ECONOMISTS WRITING HISTORY: American and French Experience in the mid 20th Century

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ABSTRACT

If one considers the fortunes of economic history in the 20th century U.S., the 1940s, 50s and 60s stand out as a particularly vibrant time for the field and economists' contributions to it. These decades saw the creation of the main association and journals - the Economic History Association, the *Journal of Economic History* for example – and the launching of large research programs – Harvard's history of entrepreneurship, Simon Kuznets' retrospective accounts, cliometrics for example. Why did American economists write *so much* history in the decades immediately following WWII, and why and how did this change with cliometrics?

To answer these questions I use interviews with scholars who were active in the mid 20th century, their publications and archival material. The bulk of the analysis focuses on the U.S., yet it relies in part on a comparison with France where economic history also experienced a golden period at this time, though it involved few economists. Instead it was the domain of *Annales* historians. This comparison sheds light on the ways in which the labels "economist" and "historian" changed meaning throughout the period of study.

Economists' general interest for history is best understood as a part of an ongoing debate on scientific method, specifically about whether and how to observe and what constitutes reliable empirical evidence. These debates contributed both to draw social scientists to history, and change the way they wrote history. In the U.S. the mid 20th century surge in *economist*-history was principally due to the post-war demand for knowledge about growth and development. The sense of urgency that came with this task increased scholars' willingness to work with estimated (as opposed to found) data. This was reinforced by American economists' experience in war planning and ensuing spread of an operations research mentality among graduate students. The issue of whether or not to estimate became a new demarcation line between "historians" and "economists". By the late 1960s, scholars who wanted to turn to the past to observe economies evolve over several decades, and let these facts "speak for themselves" had largely been replaced by researchers who used modern economic theory to frame historical investigation, and relied on quantification and estimation as their main empirical inputs.

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I would like to thank teachers and students at the London School of Economics for their wisdom, encouragement and passion. Among them both Mary Morgan, my advisor, and Tiago Mata, my friend and colleague, provided the critical distance, the conversations and the motivation to get things done, while retaining a sense of discovery and relevance. The excitement of engaging in research was also kept alive by the scholars I met in the past four years, at conferences, seminars, in archives, or as I interviewed them in their living rooms. Paul David and Malcolm Rutherford were the most precious of these acquaintances, and their willingness to hear regular updates of my work provided great encouragement. Yet many others threw sparks to my fire with their questions, their curiosity, or their generous sharing of time, documents and recollections – in particular I wish to thank the men and women who responded to my request for interviews and gave me the benefit of the doubt.

While I did do some thinking, this thesis was also an exercise in logistics and coordination –more so than I could have imagined when I first set out. Without the help of archivists in France and the U.S., the LSE's willingness to subsidize travel to conferences, generous scholarships from the Rockefeller Archive Center and the Hagley Library, and precious help from Philippe Fontaine and Eric Brousseau in France, I would have had access to much less information than our digital world would suppose.

None of this would have been possible without the love and support of friends and family. I am blessed with parents and siblings for whom curiosity is a way of life and my friends are a constant source of laughter and inspiration. I give special thanks to Shelley Lawrence de Rouvray, Amy de Rouvray and Jonathan Bruck for their unconditional support.

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Paris, March 2005

To my parents and their parents
for giving me a “room of my own”.

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CHAPTER 1.

INTRODUCTION: ECONOMIST-HISTORIANS IN THE MID 20th CENTURY

1. "Economist-historians"

The main institutions - associations, journals and committees - for economic history in the U.S. were founded in the 1940s and 1950s, just before young economists who called themselves "cliometricians" surfaced in the 1960s bringing data estimation, modeling and econometric techniques to historical questions such as the profitability of slavery, the contribution of railroads to 19th century growth, or medieval English peasants' rationale for scattering their plots.[1] This so-called "cliometric revolution" unfolded in a broad rhetoric that suggested that cliometricians were the first American economists to tackle historical questions (see chapter 2). This tended to obscure the fact that the upheaval came in the wake of earlier initiatives – creation of the Economic History Association (1941), of the *Journal of Economic History* (1941), of the Harvard Research Center in Entrepreneurial History (1948), of *Explorations in Economic History* (1948) and of Simon Kuznets' restrospective national income accounts (1949) – all of which were spearheaded by economists who, for one

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[1] Conrad and Meyer (1964); Fogel (1964); McCloskey (1975). Stanley Reiter, a mathematician and economist coined the word "cliometrics" in the early 1960s. He held an appointment at Purdue University, in Indiana, where he interacted with young economists who had an interest in economic history. The term was meant to embody their shared commitment to quantification, as it was the combination of "Clio" – the Greek muse of history – and a suffix denoting measurement. For a more detailed investigation of this term and the scholarship it denoted, see chapter 7.

reason or another *wrote* history. Thus cliometrics can be seen as one facet of a much larger mid 20th century phenomenon in the U.S. that drew numerous economists to economic history. In other words, cliometricians were one of many groups of American *economist-historians*.[2] Seen from this perspective, we may wish to know why American economists wrote *so much* history in the decades immediately following WWII, and why and how this changed with cliometrics?

The 1940s, 50s and 60s stick out as unusually eventful decades for economist-history, compared both to events that came before and after this time. Economic history's mid 20th century golden age certainly contrasts with the situation today. Scholars familiar with contemporary economics and economic history have argued that it is only a question of years before economist-historians disappear from the American (and perforce international) economic scene.[3] As an indication, a quick survey of the top-ten graduate programs in economics shows that over 50% do not list economic history as a required course for their Ph.D. students.[4] It also contrasts with the situation in the late 19th and early 20th

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[2] I will use this term to refer to economists who investigated past events while retaining their identity as economists.

[3] The argument is never this simple, and is usually in the form of: "unless we do this…economic history will continue losing favor among economists". See for example the collected essays in Field, Ed. (1987). For a more recent example, see a 2004 initiative at the Center for Economic Policy Research in London, where economic historians are urged to write more quantitative and "presentist" economic history of Europe: http://www.cepr.org/Research/Initiatives/EH.htm

[4] The top ten include: Harvard, Chicago, Stanford, MIT, Princeton, Yale, U. C. Berkeley, University of Pennsylvania, Northwestern and Minnesota. Ratings tend to be controversial, but most agree that these 10 universities would be on the top 20 list in any system of rating and weighting, Thursby (2000).
Harvard does not require its graduate students to take economic history (http://post.economics.harvard.edu/graduate/requirements.html); neither do Chicago (http://economics.uchicago.edu/about_lit_grad0405_requir.shtml), Princeton (http://www.econ.princeton.edu/grad/Requirements%2004.pdf), U. Penn (http://www.econ.upenn.edu/Graduate/Description.htm#Core) or Minnesota (http://www.econ.umn.edu/graduate/prgyrone.html). U.C. Berkeley has economic history as a required course (http://emlab.berkeley.edu/econ/grad/program-yr1.shtml); so do Yale (http://www.econ.yale.edu/graduate/requirements.htm); MIT (http://econ-www.mit.edu/graduate/core.htm) and Stanford (Gavin Wright confirmed that the requirement for economic history had not changed in 21 years, correspondence

century. In a broad sense, American economists' relationship to history dated as far back as its roots in classical political economy: Adam Smith's *Wealth of Nations* (1776) made abundant reference to ancient economic practices, Karl Marx's *Das Kapital* (1867) was built around a historical argument.[5] However classical economists were not self-conscious about their use of historical information, nor did they propose a justification for history being necessary to economics. In stark contrast, starting in the mid 19th century, several groups of economists developed a "historical" creed: the German historical school, the British historical school and, a few decades later, various strands of North American "Institutionalism"- for example the labor economists around John R. Commons at Wisconsin.[6] In America, these historical scholars did not adopt a single label: "economists", "historical economists", "institutionalists", and even "heterodox" may have applied to one group or another, at some time or another. Very few chose the label "economic historian" and did not always do so systematically.

To understand the uniqueness of mid 20th century American economist-history this thesis will proceed comparatively across time, country, but also across discipline. Historians of science often rely on comparative exercises, seeking to replicate the ideal of laboratory work: hold all factors constant save one, and observe the consequences of this variation. In our case, by contrasting situations where an event occurred and those where it did not, we may make

with Cristel de Rouvray, October 27th 2004). Northwestern has a unique program in this respect, as economic history is part of an "Economic History and Development" requirement sequence, which signifies that students can avoid taking a class in economic history and take development economics instead (http://www.econ.northwestern.edu/phd/currentreq.html). As this thesis is being defended at the LSE, the reader may wish to know that the LSE Economics Department does not offer any course in economic history (nor suggest how its research students could use the Economic History department to find advanced training there) – http://www.lse.ac.uk/resources/calendar2004-2005/programmeRegulations/mResPhD/mResPhDinEconomics.htm.

