

But, let us come back to the Received View and to their understanding of the social dimension of science as well as to the importance they attribute to the economists' practices. I will argue here that even if the authors of the Received View take into account the practices of the researchers in their analysis, they only do so to a limited extent. Their limitations reside on their main focus, which is of a rather interalist nature.

So, there seems to be a paradox here. Normally, if you take into account the practices of the researchers, then, in bringing about your study, you are also taking into account a great

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[3] We appeal here to the principle of *reflexivity* advanced by the "Strong Programme" of the School of Edinburgh, particularly in Bloor (1979, p. 7).

[4] It would not be too risky to say that today's Economics, today's Economy, and today's scientific thinking and imaging are very different from that of the 1980's. A lot of things have happened in the body and image of knowledge of economics, which have definitely changed our view about it. The further development of Neuroeconomics and Behavioural Economics are just two examples of these changes. The market liberalization and the implementation of Neoliberal Policies during the 1990's as well as the present economic crisis have certainly changed our vision of the Economy. In terms of works in economic methodology and economic philosophy, the explosions to which Hands (2001a, 2001b) refers have assuredly shaped the vision we have about this field of research. All these events and many others have, indeed, contributed to the creation of our own vision about the world. Please note that they are not only related to changes in terms of economics, but also in terms of the economy. Furthermore a deeper reflexion about the factors affecting our image of knowledge could lead to a list of a greater number of cultural, social and political events.

number of cultural, social, political and historical factors. This is so since scientific practices are dictated by culture, by society, and by the given historical context at a particular time. Hence, scientific practices are not neutral; they develop throughout time; they change depending on the modifications of scientists' priorities, which are in turn determined by factors inside and outside the scientific field. How is it possible to say, then, that the authors of the Received View present a rather internalist approach, even when I recognize that they give some importance to the practices of the econometricians themselves?

It is possible to say this, because they do not consider science (and econometrics) as a cultural activity. Even if the authors of the Received view concentrate on the practices of econometricians, they only do so at the theoretical and methodological sphere. They forget about external factors such as the historical and social contexts in which econometricians found themselves when they were working on the development of the econometric tools. And once they have forgotten these factors, they will forcedly disregard one of the aspects central to our analysis: The *interaction* between the internal and external factors[5].

When I say that the authors of the Received View forgot about the external factors and did only take into account the internal factors, I do not intend to fall into the classic discussion between the internalist/externalist approaches (see for example Braunstein (2008), pp. 88-92). Even if I consider that both, the internal and the external factors are relevant and necessary to explain the evolution of a discipline, I make this separation only because it helps us to identify the factors at stake. I make it also because it is, to some extent, the separation that the authors of the Received View did back in the 1980's. That methodological separation might have been part of their image of knowledge and so it might have been part of their vision of the world.

But this separation must be only temporary to our purposes. For the real analysis must be undertaken at the sphere of the interaction between the internal and external factors that we will identify later as the interaction between the image and the body of knowledge. So, in what follows let me first begin by presenting an argument of why I have chosen both Corry's and Hacking's frameworks. Then, let me introduce Leo Corry's framework and discuss its most essential elements. These elements are the body of knowledge, the image of knowledge,

[5] I only make this distinction between internal and external factors as a way of introducing the problem. As the reader will see further on, these notions will be rapidly replaced by a more accurate notion, namely by that of image and body of knowledge.

the interaction between them, and the concept of reflexivity. And finally, let me make the same with Ian Hacking's styles of reasoning, in order to explain his concepts of objectivity, autonomy, positivity, stability, self-authenticating techniques, and philosophical technology.

1. Body/Image Framework and the Styles of Reasoning

Leo Corry's framework of the image/body of knowledge is of great help in order to deepen the analysis of the interaction between the internal and external aspects that affect the evolution of econometrics as well as the evolution of the history of econometrics.[6] This is so, not only because it allows us to focus on the communal nature of scientific reasoning and in the practices of the scientists themselves, but also because, as we will further explain, it allows us to analyse the social and historical dimensions of econometrics' evolution, understanding the ever-lasting interaction between these factors and the theoretical advances inside econometrics.

Nevertheless, there are two issues, in Corry's framework, to which we would like to direct our attention. On the one hand, Corry's framework says nothing about the conditions that render possible the existence of a given image and a given body of knowledge. It just takes for granted that they exist, that there are some factors affecting them, and that their interaction shapes them in a particular way. But, it does not explain the process images and bodies undergo in order to be created.

On the other hand, the second issue has to do with the "local" nature of this framework. A certain image of knowledge, for example, can be particular of a little community of scientists or, even more, it can be particular of just one scientist. There is no way of understanding how the image goes beyond the local community.

The latter is of course quite interesting to our analysis if we remember that we are taking into account the people involved in the writing of econometrics. But this local focus is also somehow limiting, for there are aspects that would remain unexplained if we only kept this methodological framework. That local nature by itself, would not allow us to understand how

[6] In fact, Corry attributes the first development of the framework image/body of knowledge to Yehuda Elkana (1981), and explains that "[t]hese concepts arose in the framework of an ambition program aimed at an anthropologic characterization of scientific knowledge as a cultural system" (Corry, 1989, p. 412).

it can be possible for a rather local image of knowledge to expand and to become dominant at a larger scale. And this would constitute a serious handicap for us and for the history of econometrics in general, if we take into account that, at the beginning, a lot of the work done at the Cowles Commission was of a rather local nature.

Hence, Corry's framework is just fine to explain the events to come once a scientific community acquires one or many Images of Knowledge, or when a particular image and body is limited to a little group of scientists. Nevertheless, this framework says nothing about the global circumstances that made it possible to get a particular way of looking at the world. This kind of questions could be answered, however, by taking into account Ian Hacking's "Styles of Reasoning", which are, to a great extent, compatible and complementary with Corry's framework, while they explain some other things of a rather global sphere.

In the sections to follow, I would like to explain Corry's framework in more detail, in order to discuss some of its most important notions: *reflexivity*, *body* and *image* of knowledge and the boundaries between them. Then, I present Hacking's "Styles of Reasoning", with all its important notions, such as that of *objectivity*, *autonomy*, *new positivity*, *self-authentication* and *Philosophical Technology*. All of these concepts will appear along this section (and along the whole dissertation) complementing each other.

2. Choosing Corry's Framework

The history of econometrics, written at a given epoch, is of course subject to that epoch's image of knowledge. This is not only the case of the Received View of the history of econometrics, but it will also be the case of our own interpretation – as we have said before. Some biases representing the current image of knowledge will be at the core of our interpretation. For this reason, this work does not have the pretention of being of a normative nature. For instance, the emphasis of today's methodological works on the practices and the social dimension of science might be the milestone of our own image of knowledge.

Nevertheless, and despite our consciousness about the biases dictated by our epoch's methodological approaches, a methodological choice has to be made. Even if we are sure that this choice represents a bias of some kind, a particular structure will be necessary to undertake our investigation[7]. In this particular case we will mainly lean upon the framework proposed by Leo Corry (1989) and, we will also rely on the framework proposed by Ian Hacking (2002).

As I have already mentioned, our analysis will be applied at two levels. So will be Corry's and Hacking's framework too. On the one hand, the frameworks will be employed at the level of The History of the History of Econometrics (at a Historiographical level), and on the other hand, at the level of Econometrics itself. Applying Corry's body/image framework can make our subject matter more amenable to certain aspects of the analysis. The idea here is to take into account those aspects that were not sufficiently discussed by the Received View or those that were treated having in mind a particular interpretation. In order to do so, however, it is important to discuss first what Corry proposed, and then, to examine whether his framework is of any help to understand the Received View and the evolution of econometrics itself.

2.1 Body and Image of Knowledge

Neither Corry's framework nor Hacking's do embrace a precise definition. As Hacking himself states, "(…) concepts are never exactly defined. Instead their authors present plentiful examples together with some characteristics and differentia" (Hacking, 1992, p. 138). So, in order to draw a general picture of their frameworks, both authors give some illustrations, concrete examples, commentaries and applications of them. That is why we are not able to

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[7] Roy Weintraub (2002) also recognizes the absolute necessity of utilizing a certain framework in order to bring about a sound methodological appraisal of a particular science or discipline, even if he defends pluralism in Economic Methodology, as we also do. The choice of that specific framework does not imply its exclusive use, nor does it imply that it is a "normative" principle. This choice might be understood as just *one* possible choice under a wide spectrum of constantly evolving alternatives. In relation to this aspect I feel quite close to Weintraub. Nevertheless, I would like to clarify that I do not feel very close to him when he says that:

> "I am not sympathetic to using history in order to criticize the discipline of economics. It is not that I have no beliefs about the strengths or weaknesses of particular lines of economic analysis. It is rather that, as a historian, both my interests and my tasks are different from that of an economist who wishes to argue with other economists about current economic analysis and policy." (Weintraub, 2002, p. 7)

Taking into account that Weintraub used Corry's framework in his 2002 book, I want to stress that his claim – at least in the way that I see it – is rather distant to the spirit of present-day methodological work. What I would like to underline here is that Corry's framework actually provides a useful way of looking at a discipline, that could enable us to *talk, argue and show* (Hacking, 2002), and *listen and answer* (McCloskey, 1988) to our fellow economists, econometricians and methodologists. In short, it enables us to undertake "good conversations". So, why shouldn't we undertake these conversations?

give a formal definition of the frameworks at hand.

The absence of a fixed definition does not have to be regarded as problematic, however. Rather, it can represent a big advantage for our analysis, because we do not find ourselves bounded to a rigid framework. The framework remains only a reference and, hence, it remains quite flexible. We can mould it depending on our own needs. In short, it rests "under construction".

Leo Corry (1989) makes the following precisions when he describes his framework:

> "We may distinguish (...) two sorts of questions concerning every scientific discipline. The first sort are questions [*sic*] about the subject matter of the discipline. The second are questions about the discipline *qua* discipline, or second-order questions. It is the aim of a discipline to answer the questions of the first sort, but usually not to answer questions of the second sort. These second-order questions concern the methodology, philosophy, history, or sociology of the discipline and are usually addressed by ancillary disciplines." (Corry, 1989, p. 411)

Furthermore, Corry specifies what kind of elements are incorporated in the body of knowledge and he gives some examples about the kind of questions that define the image of knowledge of a particular discipline at a given epoch:

> "The body of knowledge includes theories, 'facts', methods and open problems. The images of knowledge serve as guiding principles, or selectors; they pose and resolve questions that arise from the body of knowledge, but are not part of and cannot be settled within the body of knowledge itself. For example, the images of knowledge help to resolve such questions as the following: Which of the open problems of the discipline most urgently demands attention? How should we decide between competing theories? What is to be considered a relevant experiment? What procedures, individuals, or institutions have authority to adjudicate disagreements within the discipline? What is to be taken as the legitimate methodology?" (Corry, 1989, pp. 411-412).

Moreover, images of knowledge can coexist within a time period. This means that different researchers at a given epoch can have a variety of images of knowledge. Econometricians at the Cowles Commission, for example, could share a quite similar image of econometrics during the 1940's, while NBER economists during the same years, would think about econometrics in a completely different way. This will be decisive in the further development of the discipline, for these two (or several) different images will shape the body of knowledge in a dissimilar way, depending on which one becomes dominant.

It is in this sense that I say that the image/body framework rests on a quite local sphere. Hence the analysis and the scrutiny of a determined image must be very careful and detailed[8]. Images of knowledge cannot coexist, however, within the mind of the same researcher, and they tend to be quite similar between the members of a scientific research group. The images retained by a scientist or by a scientific group can, though, evolve and change with time.

The constant evolution of the images happens and emerges, then, at the local sphere. Local social interactions and the changing of scientific practices cause this evolution. That is why it is so interesting for us to focus on the local and evolving character of scientific practices: because they are the source of new imageries and images. This local character of the images can, theoretically, give as a result, an infinity of images of knowledge within a scientific community. Or, at least, it can certainly provide us with a vast number of images of knowledge, which is, no doubt, a rich source of heterogeneity and diversity.

The imposition of a particular image of knowledge over the others, however, remains unexplained until now. How can it be possible that one particular image, or at best, a few particular images of knowledge finally impose themselves over the others? It also remains important to ask whether a complete imposition and dominion of a determined image is possible at all.

We will see that the imposition and dominion of a particular image only takes partially place. This imposition can only be partial, since the birth of new images and the coexistence of several existing images counts as the main agitator provoking methodological and theoretical debates and is constantly happening. They are the engines of scientific evolution[9].

[8] On how to undertake a very careful and detailed scrutiny see the concept of Microhistory developed by C. Ginzburg (1993).

[9] I would like to stress here that I write "evolution" and not "progress". In doing so, I would like to escape from a certain view of "improvement" or "advancement". Evolution in this sense seems to be

In order to understand the partial imposition of one image of knowledge over the others one has to ask what are the aspects and factors that actually affect the images. As we have seen before (in page 22) the images of knowledge are the "guiding principles" determining which questions are relevant to the discipline, which institutions or individuals have the authority in a certain matter, and what is the "right" way to pose the questions. As it happens, this process of selecting the "good" questions and methods and individuals is quite complicated and has a lot to do with philosophical, historical, sociological and cultural principles. So that all these aspects should, in a way or another, be taken into account.

Some of the aspects on which one could direct his attention are the political and financial aspects that affect the scientific field. At a certain point, for instance, some ways of looking at a problem will be politically more "correct" and they will find more financial support. However, this financial support does not have to have any necessary connection with any kind of "tainted knowledge" (Mirowski, 2002, p. 158). Knowledge produced within the context of a given fund does not have to be directly "polluted" with the interests of the funders. The effects of funding are far more complex to understand. For this funding create the particular conditions that will shape the way science is made. As Mirowski puts it, he wants to:

> "highlight the specific manner in which shifts in science funding and organization fostered an intellectual sea change in the way in which issues of communication, command, control and information – the military C^3I – came to dominate the continuum of scientific thought in the postwar period" (Mirowski, 2002, p. 158).

This is going to be the case of the Econometrics Programme advanced by the Cowles Commission during the 1940's, for example, where political and military issues and objectives legitimated the research activities at the Cowles. This legitimation was not only reflected in an ideological sphere, but it was also translated in the massive remittances transferred to that organization[10]. This fact would have provided economists at the Cowles with a particular image of knowledge about econometrics, economics and science in general.

more accurate to describe a gradual change of the characteristics of a given discipline, which does not imply any "improvement" idea or the like.

[10] "Like other sciences blessed by the postwar largesse, the Cowles budget ballooned to $153.000 a year by 1951, with RAND covering 32 percent of the total, and the Office of Naval Research another 24 percent" (Mirowski, 2002, p. 220). It is worth noting that only five years before, in 1944, the total income of the Cowles was of $ 21.234 (see Mirowski, 2002, p. 217).

While some images are imposed to particular scientific communities little by little by the bias of funding and political interests, other images can survive elsewhere. They can survive in the minds of other scientists, attached to other scientific communities, subject to other political influences. Some of them can be more or less important; more or less independent in regard to the "intellectual sea change" created by the dominant funding and system of organization; and more or less dependent of other sources of funding as well as from other systems of organization.

But the political and financial "correctness" are not the only factors to be taken into account. For, they do not decide by themselves whether an image will be dominant or not. They are a necessary, but not a sufficient condition, so to say. Additionally to the political and financial aspects, compatibility aspects between the existing body and the partially dominant image of knowledge are relevant too. The criteria advanced by the image of knowledge have to be coherent with the existing theories, facts, relevant problems and techniques.

Morgan (1990, pp. 34-39) provides a good example of incompatibility between the image and the body of knowledge. (Though she does not use Corry's framework and so, she does not think about the body/image terms, as we do). Morgan gives an explanation of why the periodic cycle programme initiated by Jevons and Moore did not prove to be very influential on further cycle analysis, even if nowadays, these authors are considered as pioneers of econometrics. The incompatibility between body and image of knowledge consisted mostly on three points.

> "First, their assumption of a periodic cycle generated from outside the economy was unattractive to most economists and the frequency methods which accompanied their assumption were found to be ill-suited for econometric work. In the second place there was a burgeoning of statistical work on business cycle in the 1920's which involved alternative ideas and methods and which were more easily accessible to business cycle students of the period. Thirdly a preference for description over explanation was a common trait of the development of statistical work on business cycles in the 1920's and 1930's regardless of the actual tools used. In the face of this strong alternative empirical statistical programme, the econometric approach advanced by Jevons and Moore, which tried to build theories of the cycle out of the statistical regularities and relationships in the data, lay dormant until revitalised by Tinbergen in the late 1930's" (Morgan, 1990, p.

39).

So, even if the body of knowledge was already there – the economic theories were there as were the statistical tools and methods – econometrics (of a more "modern" sort) did just not have a place in the mind of the majority of economists. Economists were more interested in describing the economy, and not in developing a theory that would entail external factors of explanation, such as the sunspot and the movements of the planet Venus. Even if there was the possibility of developing further work on the body of knowledge built by Jevons and Moore's propositions and methods, the image of knowledge of that epoch was not compatible with it. So, something must have happened after Tinbergen that would have rendered possible the use of statistical and econometric tools in a way closer to that of Jevons and Moore. The analysis of what rendered this possible will, however, be part of a further stage of our work. So, let us continue, for now, with the exposition of the methodological framework.

Corry's framework provides an accurate explanation of the interaction between "scientific" and "non-scientific" factors once a body and an image of knowledge are established. Yet, it still does not provide a satisfactory explanation of the formation of a particular image of knowledge. The explanation of this process of formation remains shaky even if Corry gives some clues relative to this issue. The clues are related to the interaction between the body and the image of knowledge that we will analyse further on. But the clues he gives remain more closely related to the evolution of already existing images of knowledge than to the formation or the birth of a new image. It is at this point that the Styles of Reasoning proposed by Ian Hacking can be of great help as a complement to Corry's framework. But let me finish the presentation of Corry's framework before passing to that of Hacking's.

2.2 Interaction between the Image and the Body

An aspect of the framework provided by Corry that we consider of paramount importance is the interaction between the Image and the Body of Knowledge. This interaction implies that both, body and image of knowledge exert an important influence over each other.

On the one hand, "the contents of the body of knowledge at a given stage of development of the discipline" (Corry, 1989, p. 412) will determine the criteria that steer the guiding principles, or the images. For example, statistical tests did account as a valid criterion to prove econometric models only after the development of structural econometrics. For structural econometrics did provide a particular way of expressing theories – or rather models – that would allow the formulation of this kind of tests. The existence of a determined body made it possible to think about particular criteria to appraising theories.

On the other hand, the image also exerts an important influence over the body of knowledge. Another kind of problem is at stake here. For, in this case, the possibility of thinking about an object in a certain manner already exists. The existing body of knowledge has made it possible to think about this particular object. The question, then, is whether the image of knowledge legitimates or not a theory, a method, etc., given some social, political and historical imaginaries and factors. Hence, this interaction enables us to understand the influence exerted by theoretical, technical, social, political and historical factors over the evolution of a discipline.

Furthermore, Corry suggests that a careless study of the influence of socio-historical factors could lead to overstated conclusions and relativism (Corry, 1989, pp. 411-412). This can be the case, for example, when factors of a social, political or historical nature are taken as strong and absolute explanations of the content of theories, "facts" or methods.

The advantage of the body/image interaction is that it forces us to take into account what we called earlier "internal" as well as "external" factors at the same time. And it is fine to do so, not only because it let us escape from relativism as well as from internalism, but also because the study of these interactions can lead us to discover new ways of understanding the history of a discipline. Just as the body of knowledge or a certain style of reasoning allows scientists to think about the world in a particular way, only possible once the style exists, similarly, I hope, this framework allows us to think about the history of econometrics and econometrics itself in a different way.

2.3 Reflexivity

It is important to recall here, that Corry applies the Body/Image framework to the history of mathematics. For him mathematics is a science of a peculiar sort. This peculiarity lies in the "singular reflexive character of mathematics", which consists on "the capacity of mathematics to study itself mathematically" (Corry, 1989, p. 413). Hence, mathematics is the only exact science whose subject matter is mathematics itself.

The kind of reflexivity of which Corry speaks about in mathematics cannot exist in econometrics. There is no way of studying econometrics "econometrically", and the subject matter of econometrics is not econometrics itself.

But another kind of reflexivity could be also present in econometrics, though. Boumans and Dupont-Kieffer (2011) also suggest that a certain form of reflexivity could be present in the history of econometrics. This is because the history of econometrics has been written, to an important extent, by econometricians themselves. When it is not the case that econometricians themselves were actually writing the history of econometrics, they were working very closely with the historians dedicated to this task. We will see this in further detail in *Part II* of the present work. Nevertheless, even if we can consider that there is some kind of reflexivity in the history of econometrics it is important to notice that this reflexivity is of another nature than the one present in mathematics described by Corry.

To be clear, the reflexivity criterion in econometrics would consist on the fact that historians of econometrics do evaluate econometrics and its history, from an econometrician point of view. Of course we do not have in the history of econometrics the kind of reflexivity found by Corry in mathematics, for there are no econometric parameters which would dictate the relevance, truthfulness or accuracy of the developed econometric methods or of their history. But the image of knowledge with which econometrics is regarded is one marked by econometrics itself.

In a way some degree of reflexivity is necessary to write the history of econometrics. For this moderate degree allows the historian to understand the body and the content of what she is talking about. A casual or distracted historian knowing nothing about the tool she is writing about would take the risk of forgetting several important insights of such a complex discipline like econometrics. So, a certain degree of reflexivity is absolutely necessary in order to understand of what one is talking about.

There can be a problem, however, with reflexivity, when it affects the image of the historian. For, in this case, a too optimistic, linear, progressive and internal vision of the history of the discipline could cloud the historian's interpretation. We think that there is something like this affecting historians of econometrics; there is a reflexivity that exerts an influence at two levels, both in their technical and theoretical knowledge about econometrics (which is very desirable), and also in their imaging (which dictates some criteria that can, in a way, bias their vision).

This bias can be understood in the following way. As we have said, the image of knowledge determines the criteria that will be held in a scientific community, as those which mark what is true, what is important to study, and what are the right questions to pose and to answer, etc. In a way they will give a particular shape and aspect of a discipline. But this shape is only one possible within an ensemble of possible interpretations. That is where the bias relies. The reflexivity of the history of econometrics, i.e., the history written by econometricians themselves, yields *one and only one* particular interpretation. And that will be one of our main criticisms: this particular interpretation is only one possible within a spectrum of infinite alternatives.

3. Styles of Reasoning

This section presents the concept of Styles of Reasoning according to Hacking's contributions. First, I will present a general picture of what a style is. Then, I will discuss the most important concepts, characteristics and implications of the styles of reasoning. Meaning that I will refer to the concept of objectivity, which comes along with the blooming of a given style. Then, I will discuss the way a style originates, as well as how styles become autonomous. In the following section, I will present the necessary condition that a style of reasoning should accomplish in order to actually be a style, namely, the creative production of novel facts or "positivity". Last, but not least, I will discuss the concepts of self-authentication that cause the stability of styles, as well as the concept introduced by Hacking of *Philosophical Technology*.

As I said before, Hacking's Styles of Reasoning are sensed to explain a more global sphere of the scientific activity than Corry's framework do. Hacking (1992, 2002) asserts, for instance, that various styles of reasoning can coexist, since they "(…) are interwoven. They are not contraries but simply different, and they can all be called upon in a single research project" (Hacking, 1992, p. 137). Hence, the same researcher can make use of a variety of styles of reasoning. They are not peculiar to any single person, and the use of one of them by the same person, does not hinder that person of using another style[11]. Furthermore,"(…) styles are completely impersonal, anonymous (…). They become, like a language, there to be used, canons of objectivity" (ibid. p. 139).

The existing Styles of Reasoning make it possible to think about an object in a particular way and hence the coexistence of many Styles contributes to and allows the formation of a certain image of knowledge[12]. Shortly, coexisting styles of reasoning make possible the formation of a given ensemble of images of knowledge.

Hence, as Morgan (2012) states, the adoption of a given style of reasoning by a particular science directly affects its history.

> "[A]dopting a new reasoning style into a science does not come without significant consequences for its content. There are inevitably connections between style and content (...) Any scientist's ability to reason in a chosen style is (…) clearly dependent on the contingent history of that discipline, and whether the method is accepted within it" (Morgan, 2012, pp. 16-17).

Styles provide both the philosophical and epistemological possibilities to think about an object, while they also provide the practical and technical possibilities for the actual coming into being and discussion of that object.

Along history, a lot of styles of reasoning have existed. Some of them have endured, even if they transformed themselves, other just disappeared. Hacking, quoting C.A. Crombie, refers to a list of six styles of reasoning, which, nevertheless, is not exhaustive:

> "(a) The simple method of postulation exemplified by the Greek mathematical sciences.

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[11] In this respect, styles and images are quite different. For researchers cannot have more than one image at a given time, but they can, and indeed do, use more than one style at the same time.

[12] This coexistence of styles is not without trouble, though. As Morgan says,

> "(…) typically those who would adopt a new style of practical reasoning for their science have to argue for it, as well as demonstrate its usefulness, for the acceptance of a new style generally institutes a *change* in reasoning style" (Morgan, 2012, p. 16, emphasis in original)

(b) The deployment of experiment both to control postulation and to explore by observation and measurement.

(c) Hypothetical construction of analogical models.

(d) Ordering of variety by comparison and taxonomy.

(e) Statistical analysis of regularities of populations, and the calculus of probabilities.

(f) The historical derivation of genetic development

(Hacking, 2002, pp. 181-182)".[13]

The adoption of one, or all of these styles, or even of other styles, by a given scientific community is what gives the standard for that community to reason about an object in the "right way". As Morgan puts it, paraphrasing Hacking, "reasoning rightly seems to reason in the style accepted by a given group of scientists" (Morgan, 2012, p. 17).

> "[So], we can also take from both Crombie and Hacking that adopting a new reasoning style into a science does not come without significant consequences for its content. There are inevitably connections between style and content, and while different sciences may rest on one or more of these styles of reasoning, that does not imply that any scientific system can rest on any style. For example, Quetelet's 'average man' of the mid-nineteenth century is a statistically defined concept and so unthinkable without the adoption of statistical reasoning" (Morgan, 2012, pp. 16-17).

Furthermore, the adoption of one or several styles not only affects the content of that particular science, but it is also accompanied by struggles within the scientific community. A scientist using a new style, will have to convince the rest of the community that there is something "good" about using this new style. There is something new about it from which everyone can learn, and from which the science in question will find itself enriched.

> "(…) typically those who would adopt a new style of practical reasoning for their science have to argue for it, as well as demonstrate its usefulness, for the acceptance of a new style generally institutes a *change* in reasoning style" (Morgan, 2012, p. 16, emphasis in original)

[13] Hacking criticises Crombie's list and does not think that one has to stick to this definition. We will come back to this criticism in chapter *6. Econometrics as a Style of Reasoning?* and will enlarge the notion of styles of reasoning in the context of Econometrics. (See Hacking (2002) pp. 182-186 for a more detailed description of Hacking's criticism).

For the subject of study that concerns us, namely for economics (and econometrics), there are two problems at stake. On the one hand, the changings that the adoption of certain styles of reasoning exert over the content of econometrics, and on the other, the daily problems that econometricians had and have to face with their fellow econometricians, in order to defend the introduction of new styles of reasoning. The introduction of the probability approach and of the statistical methods of analysis, are just two examples of these struggles.

But, let me go back to the global character of the Styles of Reasoning, since I find here not only an aspect of complementarity, but also one of compatibility with Corry's framework. I say that the Styles are of a rather global nature compared to the Images, because they provide the structure of reasoning that allows all scientists within a given period of time and space, to think about an object in particular ways. Nevertheless, this way of thinking about an object rises at a local sphere. "I want something both social and metaphysical", says Hacking, "and propose my concept of a 'style of reasoning'. It is an irrevocably metaphysical idea, yet styles, like all human, come into being through little local interactions" (Hacking, 1992, pp. 131-132). So, this global character is only acquired once the Style has been established and once it attaints a certain degree of stability within the scientific community. (I will come back to this aspect, when I talk about objectivity in section 3.2).

Although styles refer to a global orbit, its formation has to be retraced at a local sphere of human interaction. And it is here, again, where Images and Styles find common ground. Here, it is important to remember that Hacking defines his Styles as 'Styles of Reasoning', subtlety different in emphasis from the 'Styles of Thinking' first proposed by A.C. Crombie[14]. This difference is supposed to emphasize on the communal, microsocial, rhetorical and conflicting character of the emergence of styles.

[14] Hacking recognizes that he first took up the idea of 'style of reasoning' from the works of Alistair Cameron Crombie. Nevertheless Hacking also explains that his vision differs from that of Crombie's only on the emphasis of an important aspect: Crombie calls his styles 'styles of thinking', while Hacking calls them 'styles of reasoning'. For Hacking the word reasoning reflexes in a better way of what he means, even if he is still not completely satisfied by it. "I prefer to speak of styles of (scientific) 'reasoning' rather than Crombie's 'thinking'. This is partly because thinking is too much in the head for my liking. Reasoning is done in public as well as in private: by thinking, yes, but also by talking and arguing and showing. The difference between Crombie and myself is only one of emphasis" (Hacking, 2002, p. 180). See also Crombie A.C. (1994) *Styles of Scientific Thinking in the European Tradition*. 3 vols. London: Duckworth.

3.1 The General Picture

Styles of Reasoning are an analytical tool developed by Ian Hacking[15], which intend to allow collaboration between the History and the Philosophy of Sciences[16] (Hacking, 2002). This collaboration, says Hacking, is complementary, though it is somehow asymmetric (ibid. p. 178). On the one hand, the Historian using this framework will focus on "how we find out, not on what we find out" (ibid. p. 178). Hence, her focus "is less about the content of the sciences than about their methods" (ibid. p. 178) and so, the Historian's work is more "an account of how conceptions of objective knowledge have come into being" (ibid. p. 198). The Philosopher using this framework, on the other hand, "can describe the techniques which become autonomous of their historical origins, and which enable styles of reasoning to persist at all" (ibid. pp. 198-199).

The use of this framework from an exclusively philosophical view, however, would not allow the researcher to understand the origins that made the birth of a style of reasoning possible. That is why, "the philosopher needs the history, for if the tool does not provide a coherent and enlightening ordering of the record, then it has no more place in sound philosophy than would any other fantasy" (ibid. p. 178).

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[15] Hacking actually attributes the origins of the concept to other authors. One of them is the Historian of Sciences Alister Crombie, from whom Hacking first heard the concept of *Styles of Thinking*. "(…) I first encountered in 1978 [the idea of styles of reasoning], in Pisa, listening to a paper by the senior historian of science, Alistar Crombie" (Hacking, 2002, p.159). Their ideas about what a style means are actually quite similar. Crombie calls his styles "styles of thinking", while Hacking's styles are "styles of reasoning". Hacking makes this differentiation because he wants to make clear that styles do not only appear by thinking, but that there is a lot of social interaction and public work going on. Yet, Hacking recognizes that Crombie's idea of style is similar in this respect.

> "This difference between Crombie and myself is only one of emphasis. He writes that 'the history of science has been the history of argument – and not thinking. We agree that there are many doings in both inferring and arguing. Crombie's book describes a lot of them, and his very title happily ends not with science but with 'Sciences and Arts'. (…) Nevertheless, there may still be a touch too much thinking to my pleasure. (…) Even my word 'reasoning' has too much to do with mind and mouth and keyboard; I does not, I regret, sufficiently invoke the manipulative hand and the attentive eye. Crombie's last word in the title of his book is 'Arts;' mine would be 'Artisan.' (Hacking, 2002, pp. 180-181).

[16] Note that Hacking does not speak of the History or the Philosophy of Science in singular. He speaks of Sciences in plural.

> "The very mention of styles, in the plural, corrects the debate: we shall stop talking of science in the singular and return to that healthy nineteenth-century practice of William Whewell and most others: we shall speak of the history an philosophy of sciences – in the plural. And we shall not speak of the scientific method as if it were some impenetrable lump, but instead address the different styles" (Hacking, 2002, p .196).

This division of labour, between historian and philosopher, should not be pushed too far, as Hacking says (ibid. p. 199). The same researcher can assume both roles (the historian's and the philosopher's) and so, by using this framework, carry out this collaboration. Just like the philosopher needs history to make a coherent study of a given discipline, so the historian needs philosophy to better understand the issues studied and their relations to the story she is telling. For "(…) every sound history is imbued with philosophical concepts about human knowledge, nature, and our conception of it" (ibid. p. 199).

Furthermore, as we will see later on, Hacking's framework provides us with a powerful idea about objectivity: styles of reasoning actually settle what it is to be objective; they become cannons of objectivity. But these canons have not always been there. They have been created and this creation happened in a special way. Even if the origination of a style of reasoning has a lot to do with "microsocial interactions and negotiations" (ibid. p. 188) and with the particular context in which the style is born, styles end up by always having popular myths of origin.

Historians, philosophers and scientists in general, attribute the creation of a style of reasoning to a specific person, an institution, an instrument, or an important event. This myth is important in the later process, experienced by every style, of getting autonomous. For the myth makes people to forget about the historical and social specific conditions and interactions that rendered possible the creation of the style. This autonomy gives the impression that the style is stable and timeless. As if it has always been there, untouched by the researchers or by the social and political struggles of life.

But styles do not gain their autonomy just by the existence of a creational myth about them. It is not as easy as that. They have to invent some extra mechanisms and techniques that will render them stable and "self-authenticating". Some of these techniques will be more effective than others and their efficacy will, in the end, explain why some styles finally disappear in time, change their form, or persist.

Styles will also have to fulfil one necessary condition in order to exist. This condition consists of producing new "positivity": this means that they will "open up new territory as they go" (ibid. p.185) and hence styles create new objects, laws, evidence, sentences and possibilities. These newly created objects become candidates for truth or falsehood under the scrutiny of the given style of reasoning. "[T]he very candidates for truth or falsehood have no existence or independence of the styles of reasoning that settle what it is to be true or false in their domain” (ibid. p. 161). And this settling of what is true or false, in turn, provides the objectivity standard for the new scientific discoveries that are done under the magnifying glass of a particular style.

In order to end this general characterization of styles, it is important to “[n]ote that styles do not determine a content, a specific science” (ibid. p. 182). They can actually be used by any discipline or science, and they can even change our vision about a particular object within a science, but, again, they do not determine the content of any particular science or discipline.

3.2 Objectivity

Hacking clearly states that his goal is one that aims at the continuation of Kant’s project: that Objectivity is possible. Hacking “wish[es] to pose a relativist question from within the heartland of rationality” (ibid., p. 159). So, he wants to attaint some sort of objectivity departing from a relativist approach. Here, he makes a distinction between subjectivity and relativity.

> “An inane subjectivism may say that whether *p* is a reason for *q* depends on whether people have got around to reasoning that way or not. I have the subtler worry that whether or not a proposition is as it were up for grabs, as a candidate for being true-or-false, depends on whether we have ways to reason about it” (Hacking, 2002, p. 160).

His “relativist worry is (…) that the sense of a proposition *p*, the way in which it points to truth or falsehood, hinges on the style of reasoning appropriate to *p*. Hence we cannot criticize that style of reasoning as a way of getting to *p* or to not-*p*, because *p* simply is that proposition whose truth in value is determined in this way” (ibid. p. 160). So, for him, there is no doubt that the new discoveries are objective, just because the style determines what it is to be objective. The point here is that the candidates for truth or falsehood do not exist apart from the style of reasoning, which determines what is true or false in a given field (ibid. pp. 160-161).

> “My styles of reasoning, eminently public, are part of what we need to understand what we mean by objectivity. This is not because styles are objective (that is, that

we have found the best impartial ways to get at the truth), but because we have settled what it is to be objective (truths of certain sorts are what we obtain by conducting certain sorts of investigations, answering to certain standards)" (Hacking, 2002, p. 181).

But people do not simply decide alone in their corners what it is to be objective or not. There is much of a communal activity behind this process of settling the objectivity of discoveries.

> "I assert neither that people have decided what shall count as objectivity, nor that we have discovered what does the trick. I am concerned with the way in which objectivity comes into being, and shall shortly state how to address the question of what keeps certain standards of objectivity in place. Why do I not say that we have simply discovered how to be objective, how to get at the truth in a long haul? This is because there are neither sentences that are candidates for truth, nor independently identified objects to be correct about, prior to the development of a style of reasoning" (Hacking, 2002, p. 188).

To resume, styles of reasoning give a canon for objectivity, and the discoveries that researchers do within it automatically become objective. Objects that are possible within a given style are only possible within that specific style and do not have any existence outside of it.

3.3 The Origins of the Styles of Reasoning and their Autonomy

Styles of reasoning come into being after a process of microsocial interactions and negotiations. Hence, styles arise as a communal activity, surrounded by all sorts of political, social and historical factors. This means that styles do have a history, and a context that made them possible. Just in the same way in which styles see the light, they can also disappear if their stabilizing and "self-authenticating" techniques were not effective enough to perpetuate the style. The present styles of reasoning have not always existed, and there is no warranty that the ones that exist today will last for ever or even for much longer.

Nevertheless, once a style of reasoning has come into being, and once it has developed an effective way of "self-authentication", it can give the impression as if it has always been there. It becomes autonomous and self-authenticating. Nobody questions its providing of objectivity, or even whether it is a "good" way of looking at a given field. They become timeless and so they also "become independent of [their] own history" (ibid. p. 188). Then, researchers, scientists, philosophers and historians, leave the style's history aside, and explain their coming into being in the form of a myth.

> "It is characteristic of styles that they have popular myths of origins" (Hacking, 2002. p. 185).

These myths can take the form of a person, an institution, an instrument, or an important event. Take for instance Galileo.

> "[He] is everyone's favourite hero (…) not only for Chomsky and Weinberg but also for Husserl (…) and Spengler. Crombie's talk on styles of scientific thinking that arouse my interest was about – Galileo. (…) All these authors referred chiefly to some aspect of style (c) [Hypothetical construction of analogical models], so let us not forget that according to Stillman Drake, it was Galileo who, by the purest use of style (b) [experimental style of reasoning] established the very first experimental and quantitative law of nature. Galileo is the stuff of myth (…)" (Hacking, 2002, p. 185).

But not only persons can be taken as the "stuff of myth". Instruments can also fill this purpose. Hacking himself takes "Schaffer and Shapin's book, subtitled *Hobbes, Boyle and the Experimental Life* (1986), as setting out the myth of origin for the laboratory style. Their hero (…) is not a person but an instrument, the apparatus, the air pump" (ibid. p. 185). This transformation of history into a myth, however helpful to organize our thinking and to give it a chronological coherence, can be problematic. For it can deviate our attention and it can make us forget about the conditions of emergence that rendered possible the creation of a style of reasoning. And so we can forget that there are other possible standards of objectivity that have been either forgotten, replaced, or that have not yet come into being.

> "We can forget the history or enshrine it in myth. Each style has become what we think of as a rather timeless canon of objectivity, a standard or model of what it is to be reasonable about this or that subject matter. We do not check to see whether mathematical proof or laboratory investigation or statistical studies are the right way to reason: they have become (after fierce struggle) what it is to reason rightly, to be reasonable in this or that domain" (Hacking, 2002, p. 188)

By forgetting about its history, its context of possibility of emergence, and about its communal character, we just passively accept the vision of the world that a given style provides us. We just accept what it is supposed to be to reason rightly.

In the first place, one could think that this passive acceptance of the style of reasoning affects primarily practitioners of a certain discipline. But the point here is that passive acceptance can go far beyond the practitioners level. The passive acceptance of a certain canon of objectivity given by a style, can also affect historians and philosophers of sciences[17]. Let me first illustrate the case when practitioners are affected. As Hacking states, practitioners can sometimes, adopt a style without really understanding the fundamental ideas behind its methods.

> "This is at its most obvious in 'cookbooks' for statistical reasoning prepared for this or that branch of science, psychology, cladistics taxonomy, high energy physics, and so forth. With no understanding of principles, and perhaps using only a mindless statistical package for the computer, an investigator is able to use statistics without understanding its language in any meaningful way whatsoever" (Hacking, 2002, p. 184).

This acceptance can be more clearly seen in the case of practical work. Economics does not escape from this automatized application of statistical techniques, either. On the other hand, historians and philosophers of sciences, in general, can be also affected by this passive acceptance of the styles of reasoning. This affectation though, occurs at another level.

In the case of the history of econometrics, I will argue that historians of econometrics have accepted the vision of a hero, which sometimes takes the form of Haavelmo, and sometimes that of Structural Econometrics or that of the Cowles Commission. Even if the Received View digs deep in the understanding of the history of econometrics, its presentation of the evolution of econometrics can sometimes be seen as if these authors were, unconsciously, considering econometrics as a style of reasoning. Although this way of looking at the history of

[17] The reader will note that I am making here a neat differentiation between practitioners, historians and philosophers. This differentiation is hard to find, however, in almost any field of study. It is even harder to find in our field of research, for, as we will see later, historians of econometrics are economists themselves as well as practitioners of econometrics. If they do not do econometrics directly, then sometimes they have a very close link to economists that are eminent econometric practitioners. This separation as many others is, again, only to the sake of the exposition, and is not necessarily accurate to describe the division between the economists.

econometrics presents a quite coherent and chronological evolution of the discipline, it can also lead to a biased scrutiny. We will come back later to this point in chapter 7 (*Part II*).