[5] Marx (1976); Smith (1976 [1776]).

[6] For a relatively brief presentation of the historical nature of these "schools", see chapters 17, 18, 20 and 27 of Spiegel (1983).

sense of the factors that led to the mid 20th century surge in American economist-history.

2. The French foil

American economists' mid 20th century commitment to history contrasted with the situation in other countries, which can be used to our advantage in seeking to understand the origins of this golden age. The most frequent contrasts chosen by scholars interested in the origins of American economists' commitment to history (in particular as it related to the "cliometric revolution") have been with France and Britain.[7]

The British case could be considered the ideal candidate for a comparison with the U.S., due to the many vehicles of communication between the two countries. This leads to the plausible assumption that while Britain may not have been marked by the same social, political, and economic trends as the U.S., British scholars had enough common epistemological and scientific grounds with their American counterparts to approximate the desired controlled environment (i.e. if no cliometric revolution occurred in the U.K., the reasons are to be found in contextual elements, rather than epistemological differences). However, the existence of separate departments of Economic History since the early 20th century in Britain has come to represent a unique case in the Western world, effectively limiting the type of friction between "historians", "economists" and different "types" of economists that make economic history such an interesting space to study (see section 3 below).

To some readers, France may appear to be characterized by traditions of scholarship so radically different from those existing in the U.S. to caution against any comparative exercise. There certainly was little communication between French and American scholars in the late 19th and early 20th century, when America was still an importer of ideas from Germany and Britain. WWII

[7] See for example Coats (1980); Lamoreaux (1998).

had not significantly altered this tableau as most French WWII *émigrés* to the U.S. returned to France after the war, and did not really leave a lasting mark on American universities.[8] Such lack of mutual influence may render a comparative exercise very difficult, yet these stark differences can also be exploited to better understand the situation in the U.S. Not only was there no cliometric revolution in France, but at first glance, there also seemed to be no enduring and varied legacies of economist-history, as there had been in Germany and the U.K.

The striking feature about France in the mid 20th century was that it produced its own "golden age" of economic history with very few "economists" involved.[9] As we shall see in chapter 5, economic history in mid 20th century France was the domain of *Annales* historians, not "economists". Yet, the reminder that *Annales* history was the driving force behind a broad reconfiguration of social sciences in post-war France should serve as a warning not to adopt pre-established classifications that presume to fix once and for all what "economists" and "historians" were doing. We must use these terms with care, as they changed meaning throughout the period of study, and are only useful to the historian if she shows how they reflected and established distinctions between various members of the social science community. Thus the preceding statement about the curious absence of economist-historians in France must be read to mean that the delineation between economist and non-economist in France was different from that in the U.S. In other words, the comparative study of economic history in these countries can serve to introduce a more systematic treatment of the second fundamental question in this thesis: what exactly were "economists" and "historians" and how did these labels change throughout the period of study? Though the situation in France is principally studied in chapter 5, occasional reminders weave the geographical comparison throughout the thesis.

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[8] Fermi (1971); Novick (1988), chapter 1.
[9] For an overview of this golden age, see Crouzet and Lescent-Giles (1998).

3. The history foil

While frictions between the history and economics communities appeared more clearly in the French context, where *Annales* historians engaged in open conflict with economists who had ventured into drawing up National Income Accounts for the 18th and 19th centuries, the types of issues raised in these debates had counterparts in the U.S. In America these debates tended to be conducted within the economics community. This is consistent with most historians of economics' contention that American economics was "pluralistic" until WWII, hence more prone to conflicts among economists (rather than between disciplines).[10] While it may not be very useful to reduce explanations of differences between scholars to statements such as "X was a historian, while Y was an economist", it is useful to ask whether differences in the specific ways one approached historical study (or rejected it altogether) were not the product of a methodological stance, which at certain times united historians and economists, and at other times distanced them. This thesis explicitly avoids a narrative that defines cliometrics as history practiced by economists, while everything else was done by historians. Such narratives tend to take modern definitions of both terms and apply them indiscriminately to an early and mid 20th century academic realm that was different from ours in many respects.[11]

This movement of proximity and dissociation between "economists" and "historians" was but one facet of the much larger issue of "scientificity" for economists. The question of what it meant to be a "scientific economist", and how one went about meeting this ideal, was a recurrent issue for the economics profession, though it did not mark all individuals, epochs or generations equally. It tended to resurface at crucial times in the profession's history, and the

[10] Mary Morgan and Malcolm Rutherford (1998a).
[11] These narratives also tend to be woven by people with stakes in the cliometric debate. See for example the cliometric narratives discussed in chapter 2, or Alfred Chandler's essay in Amatori and Jones, Eds. (2003).

consensus that emerged from these debates would periodically reconfigure the parameters of legitimate practice, and continues to do so today.[12]

The importance of these “scientificity” debates has been well documented for the late 19th century emergence of an independent American economics discipline.[13] It has also been used to explain the complex professional reconfiguration that occurred in the mid 20th century, a phenomenon generally known as “the transformation of American economics”.[14] Though the broad strokes of this transformation have been painted there is still much work to be done on the nature and process of this change.[15] This constitutes one of the main goals of this thesis: to enhance our understanding of the directions towards which American economics converged after WWII, and the process by which these consensus points were enforced. I hope to accomplish this by focusing on economic history as a borderland, a stage for dialogue, debate and disagreement among different types of economists and historians. This is the main virtue of the “history foil”: by comparing certain American economists who resembled French “historians”, to those who did not resemble them at all, the analyst can distinguish between scholars who were gradually being pushed outside legitimate economics and those who became “members” of the new post-WWII economics.

[12] As an example of contemporary “scientificity” debates one may cite disagreements about the legitimacy of experimental economics. It has quite recently become part of legitimate economics, in spite of Lionel Robbins’ 1920s rejection of all inquiry relating to the psychological dimensions of economic decision-making. This is consistent with the view that these “scientificity” debates are rarely closed once and for all and tend to resurface periodically.
[13] Furner (1975).
[14] Mary Morgan and Malcolm Rutherford, Ed. (1998b).
[15] One notable exception to the majority broad-brush studies is Malcolm Rutherford’s extensive work on the evolution of “Institutional” economists, schools, research centers and universities in the interwar period. See for example, Rutherford (2003); Rutherford (2005); Rutherford (Forthcoming).

4. The contingencies of American economist – history

This thesis presents the three “golden decades” of American economist-history (1940s, 1950s and 1960s) as having been shaped by the pro-active efforts of various groups of economists, who held strong and often conflicting visions about “good” economics and the right method for the discipline. Their actions had relatively drastic consequences for American economic history, either because they resulted in the establishment of a new recognizable sub-field, or because they entailed the division or the radical transformation of the existing space. The consequences were dramatic because of economic history’s hybrid position: though a part of economics, it was always a borderland – a place that could be inhabited by scholars who were not immediately recognized as “economists” by insiders, and consequently was a stage for dialogue, disagreement and controversy not typical of “normal science” rituals inside economics. These interactions almost systematically resulted in crises whose outcome was in part determined by the epistemological coherence of each camp’s beliefs, and also by the ways in which they could relate to other groups outside the zone of friction – including other economists, foundation officers and university administrators. This added an important degree of “contingency” to the outcome of the debates.

For example, as we shall see in chapter 3 when the founders of the EHA set out to create a space for economist-history, they were confronted by Simon Kuznets who favored a predominantly quantitative and comparative view of the past. This clash revealed that the former’s commitment to historical empiricism was inextricably tied to a vision of America’s economic success grounded on a harmonious blend of individual initiative and institutional soundness. Thus they favored relatively descriptive studies of government action or entrepreneurial initiative, and invited sociologists and political scientists into their newly created realm, hoping to reconfigure research in economics to systematically include insights from these disciplines. In the early 1940s, while these views of “good” research may have been on the wane in the economics community at large, they still held sway among officers of the Rockefeller Foundation who could provide the financial means for such initiatives to be launched and sustained. However, this financial lifeline was not a neutral element in the evolution of their research, contributing no small part to its eventual focus on the entrepreneurial system as the key to understanding economic growth. By choosing the entrepreneur as a symbol of their historical work, they combined political conservatism (certainly in an age where Keynesians were radicals and communists were heretics!) with historical empiricism, a way to stay within the permissible borders of the American profession without adopting the technical tools that were quickly taking over economics.