3.4 Novel facts and "Positivity" as a Necessary Condition

The emergence of styles of reasoning renders possible the discussion of certain facts, objects, phenomena and techniques. As we have already argued, following Hacking, the existence of certain objects is only conceivable if this object is the product of the reasoning within a particular style. Hence, "[s]tyles…open up new territory as they go" (ibid., p. 185), and so:

> "Every style of reasoning introduces a great many novelties including new types of:
>
> objects
>
> evidence
>
> sentences, new ways of being a candidate for truth or falsehood
>
> laws, or at any rate modalities
>
> possibilities"
>
> (Hacking, 2002, pp. 188-189).

But, becoming a proper style is not just a matter of creatively producing new kinds of objects and opening up new territory. For, inventing new kinds of objects and sentences and words, is something that all humans have been doing since ever. It is about introducing new kinds of positivity or as Hacking more clearly says it:

> "Each new style and each territorial extension, brings with it new sentences, things that were quite literally never said before. That is hardly unusual. That is what lively people have been doing since the beginning of the human race. What's different about styles is that they introduce new ways of being a candidate for truth or falsehood. As Compte put it (…), they introduce new kinds of 'positivity', ways to have a positive truth value, to be up for grabs as true or false" (Hacking, 2002, p. 190).

Positivity is the fact of introducing new propositions, which can be considered as true or false. Remember that the canon of what it is to be true and what it is to be false is given by the style of reasoning itself.

The introduction of novelties becomes, then, a necessary condition a style has to accomplish in order to be a proper style of reasoning.

> "Each style should introduce novelties of most or all of the listed types, and should do so in an open-textured, ongoing and creative way" (Hacking, 2002, p. 190).

3.5 Stability of Styles and Self-authentication

In order to explain the provenance of the stability of styles, Hacking begins by endorsing a lemma from the *Strong Programme in Sociology of Science*. This lemma states that truth, or a fact, or reality or the way the world is, are not stabilizing factors that perpetuate styles. Here, truth, and facts, and reality, and the way the world is, has to be understood in an ontological way. As if they really existed outside our own knowledge.

> "The truth of a proposition in no way explains our discovery of it, or its acceptance by a scientific community, or its staying in place as a standard item of knowledge. Nor does being a fact, nor reality, nor the way the world is. My reasons for saying so are (…) of very traditional philosophy. I would transfer to truth (and reality) what Kant said about existence, that it is not a predicate, adding nothing to the subject. I may believe that there was a solar eclipse this summer because there was one in the place I was then staying; the eclipse is part of the explanation of my belief (a view which might be resisted in Edinburgh), along with my experience, my memory, my general knowledge, the folderol in the newspapers, etc. But the fact that there was an eclipse, is not part of the explanation, or at any rate not over and above the eclipse itself" (Hacking, 2002, pp. 192-193).

So what does actually give stability to the styles of reasoning? Hacking argues that it is the self-authenticating mechanism, which provides that stability. Remember that this mechanism has no higher standard to which it directly answers, and so the standard is produced by the style itself. The style then produces a kind of truth (different from the first kind of truth we were talking about) that is proper of the style of reasoning. Truths of this type find a way of authentication within the standard and canon of the style. Hence, these are "self-authenticating" techniques, which give the impression that there is some kind of circularity in the process.

> "The truth of a sentence (of a kind introduced by a style of reasoning) is what we find out by reasoning using that style. Styles become standards of objectivity because they get at the truth. But a sentence of that kind is a candidate for truth or falsehood only in the context of the style. Thus styles are in a certain sense 'self-authenticating'. Sentences of the relevant kinds are candidates for truth or falsehood only when a style of reasoning makes them so. This statement induces an unsettling feeling of circularity." (Hacking, 2002, p. 191)

This circularity is closely related to the fact that styles introduce novelties and has also a lot to do with the capacity of styles to authenticate themselves. The introduction of these novelties could be interpreted as if this approach would be of a constructionist nature. Scientists would construct facts and phenomena during their research and negotiations. These facts and phenomena would not have existed before they were literally constructed by the scientists. This is not the idea Hacking is trying to express, though.

> "The doctrine of self-authenticating styles is distinct from 'constructionist' accounts of scientific discovery. For in those accounts individual facts of a typically familiar kind become constructed-as-facts in the course of research and negotiation. There was no fact 'there' to discover until constructed" (Hacking, 2002, p. 191).

Hacking's idea of self-authentication is not, however, of a constructionist nature. In fact, facts and phenomena can already be there, but there can be different interpretations about their truthfulness of falsehood. These interpretations will depend on the styles of reasoning that are used in order to assess them as true or false. What is important is that the particular fact must be a candidate for truth or falsehood. Remember that styles are all about the way we find out things. They are not about what we find out[18].

[18] Here we do not dismiss the possibility of facts and phenomena being constructed. This is something that happens in sciences all the time, of course. What we want to set clear is that styles do not have the capacity to construct new objects. They just give new possibilities of seeing objects, of interpreting them, etc. The construction of objects happens outside the style. The point here is that styles produce new positivities, as we have already said it in the previous section. New Positivities bring new ways of

> "According to my doctrine, if a sentence is a candidate for truth or falsehood, then by using the appropriate style of reasoning we may find out whether it is true or false" (ibid. pp. 191-192).

Moreover, Hacking explains that this "apparent circularity" actually explains the immunity of styles towards refutation. And hence, this circularity also explains why styles are stable. Even if styles can be modified or can disappear in the long run, in the short run styles appear to be timeless and unchangeable. What actually changes in the short run are not styles, but the knowledge produced by those styles. The framework can remain the same, while the content and the knowledge produced by it changes[19]. Hacking focuses his attention on the causes of this apparent continuity of styles. Namely, on the stabilizing techniques that styles develop.

> "The apparent circularity in the self-authenticating styles is to be welcomed. It helps explain why, although styles may evolve or be abandoned, they are curiously immune to anything akin to refutation. There is no higher standard to which they directly answer. The remarkable thing about styles is that they are stable, enduring accumulating over the long haul. Moreover, in a shorter time frame, the knowledge that we acquire using them is moderately stable. It is our knowledges that are subject to revolution, to mutation, and to several kinds of oblivion; it is the content of what we find out, not how we find out, that is refuted. Here lies the source of a certain kind of stability" (Hacking, 2002, p. 192).

Stabilizing techniques differ between styles. Each style of reasoning has its own stabilizing technique. Indeed, "[a]lmost the only thing that stabilizing techniques have in common is that they enable a self-authenticating style to persist, to endure" (ibid. p. 193). But techniques also differ in their effectiveness. Some can be more effective than others, and this explains why some styles of reasoning remain more or less stable.

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being a candidate for truth or false, and styles just determine whether these candidates are, indeed, true or false.

[19] Take economics, for instance. As Morgan (2012) shows, *modelling* is one of the favourite styles of reasoning used by economists. Ignoring the fact that modelling has evolved a lot within the life of economics science, or taking just a short period as an example, we will be able to see that the use of the same style of reasoning does not impede economic theory to change. Macroeconomics, for example, experienced a big change in its content during the 1970's, passing from a Keynesian to a Neoclassical view, without changing their style of reasoning: modelling.

Let us take one of Hacking's illustrations of a self-authenticating technique: that of statistics. Why statistics? Because of two reasons: first, because Hacking considers statistics as one of the most stable styles, and second, because this style is closely related to our own field of study:

> "(…) although Mark Twain, Disraeli, or whoever could, in the earlier days of statistical style, utter the splendid canard about lies, damn lies, and statistics, the statistical style is so stable that it has grown its own word that gives a hint about its most persistent techniques: 'robust'. In the case of statistics there is an almost too evident version of self-authentication (the use of probabilities to assess the probabilities)" (Hacking, 2002, p. 194)

In his 1992 article[20], Hacking shows how the whole social dimension that entails the creation of a scientific style of reasoning is actually present in the case of the statistical style of reasoning.

> "If you want interests, we have interests. If you want theoretical devices, we have those. And institutions, modes of legitimation, takeover battles, constructions, uses of power, networks, intimations of control, and much, much more" (Hacking, 1992, p. 133).

But he also shows that once a style of reasoning becomes stable, all these social factors that were essential for its birth become less and less important.

> "Yet as the style becomes increasingly secure, these are decreasingly relevant to its status. The style ends as an autonomous way of being objective about a wide class of facts, armed with its own authority, and available as a neutral tool for any project or ideology that seeks to deploy it. It provides new criteria of truth, new grounds for belief, new objects about which there can be knowledge. It generates the very stuff about which we do metaphysics" (Hacking 1992, p. 133)

There are, however, other factors different from the self-authenticating techniques that provide stability to the styles. These factors are material and institutional factors. As I understand this, Hacking means that there must be some conditions that make the emergence

[20] "Statistical Language, Statistical Truth and Statistical Reason: The self-authentification of a style of scientific reasoning", published in *Social Dimensions of Science*, by Ernan McMullin (ed.), (pp. 130-157).

and maintenance of particular stabilizing techniques possible. Some things that would render the technique legible and accessible; some actual spaces where these techniques can be learnt and discussed; other objects that would make it possible to see, read, and publish the techniques. These spaces and things will render the practice of the techniques possible, as well as they will institutionalize them, while rendering them autonomous of their history and of their microsocial interactions that first brought them into being.

These things and spaces can be, of course, universities, laboratories, think tanks, research centres, journals, books, manuals, etc. Hence, the study of the self-authenticating techniques includes, not only the method and the technique as such, but also the material and institutional conditions that make the existence and persistence of the techniques possible. Hacking will refer to the study of the whole that contains the self-authenticating technique as *Philosophical Technology*.

3.6 "Philosophical Technology"

Philosophical Technology, it must be admitted, is not a very suitable name of what Hacking means[21]. At first sight, it gives the impression to be something like the application of some techniques, which would be the common meaning of the word technology. Hacking is actually speaking of "the philosophical study of certain techniques, just as philosophical anthropology is the study of certain aspects of man, epidemiology of epidemic diseases" (ibid. p. 197).

In short, Hacking

> "(…) invite[s] what [he] call[s] philosophical technology: a study of the ways in which styles of reasoning provide stable knowledge and become not the uncoverers of objective truth but rather the standards of objectivity" (Hacking, 2002, p. 198).

[21] Even the author himself recognizes the unsuitability of the term. "This label does not carry its meaning on its face, for I am not talking about what we usually mean by 'technology', namely the development, application, and exploitation of the arts, crafts, and sciences" (Hacking, 2002, p. 197).

But Philosophical Technology goes further. It does not only remain at the level of the study of the techniques, but it digs deep into the conditions that made it possible for a style to emanate. These conditions, however, are closely related to the place human beings have in nature.

> "The persistence of a style demands some brute conditions about people and their place in nature. These conditions are not topics of the sciences, to be investigated by one or more styles, but conditions for the possibility of styles" (Hacking, 2002, p. 196).

The description of these techniques would not be sufficient to make a serious study of a style of reasoning. The important point would be to grasp the facts that are typical of human nature, those that have always been there, but that have remained unnoticed, because they are too obvious to be seen. The kind of facts that are just taken for granted by everybody and that become "invisible" in a way. That is where the real interest lies, for these conditions, these brute conditions, would be the ones responsible for the creation of the possibilities where styles emerge.

> "An account of them [of the brute conditions] has to be brief and banal, because there is not much to say. What we have to supply are, to quote Wittgenstein, 'really remarks on the natural history of man: not curiosities, however, but rather observations on facts which no one has doubted and which have only gone unremarked because they are always there before our eyes' (Wittgenstein 1981, 47). Wittgenstein and others also called this (philosophical) anthropology (cf. Bloor 1983). (…) Wittgenstein's philosophical anthropology is about the 'natural history of man', or, as I prefer to put it, about human beings and their place in nature. It concerns facts about all people, facts that make it possible for any community to deploy the self-stabilizing techniques of styles of reasoning" (Hacking, 2002, pp. 196-97).

So, three questions will concern our work, regarding styles of reasoning. First, a relation of complementarity will be established between the body/image of knowledge framework and the styles for reasoning framework. Second, we will try to answer the question whether econometrics can be considered a style for reasoning according to the Received View and whether or not that consideration is accurate. Last, but not least, we will have to dig deep in order to deal with the unremarked facts that have rendered possible the existence of the econometrics style for reasoning – if there was one – or with the introduction of the statistical style of reasoning into economics – if we adhere to a rather "Hackingnian" interpretation.

Conclusion

In short, *Part I* deals with the methodological framework (or frameworks) that appeared more "suitable" in order to undertake this research. These frameworks can be considered as being quite coherent with the latest practices in economic methodology (see Hands, 2001b), because they allow focusing on how economics is actually done, rather than on how economics should be done. Note that there is no normative intention behind the choice of these frameworks. Choosing them is just one alternative within a large spectrum of possibilities.

Part I explicitly states that there is a similarity between the frameworks of the present work and the one chosen by the Received View. This similarity lies in the fact that both approaches focus on the practices of the econometricians. This focus is the key that contributes to the quite chaotic and multifaceted evolution of econometrics presented by the Received View at the beginning of the period studied.

Nevertheless, and despite this methodological similarity, there is an important difference between the focus of the Received View and that of the present work. For, the Received View only takes into account individual contributions in terms of theory and methodology, forgetting to study a variety of other factors affecting the evolution of science. In short, the Received View does not fully consider econometrics as embedded within the cultural activity. This fact limits their analysis of the social dimension of the history of econometrics. The present research, on the other hand, considers the theoretical and methodological factors as continuously interacting with a series of other factors (political, philosophical, social) and as being embedded within a cultural context.

Part I also served to adhere to a given methodological framework. This adhesion is necessary, for it provides a structure to the research. But it can also limit the analysis to some extent, for it forces the researcher to concentrate on some aspects rather than other. This work applies a twofold strategy in order to minimize the limits and the problems raised by the allegiance to one particular methodological framework: First, it considers two methodological frameworks, instead of only one. Both seem to be quite complementary: Corry's Image/body of knowledge and Hacking's Styles of Scientific Reasoning. Second, following these authors, this work keeps the idea that there is no rigid definition of the frameworks, and so, that these remain quite flexible and always "under construction".

Corry's framework helps focusing on the interaction of what he calls the body and the image of knowledge. Let me recall the elements of the image and body very briefly. Corry understands "theories, facts, methods and open problems" (Corry, 1989, p. 411), as being part of the body of knowledge of a discipline. The images, on the contrary, are the "guiding principles, which pose and resolve questions arising from the body of knowledge itself" (ibid.). The important point here is that both images and body are not separated or independent things. They find themselves in continuous interaction and they affect each other at all times. In fact, the separation is only conceptual, for, in "reality", they are just one and the same thing. These interactions happen at the "micro-social level" and it is these very interactions, which allow us to introduce new elements in the analysis of the history of econometrics.

The new elements I am talking about overcome the classic discussion about internal/external factors affecting a science. Instead, these new elements are a combination of both, the internal and the external factors, which find themselves in a continuous interaction. The body of knowledge cannot be understood without understanding the images of knowledge and *vice versa*. Theories must be understood at the light of the kind of methodological questions and "guiding principles" that were posed at the time under scrutiny. These latter questions, however, have to be also understood as being influenced by the body of knowledge, provided by the theories at that given time.

Furthermore, two considerations are important in order to understand how it is possible for an image of knowledge to become dominant and to be imposed to a large number of scientists. First, there is a certain amount of influences originating at the level of research funding, and political research direction. Both, the funding and the political interests can be crucial for the expansion of the material and contextual conditions that render possible the imposition of an image. Yet, these factors are not sufficient in order to understand the imposition of an image.

There is also a paramount condition of compatibility between the body and the image of knowledge that has to be fulfilled. If the image of knowledge is concerned with a number of questions impossible to resolve with the existing body of knowledge, then asking these questions will make no sense. If, on the other hand, the body of knowledge is armed with the adequate tools and theories that could resolve a question, but this question is not considered as relevant by the image of knowledge, then there will be no point and no interest in resolving it – and it will, indeed, remain unsolved.

Section 2.3 deals with the concept of reflexivity – a key concept in Corry's framework. Nevertheless, it is worth recalling the fact that Corry is a historian of mathematics and that his concept of reflexivity is one that cannot be applied directly to econometrics. He insists on the fact that mathematics is the only science that has the capacity "to study itself mathematically" (Corry, 1989, p. 413). Reflexivity, as defined by Corry, cannot be found in econometrics, because of a very simple reason: econometrics cannot study itself "econometrically".

Still, the history of econometrics presents another kind of reflexivity. The close relation held between the historians of econometrics and the econometricians themselves, is the source of this special kind of reflexivity. This relation has affected the writing of the history of econometrics, which has, consequently, been written "from within" econometrics.

This means that people writing about the history of econometrics have shared a particular image of knowledge that emerges from the minds of econometricians themselves. The point here is that the image of knowledge retained by econometricians has certainly directed the study of the history of econometrics in the direction of econometricians' interests. It is very likely that their interests lie in the internal methodological and theoretical aspects of their discipline, rather than on the social, cultural, historical, or even political aspects. In short, it is plausible that this exercise of "common" writing had yielded a biased narrative of the history of econometrics.

It is worth emphasising that a major criticism of the present work resides in this particular concept of reflexivity. This criticism consists on the fact that the reflexivity of the history of econometrics, i.e., the history written by econometricians themselves, yields *one and only one* particular interpretation of this history.

Part I also analyses two additional issues related to Corry's framework. The first issue concerns the fact that this framework takes for granted the *a priori* existence of a certain body and a certain image of knowledge. This fact can be somehow problematic, because it does not study the conditions that made possible the emergence of a particular body and a particular image. The second issue that attracts attention has to do with the framework's local nature.

The problem with this locality resides in the fact that it would not allow the researcher to understand how could it be possible for an image and a body to expand beyond its original scientific roots and to find echo in a broader community.

These issues can be complemented by another methodological framework: Hacking's styles of reasoning. These styles explicitly talk about the creation of certain conditions that render possible the emergence of a given image and body of knowledge, while they remain of a rather global nature. Thus they overcome both problems: that of the emergence conditions of a certain image and body, and that of locality.

Even if styles also emerge from local and social interactions "like all human" (Hacking, 2002), they become global and commonly accepted: a canon for objectivity. And so, Hacking's framework provides us with a solution allowing us to jointly resolve Corry's issues: the styles of scientific reasoning are those responsible of bringing about the possibility conditions of thinking about a problem in a certain way; they contain the material and theoretical "global" possibilities of existence that allow scientists to give a particular interpretation to given facts, at the light of the style at hand.

So, the complementarity elements between these two frameworks lie in several aspects. First, in the fact that Corry's framework stays at a rather local sphere, while Hacking's reaches "globality" (even if it emerges from "locality"). Second, while Hacking's styles become stable in the short term and so they become objective, Corry's images and body are continuously changing as a result of their never-ending interaction. It is true, though, that styles are also subject to change. But only in the long run, for in the short run, they give the impression of being rather stable. And finally, these frameworks are complementary because of their cultural and social origins. Science, seen from the perspective of these frameworks, is a cultural and social activity, which implies the existence of conflicts, passions, beliefs, political and economical interests, and to some amount the existence of reason as well, of course.

There are other additional interesting aspects that the concept of styles of reasoning brings up. These are the concepts of objectivity, autonomy, positivity, stability and self-authentication, and philosophical technology. A detailed account of these concepts has been provided and it would make no sense to repeat this characterisation. Rather, it can be more helpful to recall

Hacking's argument about the possibility – and necessity – of collaboration between the philosopher and the historian, allowed by the styles of reasoning.

This possibility of collaboration is given by the fact that Hacking's concepts do not only remain at the philosophical sphere. They have to be studied taking into account its historical path, from its origination to its perpetuation as standards of objectivity. And so,

> "[the work of the historian] will (...) be read in part as an account of how conceptions of objective knowledge have come into being, while the philosopher can describe the techniques which became autonomous of their historical origins, and which enables styles of reasoning to persist at all" (Hacking, 2002, pp. 198-199).

Last but not least, both frameworks allow the undertaking not only of a historiographical study– as is the case of the present work – but also the undertaking of a study of the history of econometrics as such. This is quite convenient, because, even if the present work focused on the historiography of econometrics, a further stage of the investigation will demand a focus on the history of econometrics itself. Yet, there are of course some historical aspects about econometrics that are unavoidably treated in the present dissertation, even if tangentially. These aspects will be subject to an analysis following Corry's and Hacking's perspectives.

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Part II

Part II

What is the Received View of the History of Econometrics?

Publications and works in economic methodology experienced an "explosion" (Hands, 1990) during the 1980's and a "multiple explosion" (Hands, 2001a) during the 1990's. This massive blast of methodological working did also include an important reflection about econometrics. Although many articles and books concerning the history and methodology of econometrics were written during these years[22], three of them capture our attention, because they grasp, in some extent, the novel and creative "spirit" of the new methodological approach of this period[23]: *A History of Econometrics* (Epstein, 1987), *The History of Econometric Ideas* (Morgan, 1990) and *The Formation of Econometrics* (Qin, 1993).

It is worth noting, however, that each one of these books has its own peculiarities. For example, they do cover diverse periods of time. Morgan, for instance, focuses on the early period of econometrics, emphasizing on the decades of the 1930's and the 1940's, while Epstein, and Qin, concentrate on the period subsequent to the 1940's, making an emphasis on the approach of structural estimation developed mainly at the Cowles Commission. Another difference lies in the contrast between the internalist approach adopted by both Morgan and Qin, and the more externalist vision in Epstein's *A History*.

On the one hand, Epstein attributes more importance to the social and historical context in which econometrics was developed. He discusses the effects that different social and political factors played in the growth of econometrics. Some of these factors are for example: social class conflicts, the search for instruments of control and the implementation of the Welfare

[22] Many of these publications on the history of econometrics were actually undertaken by the econometricians themselves (see Boumans & Dupont-Kieffer, 2012). This fact should be kept in mind, for it is important in a further stage of our analysis.

[23] We basically refer to the "new methodological approach in economics", having in mind Hands' (2001b) *Reflection without Rules*, where he argues that economic methodology has abandoned its emphasis in appraising economics under a "rules-based" framework inherited from positivism. Economic methodology has, then, passed from this rigid view to a more wide "pluralist approach" that takes into account not only the traditional philosophical and methodological aspects, but also reinforces the role plaid by sociological, economic, rhetorical, practical and historical factors, as well as the interaction between economics and neighbour social sciences or with natural sciences.

State (Dharmapala, 1993). Morgan and Qin, on the other hand, emphasize on aspects of a rather internal nature. The theoretical difficulties of concealing mathematics, statistics and probability calculus, the identification and estimation problems, the introduction of testing criteria or the way econometricians did manage with the data-theory gap, are all problems treated by Morgan and Qin.

Even if the three books can be considered, in general, as "complementary" (see for example Dharmapala, 1993), to some degree, Morgan's *History* and Qin's *Formation* are more closely related to each other than they are to Epstein's *A History*. As Qin herself says, "[t]his book can be considered, in many ways, a complement and sequel to Morgan's (1990) work" (Qin, 1993, p. 2). This complementarity can be found at two levels: (1) in several aspects of their books (theoretical, methodological and conceptual affinities), but also (2) in the proximity of the process of research and writing that, presumably, influenced both authors (cultural, institutional and academic context). For the purpose of this work, I would like to concentrate on the similarities presented by these two books, letting aside many important differences between them.

4. Who are the authors of the Received View?

There is not too much biographical material available about Mary Morgan or Qin Duo. I have mainly used their web sites at the LSE and at the University of London in order to get a first picture about the authors. Furthermore, I have extracted other interesting facts from the interview that Ericsson (2004) made to David F. Hendry and from the article written by Boumans and Dupont-Kieffer (2011). Nevertheless, many aspects that remain unclear or incomplete could be figured out by interviewing Morgan and Qin directly in a further stage of this work.

4.1 Mary S. Morgan

Mary S. Morgan is Professor of History and Philosophy of Economics at the London School of Economics and Political Science. She was appointed Professor at the LSE in 1999, although her relation to the LSE goes back to the mid 1970's at the beginning of her academic life. The LSE can be considered her *alma mater*, since it was at that prestigious university where she got her B.Sc. in 1978 graduating with honours in "Economics and Economic History" and it was also there where she got her Ph.D. in 1984. Morgan wrote her thesis under the supervision of David F. Hendry, a very well known econometrician, supported with the financial aid of the Social Science Research Council[24].

Two facts are worth noticing with respect to the academic relation between Hendry and Morgan. On the one hand, it is interesting to notice that Morgan's (1990) book originated from her doctoral thesis. Hendry's direction must have been of great value for Morgan to get a clearer idea about econometrics and for her to get a deeper understanding of the econometric models and concepts. Nevertheless, one cannot forget that Hendry is an econometrician himself, and that he begun to work on the history of econometrics practically at the same time that Morgan started her Ph.D. thesis (Ericsson, 2004, p. 779). This fact can be of relevance in order to understand Morgan's (1990) emphasis on the internal aspects of the history of econometrics.

On the other hand, Morgan came back to the LSE in 1979 to embark on her doctoral thesis, only after having "lost her job at the Bank of England when Margaret Thatcher abolished exchange controls in 1979" (David F. Hendry, in Ericcson, 2004, p. 779). It seems that Morgan was back then an applied economist (and econometrician), enrolled on a job that demanded her practical skills[25]. Her position at the Bank of England was, however, not the only practical job in which she had been involved. She had also occupied a position as Research Assistant at the Citibank from 1973 to 1975.

4.2 Qin Duo

Qin Duo is a Reader in Economics at Queen Mary, University of London. Even if her publications combine applied and historical work in econometrics, her lectures focus on the

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[24] The Social Science Research Council became the Economic and Social Research Council in 1983 (see Boumans *et al*. 2011, p. 12).

[25] I could not establish whether Morgan's work in the history of econometrics goes to a previous period before her Ph.D. thesis. It is true that she graduated in "Economics and Economic History", but there is no information about her early work on the history of economics or econometrics. I could not find any early paper published on that subject. This makes me think that even if Morgan was interested in the history of economics, she did not really begin to work on it, before she started her doctorate programme. This means, that she would have started to work on the history of econometrics more or less at the same time that Hendry did.

applied side of quantitative methods and do not include any course in the history of econometrics. She is in charge of three courses at the University of London: two in econometrics and another in economic development. The econometric courses are: "Applied Econometrics" and "Quantitative Methods III". (The third lecture is "Economic Development in South East Asia"). Roughly, both econometric courses define as their main objective, "to learn how to apply economic theories to real economic data by means of empirical models" (http://www.soas.ac.uk/courseunits/153400119.html). No further relevant information about Professor Qin was possible to obtain.

In short, both authors have always had a strong link to econometrics. This is so, even if one takes into account their early formation years, where the link to econometrical practical work seems to be stronger than the link to historical studies. This statement is quite risky, of course. But it seems to hold even if the work of both authors has evolved throughout the two precedent decades (see for example Morgan 2012). Morgan's new work shows a broader and more open view, where she takes into account not only internal historical factors, but where she makes use of Hacking's framework, which allows her to present *modelling* as a style of reasoning, with all the cultural, social, historical, theoretical and political implications that come with it.

But even if the authors' thinking about the history of econometrics evolved, the point here is that their attachment to econometrics during the writing of the books, had an effect on their vision about econometrics. Again, there is nothing necessarily "wrong" about being attached to econometrics when you are writing about its history. It is just that the vision of an attached historian yields a particular vision of the history of econometrics, which can be labelled as biased, by reason of the reflexivity I talked about in section 2.3.

5. The "Funnel Vision" of the Evolution of Econometrics

Morgan and Qin use a very appealing methodology in order to study the history of econometrics. Rather than focusing on the economic schools of thought or on the history of the method itself, they focus on the individuals involved in the construction of econometrics. Or, as Morgan clearly says:

> "It might be argued that we should look not to the history of economic methods

> but to the history of the people, the economists themselves, in order to understand where econometrics came from" (Morgan, 1990, p. 5).

Early econometricians belonged to an eclectic spectrum of economic traditions, so that no particular school of thought or research programme can be accounted for being at the heart of the construction of econometrics. That is why a linear or cumulative description of this process does not fit to the history of econometric ideas. Instead, a rather unclear process with many drawbacks and dead-ends gives a more accurate description of the history of econometrics.

Even if the Received View advocates for a quite complicated vision of the evolution of econometrics, due to its eclectic origins, the image they transmit is one that ends up in the best possible manner: with the New Consensus after Haavelmo's contribution. In the sections that follow I will present an image and a synthesis of the vision of the evolution of econometrics transmitted by the Received View. First I will present that image in the form of a diagram, and then I will make an outline of the evolution of econometrics, as presented by the Received View.

In Diagram 1 I will try to show that the process and evolution that appeared to be very complicated at an early stage, progressively finds its way to make a unified whole of techniques and of interpretation: Haavelmo's Probabilistic Approach. The rather confusing beginning of the development of econometrics would converge to one point; it seems that every contribution would point to the achievement of what Morgan calls the New Consensus. Before describing the vision of the Received View in a more detailed way, let me suggest a way to read the diagram and then let me present the diagram itself.

5.1 A Reader's Guide to the Diagram

The central idea of Diagram 1 is to show the general picture of the history of econometrics transmitted by the Received View. I would like to show that, at the beginning, the Received View presents this history as a rather eclectic and chaotic one, which, little by little, gets closer to an ideal synthesis. It is only after the works of the Cowles Commission that econometrics would have attainted this synthesis, notably by means of Haavelmo's 1944 contribution. According to Morgan, econometricians themselves would not have been conscious about the fact that their own contributions were forging the way to such a consensus.

The reading of the following diagram is not very easy at a first glance. That is why I suggest looking at it in two stages. First, I suggest a global look. Just take a step back, look at the graphic from the top to the bottom, and notice that the diagram has the form of a funnel. Do not yet pay attention to the legends that are between the frames. Try, on the contrary, to simply focus on the legends inside the biggest and colourful frames.

Quite bright at the very beginning, the funnel contains a lot of things, contributions, views, and interactions. Little by little, though, the neck of the funnel gets narrower and narrower, until it gets the best possible result at the very bottom, the synthesis of all: the "New Consensus".

Take now the colours into account. Direct your attention to the green frames, situated at the very top and at the very bottom of the diagram. There are only five green frames. Four of them represent the disciplines that were supposed to be separated at the beginning of the period. These disciplines are: "Mathematical Economics", "Probability Calculus", "Statistical Analysis", and "Descriptive Statistics". The last green frame, the one at the very bottom, represents the synthesis and harmonization of the four on the top: "The New Consensus and the Cowles Commission Econometrics".

Now, take a look at the three blue ovals. They represent the research areas that were most relevant for the evolution of econometrics. One of them is less relevant, according to the Received View. It is "Keynesian Economics". While Morgan (1990) studies "Demand Analysis" and "Business Cycles" in more detail, Keynesian Economics only appears tangentially in her book. On the one hand, she just mentions that Keynesian Economics was responsible for the changing of the unit of analysis – passing from the business cycle to macroeconomics – and on the other, that Keynes himself was the leading character of one of the harshest criticisms ever directed to econometrics.

From this point, I would suggest directing your attention to the orange ovals. These represent two of the most important controversies econometrics got to go through. The first, less studied by the Received View, is the "Keynes *versus* Tinbergen" debate. The other, "The Measurement without Theory Controversy" at the right side of the diagram, comes from the only alternative movement to econometrics presented by the Received View: "Statistical

Economics” (in yellow). It is worth noticing that, for the Received View, both controversies contributed to the gestation of the final consensus. The Received View does not present any kind of divergences that would come off these debates.

At this point, direct your attention to the last colour: violet. The violet frames delineate the apparition of some models in the evolution of econometrics. These models are the Formal Stochastic Models (in the early 1930’s), the “Errors-in-Equation-Models”, the “Errors-in-Variables-Models”, and the first “Macroeconometric Models”. They are all presented as important intermediate steps for the development of the more sophisticated kind of models provided by Structural Econometrics after 1944.

The second stage of looking at the graph consists on a more detailed study, where I suggest picking out one of the top frames, and then following the lines all their way down to the bottom. Take for example the green frame “Descriptive Statistics” on the right top of the diagram. Three lines come out of that frame. The first line – a weak and stippling line – goes to the left and unites “Descriptive Statistics” with “Statistical Analysis”. This line recognizes the interrelation between these disciplines, even if they are presented as belonging to different spheres.

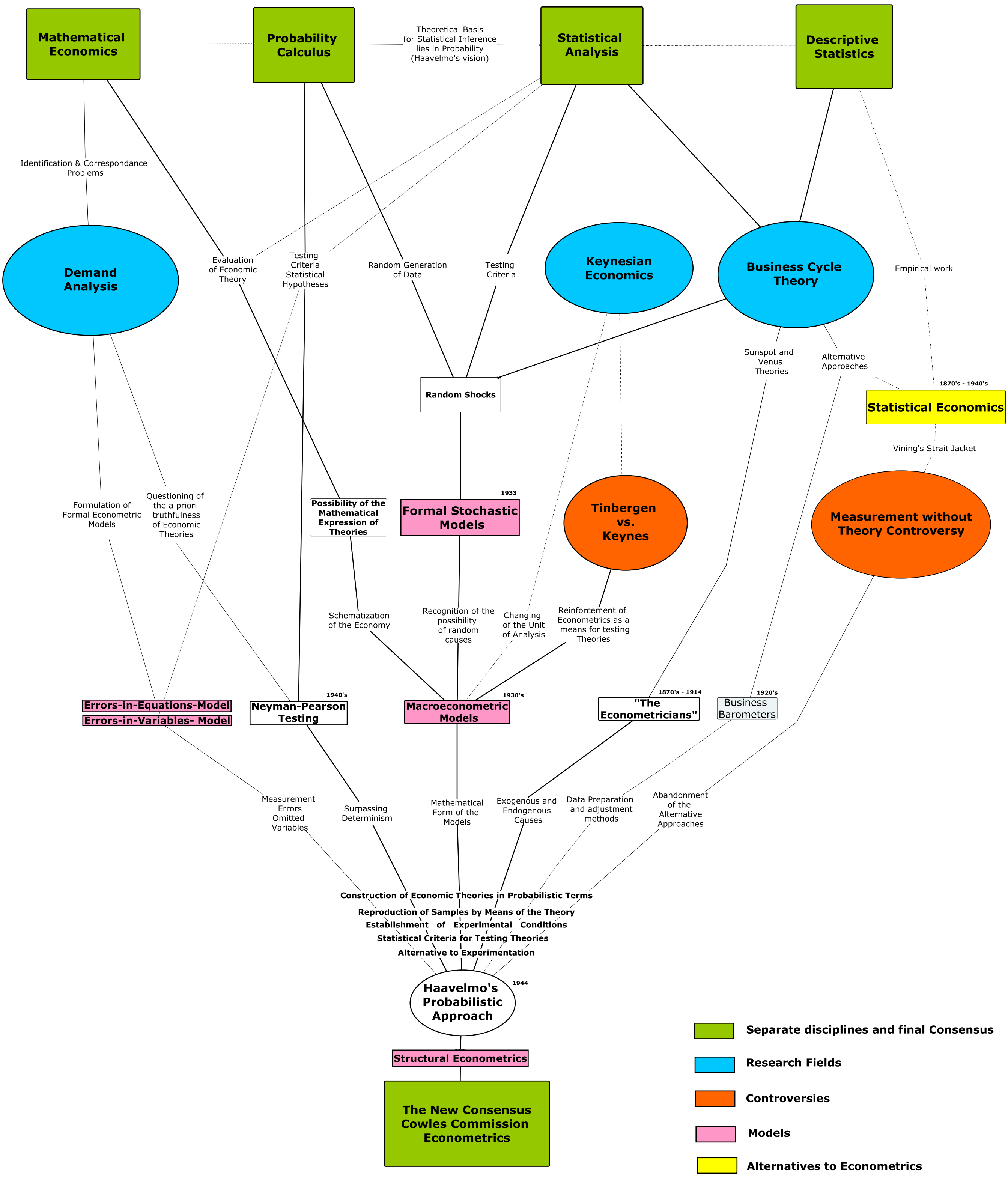
The other two lines, which go downwards, are susceptible of a richer analysis. The first line – thick, continuous and black – goes directly to the research field “Business Cycles”, while the second – stippled and thin – passes through the legend “Empirical Work”, and arrives at the frame “Statistical Economics”. The idea here is that the thick and continuous lines go in agreement with the general guidelines that will, in the end, arrive to the Consensus. The stippled lines, on the contrary, are developments that did not directly go in that direction, and that, in the case of “Statistical Economics”, show a divergent path, moving away from the centre of the funnel, almost coming out of the page.

This first attempt of divergence, however, is rapidly neutralized and redirected to the centre of the funnel. The Received View presents the “Measurement without Theory Controversy” in such a way that this controversy appears to have contributed to the abandonment of alternative approaches to econometrics. Hence, the “Measurement without Theory Controversy” would have “done its bit to help” econometrics to achieve the “New Consensus”.

From this moment on, I invite the reader to follow the reading and interpretation of the diagram in the way I have just explained. Take another green frame on the top of the graph, and then follow the lines until you get to the “New Consensus”.

Diagram 1

The Evolution of Econometrics according to the "Received View"



5.2 The Origins

After the presentation of the diagram the next three sections give a quite detailed description of the evolution of econometrics according to the Received View. First, in this section, I briefly talk about some general methodological problems that marked the origins of econometrics. Then, I pass to the study of the Business Cycles, and finally I describe what happened in the field of Demand Analysis. In these last sections I discuss, following the Received View interpretation, some of the most important contributions to econometrics, developed in these fields of research.

The Received View begins its interpretation by saying that econometricians of the late nineteenth and early twentieth centuries, had to face the entrenched idea that mathematical economics and statistical work belonged to different methodological spheres.

> "[N]ineteenth-century economists believed that mathematics and statistics worked in different ways: mathematics as a tool of deduction and statistics as a tool of induction" (Morgan, 1990, p. 4.).

This idea of separation between the two methods is evidenced by the positions taken by many economists. Cournot as well as Walras and Marshall for instance, were aware of the importance of mathematics in constructing economic theory, while they did not pay much attention to any attempt of introducing statistical notions into their work. On the other hand, the German Historical School and the American Institutionalist Movement for example, emphasized on the importance of statistical work, while they "were often those who most strongly rejected mathematical methods and models in economics" (Morgan, 1990, p. 6).

Besides the impossibility of conciliating mathematics with statistics, economists also had to go beyond the entrenched idea that probability calculus could not be an adequate way of describing economic theory. Nineteenth century economists believed that probability calculus could not be of any help for economic theory, because economics was all about well-determined relations. Probability, on the other hand, reasoned in terms of chance and randomness. That is, according to Morgan, one of the most important contributions of Haavelmo's 1944 paper, which also explains why his approach is most referred to as the Probabilistic Revolution. It can be called a revolution in the sense that it changed the old scientific view of the world, passing from a deterministic to a probabilistic way of looking at it (ibid. p. 261).

Another important characteristic of the interpretation of the Received View, is that practical work played a major role in the development of econometrics. In *Diagram 1* we can see three fields of applied work that were crucial. They find themselves in oval-shaped (blue) frames in the upper level of the diagram: one on the left side: Demand Analysis. The next one, Keynesian Economics, in the middle of the graphic. And the last one, at the very right side: Business Cycle Theory.

Keynesian Economics only appears in a tangential way in the description of The Received View. First, it appears as an important stage for economists to change their unit of analysis from business cycles to macroeconomics. Second, it appears briefly at another source of criticism of the econometrics approach. This criticism, however, disappears very fast with the contributions provided by Tinbergen.

The Received View focuses its attention, then, on the other two fields of research: Demand Analysis and Business Cycles. It is worth noticing that economists had different conceptions about these two research fields. While economists did not understand Business Cycles quite well and hence no theory could satisfactorily explain this phenomenon, Demand Analysis, on the other hand, presented a wide theoretical consensus among economists.

This, of course, changed the way in which econometrics and quantitative methods entered the analysis of both fields. In the case of Business Cycles, data would play a major role as a means for discovering the principal causes and characteristics of the behaviour of the phenomenon. That is why, in *Diagram 1*, this field of research is closer to the Descriptive Statistics frame on the right-upper side. On the contrary, as a theoretical consensus was provided in the case of Demand Analysis, disagreement was present at the level of more technical problems of narrowing the data-theory gap and on identification questions, relegating the role of raw data to a secondary level.

Statistical Analysis entered in both fields of analysis as well. Yet, in the case of Business Cycles, Statistical Analysis only entered in the works of Jevons, Moore and Persons. It did not completely enter into the work advanced by Mitchell and the NBER, though. So, let me continue with the detailed description of econometrics evolution that can be followed to some extent with the diagram. I will begin at the right top of the diagram, more precisely at the frame Business Cycle Theory.