As we shall see in chapters 6 and 7 when cliometricians entered the field two decades later, they had to reckon with these representatives of "older" epistemological views, and their clashes revealed the degree to which notions of good empirical practice had changed in one generation. But the disagreements also revealed the extent to which the barriers of permissible behavior had evolved. For example, "scientific" economists in the U.S. were by then nearly offended by studies that displayed too close an association with business schools (which incidentally, were being heavily sponsored by both the Rockefeller Foundation and the Ford Foundation). As might be expected from crises in borderlands, those who "lost" were pushed out of both economist-history and economics, though ironically the act of pushing them out was also the genesis moment for a new group. Thus, when cliometricians sealed the borders of acceptable economist-history, and excluded entrepreneurial historians, this considerably strengthened the emerging field of business history.

In France, economic history was also a border place though the spatial dynamics were different from those in the American case. It was a stage for interactions among members of various social sciences, but it was not a place predominantly inhabited or ruled by economists. Rather, it was the stronghold of

Annales historians, some of whom strove to reshape this overlap with economics into a new model for the social sciences. Thus the momentum really came from Fernand Braudel and his colleagues, gathering economists and other social scientists along the way. The *Annalistes'* ability to create such momentum was greatly aided by the patronage of the Ford Foundation, whose interest in French economic history was intimately tied to the actions and reputation of pre-cliometric economist-historians in the U.S. Hence, while the dynamics of this borderland may have been different between countries, the stories met at several crucial junctions. These encounters are reminders of the high degree of contingency inherent in the events presented in the thesis.

5. Sources and Outline

As we shall see in chapter 2, there is little secondary material about economist-historians in either France or the U.S. The bulk of material comes from three other sources. The first is published works by protagonists in these debates, as they reveal epistemological dispositions and other elements economists took for granted (for example their implicit definitions of "economist", "historian", or"social scientist"). In this search for published materials I sought out instances of open controversy between members of various schools of economic historians, even when they involved scholars who set themselves up against "straw men" – like "orthodox" or "deductive" economics.

The second source is archive material (correspondence, unpublished papers, grant applications) principally drawn from the records of the Ford and Rockefeller Foundations - two American philanthropic organizations that played a crucial role for economist-history in the mid 20th century – and from the archives of the Economic History Association. I also used the papers of various economists. A list of the archives and collections I consulted is presented in Appendix 1.

The third source of information consists in interviews with protagonists or their students, in both France and the U.S. In the course of two years, I interviewed tens of people in each country. I use these unpublished materials to further document the implicit beliefs about good science. But I use these materials cautiously, mindful that recollections of events that happened decades ago are subject to memory lapses (a problem that can sometimes be circumvented by triangulation of evidence from other sources), and distortions due to narratives that interviewees construct when placed in the relatively artificial role of being supposedly objective witnesses of a past they had high stakes in (and sometimes still do).[16] Thus, in the following chapters quotes from these interviews will principally be used as evidence that narratives are constitutive of identity and border creation rather than reflections of a past as it happened, or as it was experienced then. A list of all the people I interviewed and my methodology for interviews is included in Appendix 2.

This thesis begins with a review of the literature pertaining to the history of economist-history (and perforce economic history) in the 20th century. Thus, chapter 2 develops themes that are only hinted at in this brief introduction. The thesis then proceeds chronologically. Chapter 3 begins in the late 1930s and chapter 7 ends in the late 1960s. Each chapter tells the story of a momentous controversy, or scientificity debate in economic history. Chapter 3 is an account of the motivations and circumstances that led a group of second-generation German-inspired economists to create the Economic History Association (EHA) and *Journal of Economic History (JEH)* in the early 1940s, with help from the Rockefeller Foundation and in opposition to what they considered to be bad economics. Chapter 4 then investigates the relationship between this form of historicism and the comprehensive, quantitative and comparative use of history advocated by Simon Kuznets. The difference between these two types of economist-history is confirmed by an analysis of the situation in France in

[16] Scott (1991); Weintraub (2005).

chapter 5, where *Annales* scholars held many epistemological and personal ties with the founders of the EHA and the *JEH*, and clashed with Kuznets' chosen French colleagues. Chapters 3, 4 and 5 suggest that differences between so-called "historians" and "economists" are perhaps better understood as differences among economists, and as the result of changes in accepted methods in economics. The comparison with France brings to light the strategic nature of economic history in the mid 20th century, as it constituted an open door for historical empiricists to lay claims on economics.

Chapter 6 investigates economic history at Harvard in the late 1950s and the battles that opposed Alexander Gerschenkron to proponents of an entrepreneurial view of social change. The full effects of his efforts to recruit young economists to economic history are analyzed in chapter 7, where we get a measure of the transformation of American economics, and its implications for economist-history. By the mid 1960s, economists had changed the way they wrote history, reflecting their belief that they could no longer spend time on the laborious and decade long compilation of "facts" - whether quantitative, qualitative, historical or contemporary. This compression of time horizons is discussed in the concluding chapter where I tie the mid 20th century surge in economist history to the rapid emergence of growth and development economics and to an operations research mentality widely diffused after WWII.

CHAPTER 2.

ECONOMISTS' RELATIONSHIP TO HISTORY

1 Introduction

The purpose of this thesis is to understand the origins of American economists' mid 20th century enthusiasm for historical study and the apparent changes it underwent with the "cliometric revolution". This may be seen as one facet of a much larger study: namely the history of economic history. Consequently, the first literature to be surveyed in this chapter deals with the origins and evolution of economic history (or economic *and* social history) in the Western world. These broader narratives have primarily focused on European countries, notably Germany, Britain and France, where the field emerged as an autonomous space in the late 19th and early 20th century. These accounts considered the actions and motivations of stakeholders in numerous disciplines, though they often acknowledged economists' fundamental role in creating and shaping this space. This was in large part due to the perceived weight of the *Methodenstreit* – the mid 19th century dispute that pitted the German economist Gustav Schmoller against the Austrian economist Carl Menger. In this schematic view, historical economists inspired by Schmoller created economic history to break away from classical and marginal economics.

In reviewing this literature we will look out for issues that pertained specifically to economists' stakes in the creation of this new space. Yet, we should be careful not to let ourselves get caught up in a dichotomous view that artificially divided economists between presumed inductivists and deductivists, thus claiming that economic history was the necessary outcome of this divide. Instead, we should privilege narratives that recognized that most early 20th century economists were rather hybrid in their views of good scientific practice, and that economic history's creation corresponded to many lines of divide and disagreement among them, not just methodological ones.

The second literature this chapter surveys is made up of accounts of the 1960s "cliometric revolution". These studies tended to interpret this event within a framework similar to the *Methodenstreit* dichotomy. The cliometric revolution was seen as the deductive economists' takeover of a space that historically inclined empiricists had forged earlier. There was more than a grain of truth to such views, yet their reliance on supposedly irreducible deductive and inductive (or theoretical and a-theoretical) oppositions was problematic. Indeed, even in the 1960s, such labels fitted few economists. In the cliometric debates, the "old" economic historians were certainly not a-theoretical and the cliometricians were empiricists of a certain kind. Once we break free of dichotomies that pit theory users against pure fact collectors, we realize that the lines of dispute were more about different types, methods and shades of empiricism than about the proper origin of theory.

The third literature this chapter surveys is made up of key contributions in the history of science and the history of economics. By broadening the lens to take into account phenomena that were occurring in economics and history at the time, and more generally by thinking of economic history in ways historians and sociologists of science frame their studies of scientific practice, we can uncover different (non *Methodenstreit*) ways of thinking about economist's relationship to history.

This chapter aims to develop a framework that can help make sense of economists' mid 20th century interest in history. After examining the *Methodenstreit* view and assessing its actual role in explaining the origin of

European economic history (section 2), this chapter looks at its counterpart in traditional accounts of the origin of American cliometrics (section 3). In both cases, the framework does not fully account for either the motivations or the context of economists' commitment to history. Section 4 suggests new ways of thinking about the issue. Generally, the chapter argues that economists' mid 20th century enthusiasm for history is best seen as an episode in a lasting debate about the proper scientific method in economics, but that the actual course and outcome of this debate was shaped by the political and ideological environment of the early 1940s-mid 1960s period, and the changing status of economic science in America. The challenge for the historian is to bring to light the mechanisms by which these external factors interacted with the methodological debate.

2 The Methodenstreit and economic history

2.1 The Methodenstreit in mid 19th century Germany and similar movements outside Germany

The emergence of economic history in Europe has been portrayed as intimately linked to methods battles (*Methodenstreit*) between representatives of an increasingly abstract mathematical procedure and challengers defending an empirico-historical method.[1] This polarized view of economic history's genesis was partly inspired by the situation in mid and late 19th century Germany, where "historical economists" sustained vigorous disagreement with British political economists and Austrian marginalists, ultimately resulting in the absence of dialogue between the camps, and the emergence in Germany of economic history as an activity quite separate from theory.[2]

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[1] Schumpeter (1954), 804-19.