5.3 The Business Cycle Analysis

A first attempt of econometric work is found as early as in the decade of the 1870's with a series of publications by William S. Jevons and in 1914 with Henry L. Moore's *Economic Cycles – Their Law and Cause*. These authors are labelled as "*The Econometricians*", for the Received View recognizes their efforts as "pioneering" (ibid. p. 31) of the modern econometric approach. Both approaches represent a breaking idea with the dominant economic methodology, as both, Jevons and Moore, abandoned the well-established method of introspection and entered into the logic of induction by relying on external data. Despite the fact that the theories defended by Jevons and Moore were largely rejected by their contemporaries, Morgan suggests that the methodological contributions of these authors constituted a major advance, which dictated important outlines for the econometric work to come.

First, Jevons envisaged a combination of endogenous and exogenous causes, which together would determine the movements of the Business Cycles. These reasoning and separation of endogenous/exogenous causes and variables proved to be very influential for Haavelmo's Probabilistic Approach and the Structural Econometrics developed at the Cowles Commission. The endogenous causes were, of course, due to the internal economic forces, while the exogenous causes were marked by the sunspot cycle, which affected the harvest cycle and this in turn the cyclical motion of the economy.

While many contemporary economists were busy studying the crises in a separate way – and did not really pay much attention to Jevons' unhinged idea of the sunspot explanation of the business cycle– Jevons was searching for the existence of an underlying exact cycle. Separate crises were not important. The really significant aspects were the "standard or averages characteristics of data" (ibid. p. 26) from which one could obtain a general explanation of a general cyclical phenomenon.

Moore's paramount contribution, on the other hand, lies in the abandonment of the *a priori* methodology and the *ceteris paribus* reasoning, which consisted in comparing two different static situations. Instead, he was interested in the dynamics of the economy and in the way the economy passed from one situation to another. For the mere exercise of comparative statics would tell the economist nothing about the real dynamics of the economy, which "was constantly shifting and changing like the sea" (ibid. p. 27). In his approach, like in that of

Jevons, Moore assumed a periodicity of the Business Cycle, but his focus was on the "statistical evidence of the economic interactions involved in the business cycle and the statistical analysis of these relationships rather than on the relation between the economic cycle and the exogenous causal factor" (ibid. p. 33), i.e. the movements of the planet Venus.

In general terms, Jevons' and Moore's works were not very well received by contemporaries, in part because their theories stated that the ultimate causes of the business cycles could find their underpinnings in astronomical phenomena, such as the sunspot variations or the movements of the planet Venus, but also because the contemporary alternative method of statistical economics enjoyed of a well credited prestige. Morgan presents this alternative method at the light of the works of Clément Juglar, Wesley C. Mitchell and Warren M. Persons. Although their approaches differed in a considerable way, they shared a common aspect: an empirical approach.

> "[T]hey believed that numerical evidence should be used on a large scale because it was better than qualitative evidence, but their use of data was empirical in the sense that they did not use statistical features of the data as a basis for building cycle theories" (Morgan, 1990, p. 40).

Juglar's theory, which was contemporary to Jevon's work, considered the credit cycle as the main cause of the business cycle. He studied all crises in a separate way and tried to find the fundamental factors that caused them, isolating or ignoring secondary causes. For him, every cycle was unique and presented differences in their lengths and amplitudes. Nevertheless, the sequence of activity was regular, which means that the sequence repeated itself (ibid. p. 43). The repeated sequence was as follows: prosperity, panic or crises, and liquidation or depression. The regularities Juglar found were built up by the "aggregate of individual occurrences" (ibid. p.44) and he supported his theory by counting the repetition of an event, rather than by finding the regularities in the data by means of pure statistical analysis. He did not think that the common features of all business cycles could be found in any form of statistically measurable regularities of the data (ibid. p. 44).

A much more striking form of empiricist work is represented by Mitchell's approach. Considered as one of the founders of the American Institutionalist Movement, his opinions about methodology and economic theory were of a reformist spirit. Although he does not "speak about qualitative versus quantitative analysis" (Mitchell, 1925), his practices showed an emphasis on the quantitative methods. He recognized, however, the importance of the conclusions of earlier business cycle studies, though he could not accept them as defining theories, nor could he completely reject them. This is why he did not make use of Statistical Analysis: for, testing, verifying or refuting theories did just not make sense to him.

> "[N]one of the business cycle theories (…) seems to be demonstrably wrong, but neither does any one seem to be wholly adequate" (Mitchell, 1913, p. 579, cited by Morgan, 1990, p. 46).

The conclusions obtained by other theories should rather be taken as mere hypotheses that would indicate where the researcher should address her analysis when facing the raw data. No preceding theory of the business cycle should condition the study of the data. Nevertheless, this position led Mitchell and his collaborator Arthur Burns to an important difficulty when trying to describe and measure the business cycles.

According to Morgan, in their 1949 book "Measuring Business Cycles" the authors established that "measuring was a prior requirement for testing the theories of other economists" (ibid. p.51). The difficulty then, consisted in trying to measure a phenomenon they had not yet clearly defined. How could it be possible to measure something whose form and content have not been determined and delimited? This problem gave rise to the critical review by Tjalling C. Koopmans "Measuring without Theory" (1947), which ended up with the decisive Controversy of the same name between the two leading institutions of the moment: The National Bureau of Economic Research (NBER) and the Cowles Commission. This Controversy catapulted the "more sophisticated" econometric methods undertaken by the Cowles, while it reduced the great influence that the National Bureau had exerted in the precedent two decades. This is a branch in our diagram that gets very rapidly forgotten by the Received View and that regains attention only until this decisive Controversy[26].

The last of the empiricists presented by Morgan is Warren M. Persons. His work focused on the development of a series of "Business Barometers", which should have had the capacity of forecasting the future behaviour of the economy. These barometers were based on the

[26] One possible interpretation of the fact that Morgan's (1990) presentation of this controversy has to do with the fact that the controversy was actually important for the final imposition of the methods developed at the Cowles Commission. Morgan does not present the controversy in the spirit of the School of Edinburgh. Namely, she does not present the controversy in a symmetrical and impartial way, and so, she does not attribute the same importance to both approaches. Her goal is rather to present the benefits that the Cowles Commission took out of the controversy.

behaviour of certain economic variables and of some arbitrarily chosen stocks. The importance of Persons' investigations for econometrics lies on the data preparation and data adjustment methods that are, at least in a certain manner, still standard in today's econometric proceedings. These methods consisted in "detrending data, (...) removing seasonal variations and (...) smoothing out erratic fluctuation" (ibid. p. 63). It is also worth mentioning that Persons' work represented important contributions to the development of today's economic indicators.

Some other remarkable contributions were the ones made by George U. Yule and Eugen Slutsky in the 1920's. These authors used statistical procedures to generate artificially economic data "under theoretically known and controlled conditions, to provide a standard for comparison with empirical data or to investigate the behaviour of data processes under certain conditions" (ibid. p. 73). Yule proposed a linear difference regression model (of the form $X_t = B_1 X_{t-1} - B_2 X_{t-2}$), in order to simulate the behaviour of economic data. These equations proved to be very influential in further time series analysis.

Slutsky undertook an "as if" experiment to prove his hypothesis that business cycles could be caused "by the combination of random events" (ibid. p. 80) or random shocks. It is interesting to note, however, that Slutsky's aim was to show that "such data generation process could give very similar data to that produced by economic activity" (ibid. p. 82). He was not suggesting that economic business cycles were caused by random events, nor was he saying that business cycle data were produced by random shocks. His suggestion was just that the form of randomly generated data could be akin to the form of data produced by the economic activity. "Slutsky's work not only removed the necessity for periodic cause of economic cycles, (...) but might also, if the cumulative mechanism were understood, make any further discussion of the cause of business cycles superfluous" (ibid. p. 83).

Contrary to most of the preceding authors, Frisch's interests focused on the pure methodology of econometrics, rather than on applied econometrics. His Rocking Horse Model (1933) intended to be a guide for econometricians, while it did not intend to represent a satisfactory business cycle theory. Morgan identifies that the two constituent parts of Frisch's model represented important aspects of the econometric work to come. On the one hand, he developed a system of interrelated equations, which could generate a cyclical behaviour depending on the estimated parameters. On the other hand, he recognized the importance of random shocks that were seen as "real disturbances (...) rather than measurement errors" (ibid.

p. 99). In Morgan's opinion, the idea of a conjunction of a deterministic system with the random shocks to represent business cycle behaviour constitutes one of Frisch's major contributions to econometrics.

This line of evolution ends up with Tinbergen's significant work, which gave rise to the first Macroeconometric Models. For the Received View the works of the 1930's yielded an important result, thanks to a decisive combination of theoretical advances. This result consisted of the combination of the works by Yule, Slutsky and Frisch (just described), the changing of the unit of analysis passing from the Business Cycle to Macroeconomics (after Keynes's influence), and the possibility of expressing economic theories in mathematical terms through the schematization of the economy (provided by the increase of the influence of Mathematical Economics). So, the possibilities for the development of Macroeconometric Models were provided. For the Received View, it was only a matter of time for somebody to come and to make this combination effective. Tinbergen would be the one to materialize this project.

> "Amongst all the econometricians of his day, Tinbergen was ideally suited for such a task [of building a macrodynamic model]. He had been experimenting with small-scale models of the cycle since the late 1920's and was well versed in the ways of dynamic models. He also had a wide knowledge of quantitative business cycle research from his experience as Editor of *De Nederlandsche Conjunctuut* (the Dutch Statistical business cycle Journal). (…) He had taken as his starting point Frisch's (1933) idea that business cycle model should consist of two elements, an economic mechanism (the macrosystem):
>
> > '*This System of relations defines the structure of the economic community to be considered in our theory'* (Tinbergen, 1935, p. 242, cited by Morgan, 1990, p. 101).
>
> and the outside influences or shocks" (Morgan, 1990, p. 101).

Tinbergen's major concern was about investigating and considering the impacts of economic policy. "He saw (...) that a model would only be amenable to policy analysis if it were relatively simple" (ibid p. 102). Following this idea a model only represented a "stylised version of the economic system" (ibid. p. 103). This reasoning sounds very familiar to modern economic analysis, where reality passes to a secondary stage of importance. But it also sounds coherent, as Tinbergen's use of mathematics obliged him to schematize the economy in simple terms. Statistical work would then have the task of evaluating economic theory, not to say whether it was correct, but merely to determine whether it was incorrect or incomplete. This attitude towards the evaluative capacity of statistical theory over economic theory, proved to be very influential in further econometric work, in particular in the works directed by the Cowles Commission.

Many theoreticians rejected the role that Tinbergen attributed to econometrics as an instrument for testing economic theory. For instance, John M. Keynes (1939) addressed a hard criticism on this aspect. Yet, the position held by Keynes is somehow made stricter by the Received View than it actually was. We will come back to this point in a further stage of this work[27]. Lord Keynes "(...) was unwilling to concede to econometricians any role in developing theory and regarded the measurement of already known theories as their only task" (p. 123-124). According to the Received View, Tinbergen was convinced of the critical and innovative nature of econometrics, while Keynes was not. Tinbergen believed that if the results did not confirm the theory then, logically, it followed that theory was wrong or incomplete and had to be revised[28].

5.4 The Demand Analysis and the Data-Theory-Gap

Morgan's focus on the Demand Analysis as the second field of practical work is accurate in the sense that econometricians found a new kind of problems in studying this field. Unlike Business Cycle Analysis, Demand Analysis presented a general agreement of its explanatory economic theory.

[27] It is not completely fair to say that Keynes was absolutely hostile to econometrics. In fact, Keynes was a member of The Econometric Society and was very interested in the progress that this discipline could achieve. There are, certainly, many interesting facts to be explored in this direction, like for example Keynes's notions of probabilities, or the econometrics work undertaken at Cambridge, following Keynes's vision. Two preeminent examples would be the works by Richard Stone and James Tobin. Their positions towards Tinbergen's work (and towards econometrics in general) will be studied in more detail in a further stage of this work, for an analysis of it would largely overpass the pretention of the present dissertation.

[28] Morgan does not present this Controversy in the spirit of the Edinburgh School. Just like in the case of the Measurement without Theory Controversy, Morgan only presents it partially. This partial presentation of the controversies give the impression that "reason" and "accurateness" was at the side of econometricians, and that neither Keynes nor Vining had too much to say about the introduction od quantitative methods in economics. Or, if they had something to say it was wrong and therefore it is just fine to focus on econometrics and not on the alternative methods.

> "The idea that price varies negatively with quantity demanded and positively with quantity supplied [was] a long-established one" (Morgan, 1990, p. 133).

Hence, the problems faced by economists in this field of research were not those of discovering the underlying economic relationships. The questions raised would be rather of identification and correspondence problems, which entailed two main obstacles: (1) the data-theory gap, which consisted in fitting the theoretical models and its measured counterparts. And (2) the identification problem, which consisted, in this specific field, in "the ability (...) [of] identify[ing] an empirical relationship as a demand curve rather than some other relationship" (ibid. p. 135). Demand Analysis brought econometricians' work closer to Mathematical Economics.

If there was such an agreement about the "truthfulness" of economic theories, these could be mathematically expressed. For it would be just a matter of expressing a "true" relationship in a language easily understood by everyone, and more importantly, that would allow economists to treat these relationships in models, and to assign them numerical values.

This possibility of expressing economic theories mathematically had two effects on econometricians. On the one hand, it allowed econometricians to bring about statistical tests to verify theories. On the other hand, it provided a new vision about the Data-Theory-Gap, in which two problems were treated in depth. First, measurement errors, and second the variables omission. Morgan explains the way econometricians initially understood the data-theory gap question citing Mark Blaug in a footnote of her book:

> "Blaug's (1980) survey of methodology in economics suggests that at the beginning of our period, economic theory would be assumed correct and verification would be the order of the day. By implication then, to close the data-theory gap the data would have to be adjusted by the theory. Later, as the verificationist school retreated under the influence of operationalism, the process would reverse." (Morgan, 1990, p. 159-160)

It seems that Blaug's description fits quite well to the behaviour of econometricians relative to demand analysis. As early as in 1914 Moore had already criticised the fact that economic theory was conceived as being static, while data were composed by "single point realizations of demand and supply interactions over a long time period, when other things were not constant" (ibid. p. 143). Moore developed different techniques for clearing the data, which proved to be very influential in later econometric work, but he left the theory untouched. These techniques consisted of a 'single statistical devise' that was nothing else but the use of first differences of the data in order to partially remove or at least correct, the dynamic effects. Another technique consisted in "removing trends by taking 'trend ratios'" (ibid. p. 145).

The realization by econometricians that economic theory could not always be considered as "true" drove them to the necessity of introducing some external criteria for testing the theory itself. It was not only a question of transforming the data, but theory was now becoming more flexible and susceptible of change[29]. Statistical Analysis and Probabilistic Calculus would be appealed in order to get these testing criteria for economic theory. By introducing statistical criteria of the Neyman-Pearson type, econometricians found another problem, as this kind of tests depended on probability calculus. Economists and econometricians in general, according to Morgan and Qin, did not fully accept probability calculus until the late 1940's under the influence of Haavelmo's Probabilistic Approach.

A second and more statistically technical solution appeared as the errors in measurement were taken into account. From this point of view economic theory was only important in order to determine the existence of the relation between two variables. Authors like Gini (1921) or Schultz (1925) (ibid. p. 199), stated that the standard regression model only minimised the effects of measurement errors from the side of the dependent variable. But measurement errors where found in both, the dependent and the explanatory variable, so that another kind of regression should be used in order to overcome this problem. The orthogonal regression or "line of best fit" (ibid p. 221) was then proposed as one type of regression that would take into account the errors in both elementary regressions and so, by "minimis[ing] the shortest

[29] This attitude of a more flexible economic theory susceptible to change is to be understood as new only in the case of the econometric approach. As noted before, the statistical economics approach, where data as such were to play a more important role in the development of economic theory, was from the beginning more flexible and susceptible to changing the pre-conceived economic theory (in the case where there actually was a theory beforehand). John Maynard Keynes too, showed his willingness to remain flexible to theory. G.E. Moore, the philosopher, who thought that no fixed definition of what is "good or bad" really exists, influenced him. G.E. Moore would say in his *Principia Ethica* (1903) that "good" depends on the circumstances in which an issue is to be decided (Meltzer, 1988). Thence, what seems to be "good" or sound in terms of economic theory or policy today, might be completely useless or ineffective in the future. So, this readiness to change theory and the fact of theory become more flexible might have been new for econometricians at that point, but it was certainly not new for economists of other schools.

distances from the observation of the fitted line" (ibid. p. 199), it would represent a better fitted relationship between the variables.

On the other hand, econometricians discovered that the effects of some hidden factors could be also disguised in the errors. This was mainly because econometricians were using simplified models to simulate a more complex economic theory. Errors not only contained the effects of measurement errors, but also the effects of variables that had not been taken into account when doing the regression. Omitting important explanatory variables would conduce to biased parameters. This question raised a number of works trying to find a way to "unravel and find the hidden and intertwined relationships or components in the observed data" (ibid. p. 212). Some of these works were those of *Two Equations Systems* developed by Moore (1914), *Confluence Analysis* proposed by Frisch (1929) and *Causal Chain Models* set forth by Tinbergen (1937).

Although intensive statistical work in economics goes back to the early 1900's, probability calculus did have to wait until the first half of the century for being introduced in the field. Probability calculus did not have any room in the terms economists thought about economic theory, for their reasoning was of a deterministic type. "At the beginning of the century, applied economists believed that there were real and constant laws of economic behaviour waiting to be uncovered by the economic scientist" (ibid. p. 229). In addition, "economic data did not constitute raw material to which probability reasoning could properly be applied" (ibid. p. 230), for economic data was not the result of experiments where the investigator could control all the conditions (Frisch, 1934, cited by Morgan, p. 234).

In short, at the beginning of the period econometricians explained the fitting differences between the observations and the theory in terms of errors made by the researcher or by the regression model (omitted variables) or in terms of statistical problems (measurement errors). Economic theory was supposed to be true. It is worth noticing that even statistical economists, such as Persons, rejected the use of probability calculus. He justified this rejection because he believed that economic observations could not fulfil probability characteristics, for they were not independent from each other. It seems quite clear that Persons' field of work, the business cycles, is all about time-series and inter-temporal relations between variables, which hardly present an independent behaviour.

As correspondence questions and theory testing became more and more important, “econometricians were becoming more sophisticated about matters of inference” (ibid. p. 241). Nevertheless, the economists’ attitude towards probability calculus constituted a contradiction. This contradiction is given by the fact that “the theoretical basis for statistical inference lies in probability theory and economists used statistical methods, yet they rejected probability” (ibid. p. 229).

Haavelmo, together with other workers mainly established at the Cowles Commission, criticised this contradiction in the 1940’s. He argued that independence and homogeneity problems of economic data did not necessarily impede the use of probabilities:

> “(...) it is *not* necessary that the observations should be independent and that they should all follow the same one-dimensional probability law. It is sufficient to assume that the *whole set* of, say *n*, observations may be considered as *one* observation of *n* variables (or a ‘simple point’) following an *n-dimensional joint* probability law, the ‘existence’ of which may be purely hypothetical. Then, one can test hypotheses regarding this joint probability law, and draw inference as to its possible form, by means of *one* sample point (in n dimensions)” (Haavelmo (1944), Preface, p. iii, in Morgan, p. 243, emphasis in the original).

If the difficulties of applying probabilities to economic data were superseded, Haavelmo’s approach would provide economists with an “adequate framework for conducting economic research and rigorous testing of theories” (ibid. p. 243). This new framework, however, would change the way economic theories were to be presented, for they would have to be formulated in a way in which they could represent statistical hypotheses (ibid. p. 243).

Morgan directs her attention to Haavelmo’s contributions in the fields of (1) comparing economic theory to data and (2) theory testing. Haavelmo’s idea on the correspondence problem was that economic theories should be expressed in a logical way, in which

> “observations will as a rule cluster in a limited subset of the set of all conceivable observations, while it is still consistent with the theory that an observation falls outside this subset ‘now and then’” (Haavelmo (1944) pp.1, 2 and 40, cited by Morgan, p. 244).

This idea is more clearly expressed in the form of an example:

> “No economist (...) would want to work with an economic theory that predicted

> that national income would be exactly $X million next year, because it would almost certainly be contradicted by fact. Instead, applied economists prefer to work with the type of theory that predicts that the level of national income next year will be close to $X million. So, though they might not admit to it, (...) econometricians already worked with an informal probability scheme and (...) probability theory merely provides a formal way of specifying such theories" (Morgan, 1990, p. 244)

Haavelmo stated that it was necessary to establish the experimental conditions under which the proposed model would hold, in order to compare theory with data. This means, that the econometrician should describe how to measure a system of "true" variables or objects, which would be identified with their corresponding theoretical variables or objects. Yet, almost all of the economic data came from "Nature's experiments*",* in which case economists are mere "passive observers*"* (ibid. p. 245), who cannot establish the experimental conditions, which gave place to the obtained data. According to Morgan, in saying this

> "Haavelmo *reached to the heart of the fundamental problem of econometrics. Economists are not in a position to isolate, control and manipulate economic conditions: they cannot undertake experiments"* (Morgan, 1990, p. 246, our emphasis).