[2] Carl Menger's criticism of the German historical position dated from the end of the 19th century (1884). However the debates started much earlier - as early as 1843, the year Wilhelm Roscher published his seminal work *Grundriss zu Vorlesungen uber die*

The German economist Gustav Schmoller was the most adamant advocate for turning economists into economic historians. He argued that theorizing could only take place on the basis of the accumulation and comparison of numerous historical monographs: empirical building blocks such as studies of a particular industry, long term series of prices and wages or descriptions of various institutions. He convinced his colleagues and students to produce enough monographs that differed from each other by only one or two factors, and establish causal statements via comparison. In arguing for such a method, he claimed to be drawing from proven scientific processes in the natural sciences. In his view, systematic comparisons across time and place - using the variation contained in this collection of historical descriptions – amounted to running laboratory "experiments".[3]

Schmoller's own work on 17th and 18th century guilds and Prussian financial policy exemplified the belief that a good economist needed to be a good historian. This was due to the fact that historians excelled at describing the material world:

> "The more incomplete the descriptive part of a science is, the more the theory consists in mass generalizations (…) The way to make progress consists first and foremost in adding to the number, precision and thoroughness of observations, so that with the assistance of more comprehensive and more perfect descriptive material of every sort gathered from experience, the classification of phenomena, the elaboration of categories may be improved, finally the typical phenomena series and

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Staatswirtschaft nach geschichtlicher Methode. Historians tend to distinguish between Roscher's "old" historical school, and Schmoller's "younger" historical school, the latter being more prominent in the *Methodenstreit* against Menger. Possibly because Menger was the most articulate defender of the non-historical position at the time, his ideas tended to be applied to the refutation of the entire German historical agenda. See Koot (1988); Hodgson (2001).

[3] Schumpeter (1954), 804-820; Schefold (1987).

> their interconnections, the causes in their entire scope may be more clearly recognized."[4]

Thus for Schmoller description and historical inquiry were one and the same. The end goal was not a collection of curious and mildly interesting stories about the past, but an extensive empirical base from which generalizations could be made.

Schmoller's methodological call was heard outside Germany. Indeed, there were many similarities between Schmoller's agenda and methodological claims made by British historical economists like Thomas E.C. Leslie, William Ashley or William Cunningham and by American historical economists like Richard T. Ely or John Commons in the late 19th century, or even by American institutionalists like Wesley Mitchell and Simon Kuznets in the interwar period.[5] This connection was substantiated by these non-German historical economists' ample reference to Schmoller and his colleagues, though the question of "influence" deserves a better look than just counting the references.[6] These Anglo-Saxon proponents of "realistic" economics were principally concerned with the usefulness and descriptive accuracy of economic statements. They contrasted this with another type of economics whose logical consistency and exactitude they appreciated, but whose usefulness they seriously questioned. In general, the late 19th and early 20th century saw the recurrent emergence of debates about the role of observation, and more narrowly the role of history, in economic science.

In some instances these debates took the shape of methodological bi-polarism, as for example in Britain with Cunninghman's 1892 attack on Alfred Marshall's use of historical evidence, in France with Francois Simiand's favorable

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[4] Schmoller (1893), *Zur Methodologie der Staats- und Socialwissenschaften* cited in Small (1924), 220.

[5] See Koot (1988); Kadish (1989); Hodgson (2001); Backhouse (2002), chapter 8.

[6] For a closer discussion of German influence on American economists, see chapter 3.

reviews of Schmoller's work and critiques of "speculative abstraction", or in the U.S. in the acrimonious duals opposing Richard T. Ely to Simon Newcomb.[7] In these debates, historical economists often made points about the need to erect economic history as a separate activity for true scientists. For example, Ashley made such a claim in his provocative 1893 inauguration speech for the first professorship in economic history (established at Harvard), when he declared that:

> "The historical movement has pursued its way, and is now settling down into channel of its own. This is none other than the actual investigation of economic history itself".[8]

Such episodes of polarized exchange have been used to confirm the view that historical economists created economic history as a survival (or victory) strategy in a methodologically divided world. Figure 2.1 illustrates this view. Seen from this perspective, economic history was the necessary outcome of a fundamentally divisive methodological battle that made it impossible for historically inclined economists to share the same title and institutions as entrenched deductive economists, as each side considered the other to be "non-scientific". In other words, the genesis of a separate economic history was solely (or principally) linked to methodological disputes within economics (to the exclusion of other disciplines, or to other terrains of dispute).

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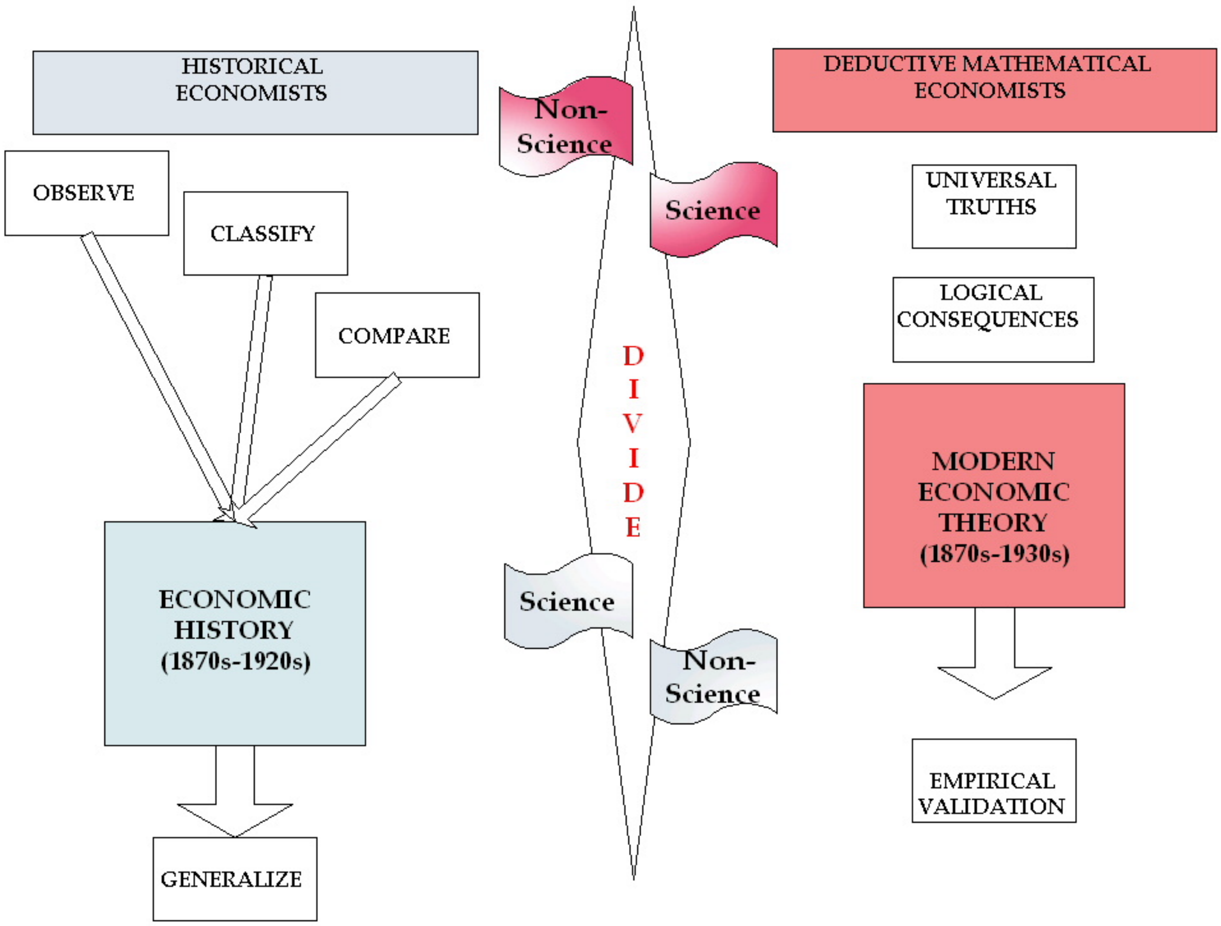
[7] Cunningham (1892); Simiand (1903); Furner (1975). See chapter 3 for a discussion of the Newcomb v. Ely debates and chapter 5 for Simiand's arguments.
[8] Ashley (1893), 6.

Figure 2.1. Taking the *Methodenstreit* at face value to explain the origins of economic history.