That is why Haavelmo proposed the following solution:

> "If we cannot clear the data of such 'other influences', we have to try to introduce these influences in the theory, in order to bring about more agreement between theory and facts" (Haavelmo (1944), p. 18, cited by Morgan, p. 246).

The second aspect to which Morgan directs her attention is the possibility of theory testing provided by Haavelmo's system. The new way of constructing economic theory in probabilistic terms, did "not exclude any system of values of the variables, but merely [gave] different weights or probabilities to the various value-systems*"* (Haavelmo (1944), p. 9, cited by Morgan, p. 248). Hence, it was no longer necessary to reject a hypothesis if the facts contradicted the theory "now and then*"*. It was only a matter of which theory would reproduce samples of the type given by "Nature" in a more probable way (Haavelmo, 1944, in Morgan, p. 248). This capacity of reproduction of samples implied a new criterion to compare different theories. In fact, the new probabilistic formulation of economic theory provided the possibility of running statistical tests of the Neyman-Pearson type.

This turn towards "more sophisticated" statistical testing, did prioritize in a way, statistical theory over economic theory. And here we encounter again the second part of the "Measurement without Theory Controversy" of the late 1940's. Koopmans had addressed a double criticism on the National Bureau's programme in terms of their lack of theory when measuring Business Cycles. The first criticism has already been mentioned earlier in this section. It concerned the lack of economic theory when Burns and Mitchell (1946) were measuring Business Cycles. They did not have a well-predefined theory of the cycle and hence they could not know what to measure, according to Koopmans.

On the other hand, the second criticism lied in the National Bureau's lack of statistical theory in order to carry out theory testing. Vining, in defence of the National Bureau's interests, recognized the importance of the introduction of probabilistic calculus in economics. He only did not agree with the "narrow scope" that the Cowles Commission and Haavelmo gave to probabilities.

> "The critical review by T.C. Koopmans of the (...) study by Burns and Mitchell would apparently cast doubt on the efficiency of almost any method of analysis that is not essentially identical with the methods adopted and developed by Koopmans (...). Acceptance of them as the only, or the best, method for reaching economic truth must hinge on results, not on any advance statement (...). Until such evidence is available, they must be considered an exceedingly narrow class of methods, and an insistent appeal to use them, and them alone, as an invitation to put a *strait jacket* on economic research" (Vining, 1949, p. 77, our emphasis).

Vining would defend the postulate that probability calculus should be used in order to help economists in the process of discovery of economic relationships, rather than just in testing some preconceived hypotheses or in generating samples to estimate preconceived parameters (Vining in Vining, R., & T.C. Koopmans, 1949).

But, despite the Controversy and the justified criticism that the National Bureau addressed to the Cowles' methods, or perhaps because of it, Haavelmo's contributions marked the beginning of a new conception of economic theory and of econometric procedure. This is, at least, what the Received View argues. Economic theory was now formulated in probabilistic terms, while econometrics was conceived as *the* method with the capacity to provide economics with an alternative to experimentation. Morgan's last paragraph of the last chapter of her book, expresses in a very neat way what Haavelmo's probability approach means to the Received View:

> "Haavelmo's work marks the shift from the traditional role of econometrics in measuring the parameters of a given theory to a concern with testing those theories. He had argued that if a theory were treated as pregnable – rather than as an unchangeable truth – it could be treated as a hypothesis about a probability distribution, and the non-experimentally obtained data could be considered a sample from this distribution. This formalisation of the problem allowed applied economists to be more flexible in their attitude to theory, since any chosen hypothesis might be incorrect and an alternative model correct. By laying out a framework in which decisions could be made about which theories are supported by data and which are not, Haavelmo provided an adequate experimental method for economics" (Morgan, 1990, pp. 257-8).

A final commentary could be of help, which describes the lower part of diagram 1: the combination of different factors and the eclectic contributions rendered possible the formulation of econometric models and the introduction of probability notions. These factors like the possibility of the mathematical expression of economic theories; the study of identification and correspondence problems found in the Demand Analysis; and the development and fit of Statistical tools for testing theories and estimating parameters, provided the theoretical and material conditions for econometrics models to appear.

6. Can Econometrics be considered a Style of Reasoning?

Before we start with the development of this section, it is important to make a substantial note. As we have stated in the introduction, citing McCloskey (1988), we do not intend to study whether the history of econometrics fits to a certain framework proposed by any Historian, Philosopher, or Sociologist, whatsoever. For this kind of comparison would lead us to a "thin" analysis of the history of econometrics. So, why are we asking whether Econometrics can be considered a Style of Scientific Reasoning?

We are asking this question because it seems to us that the Received View has, certainly unconsciously, provided the general public with an interpretation of the history of econometrics that is closely related to the styles-of-reasoning framework. In their defence, it is important to clarify that the authors of the Received View never mentioned the possibility of considering econometrics as a style of reasoning. They do not even use Hacking's or any similar framework in their study. So, my assertion is quite risky and very provocative, and can also be considered exaggerated. But, in my opinion, this is a risk worth taking, because it can lighten some important facts about the history of econometrics.

For the point here is that the Received View contributes to the presentation of Econometrics as a standard for objectivity. This means that econometrics is presented so, that it gives the impression that, when talking about it, we are talking about a timeless, neutral, and objective tool, that provides economists (or any other scientist) with the possibility to analyse facts, phenomena, data, etc., in a rigorous and scientific way. In fact, we will argue that the Received View interpretation of the history of econometrics attempts to contribute to the presentation of econometrics as a sort of style of reasoning for economists. This is not an exclusive task fulfilled by the Received View, however, but by numerous eminent economists, economics professors, historians of economics, economists-practitioners, and institutions[30].

In defence of the Received View, again, we could say that these authors make a huge effort to recognize the extreme complications that early econometricians had to face in order to develop what we finally know as Structural Econometrics. They certainly did a huge effort and a great job in this matter. But the thing is that once they have presented this highly complicated and convoluted evolution, they present Haavelmo's 1944 paper and the research undertaken at the Cowles Commission as the "philosopher's stone" that would solve all the old problems and would relaunch a new programme, free from its previous vices. The new programme, armed with its own creational myth, self-authenticating techniques, and with a long-run stability lasting until today, would resemble something similar to what Hacking defines as a Scientific Style of Reasoning.

[30] In fact, the way in which econometrics is used nowadays by almost every economist, contributes to the construction of this idea that econometrics is a timeless, neutral, and objective tool that can be used in academic, practical or educational work without thinking too much about its history or special conditions as a canon of objectivity. Econometrics is just taken for granted by a great majority of them. Though, there might be no consciousness about this particular use or about the fact that they are contributing to the construction of such a view about econometrics.

Such a presentation and interpretation of econometrics as a style of reasoning implies, of course, many problems: if econometrics is presented as a style of reasoning that would have emerged almost exclusively from inside economics, then we would be forgetting the importance of a great number of sciences and disciplines that played a decisive role in the development of some of the most important constitutive pieces of econometrics. Such constitutive pieces are: descriptive statistics, inferential statistics, mathematical economics, probability calculus, modelling, biometrics, psychometrics, eugenics, etc.

This particular presentation would also make us ignore the existence of the Statistical Style of Reasoning. Hacking introduces the statistical style of reasoning as one that would entail probability calculus, descriptive statistics and inferential statistics altogether. This grouping of the three disciplines would not be free from tuff struggles and debates, however. Hacking recognizes that there were certainly many discussions within statistics and mathematics about the harmonic use of the three disciplines. Nevertheless, as early as in 1844, Quetelet had already imported probabilities into the statistical analysis[31], something that, according to the Received View, was not achieved in economics until 1944, hundred years later. This importation would have rendered possible the using of the statistical style, combining statistics and probability calculus. There was, however, no such a use of the statistical style in economics, at least not until 1944, according to the Received View.

Another problem of this presentation is that econometrics would constitute *the* standard for objectivity in economics. Economists, thus, would take this standard for granted, without reflexion whatsoever of what the econometrical results actually yielded, or without reflexion about the provenance of the objectivity canons, which would appear to be "naturally" given. The interests that once lied behind the development of these methods would be forgotten, as well as the ideologies and the individuals that rendered econometrics possible. And once all these things have been forgotten, the style "becomes fixed as a new way for truth, it needs no support or rhetoric, for as it assumes self-confidence it generates its own standard of objectivity and its own ideology" (Hacking, 1992, p. 132).

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[31] Hacking states that:

> "Quetelet's move in 1844 was a decisive advance for the statistical style of reasoning, because it created discourse about a new class of entities and their measurements. This discourse could not exist without the importation of probabilities and the Gaussian law of errors. Had it not been for this move, there might be no such thing as Crombie's 'the statistical analysis of populations and the calculus of probabilities'. There might have been two distinct things, statistics and probability" (Hacking, 1992, p. 149).

Diagram 1 of chapter five can also be useful in order to clarify this point. This diagram recreates the vision of a very complicated process of development at the beginning of the period that is marked by many-sided contributions coming from all kind of economists – from the most Orthodox, to the more Heterodox, European, European Immigrants, North American, or even Soviet economists. This very complicated process, however, starts to clear up itself little by little and leaves way to the "synthesis" and the New Consensus, which provides a new standard of objectivity. But the following question raises: if there is a new standard of objectivity, one that nobody discusses and that has been "cleaned" of its previous social, political, and historical "imperfections" and "stains", then, why worry about them? Let us just forget about them. This is precisely the opposite spirit of this work.

For what preoccupies me is this perception of econometrics as a tool that does not move, that has always been there and that has solved its most deep methodological and epistemological problems for good and for all. Paraphrasing Hacking, what worries me is the Braudelian look that we – economists and economic historians – have given to econometrics. "Styles are Braudelian" (Hacking, 1992, p. 153) and if we have given a style-like appearance to econometrics, then econometrics is Braudelian too. But, what does it mean and what are the implications of styles being Braudelian?

> "The Braudelian aspects of science [are] the long-term slow-moving, persistent, and accumulating aspects of the growth of knowledge. Braudel, in caricature, wrote of the Mediterranean as a Sea around which nothing much happens besides shifts in climate and topography; the chief effect of civilization in Greece was to turn a forested peninsula into a rockheap" (Hacking, 1992, p. 130).

Economics and econometrics, even if they sometimes appear to be Braudelian, are certainly not like that. Not for the Economic Historian, at least. We actually would like to emphasize that econometrics have been given this style-of-reasoning-look by the views brought by many economists, historians, practitioners, books, manuals, reviews, etc. But this is no more than a look, an appearance. For econometrics is far from being the standard of objectivity deprived of a history or a past.

We want to recover the essential role that historical, social and political factors do play in the development of any scientific discipline, and make clear the fact that the implementation of any style of reasoning (even if it is not econometrics) defines just *one* standard of objectivity.

It does not define *the* standard. For there can (or could) be many other standards of objectivity. In fact, standards have changed throughout time and will change in the future.

Ours can be considered a claim to reduce the optimism about a given standard of objectivity and to recognize its limitations as well as its social, historical, and political origins, which make of these standards changeable ones. But most importantly, it can be considered a claim that wants to vindicate the plurality and the possibility of a greater number of canons of objectivity and scientific standards, always recognizing that they are not neutral, not timeless, and that they have a history and a past that should be interesting and useful to scientists (in our case to economists), in order to better understand the bases that lie behind our ways of reasoning.

6.1 The Received View Interpretation

As we have showed in Diagram 1 in chapter 5, the Received View attributes to econometrics the harmonization of four major disciplines, which had been considered hitherto incompatible. These were mathematical economics, probability calculus, inferential statistics, and descriptive statistics. This process of harmonization could be studied following the idea of Peter Galison taken out from linguistics, cited by Hacking (Hacking, 2002, p. 182): the development of a new language by means of a *trading zone*. This notion supports the idea that

> "(…) a new language develops, for purposes of trade and social intercourse, at the interface between two established languages. The trading zone idea will be useful in the study of styles of reasoning when we begin to describe any inquiry that employs several styles. It is often not the case that a single investigator is at home in more than one style of reasoning. Instead, there is collaboration in which a person expert in style X makes use of a handy robust core of techniques from style Y" (Hacking, 2002, p. 184).

This trading zone between many languages, styles or disciplines, can quite accurately suit the economics' methodological situation in general[32], and it can, in particular, quite well describe

[32] It would not be taking too high a risky at all if I say that an important number of styles of reasoning have coexisted in the economists' minds since the times of Smith, Ricardo, and Marx, until today. Even if some styles have dominated the scene during different periods, while others have lessened their influence, economists have always helped themselves with the tools at hand provided by a

the situation during the first half of the twentieth century, when econometrics was developed. At this point, there was a huge concern from the part of the economists to render their discipline more "respectable" and "scientific". What it means for a science to be respectable and scientific is a matter of taste, beliefs, prejudices, social acceptance, and, of course, debate. But, to some extent, some economists understood that their respectability and scientificity lied in their capacity to approach economics to the queen of sciences: physics[33]. And to approach physics meant, at that time, to leave aside this tradition of making literature-based economics, and pass to the introduction of more "serious" and "objective" methods, such as the quantitative methods.

> "For the econometricians of the first half of the twentieth century, the union of mathematics and statistics with economics was the ideal way to practising scientific economics" (Morgan, 1990, p. 2).

But, there were various problems concerning the introduction of these methods. One of them has already been treated earlier in this work. Economists had the impression that these four disciplines were incompatible. Yet, another problem, not treated until now, comes into scene. This problem has to do with the impossibility of undertaking experiments in economics.

If experimentation would have been possible in economics, this could have been another way of rendering economics more scientific. But, even if the experimental style of reasoning had

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variety of styles. Taking Crombie's list as reference, one would not be able to deny that the method of postulation exemplified by mathematical sciences, the hypothetical construction of analogical models, the ordering of variety by comparison and taxonomy, and the statistical analysis of regularities of populations, and the calculus of probabilities, have been present almost at all times in economics. The deployment of experiment, both to control postulation and to explore by observation and measurement, would have been quite recently introduced, but it also makes part of the available styles of reasoning for economists. The laboratory style of reasoning of which Hacking speaks would also have been recently added to the list. The modelling style of reasoning proposed by Morgan (2012), on the other hand, can be put in the first list, for it has been available for economists since a great number of decades.

[33] It suffices to see the terms in which Koopmans and Vining held the discussion on the *Measurement without Theory Controversy*, in order to grasp the idea of what it meant to be scientific at that time. Both authors take physics and astronomy as the standard that would allow to make comparisons and to identify the "stage of development" of economics. Whether economics still found itself in the "Kepler Stage of Development" or whether it had already reached the "Newton Stage of Development" was a matter of debate. The fact that physics was the standard towards which every science should converge was in no way controverted. Economics should introduce quantitative methods in order to become a respectable science. But this introduction could not be in whatever way. It had to be following the model of physics. On this topic see Mirowski (1989).

already been used by many other sciences, it was just not possible to implement it in economics. And hence,

> "(…) economists seeking a more scientific profile for economics regularly bewailed the fact that the experimental method available to the physical sciences was inapplicable to economics" (Morgan, 1990, p. 9).

So, the impossibility of introducing experimentation in economics hindered economists to render their discipline more scientific (at least for the contemporary imaginary of what scientificity meant). But, again, economists needed to render their discipline more scientific, in order to gain respectability and to better understand the economy. What could they do? Morgan (1990, p. 9) suggests that economists used the new statistical methods in order to substitute the experimental method.

> "The idea of a scientific or controlled experiment is to reproduce the conditions required by a theory and then to manipulate the relevant variables in order to take measurements of a particular scientific parameter or to test the theory. When the data are not collected under controlled conditions or are not from repeatable experiments, then the relationship between the data and the theoretical laws is likely to be neither direct nor clear-cut" (Morgan, 1990, p. 9).

So, Haavelmo's 1944 Probability Approach provided economists with the accurate tool that would render unnecessary the undertaking of experiments. And so, economists at the Cowles Commission

> "(…) saw themselves as armed with very powerful, precision-engineered tools based on the probability approach (in comparison with the previous tools of econometrics fashioned in the bronze age) which they believed would solve a host of econometric problems" (Morgan, 1990, p. 251).

The statistical methods allowed economists to "extract regularity or repeated pattern, or constant relationships, from a mass of data (instead of taking one observation from an individually manipulated or varied event)" (Morgan, 1990, pp. 9-10)[34].

[34] Morgan recognizes, however, that economists were not always conscious of what they were doing. They did not know that they were searching for a substitute to experimentation. This interpretation is, then, rather Morgan's reconstruction of the history of econometrics.

Hence the harmonization between the four disciplines brought into being econometrics: a mixture between the statistical and the mathematical styles of reasoning, which would have the capacity to replace the experimental style of reasoning, impossible – or unimaginable – to introduce in economics at that time.

We have stated that styles are characterized by a creational myth. The creation story of every style is told in such a way that a myth comes into being. Historians and practitioners forget about the difficulties that a style had to face in order to become a standard of objectivity and in order to become stable. A person, or a tool, or an institution (why not?) is taken as the crucial event that marks the creation of a style of reasoning. The word myth is here used in the sense that the importance of the single person or event is exaggerated or idealized.

So, the Received View has created its own myth about the creation of econometrics. It is Haavelmo's 1944 paper *The Probability Approach in Econometrics*. In this case, the myth becomes the method developed, rather than Haavelmo himself. But Haavelmo plays a crucial role, at least as a member of a selected group of elite economists all working together at the Cowles Commission. His work, according to the Received View, would be the turning point that would mark the way econometrics should continue its path from that point on.

> "*Haavelmo's 1944 paper marks* the end of the formative years in econometrics and the beginning of its *mature period*. *His treatise set out the best methods for the practice of econometrics* and explained the reasoning behind these rules. His novel ideas on the role of probability *pointed the future way for econometrics*, but in many other respects the concepts and approach of Haavelmo's programme were firmly rooted in the past. In his hands, the individual practical solutions and insights generated by the earlier work were *fully fitted together as in a completed jigsaw puzzle, showing one single econometrics applicable to all branches of economics*" (Morgan, 1990, p. 259, our own emphasis).

There can be no doubt that Haavelmo's paper was of great importance and that his work as well as that of the Cowles Commission proved to be very influential. But to say that it "marks the mature period of econometrics", to call his paper a "treatise that sets out the best methods

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"It would be wrong to suggest that, in doing so, econometricians were consciously making their own substitute for experiments, rather they responded to the particular problems of measurement and control, the particular mismatch between theory and data, which occurred in their applied work. Only later did they begin theoretical discussions and seek general solutions to their practical problems" (Morgan, 1990, p. 11).

for the practice of econometrics", that "pointed out the future way for econometrics", and that was able to "fully fit together all the individual solutions" that had been developed by tens of economists, statisticians, mathematicians, and econometricians, seems to be a little exaggerated. It also seems to be a very idealized vision of the paper. This is what myths do. They exaggerate and idealize events.

But the dangerous part of making such an assertion is that, if we would just passively "take the bait, the hook, the line, and the sinker", completely obeying the hidden message, then, there would be no interest in studying the history of econometrics before Haavelmo. If Haavelmo said everything what had to be said, and set the best possible methods for econometrics, then studying the history of econometrics, apart from Haavelmo, would make no sense at all. It is as if the Received View wanted to tell us something like this: "Haavelmo embraces everything that you have to know about econometrics. If there was something else before 1944 that could have been interesting (in order always to arrive to Haavelmo), the Received View of the History of Econometrics has already said it. So, do not bother in studying the prehistory of econometrics".

This assertion is not only discouraging and pretentious. (And I must say, I might, again, be exaggerating the attitude of the Received View with the intention of being provocative at this point[35]). This assertion is also part of the self-authenticating-strategy of econometrics. Not only econometricians and practitioners would take econometrics for granted and as a standard of objectivity. The work of historians would also do its part in this task. Do not ask whether there were some problems before, during or after Haavelmo, for he has already solved them and we have studied those imperfections and stains.

Last but not least, there is still another important point to mention here. Even if one could say that econometrics is actually the application of a broader style of reasoning (namely, the statistical style of reasoning) the Received View has defended the idea that the objectivity criterion is provided by econometrics itself and not by statistics. Even if they repeatedly recognize that econometrics is more about statistics than about any other discipline (like

[35] None of the authors of the Received View has ever pronounced such an assertion, and I am sure that they would not agree with me in this point. On the contrary, I am sure that they would promote the study of the history of econometrics in all the periods. But, again, I am exaggerating and I am pulling the Received View to an extreme position, in order to make my criticism more clear.

mathematics[36]) the standard of objectivity would be given from within econometrics, not statistics.