The basic problem with figure 2.1 is that it takes dichotomous methods statements at face value, without asking if they corresponded to the actual work done by these economists, or if they overlapped with other motives for the creation of a separate economic history. For example, Mary Furner has shown that late 19th century American economics did not correspond to the picture that one might draw from reading only Ely and Newcomb's methodological rants. True, Newcomb did write about the necessity for a scientific economics to resemble astronomy; and Ely did mock rational economic man and practically every other abstract device in economic reasoning. Yet, these extremisms were not representative of American economists at large, who would be more accurately described as hybrids.[9] Thorstein Veblen could be taken as one example of this hybridity: he was a self-identified "historical" economist but not really an "empiricist" in the meticulous, monographical sense of the term, and he certainly emitted some broad, rather speculative theoretical statements based on hypotheses about human nature, such as his concept of "conspicuous consumption".[10]

In general, a more detailed analysis of the origin and spread of historical claims in and outside Europe has shown that very few scholars adopted one or the other methodological stance in its pure form. Alfred Marshall was often cited as an example of this hybridity. Alon Kadish's fine study of the separation of "economics" from "economic history" in the U.K. in the early 20th century has shown that it was not driven by fundamentally incompatible epistemologies but rather by more prosaic factors, such as different political stances and administrative rivalries between Cambridge departments.[11]

2.2 Economic history in early 20th century Britain

As Kadish has shown, Marshall actually wanted to keep economic history in the economics degree ("Tripos") he set out to design in the late 19th century. Marshall had a vision for economics that rested in part on formal laws (deduced from basic assumptions about the individual) and on careful empirical validation. His own work embodied his commitment to economic history, and he maintained that certain institutions could not be understood via universal causal statements but needed to be set in historical sequence.[12] As he was

[9] Furner (1975), 60-66.
[10] Hodgson (2001).
[11] Kadish (1989).
[12] Ibid, 174-5.

primarily interested in explaining contemporary phenomena, he favored the history of events that had occurred after the industrial revolution. Yet this did not lead him to any outright rejection of earlier economic history, only to suggest that this area of inquiry was potentially less crucial to an education in economics.[13] This division of labour did not seem to please the Cambridge historians, who were not particularly keen on seeing economic history (of any century) be included in the Moral Sciences Tripos (Marshall's economics degree was first institutionalized as the Moral Sciences Tripos, Part II), in particular if it came to focus on the 19th century. Indeed this subject was an entry point for training students for the public examinations (Civil Service, Foreign Service etc.) and thus strategic for drawing more students. Consequently, the History Tripos held fast to all economic history, and Marshall had to give it up (or retain a very reduced version of it in his degree). [14]

In the late 19th century, when Marshall was laboring to design this first economics degree, he was in a vulnerable place. He was convinced that his ability to establish this separate track was a necessary condition for the eventual emergence of economics as a science – enabling the creation of a new kind of scholar trained in the numerous skills Marshall deemed crucial. Thus, he was not in a strong position to negotiate the exact composition of his degree – which led Kadish to conclude that Marshall's creation of a high-theory track may well have been the result of negotiations with the Moral Sciences and History boards, rather than the direct consequence of his epistemological and methodological beliefs. Notice also that the story Kadish told was that of the "liberation of economics", not the liberation or creation of economic history.

Once the division between those who trained to become "economists" and those who wanted to focus on actual events in historical perspective (i.e. study economic history) was enacted, it practically gained a life of its own. Economic

[13] Ibid, 200-1.
[14] Ibid, 168-219.

history professorships in History were often staffed by anti-theoretical (in particular anti-neoclassical) scholars, who were strongly opposed to Marshallian (and later Pigouan) economics. This made meaningful dialogue across fields rather difficult (Cunningham was a good example of an economic historian who hampered dialogue, and he had counterparts among economists, like Pigou himself).[15] The subsequent creation of separate economic history departments, the Economic History Society (1926) and the *Economic History Review* (1927) was more a matter of failure to keep open a dialogue between those who had trained to teach theory and those who had trained to teach history, than the purposeful result of historical economists' will to secede from neoclassicism, or the reverse.[16] By then, a political component had also contributed to worsen the lines of division, as "historical economists" (or economic historians) accused Cambridge theoretical economists of being excessively conservative and naively in favor of free trade.[17]

In essence, Kadish turned upside down the story told in Figure 2.1 – in the U.K. it would seem that economic history was separated from economics before the real methodological divide set in, and the divide was made possible by the actual lack of conversation between camps! Kadish's study showed that the *Methodenstreit* was useful neither for describing the complex methodological lines that criss-crossed through early 20th century British economics, nor as a causal account of the creation of a separate economic history. Yet, this should not lead us to completely ignore methodological statements that referred to such dichotomous views. As we shall see in section 3, the methodological divide may not have been an accurate description or explanation for the events, but it certainly served to consolidate identities once other factors had opened the way for their creation.

[15] Ibid, 214-5.
[16] Ibid, 241-5.
[17] Ibid, 234-5.

3 Strategic uses of the *Methodenstreit* view

3.1 Participants' accounts of the cliometric revolution

The 1960s cliometric revolution was the object of much attention among economists and economic historians. For the most part, these accounts tended to be produced by scholars who participated in the movement (or were their direct descendants): prefaces and introductions to works of cliometrics or "new" economic history often delved into brief recollections of the movement's origins, antecedents and founding moments.[18] In the 1960s and 1970s, many articles published in the *Journal of Economic History*, *Explorations in Economic History* and the *American Economic Review* briefly alluded to the history of cliometrics and the events that led to its creation. These biographical narratives tended to be anecdotal rather than attempts to provide any comprehensive intellectual history, yet they did contain implicit explanations of how and why cliometrics emerged. These explanations were predominantly "internalist" and somewhat "Whiggish" - arguing that the superiority of their scientific method was at the root of the revolution and its success – and paid very little attention to the general academic and social context of the time.[19] The basic premise was that there existed a natural transfer of improved methods from theory (and this included behavioral and statistical theory) to the lower field of applied work,

[18] Examples include Davis, Hughes and Reiter (1960); North (1963); Fogel (1965); Hughes (1965); North (1965); Fogel (1966a); Hughes (1966); Parker (1971); McCloskey (1976); McCloskey (1978); AEA (1997). See also Encyclopedia articles like North (1968); Engermann (1996). For examples in textbooks see Andreano, Ed. (1970); Temin, Ed. (1973); Lee Susan Previant and Peter Passell (1979); McCloskey (1987); Atack Jeremy and Peter Passell (1994).

[19] The term "Whig history" was coined by Herbert Butterfield in *Whig Interpretation of History* (1931), where he described it as "the tendency in many historians… to emphasize certain principles of progress in the past and to produce a story which is the ratification if not the glorification of the present". The term "internalist" denotes explanations that rely principally on the internal course of ideas (contradictions, anomalies for example) to account for change in science; see the introductory chapter in Novick (1988) for a brief account of this position in the history of science.

including history – and cliometrics was simply the application of modern economic theory and methods to historical inquiry.

A good example of this type of reasoning may be found in the preface to collected essays in honor of Douglass North - a renowned cliometrician, and co-winner of the 1993 Nobel Prize for economics (awarded to two economic historians): "there can be no doubt that the power of the philosophy espoused by the New Economic History explains the movement's eventual triumph".[20] North himself had expressed this feeling two decades earlier:

> "A Revolution is taking place in the United States. It is being initiated by a new generation of economic historians who are both skeptical of traditional interpretations of U.S. economic history and convinced that a new economic history must be firmly grounded in sound statistical data. Even a cursory examination of accepted "truths" of U.S. economic history suggests that many of them are inconsistent with elementary economic analysis and have never been subject to-and would not survive testing with statistical data." [21]

These participant histories traced the birthplace of this inevitable revolution to a 1957 meeting where two young economists Alfred Conrad and John Meyer gave two joint papers.[22] The first was an attempt to test the profitability of slavery by comparing the actual return on slave labor to the

[20] Ransom, Sutch and Walton, Eds. (1981), xiii.
[21] North (1963), 128.
[22] See North (1965); Fogel (1966b); Temin, Ed. (1973), 12; Lee and Passell (1979); Ransom, Sutch and Walton, Eds. (1981). All of these accounts mentioned at least one of Conrad and Meyer's two papers; many mentioned the 1957 meeting as well, though did not always specify who had organized it. The fact that it was jointly sponsored by the Economic History Association and the National Bureau of Economic Research seemed relatively unimportant to these authors who presented their movement as something that was bound to happen, regardless of who mobilized the scholars. See chapters 6 and 7 for more information about these sponsors and their motivations.

interest that could be earned on alternative uses of capital.[23] The second was a methodological argument that called for a rigorous study of the past, using recently developed economic and statistical theory to frame and investigate explicit hypotheses.[24] Their slavery paper was meant as an illustration of the power of this new method. According to anecdotal histories, this agenda appealed to many and the enthusiasm it triggered was taken as proof of its merit. However such histories did not credit Conrad and Meyer for being the creators of this agenda; rather they were remembered for having blown the battle horn. Most biographical recollections suggested that the revolution was bound to happen, given the multiple currents that were pushing in this direction. This feeling of inevitability was well expressed by McCloskey in 1976:

> "At about the same time [as the debut of Gerschenkon's workshop in economic history], another example of simultaneous discovery so common when an idea's time has come, similar centers had sprung up at Rochester (…) and at Purdue."[25]

For McCloskey, there was something natural about the advent of new economic history, as it sprung up in many – supposedly independent - places, like Harvard and Purdue University. This was consistent with a view of its origins: new economic history was created to replace "old", obsolete studies whose findings were inconsistent with modern economic theory. Hughes, for example, argued that economics had undergone a great leap forward beginning in the 1930s and culminated in the prestige of post-WW2 mathematical economics. Given this change, he believed that economic history should be rescued by this new knowledge:

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23 Conrad Alfred and John Meyer (1958).
24 Conrad Alfred and John Meyer (1957).
25 McCloskey (1976), 441.

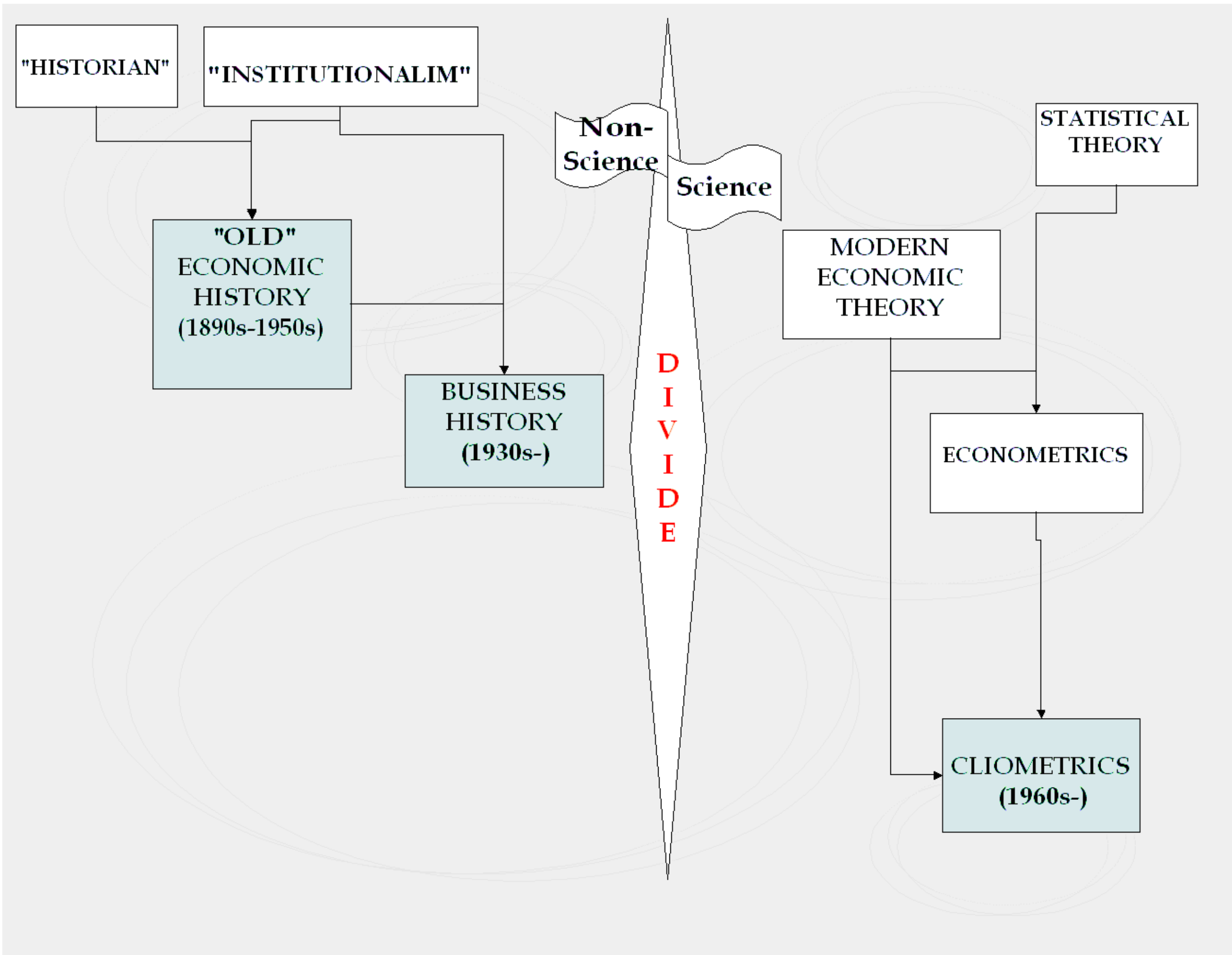
> "The generation of economic historians contemporaneous with the older, pre-Keynesian theorists [those before the great leap forward] continued their work with ever dwindling number of recruits from economics departments. By the early 1950s the profession of economic history was largely being recruited from the ranks of graduate students in straight history. The small number of ranking economic historians trained as economists aged 45 to 65 is dramatic evidence of this history. The revolutionary changes in the logic of economics analysis had passed American economic history by."[26]

For Hughes, cliometricians were on the cutting edge of economics – which included Keynesian macro economic theory - while the people they overturned had missed the boat. Figure 2.2 summarizes this participants' view of events.

On the right side of Figure 2.2 we see a schematic representation of Conrad and Meyer's methodological piece. A combination of statistical theories and tools (new ways to observe and extract from observation) with explicit theoretical hypotheses (about the behavior of agents) generated a demand for cliometrics. Implicit in this figure is a historical argument: progress in these tools had been so considerable in the preceding decades that post-war economists just could not resist the duty to apply this knowledge to badly posed and inadequately answered historical questions. Recent progress in economics had confirmed that scientific economics could only proceed via the progressive formalization of abstract statements, and the subsequent validation or falsification of these propositions.

[26] Hughes (1966), 48.

Figure 2.2: A participant's view of the Cliometric Revolution



In this view, the patient accumulation of facts was an inadequate and unproductive use of economists' time. Hence economists who had once argued in favor of this method, and had been broadly labeled "institutionalists" were placed on the same side of the divide as "historians" – i.e. they had no chance of ever explaining anything!

Labels, "name calling" were a particularly important feature of the cliometric debate, and changes in the names used by participants were a first indication that there may be more to the narrative than is expressed in Figure 2.2. Though three decades of anecdotal histories produced by cliometricians tended to agree about the main causal claim, there was a significant change in the presentation of the argument in the late 1970s. Before then, anecdotal histories were predominantly told in the frame of "old" versus "new" economic history. Beginning in the 1980s these anecdotal histories were much less schematic. This shift in perspective could be seen in changes between the 1st and 2nd edition of the main cliometric textbook: *A New View of American Economic History*. In the first (1979), the authors spent a sizeable part of their introduction describing economic history before 1960 and ways in which the new economic history challenged the "old". There was practically no mention of this in the second edition (1994). Instead, the authors focused on cliometricians' relationship with economists. This focus was amplified as time went by. For example second generation cliometrician Claudia Goldin (she was Robert Fogel's student) began a 1995 article on her teacher's Nobel Prize with a telling question: "what is this cliometrics, and how have these two Nobel Prize winners furthered the discipline of economics"?[27] According to Goldin, the cliometric revolution might have been about getting history right, but for the sake of economics and policy, not history. This constituted a clear reversal of emphasis – remember Douglass North's earlier quote, where he presented economists as rescuers of an obsolete history profession, with no explicit call to contribute to economics.

This change in narrative suggests the purpose these participant histories served: they created a set of anecdotes (myths) to lay the bases for a shared identity and memory.[28] The 1980s shift in focus was an indication that cliometricians' self identity was changing. In the early years, identifying a challenger was one of the pillars of identity construction. Once the cliometricians had won the battle with "old" historians, and established their way of doing economic history as the professional norm, they encountered a new challenge –

[27] Goldin (1995), 208.
[28] For a detailed presentation of the ways in which intellectual histories can root the identity of factions in economics, see Mata (2004).

namely to appeal to economists who seemed less and less interested in historical study. These shifting agendas were reflected in the names used for the various protagonists mentioned in these accounts. Pre-1980 narratives insisted on cliometricians' historical purpose: they were alternatively "cliometricians"[29], "new economic historians"[30] or "econometric historians"[31]. In contrast members of the opposing camp were alternatively called "old economic historians"[32], "traditional economic historians"[33] or "institutional historians"[34]. Labels used in later accounts emphasized cliometricians' *economist* identity, even adopting names that had once belonged to the supposedly unscientific side of the divide, such as "historical economist"[35] or "economist economic historian"[36].