The Received View treats econometrics as a depending, but separate discipline, different from statistics. And they are right in doing so. Nevertheless, when one uses the styles of reasoning framework, things can be seen differently. We can understand econometrics as the application of the statistical style of reasoning by economists. This would imply a canon of objectivity coming from statistics and not necessarily from econometrics. This is, in fact, the second criticism that Vining addressed to Koopmans in the Measurement without Theory Controversy (see section 5).

My point here is that a style of reasoning, or a standard for objectivity, does not stabilize itself only by internal self-authenticating techniques. There are other, rather external, self-authenticating techniques that play an important role in the trajectory of a style to becoming autonomous and stable. In this case, the Received View of the History of Econometrics plays that role of stabilizing factor and self-authenticating technique.

6.2 Our own Interpretation

In the end, the story told by the Received View is one that helps to consolidate the idea that econometrics produces some kind of objectivity. This Story helps to increase the feeling that once econometrics was "created", it has always been there, acting as a standard for objectivity, even if the contents of economic theory have changed and continue to change. The problem with this interpretation is that it would produce a biased and clouded concept of econometrics for both econometric students and econometric practitioners, who are not devoted to methodological and historical work (which is the great majority).

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[36] On this point see Morgan:

> "The most fruitful place to start such a search might well prove to be amongst the statistical thinkers, for the content and evolution of the econometric programme in its formative years was much influenced by developments in statistics. And in the long run, mathematical economics and statistical economics divided again, leaving econometrics firmly on the statistical side of the fence" (Morgan, 1990, p. 7).

And then she continuous in a footnote:

> "Although the econometricians aimed to synthesise mathematical and statistical economics, it is not clear whether mathematical models were necessary for the successful application of statistics to economics" (ibid. p. 7).

If the general belief implicitly presents econometrics as something equivalent to a style, then very few will feel the necessity of digging deeper into the history of econometrics to discover that it is not a neutral tool and that its objectivity is of a complicated kind. Furthermore, econometrics will be used mechanically, and thoughtlessly, without paying attention to the epistemological tenets that lie behind it, or to the actual origins and primary uses of statistical and mathematical tools, not originally developed for (or within) economics.

My interpretation is rather different, but it is very simple. I do not think that econometrics is a style of reasoning in itself. Rather, I would say that econometrics makes use of other styles of reasoning such as the statistical, the modelling, and the mathematical styles. This, of course, says anything new and does not resolve any problem.

However, in my opinion, the importance of this claim lies in the vindication of the study of the history of econometrics, as one where cultural, social, political and theoretical aspects matter. And, more precisely, I claim the importance of the study of the history of econometrics, using a framework like Corry's[37]. For such a framework, provides us with an important aspect for better understanding the continually moving and controversial nature of a discipline like econometrics: the interactions between the body and the image of knowledge.

I also call for a more careful understanding of the application of econometric methods in all-day economics. And, finally, I claim that such a framework allows us to understand the position of the Received View as one way in which stability is provided to econometrics, and hence, it makes of the Received View a subject matter of study under the lens of "philosophical technology" (see section 3.6 and Hacking, 2002, pp. 197). This is to say that the Received View becomes a stabilising technique of econometrics, worth of being studied using Hacking's philosophical technology.

7. Conceptual, Theoretical and Methodological Complementarities

After having defined the Received View in the precedent chapters, we turn now to the analysis of the conceptual, theoretical and methodological complementarities between the two books. I will try to establish some common traits between both authors, which will be helpful

[37] This is, of course, in no way a claim for exclusive use of Corry's framework. There are many other frameworks there to be used.

in order to identify a joint vision about the evolution of econometrics. This common or joint vision, will be reflected in a series of concepts and images about these concepts, all of them treated in a separate section. In the end, chapter seven will hopefully allow us to complete our understanding of what is the image of the Received View in regard to the history of econometrics.

7.1 Econometrics, Scientificity and Theoretical Flexibility

Morgan and Qin utilize some concepts in a complementary way. For both authors Econometrics was the key discipline that allowed economics to become "economic science", through the introduction of "scientific means and methods" (Qin, 1993, p. 1). Econometrics also provided an alternative method to experimentation, which rendered the economists' profession more respectable[38].

Furthermore, Haavelmo's approach rendered possible the formulation of economic theory in a new way. This new way of formulation allowed economic theory

> "(…) to be treated as an hypothesis about a probability distribution, (…) [where] the non-experimentally obtained data could be considered a sample from this distribution" (Morgan, 1990, p. 257).

This treatment of economic theories provoked a change in applied economists' attitude towards theory. Their attitude became more "flexible (…), since any chosen hypothesis might

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[38] The *respectability* that the quantification of economics brought to economists and econometricians remains quite unclear and unexplained from the side of the Received View. No clear cut argument is mobilised that would explain and convince us that the quantification of economics would render this science more respectable. It is not clear, for example, whether a highly mathematized sort of economics would necessarily lead to a better understanding of the economy or of the economic theories. Quite the contrary. One can find for example contemporary reviews of books and articles that would complain about the exaggerated use of mathematics in economics (see for example E.B. Wilson's Review of Haavelmo's 1944 article).

> "There is a small group of econometricians who are well trained in mathematics and who apparently choose to write for one another rather than for economists (or even econometricians) in general" (Wilson, 1946, cited by Armatte, 2012, p. 21).

The advanced reasoning remains at the flat and simple idea that quantification is "good", for it increases rigour and scientificity. But there is no solid explanation behind the desirability of quantifying. It seems to me that it is just desirable, because of a matter of beliefs, and maybe because of a matter of being well regarded by colleagues from both economics and other sciences. Hence, it seems to me that understanding the image of knowledge of the time can be of great help at this point.

be incorrect and an alternative model correct" (ibid. pp. 257-58). Both authors, Qin and Morgan, defend the idea that there was a gain in economists' flexibility towards theory.

Nevertheless, this is one of the points that remains problematic and worth studying. Since, it is possible that once a body of knowledge for econometrics was established, it endured in a quite stable form and the way of doing econometrics became rigid again. Here, it is interesting to make a differentiation between the *fathers* (or the founders) of the discipline such as Haavelmo, and the *disciples* (or the practitioners) that came later on.

The difference between them lies, mainly, in their beliefs. Whereas the founders are more concerned with methodological, theoretical, semantic and philosophical questions about econometrics, practitioners are more concerned with the viability, operationality and practicality of econometrics. This means that, once a method has been established and institutionalized, practitioners will take it and use it, concentrating themselves on the practical sphere of the method. They will very often forget about the original questions posed by the founders, as well as about the essential characteristics of the method in question[39].

This resembles what we have said in *Part I* about the styles of scientific reasoning. Whether the style of reasoning was econometrics – as the Received View could argue - or whether it was statistics – as we would – the important thing here is that when time passes by, once the style has been introduced, once it has established a creational myth, once it counts with self-authenticating techniques, and once it is stable, practitioners and even theoreticians (which sometimes are the same persons) forget about the essential questions and problems that enjoyed of a quite high status in earlier stages of development of the method.

7.2 An Internalist View and a Notion of Progress

The approach of the Received View is of a rather internalist nature. This approach leads the authors to ignore some important aspects of the social, institutional and historical context, but also of the interaction between econometrics and other disciplines and sciences. They do not take into account, for example, the role that the WWII period and the afterwar period played in the development of econometrics. And so, they forget about the military objectives of

[39] We will scrutinize this rigidity further on under Vining's (1949) proposition about putting a *straight jacket* to economic methodology.

control and planning that were introduced in economics and in many other sciences (see Mirowski, 2002).

They also overlook the interactions and the influence that other sciences exerted over economics and econometrics. Even if Morgan recognizes the importance of biometrics and psychometrics in influencing the development of some of the econometric methods, she does not study the interaction between these disciplines and econometrics[40]. She rather concentrates on the internal efforts that econometricians were forced to do, in order to render the statistical methods suitable to econometrics.

Moreover, the influence that physics exerted over the development of economic theory or econometrics is barely mentioned. Qin refers to Ménard (1987) and Mirowski (1989a and 1989b), when trying to explain why it was so difficult for probability theory to enter into economic analysis. She says that "a[n] (…) antagonistic force came from theoretical economics, where there prevailed a deep-rooted deterministic viewpoint under the influence of nineteenth century mechanical physics" (Qin, 1993, p. 10). But that's it. She does not develop any further analysis about the relations between physics and econometrics.

The Received View retains a certain vision of progress too. This vision of progress, combined with the internalist approach, results in a particular reconstruction of the history of the evolution of econometrics. This particular reconstruction shows, on the whole, a quite logical and straightforward growth of econometrics. As we will see later, both authors defend the idea of a passage from an immature but creative stage of development of econometrics to a more stable mature stage. This passage was due to the Probabilistic Revolution provided by Haavelmo's (1944) article, which "freed" economics and econometrics from its "deep-rooted deterministic viewpoint" (Qin, 1993, p. 10). Furthermore, Morgan and Qin suggest a particular kind of progress in their presentation of how econometricians faced the data-theory gap problem:

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[40] Morgan recognizes the parallel development of other disciplines based on statistics in other sciences, but she does not study the interaction between econometrics and these other disciplines in more detail:

> "When we look at the history of statistics we find that econometrics was far from an isolated development, for statistical methods and probabilistic thinking were widely introduced into nineteenth- and early twentieth-century science. Few scientific fields remained unaffected, although statistical thinking and probability did not always enter at the same level in each discipline, as I have helped to argue elsewhere. The econometrics movement was paralleled in particular by biometrics in biology and psychometrics in psychology (…) and all three developed roughly at the same time" (Morgan, 1990, p. 7).

> "We see that (…) [econometricians] developed three formal models of the way in which observed statistical relationships might correspond to their expected theoretical relationships. Each model provided an explanation of the data-theory gap and a rationalisation of the approximate, rather than exact, fit found in measured economic laws. One model explained these approximations in terms of measurement errors: the errors-in-variables model. Another explanation rested on variables omitted from the measurement equation: the errors-in-equation models. The third model provided *a more general explanation* of the relationship between empirical results and economic theory by treating the theoretical relationship as probabilistic." (Morgan, 1990, p. 193, our emphasis)

In a more local sphere, however, some subtleties are to be retained. As we have already emphasized Morgan and Qin focus on the individuals involved in the construction of econometrics, rather than focusing on the economic schools of thought or on the history of the method itself. This focus reveals, at a local sphere, again, a somewhat unclear construction of econometrics, characterized by errors, drawbacks and dead-ends.

> "It might be argued that we should look not to the history of economic methods but *to the history of the people*, *the economists themselves*, in order to understand where econometrics came from" (Morgan, 1990, p. 5, our emphasis).

> "My prime impression of the history is that most important econometric advances have resulted from *ingenious* interfusion of statistical ideas with economic thinking, in regard to applied problems of interest" (Qin, 1993, p. 184, our emphasis).

But, again, this focus on the history of the people is only directed to the contributions that this people could have done to the evolution of a body of knowledge and, hence, this analysis remains at a rather internal level. For the contributions that they talk about, would always arrive to the New Consensus proposed by Haavelmo and presented in diagram 1 in chapter 5.

7.3 Optimism about Haavelmo's Probabilistic Revolution

This somewhat internal approach not only makes the authors overlook some of the aspects we mentioned, but it also leads the authors to make a quite optimistic claim. Their optimism lies in the interpretation they give to Haavelmo's *The Probability Approach in Econometrics* (1944) as being the basis of a "Probabilistic Revolution", which would have changed the way econometricians and economists looked at the world.

Before 1944, so they say, economists, and particularly econometricians, looked at the world through a deterministic lens. This means that econometricians believed that economics was all about well-determined relations, and so, "there were real and constant laws of economic behaviour waiting to be uncovered by the economic scientist" (Morgan, 1990, p. 229). On the contrary, after Haavelmo's (1944) paper, econometrics and economics were "freed" (Morgan, 1990, p. 8 and p. 260 and Qin, 1993, p. 1) from this deterministic view, favouring a probabilistic approach, which improved the process of inference in econometrics.

This vision is problematic for two reasons: (1) it suggests that before 1944 there only existed a deterministic view of the world in economics, and (2) it also suggests that after 1944 all the econometric work based on the Cowles' structural equations got rid of determinism. The story is, however, of a far more complex nature.

On the one hand, before Haavelmo, there was much more in economic thinking than just a deterministic vision of the world. For instance, the American School of Institutional Economics (to which Morgan refers at the beginning of her book) did in any way have a deterministic view of the world, even if they did not make use of probability calculus. Additionally, there was also Keynes whose non-deterministic view of the world can be seen in his willingness to changing his theory (as many times as it was required) if the conditions demanded for it[41].

Saying that economists and econometricians only saw the world in terms of well-defined relations oversimplifies a bit too much what was happening back then. It seems that Morgan

[41] Keynes's reasoning and theorizing was of an evolutionary nature: his views about the world were not static and changed depending on the particular circumstances (Meltzer, 1988). His readiness and willingness to change are explicitly conscious and constitute, perhaps, some of the main legacies passed on to Keynes by philosopher G.E. Moore at the beginning of the twentieth century in Cambridge. No fixed definition of what is "good" or "bad" really exists. Moore would say in his *Principia Ethica* (1903) that "good" depends on the circumstances in which an issue is to be decided (Meltzer, 1988). Thence, what seems to be "good" or sound in terms of economic theory or policy today, might be completely useless or ineffective in the future. Furthermore, as Backhouse and Bateman (2010, pp. 15-16) state, Keynes was aware of the fact that economic theory should evolve with time and he was not concerned about the changes and arrangements that other authors such as Hicks, Meade or Harrod were introducing to his theory. Keynes's vision is in no way a deterministic one.

and Qin arrive to this conclusion because they take probability calculus as being the key element that marks the differentiation between having a deterministic view or not. This argument seems shaky, for probability calculus is neither a necessary nor a sufficient condition to escape determinism.

On the other hand, Morgan's and Qin's view remain problematic, because it is not clear that the introduction of the probability calculus after Haavelmo's (1944) article provided a framework for economists to escape determinism. The authors of the Received View take it for granted that the change in the body of knowledge of econometrics would automatically induce a modification of the image of knowledge of the scientific community. This modification, if there was one, is more complicated than that. The deterministic or the non-deterministic view of the world is an element of the images of knowledge rather than one of the body of knowledge[42].

Certainly, changes in the body of knowledge can produce modifications in the image of knowledge. But this does not happen automatically. Changes in the body of knowledge can generate the possibility of thinking differently about the world[43]. Those changes provided by Haavelmo can be understood as the tool that allowed econometricians to think in non-deterministic ways. But this change in the body by itself cannot be considered as the only factor responsible of a bigger change in the discipline. The bigger change has to be studied, not only within the body, but also under the lens of the changes in the image of knowledge.

The Received View does not take seriously into account, the fact that other things different from the theoretical framework should change, in order to produce a new kind of theory. We have the possibility of studying these other kinds of factors, in the form of circumstances, facts, events, etc., that affect the image of knowledge of the scientists. What we can say until now is that even if there was a real and important change in the body of knowledge of econometrics, this was not sufficient to modify econometricians' image of knowledge in such a way that determinism could be completely overcome. We hope that we will be able to discover which were the factors not allowing the image of knowledge to precipitately turn towards an essentially non-deterministic view.

[42] But even if the body were modified, it is important to evaluate the nature of this modification. One should differentiate between the *epistemological* and the *ontological* kinds of uncertainty that probability calculus is supposed to represent (on this see Armatte (2012) and Davidson (2010)).

[43] See Hacking (2002) and the way the styles of reasoning create the possibilities of thinking about the world in a way that was not possible before under the regime of another style of reasoning.

7.4 From a Creative to a Mature Stage of Econometrics

For the authors of the Received View, Haavelmo's paper marks the passage from a creative phase of econometrics to a mature phase. They understand this passage as if economists had abandoned the idea of discovering new underlying principles between the variables by fine-tuning econometric models. Economists were now armed with a model that provided a protocol and a series of rules that should be followed. Hence, a consensus of the way econometrics should be carried out was attaint after 1944: "The New Consensus" (Morgan, 1990). This consensus consisted in the adoption of very specific rules providing a procedure about how to test hypotheses, how to estimate parameters or some directions about how economic theory should enter the econometric analysis.

Yet, the authors suggest that the 1944 paper's influence surpassed econometrics and actually had an important impact in the whole of economics. According to them, economic theorists were now compelled to express their theories in a probabilistic way, which was amenable to econometric and quantitative analysis.

This consensus contrasts, however, with the argument about the liberation of determinism discussed before. It will be interesting to study the relation between these two phenomena. On the one hand, econometricians were provided with a model and a protocol of how to do econometrics. On the other, they had presumably overcome determinism and were flexible towards theoretical propositions. How is this relation to be understood? Was there actually such flexibility in theoretical terms?

What is somehow problematic here is that the Received View holds a notion of creativity and maturity very similar to that held by Koopmans in the Measurement without Theory Controversy, without questioning it. Even if the Received View's interpretation is not as extreme as is Koopman's, the fundamental idea is quite the same.

In his attack, Koopmans makes an analogy between economics and astrophysics, and compares the NBER procedure with the methods used by Kepler in the very early stage of measuring the position of the planets.

> "It appears to be the intention of Burns and Mitchell – in any case this is the opinion of the present reviewer – that their book represent an important

> contribution to the 'Kepler stage' of inquiry in the field of economics" (Koopmans, 1947, pp. 161-162).

Hence, the only contribution of the NBER economists would be that of measuring the "empirical regularities" of the economy, rather than the more Newtonian task of discovering the "fundamental laws" underlying them. This last task would though, be fulfilled by the Cowles Commission's approach. Somehow, econometrics would have reached maturity with Haavelmo's 1944 paper, for it would have ceased to look only for empirical regularities and it would have provided the passage to a more mature stage, which would combine both the Kepler and the Newton stage. Vining explains Koopmans position in the following way:

> "(…) Koopmans seems convinced that without our Kepler we have witnessed the emergence of what will pass as a first approximation to or a supporting framework for a Newtonian phenomenon. Some of his discussion suggests that we have already at hand a theoretical model that is a sort of social counterpart of Newtonian mechanics. But this is not asserted; rather, he argues that a way must be found (or has been found) to perform the Keplerian and Newtonian tasks together" (Vining, 1949, p. 79).

Even if Morgan and Qin do not use the same grid of comparison in terms of Kepler and Newton stage, they held the idea of econometrics passing to a mature stage after Haavelmo's paper.

> "Haavelmo's 1944 paper marks the end of the formative years in econometrics and the beginning of its mature period. His treatise set out the best methods for the practice of econometrics and explained the reasoning behind these rules" (Morgan, 1990, p- 259).

7.5 The Eminence of the Cowles Commission

The fact that both authors, Morgan and Qin, emphasize on the role played by the Cowles Commission in the construction of econometrics should be listed in a list of external-institutional factors, rather than here, in the methodological, theoretical and conceptual factors. Nevertheless, the way in which the role of the Cowles is presented by both authors corresponds more to the logic of the internal factors.

The discussion of the role played by Cowles can be labelled as an internal factor, because the authors of the Received View do not discuss the events that were at stake at the Commission in terms of the political interests, military strategies, funding provenance, etc. (see Mirowski, (2002) for a further discussion of the events happening at that time).

On the contrary, the importance that they attribute to the Cowles Commission is related to the methodological and theoretical framework that this important group of economists and thinkers provided during the 1940's in terms of econometric modelling. No real importance is given to the Commission as an institution, that represents different kind of interests and that follows its particular goals. The Cowles Commission is treated just as a neutral place, where may brilliant economists came together and started working on economic problems. The result, by 1944, was a paper that defined the rules of how to make "good" econometrics[44].

Furthermore, Morgan does not take into account the fact that the dominance of the Cowles Commission meant a reduction in the heterogeneity of the contributors in the construction of econometrics. At the beginning of the period people involved in the construction of econometrics were of a rather eclectic nature. This spirit of heterogeneity is present, for example, at the first Editor's Note of *Econometrica*, by Frisch in January 1933:

> "Complete freedom of thought will rule in the columns of ECONOMETRICA, and candid discussion on the surveys or on other material appearing in its pages will always be welcome. To judge from the stimulating discussions that have taken place both at the European and the American meetings of the Society, there is no danger of inbreeding of thought amongst econometricians. It is true that those attending the meetings have shown a veritable enthusiasm for the common cause, econometrics. But, together with this community in general interest and attitude, there has been manifest a variety of ideas and a frankness in mutual criticism that guarantee the broadness and freshness of the future work. This vigorous spirit will, no doubt, be reflected in the columns of ECONOMETRICA."