Another indication that these biographical accounts were principally constructed to forge group identity can be gleaned from responses cliometricians gave when I interviewed them (see Appendix 2) and asked them to recall the people and scholarship that lay behind such labels as "old economic history" or "institutionalism". North's responses were fairly typical of most interviewees. When probed about institutionalism he acknowledged that it was a "bad word back then", though he had since changed his mind, as he now embraced the "influence" of Commons and Veblen. He defined "old economic history" as economic history practiced mostly by historians. When prompted about specific names (like John Nef, Earl Hamilton or Arthur Cole) he agreed that they would have been called "old". As we shall see in chapter 3, they were actually all economists (insofar as they were trained and taught in economics departments, and they were eager to engage other economists in conversation). In general,

[29] See for examples Fogel (1966), Temin (1973).
[30] See for example Hughes (1965), North (1965), Davis (1968), Temin (1973).
[31] See for example: Fogel (1966), Temin (1973).
[32] See for example North (1965), Temin (1973).
[33] See for example Lee and Passell (1979).
[34] This label is quite rare in the literature. See Temin (1973).
[35] See McCloskey (1987).
[36] See Field, Ed. (1987).

most respondents recognized that there had been some exaggeration in the depiction and rejection of past scholarship in economic history. As Fogel said, "we probably exaggerated too much the break between us and our predecessors in economic history".[37]

Because these anecdotal histories were principally aimed at identity creation they constitute primary material for my research rather than reliable causal accounts of scientific change. Indeed, in the process of focusing on challengers, identities and labels, these accounts generated incomplete explanations. As they took the movement for granted, these ad hoc accounts did not inquire into the social, economic and political context that might have made it more likely, or even possible. They did not discuss potential differences between the approaches embodied by the different groups involved in the "revolution", or the role individual scholars might have played, or the projects and sponsors that brought them together. Even if one agreed with them that the idea was ripe, they gave us little sense of where this ripeness might have come from. They claimed it came from the internal necessity of scientific progress. Yet it was unclear why economists should have felt the need to reform history, or even to do history. Why was it so natural for economists to want to turn to history, then and there? What was this "idea" whose time had come: the idea of reforming history, or the idea of doing economics differently? What did identity wars have to do with these methodological and substantive debates?

3.2 A broader approach to the cliometric revolution

Once we recognize the polarized nature of these accounts, we can start considering other factors that could not fit within a Figure 2.2 vision of events. Extra *Methodenstreit* factors have been suggested by non-participant historians

[37] Robert Fogel (2004), Interviewed by Cristel de Rouvray, via telephone, Chicago, February 2004; Douglass North (2004), Interviewed by Cristel de Rouvray, via telephone, St Louis, February 2004.

such as Coats (1980), Lamoreaux (1997) and Schabas (1995). Each wove a multidimensional narrative, pointing to a variety of causes that could have made the cliometric revolution more likely to happen in the U.S. in the early 1960s.

Coats' essay started with a contrast between Europe and the U.S. and asked why cliometricians had been more successful in America than in Europe, particularly Britain (whose economic history profession he knew well). His explanatory factors were: youth, size of profession and different cold war politics. There were many more economists in the U.S. – in particular young ones – who needed to make a name for themselves. Thus, Coats argued, it might be expected that some of them would turn to economic history as their field of specialization (as they could have turned to labor economics or econometrics). Their initial rhetorical verve and eagerness to downplay earlier contributions was both a manifestation of their youth and of an increasingly competitive profession.

Instead of attributing their use of mathematical models and statistical tests to the superiority of new methods and theories, Coats made reference to Cold War politics to explain why these young economists should have been so successful in moving economic history away from literary forms of expression. Technical language was obscure not only to older scholars; it was also impenetrable to a wider audience. This could be an advantage in the immediate post-McCarthy era:

> ”Broad, vague, potentially sensitive themes like the history of capitalism, the social consequences of industrialism and the nature and causes of poverty were eschewed in favor of politically safe exercises deploying positivistic techniques and orthodox neoclassical models.”[38]

[38] Coats (1980), 203.

Curiously, Coats was writing in 1980 – a few years after the explosive public controversy on slavery spurred by Fogel and Engerman's cliometric study *Time on the Cross*.[39] Yet Coats' description of "safe" scholarly exercise is inconsistent with the public outcry their conclusions generated (in spite of the fact that they were expressed mathematically and substantiated statistically). Hence it was not so clear that a desire for safety and isolation from public scrutiny explained American economists' proclivity to do history in a statistical and theoretical way. If anything, a strong desire to participate in the increasingly popular conversation about underdevelopment and economic growth seemed to be at the root of their interest in history. As we shall see in chapters 4 and 6, there was considerable overlap between economist-history and development economics in the 1960s.

Naomi Lamoreaux focused less than Coats on the social and political context of the American 1960s, and started with the methodological arguments caricatured in Figure 2.2. However, as Kadish did for late 19th century U.K., she argued that events in economic history had to be situated with respect to scientific trends in economics *and* in history. She reminded us that cliometrics was not the only instance when scholars called for quantification, greater objectivity and use of social science theories in history (she cited *Annalistes* in France as another example of such scientific historians).[40] Seen from this perspective, cliometricians' difference lay in the theory they adopted - only economics – as opposed to other scholars for whom other social scientific theories could be used. Thus she reinterpreted the "old" v. "new" economic history disputes as a battle between Parsonian sociologists and neoclassical economists, rather than the no-theory v. theory battle depicted by participants in their anecdotal histories.

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[39] Engerman Stanley and Robert Fogel (1974).
[40] Lamoreaux (1998), 59.

She substantiated this point by looking at the work cliometricians dismissed as "old" (in particular the work being done at the Harvard Center for Entrepreneurial history) and gave concrete examples of the disagreements that surfaced between the groups. For example, while the "old" historians explained technological change as the result of the personal actions of key entrepreneurs or the features of the entrepreneurial system, the cliometricians explained it as a:

> "response to demand-side stimuli: changes in relative factor prices (…) As an automatic response to the successful expansion of industries in an acquisitive society under competitive market conditions. [Thus] if X had not invented the cotton gin, someone else would have."[41]

By placing cliometrics in a larger debate about the relationship between theory and history, Lamoreaux opened up fresh perspectives, and invited new questions. Who really were these "old" economic historians: fellow economists, sociologically minded historians? What were they really arguing about: the proper role of high theory, the proper kind of high theory? What might have made explanations based on entrepreneurial spirit and initiative unacceptable to young 1960s economists?

Schabas' analysis of the cliometric revolution also started from the methodological debate, though she placed it in the larger context of the post-war transformation of American economics, and the progressive disappearance of certain types of economists in favor of a much more homogeneous neoclassical practitioner.[42] One of the important lines of division between these new and old economists was their willingness to accept the universality of the laws, models and principles they developed. According to Schabas, the young neoclassicals were convinced that individual and social behavior could be explained by the

[41] Ibid, 61.
[42] Schabas (1995).

same causes in all times and places. Whereas many earlier economists rejected this proposition outright, arguing instead that different times followed different laws. Schabas' study reminded us that cliometrics was not a battle among entrenched, unchanging opposed factions of economists, or between economists and historians but rather the manifestation of changing standards of "scientificity" in the economics profession at large. This encourages us to take another look at the debates that raged among cliometricians and non-cliometricians, and relate them to wider debates taking place in economics, acknowledging that all these debates had both epistemological and rhetorical dimensions.

The explanatory factors mentioned by Kadish, Coats, Schabas and Lamoreaux seem convincing enough to encourage us to include them into any account of the cliometric revolution and consider their counterpart in other episodes of mid 20th century economist-history. For example, to borrow Coats' questions, we can ask whether a particular feature of the late 1940s political landscape shaped economists' desire to study entrepreneurial history? Or to consider Schabas' explanation, we may search for changes and tensions within economics in the late 1930s that might explain why certain economists felt the need to set up a journal and an association for a separate economic history? If we were to adopt Lamoreaux' point of view, we might look at differences between the core theories (explanations of human behavior) espoused by different economic historians.

These are all good questions, and this thesis will explore each of them. However, they don't provide an exhaustive view of economists' changing relationship to historical study, and seem rather like a list that critical readers may be tempted to add to (how about the religion, ethnicity, ideologies, scientific reputation etc.). In other words, we are still left with the challenge of framing mid 20th century economist-history in more operational terms. Can we develop a framework that helps us think about the origins of a separate economic history in mid 20th century America and its transformation into cliometrics less than two decades later? The following section surveys landmark texts in the history of science to suggest a few clues for thinking systematically about the relationship between knowledge (science) and society (context).