[44] Haavelmo's paper not only introduced the probability calculus into econometrics, but it also provided a guide of the way how model hypotheses should be tested, parameters estimated and models identified.

(Frisch, 1933, p. 4)

The eclectic spirit was lessened, however, at the end of the period studied by Morgan when the Cowles Commission takes the lead of econometric research. This decline in the heterogeneity supposes the facilitation of the establishment of an "orthodoxy", not only in econometrics, but also in economics as a whole. This is an aspect of the *Measuring without Theory Controversy*[45], which is not mentioned by Morgan at all.

Morgan only refers to the rejection of theory by the National Bureau's workers, notably by Mitchell. She does not say anything, however, about the kind of theory that Mitchell and his fellows were opposing to. Mitchell opposed to *Neoclassical Theory*, not only in methodological terms, but he also opposed to the theoretical and even to the political neoclassical positions[46].

As Mirowski (1989b) argues, econometrics played its part in the battle between Economic Schools, in order to become dominant. The development of econometrics cannot be separated of this contention, as if its development had been neutral towards economic thinking. The Cowles Commission, even if it embraced a great number of immigrants and economists of a diverse background, clearly opposed the Institutionalist School. The latter was far more influential at that time than it is nowadays, and so the development of a powerful tool (like econometrics) that would convince other economists, politicians and scientists in general of the superiority of the Cowles' methods, would put Neoclassical Economics at the top again. The stab-wound produced by the Measurement without Theory Controversy, together with the death of one of the most influential figures of the Institutionalist Movement, Wesley C. Mitchell, prepared the ground for the expansion of econometrics throughout the economic

[45] We have already sketched the main points of this controversy in earlier passages. For a study of the original controversy see Vining, R., & T.C. Koopmans (1949). 'Methodological Issues in Quantitative Economics'. *The Review of Economics and Statistics*, pp. 77-94.

[46] It is clear that the definition of the term "Neoclassical School" is quite problematic, vague and polemic. For the purpose of this work we adopt Mirowski's general notion of the Neoclassical School, which consists mainly of the works of authors using the *marginal utility approach*, such as A. Marshall, L. Walras or W.S. Jevons. It remains very problematic, however, to try to conceal the thinking of these authors in terms of theory or politics. Hence, it would be prudent to say that the main author to which Mitchell was opposing was Alfred Marshall, whose work was the most influential, at least during the first decades of the twentieth century, in the Anglo-Saxon world.

science[47].

Conclusion

Part II is devoted to the study of the Received View of the History of Econometrics. It begins, in chapter four, with the presentation of the main contributors to this view: Morgan and Qin. There are two important facts worth mentioning here.

On the one hand, there is the proximity of Morgan and Qin, during the gestation of their books. This proximity not only consists of the fact that they knew from each other's works or that Morgan was a member of the jury that evaluated Qin's Ph.D. thesis, neither does it only consist of their geographical proximity. There is much more behind these facts. For instance, David F. Hendry, and other LSE econometricians, seem to have played an important role in the interpretation of both Ph.D. theses.

On the other hand, the own academic formation and early practices of Qin and Morgan are interesting too. Both were involved in econometricians' practical work since their early years as economists. In a way, these two facts give, of course, the feeling that Qin and Morgan knew (and know) very well the tools they were talking about, which increases the credibility and rigour of their works. Nevertheless, and this is where our methodological framework enters the scene, their own formation, their academic, institutional and cultural context, as well as their tasks as econometricians themselves, has certainly shaped their images about econometrics. The study of all these factors will be part of a further stage of this work.

So, we are dealing here with what we have called the problem of reflexivity, following Boumans and Dupont-Kieffer (2011), presented in section 2.4 of this work. The authors of the Received View were embedded in a particular way of looking at econometrics. This was a way proper of econometricians and not necessarily one of "pure" historians. This means that it was econometricians themselves who were writing the history of econometrics. As I have

[47] The Measurement without Theory Controversy was, of course, not the only factor responsible for the decline of the Institutionalist Movement. Additionally to the strengthening of Neoclassical and Keynesian Economics as well as the growth in the criticisms against Institutionalists coming from everywhere (like for example Homan (1932), Bain (1944), Koopmans (1947) or Friedman (1953)), there are also internal dynamics of discussion and separation that played their part and contributed to the weakening of this group. There was at least a threefold separation between Institutionalists since the 1940's (see Rutherford 2013).

already mentioned, this is convenient in a way, because of the first-hand knowledge of the tools and methods they are talking about. It is problematic, however, because it can lead to a quite internalist approach of the history (which in fact occurred), and can give way only to one particular – and perhaps biased – view about econometrics.

Chapter five discussed the presentation of the history of econometrics by the Received View and criticised it by labelling it as "the Funnel vision". Here, the chapter is composed by a quite long analytical work of the evolution of econometrics, according to Morgan, which in the end, always leads – like a funnel – to Haavelmo's probability approach. It does not matter, for example, whether there were two paramount debates between "the econometricians" and other Schools of thought, like in the case of the controversy between Tinbergen and Keynes, or that between Koopmans and Vining. The Received View presents both controversies in a short way. Additionally, their interpretation of these controversies is of a rather optimistic character in favour to econometrics, while the alternative approaches are not given the importance they merit, and so, they are not treated in an impartial way, as would be suggested in Edinburgh.

Then, chapter six discusses whether econometrics can be considered a style of reasoning, and if so, what would be the implications of this consideration. I claimed that the Received View, certainly in an unconscious way, *contributes* to the consideration of econometrics as a style of reasoning in the mind of economists, econometricians and historians. The problem with this interpretation is that, in the end, econometrics *appears* as a neutral standard of objectivity, deprived of a history and so, deprived of a complex and heterogeneous origin.

The Received View contributes then to present econometrics' origins in the form of a myth, exaggerating and idealizing the contribution of one person, one method and one institution. The Received View would, in my opinion, exaggerate the role played by both Haavelmo and the Cowles Commission.

Note that the present work does not suggest that the Received View actually forgets about the heterogeneous character of the origins of econometrics nor does it suggest that it forgets about its history. The point here is that the final presentation (the idea of the funnel), *contributes* to the imaginary of econometrics as a neutral and objective tool. The Received View has of course dealt in a very extensive, analytical and rich way, with the history of econometrics.

The argument of this work claims that their particular interpretation of the history contributes to an existing image of econometrics, which concentrates on internal factors. This image is provided and reinforced by the way econometrics is used in all-day economics and by the way it is taught in most universities around the world. Econometricians and economists not interested in a deep study of the history of econometrics will keep in mind the image of the hero of the myth – Haavelmo's Approach or Haavelmo himself. There is, however, much more about the history of econometrics that econometricians (and economists) would be willing to know.

It is at this point where I would like to insist on the further study of the history of econometrics taking into account, not only Hacking's styles, but also Corry's images and body of knowledge. For, it is the interaction between images and body, which would render possible the understanding of the continuously moving nature of disciplines such as econometrics: a discipline, which has a complex history, full of social interactions, political struggles and even economical interests.

The last chapter (Conceptual, Theoretical and Methodological Complementarities) is important, because it reinforces both the funnel vision presented in chapter five and the style-of-reasoning-interpretation of econometrics (implicitly) provided by the Received View. This last chapter presents some of Morgan's and Qin's most essential complementary concepts that have given this funnel-shaped image of the history of econometrics. These include the notion of sciencifity and progress, as well as their internalist approach, their optimism about Haavelmo's contribution, the division between a creative stage and the mature stage of development of econometrics, coinciding with the period before and after the Cowles's devotion to econometrics.

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Not the history, but... a history

The history of a discipline cannot be considered as being only one. Quite the contrary, the history of a discipline is a working process constituted by an ensemble of histories, some of which are accepted at a certain epoch, some of which are not; some of which are told, some of which are not told; some of which are imagined and conceived, and some of which never even come into existence.

Thus, history, as a field of study, cannot be considered as static or unquestionably true. In the short term, however, history can appear to be quite a safe terrain, stable and "true". Its stability will depend on the establishment of a particular series of conditions and techniques, which will make it endure. Yet, in the long run history is subject to change.

It is not, of course, that the facts and events that occurred during the past will change. They already happened and they cannot be changed. It is rather the changing of the actual ways of thinking, which conditions our possibilities of reflecting about past events. This change consists of the emergence of other possible ways of interpreting history.

The fact that there would be other possible interpretations to write about the history of a discipline has nothing to do with subjectivism, though. It is not a matter of a historian having a partial interpretation with a subjective point of view. Furthermore, it is not the case that the meaning of history would be dependent of the evaluation of distinct human beings who perceive history in a different manner. For history, again, is not some kind of entity waiting to be uncovered. It is rather a working process.

The point here is that there is no true history that would be considered as true or false by a particular historian. On the contrary, paraphrasing Hacking, "[history] is either true-or-false but thinking makes it so" (Hacking, 2002, p. 160)[48]. So, it is the ways we have to reason about something which make that thing (in this case history) a candidate for truth or falsehood. And these ways to reason about something go beyond the individual historian. They depend on the acceptance and use of certain ways of thinking by a given scientific community at a given time.

For Hacking, it is styles of reasoning which constitute these ways of thinking. And we follow him in that assertion. But the existing images and bodies of knowledge at a given epoch

[48] In fact Hackings quote is as follows:

> "(…) nothing's either true-or-false but thinking makes it so" (Hacking, 2002, p. 160).

influence these ways of thinking too. For they also make possible the emergence of different interpretations of history.

The blooming of the works in economic history, economic philosophy and economic methodology, to which Hands (1990, 2001a, and 2001b) refers, constitutes a changing in the ways we look at the history of science in general, and at the history of econometrics in particular. This changing, expressed in the emergence of a plurality of methodological frameworks for economic methodology[49], constitutes the breeding ground for the growth of new possibilities of interpreting the history of econometrics, in which the Received View is only one possible history among many others. So, there is no such thing as *the* history of econometrics, but there are *many* histories of econometrics.

Writing history: a matter of imagining, reasoning and interacting

Studying and writing history involves a lot of work. Not only practical, but also intellectual work, like reasoning and reflecting. But it is more than just that. It is an activity embedded in a social and cultural sphere, full of human interactions. Talking, and discussing, and interacting are fundamental actions for the historian. But also imagining and inventing makes part of the exercise of writing history.

Writing history is like telling a story. In fact these two words, history and story, are more closely related than they appear. Even if they do not mean exactly the same, telling stories constitutes an important part of studying and writing history. For if someone is willing to write history, she has to create that history in her imagination and then find a way to tell it. So, writing history involves imagination, practical work and social interaction. Thus, culture, reason and society make an important part of writing history.

So, the historian has to create her own image in her head about the history of her object of study. But, the object of study is not the only factor that will affect her image, for it is not only historical factors, which affect the construction of such an image. There are other factors, cultural, social, political, and philosophical, that certainly affect the creation of that history.

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[49] Some of the methodological frameworks that make part of this plurality are for example, those provided by the Edinburgh Strong Programme, the Rhetoric of Economics by McCloskey, Feminist Epistemology, New Pragmatism, the Economics of Scientific Knowledge, and so on. On this, see Hands (2001a, 2001b).

For historians are people embedded within a particular context in which they find themselves exposed to the direct influence of these factors. This is, in short, what the present research intended to do: to take into account the historical, philosophical, social, cultural and political factors that could have affected the historians' own image about the history of econometrics.

With that objective in mind, the present work set up a methodological framework allowing the researcher to take all these factors into account: the image/body of knowledge and the styles of reasoning. The accuracy of these methodological frameworks lies in the fact that they allow the researcher to take into account not only historical factors, but also philosophical and social factors. The image/body framework allows focusing on the social factors, while the styles of reasoning allow focusing on the philosophical factors. But both frameworks allow the collaboration between the historian, the philosopher and the sociologist of science. Their work necessarily becomes complementary with the use of these frameworks.

Last but not least, these frameworks provided an accurate way of doing a historiographical work, which was the principal focus of the present dissertation. But this historiographical work was of a particular nature. For it considered the writing of the history of econometrics as essentially human, and so, embedded in a social and cultural context, bringing into the analysis important philosophical concepts. And, again, the use of the body/image of knowledge and of the styles of reasoning allowed the researcher to take into account all these kinds of factors.

What about the history of econometrics?

Historians of econometrics are not an exception and so, they also find themselves affected by these kinds of factors. The writing of the history of econometrics is also embedded within a cultural, social and historical sphere. This insertion within a wider context affects the ways in which econometric historians rely on, and so, they influence their vision about their object of study, about econometrics.

This is the case of the Received View of the history of econometrics. Immersed in a social, cultural, scientific and historical context back in the 1980's and 1990's, the writers of this history yielded a particular view of econometrics. Its peculiarity resides in its internalist approach, marked and directed by the close relation that historians held with econometricians and with the practice of econometrics itself.

The principal conclusion of this work is that the Received View of the history of econometrics is just one possible view among others. But, more importantly, this work stated that the criteria that shaped the interpretation of the Received View are not entirely subject to the historian herself. Rather, they are given by the image of knowledge that a scientific community holds about a certain discipline in a certain epoch. The body of knowledge and the ways in which historians (and scientists) think about the history of econometrics also affected the history told by this view. Furthermore, the social interaction with a particular scientific community (econometricians at the LSE) also marked these interpretation criteria.

In the case of the history of econometrics we find an important interaction between the historians and the econometricians. This interaction certainly exerted an influence over the historians willing to write about econometrics. First it was Mary S. Morgan who was appointed to study the history of econometrics. A young economist, economic historian and applied econometrician graduated from the LSE some months before, Morgan started her Ph.D. thesis on the history of econometrics in 1979 under David Hendry's supervision. It is hard to imagine a relationship between these two scientists, in which the well-known econometrician would not have exerted an important influence over the young economic historian.

Then, it was Qin Duo who embarked on the historical investigation about econometrics. As she states is her book, her research was a complement to Morgan's, and so their working and writing was quite close. Qin was also exposed to an important influence from the part of econometricians. In fact, between Morgan and Qin it was Qin who continued working to a greater extent in econometrics after the publications of their books. It suffices to have a look to her courses and papers in order to see that she remained quite attached to practical econometric work.

This close collaboration with econometricians, but also the fact that the historians were econometricians themselves, certainly shaped the image of knowledge they had about econometrics. We find here what Boumans and Dupont-Kieffer (2011) call the problem of reflexivity in the history of econometrics. It is a history viewed from the inside of econometrics, interpreting its history by means of the image of knowledge of a certain epoch

(Boumans and Dupont-Kieffer, 2011, p. 32). This particular image of the 1980's jugged the historical internal factors as more essential, describing the evolution of econometrics as a "funnel-shaped-process" (see diagram 1).

The problem with the exclusive spreading of this funnel vision lies in the fact that it constitutes a way of shaping the body of econometrics in a determinate way. As *Part I* of this work states, the image of knowledge exerts an influence over the body of knowledge, determining the questions that should be asked and responded, and deciding which should be the guiding principles that would pilot the discipline.

So the Received View, being the most expanded vision about the history of econometrics, contributes to the creation of an image of econometrics that overvalues a particular vision. It contributes to the perpetuation of the idea about econometrics as a tool whose history is marked by a trace of progress, by a creative and mature stage of development, and by a major event: Haavelmo's 1944 synthesis and the birth of structural econometrics.

Our aim was to show that this view is just one among others. There are other ways of thinking about the history of econometrics and there are other images of knowledge today as well, which would affect the vision we have about the evolution of econometrics. Both, the images and the ways of thinking change the interpretation criteria and create the breeding ground for the emergence of new possible histories.

Nevertheless, one cannot be too optimistic and one has to be aware of the fact that today's image also biases our own interpretation. There is no doubt about that. Today's interpretation criteria are also shaped and biased by an image. But this should not be that problematic. Today's image encourages the plurality of interpretations, and these could complement and enrich the Received View's interpretation, even if they all present a bias of some kind.

The objective is not to deny the Received View of the history of econometrics. It is only to give that view, as well as any other and ours, their correspondent places: they are just one possibility within a wider spectrum of complementary, though sometimes contradictory, views about the same object. And it is this complementarity and even this contradiction, which enriches the vision of the history of econometrics. For it encourages the debate, rendering the images of knowledge more dynamic and changeable, as well as the body of knowledge and the ways of thinking.

Nevertheless, we encounter another problem here. This work started with the statement that econometrics is one of economists' most popular tools. Its popularity lies mostly on the touch of scientificity and seriousness that econometrics gives to economics and to economists using that tool. Most economic policies are suggested following an econometric model of some sort, and so econometrics is supposed to be a clear-cut and neutral tool, capable of providing accurate criteria to take any kind of decision.

If this work, together with many others, states that econometrics presents a history biased by social, philosophical, cultural, and historical factors, then its credibility and more importantly, econometricians' credibility could be weakened. For this vision about econometrics would reveal its social, historical and human character. It would show its neutrality and robustness just as the product of an image. An image, partially produced by historians, which shaped the econometricians' vision about the history of econometrics as having the form of a funnel, which seems to be quite complicated and eclectic at the beginning, but which little by little finds its way to get at the best possible result: "Modern Econometrics".

What comes next?

Every history of econometrics is subject to a certain bias. For every historian of science is embedded within the social activity of writing histories. However, if the biases shaping historians views about econometrics are so eclectic and so different, then it might be possible that the history of econometrics will be finally recognised as a working process, and not as a field that has already been written.

One possibility opened by the Received View itself, invites researchers to study some of the authors, controversies and main events treated by the Received View in another way. Haavelmo and the Cowles Commission should still have an important place in the study of the history of econometrics, but this time they could be regarded under the light of another magnifying glass; a less powerful one, maybe. This less powerful magnifying glass would show, not only the history of the methods and techniques, but other things that were happening in the hectic social and scientific context of the epoch. But most importantly, this magnifying glass would not "magnify" that much the role that Haavelmo and the Cowles Commission played in the development of econometrics.

Both controversies, to which the Received View referred, could get another look too. These are the "Measurement without Theory Controversy" and the "Keynes *versus* Tinbergen" controversy. The researcher could leave aside the emphasis on the effects that both controversies had on the construction of the New Consensus and modern econometrics, in order to concentrate on the developments that the followers of the "defeated" sides of these controversies certainly tried to continue.

For instance, a further scrutiny of the works of the Institutionalists after Vining's defeat would be worth studying. Economists belonging to that school of thought did not just give up their methods in order to join the econometrics approach. They continued, at least for a while, to use the methods originally knew as statistical economics. It would be a way of enriching the history of econometrics, to know what really happened to these practices, and whether they exerted a further influence over other research fields in economics. The study of the works in quantitative economics followed by some Keynesian economists and their vision about econometrics would open up new possibilities of interpretation too. Some suggestions here are constituted by the study of the works by Richard Stone and James Tobin, for instance.

Last but not least, the study could be also directed to some other econometricians who worked very closely with the Cowles Commission, but who showed, apparently, a different vision about the methods developed at that institution. The works of Lawrence Klein might provide such an alternative vision about the methods of the Cowles, from within the Cowles itself.

The former constitute only a few suggestions and hints that could mark a possible way to carry on the study of the history of econometrics. However, there might be tens of hints like these, all of which are valid and worth pursuing. The principal idea here is to realise that there is an infinite spectrum of possibilities of writing the history of econometrics and so, that this spectrum can be expanded and explored. Its expansion depends on the ways of thinking that historians have at hand at a given epoch.

As Hands (1990, 2001a, 2001b) showed, there has been an explosion in the ways of thinking available for historians in the last two decades. Thanks to this explosion, economic methodologists promote the plurality of visions and methods. So, let us take the most of this explosion of plural methodological and historical works, and let us take a bunch of new looks to the history of econometrics from within other perspectives. The involvement of historians with other ways of thinking in the study of the history of econometrics will open up the possibility of expanding and exploring the infinite spectrum of possible histories of econometrics.

Until now, econometricians themselves have been in charge of writing the history of econometrics. And, as we have seen, this fact has conditioned our vision of that history to the image of knowledge that econometricians have about their own discipline. In a way, this conditioning has been enriching, for it has provided a technically well-informed survey of econometrics' evolution. But it can be the right time now to open up new views that would value other aspects of this history, different to those already studied.

Thus, the history of econometrics remains a rich field of research, waiting to be explored. Its richness, basically, resides in two aspects. First, there are still many contributions, periods, authors and institutions to be studied. Second, those who have been already studied can be reanalysed taking into account new approaches and emphasising on new elements and factors. Both alternatives could yield fascinating results, which would complement the existing histories, but most of all, which would perhaps contradict them, generating debate. And that is, precisely, what science and history are all about: debating, conversing, thinking, reanalysing, and reinterpreting. Then, rewriting.

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