4 Perennial debates and their temporary resolutions

4.1 Perennial debates

Certain scientific debates recur because they are never solved. This was Peter Novick's claim in his insightful analysis of 20th century American historians, and their perennial debates about "objectivity".[43] According to Novick, changes in the historical profession could usefully be understood as changes in the consensus about whether or not historians could depict the past "objectively". According to Novick, this question recurred because it was, in large part, unanswerable, and generations of historians' efforts to wrestle with the "objectivity" question did not result in any meaningful progress or solution to the issue; yet the debate fundamentally changed the profession every time it erupted.

To account for eruptions and temporary solutions to this long-standing issue, Novick highlighted the links between the epistemological resolution and the political, economic, demographic and social composition of professional historians. He argued that the latter were the key to understanding the former. For example, while late 19th century American historians hardly questioned their potential for objectivity, post WWI historians became increasingly uncomfortable with a professional rhetoric that blindly accepted the difference between "fact" and "interpretation". Much of this interwar skepticism was grounded in the growing heterogeneity of academic historians, in terms of age and political disposition – and the most memorable critiques of pre-WWI epistemology came

[43] Novick (1988), 250.

from young, radical (or at least meliorist) scholars like Charles Beard and Carl Becker. They awakened doubts among their colleagues, whose self-confidence had been seriously undermined by the propagandist nature of much historical work published during WWI. They argued for a confrontational mode of discovery, where historians firmly critiqued each other and could collectively provide an objective depiction of the past. This commitment to argumentation contrasted sharply with what Novick described as a strikingly homogeneous late 19th century white, male, politically and religiously conservative profession, which had believed that each individual historian could provide a "true" picture of the past.[44]

Novick's work is relevant to this thesis inquiry in two separate ways. The first is as a template for studying the history of social science. His insistence that certain debates (be they methodological as in his study, or over specific issues, like the question of whether or not man has free will) resurface periodically, are never intrinsically solved, but settle on consensus points reflecting the context in which they emerged, seems pertinent to our investigation. In earlier sections we encountered many debates about the proper role of economic history in economics, and they seemed to overlap in more than one way. For example, there is an obvious parallel between Figures 2.1 and 2.2, as if economists had always been divided between those who favored an approach that started from universal principles and those who wanted to start from the systematic observation of their world. Though we may want to highlight nuances in both these positions, and acknowledge that they had a strong rhetorical component, they were nonetheless recurrent (and did correspond to different overall dispositions about proper scientific method).

The second way Novick may be of use is via parallels between our economic history debate and the "objectivity question" raised by historians. As we shall discover as early as in chapter 3, debates that troubled economic

[44] Ibid, 111-250.

historians originated in economics, rather than history (and Novick's own account has very little information on the protagonists will we encounter in subsequent chapters), yet the problem of whether or not the analyst could provide an "objective" depiction of reality (and of the past) was very much a part of the debates around economic history. As with Novick's historians, economists' notions of what constituted an "objective" statement changed throughout the 20th century and was frequently a matter of dispute.

The question of the proper place of history in economic investigation seemed to have originated in the 19th century, at least as far back as John Stuart Mill, possibly the most influential writer on the subject. Mill had set up the terms of the debate in his 1836 treatise *On the Definition of Political Economy; and on the Method of Investigation Proper to it.*[45] His starting point was that economic reality was intrinsically complex, by which he meant that every phenomenon was the net result of multiple causes exerting their influence at once. Unfortunately, economists - like astronomers - could not run controlled experiments with the phenomenon they studied, and could thus not rely on observation to draw out the nature and relative importance of all these causes. Consequently, as Mill argued, the only scientific way to reason in economics was to start with simple, verifiable first principles about the individual and deduce the aggregate social consequences.[46] For example, Mill started with "universal" truths he had obtained from introspection – namely that man desired wealth and that this pursuit was systematically impeded by the aversion for effort and the preference for present enjoyment – and deduced certain necessary consequences about the nature of economic behavior in society. However, Mill insisted on the fact that the "laws" linking cause and effect could only ever be formulated as contingent laws (as opposed to universal ones), usually described as "tendency laws":

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[45] Mill (1986).
[46] This methodological argument became one of the three standard "philosophical" readings for economists. The two others were Koopmans (1947); Friedman (1953).

statements about one variable's tendency to cause another, but interference from the many other factors that affect human life made these predictions necessarily inexact.[47]

As De Marchi has shown, Mill struggled for a decade before writing this methodological tract - and was never entirely comfortable at having rejected empirical evidence as a source of demonstration, or reliability in economics. If anything, Mill was an able user of "fact" to make arguments about the likelihood that one event had been caused by another – for example he did this admirably in a study of the relationship between true testimonies and oath taking, analyzing dozens of court cases. Were oaths a sufficient guarantee that a witness would speak the truth? Mill showed that truth was only obtained when oaths were combined with a strong judge and a strong popular interest in the trial, whereas false testimonies were more frequent when only the oath was operative. Thus, he concluded that truth could not be obtained by the swearing of oaths.[48] According to De Marchi, Mill was content with such a reasoning, provided it did not require 100% certainty on the part of the economist: induction could only produce statements about the particular situation that had been observed, and these could not straightforwardly extend to other contexts (other countries, other epochs). Yet, Mill was deeply committed to creating a science that had "certainty"– i.e. a degree of scientific respectability that he thought the natural sciences had earned. Considering the impossibility of experimentation, and the consequent contingency of empirically derived generalizations, economics could only be a hypothetico-deductive science.

According to De Marchi, Mill's progressive distancing from empirical work was the product of his repeated attempts to evaluate the scientific bases of history – a process that culminated in his rejection of history as a model for the social sciences. Not only was it plagued with the shortcomings of all empirical

[47] Cartwright (1989).
[48] De Marchi (2002), 311.

work (namely the impossibility of generating sure statements) but it worked with incomplete material: one could never know the past as well as one knew the present. De Marchi explained Mill's reasons for believing this:

> "This is a matter of not having the necessary information. A contemporary observer has access to numerous facts of common experience, but these are rarely thought significant enough to record. What tends to be recorded is the exceptional occurrence. Sometimes an imaginative historian might infer the rule lying behind a recorded exception, but in this way history also becomes beholden to happenstance."[49]

This quote hints at the overlap between the issue of whether an economist can or should observe, and whether some observations are better (more objective) than others. From Mill's conclusion, one might think that in 1836, history was removed from the economist's legitimate working materials. However this was not the case. Several generations of economists openly disagreed with Mill's methodological recommendations and his sacrifice of relevance for certainty. The most famous of these were the German historical economists who launched the *Methodenstreit*. As we saw earlier, they opposed both Mill's conclusion that general statements derived from empirical investigation were inadequate, and his belief that historical evidence was somewhat inferior to contemporary evidence. As may be gleaned from Schmoller's stance, his reasons for encouraging historical monographic work were not in defiance of Mill's point that they could only produce contingent knowledge but rested on the conviction that the multiplication of monographs and careful comparison among them would strengthen results that had previously relied only on correlation of factors within a single monograph. Thus

[49] Ibid, 321.

where Mill had recommended tendency laws and the hypothetico-deductive method, the German historicists advocated a much vaster empirical enquiry.

The divide was consequently not between strict inductivists who believed that facts would spontaneously speak to them and deductivists who completely ignored reality but between scholars who had different degrees of commitment to observation and how much time they should devote to it. Schmoller's group was made up of people who continued to think about useful ways of observing. Thus, for example, historical economists contributed disproportionately to importing statistical techniques and statistical theory into economics in the late 19th and early 20th century (at the NBER, for example).[50] Note that they often presented this emphasis on quantification as a means of obtaining more objective pictures of the economy.

The issue of "whether, how and how much to observe" was, in principle, larger than the issue of whether or not to do historical work, though in effect these issues overlapped quite considerably. However, there were other facets to the historical question, most of which stemmed from certain scholars' commitments to stage theories or evolutionary views of social change. Many 19th century political economists had explained the wealth of nations in historical perspective, with reference to a set of stages that each economy needed to undergo in its path towards prosperity (or harmony). This view of social development went hand in hand with the belief that each stage followed its own laws and that economists needed to spell these out (in particular to aid countries who were in earlier stages of their development, hence did not operate along the same lines as 19th century Britain). In Germany, one of the more vocal defenders of these views was Friedrich List (1789-1846).[51] A related, though perhaps less relativistic view was the belief that human societies could only be understood in the long term, with reference to their historical development. In other words,

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[50] Morgan (1990); Porter (2001), 16.
[51] His most famous work was the *National System of Political Economy* (1841).

studies of the present could not be divorced from studies of the past. This roughly characterized Wilhelm Roscher's work and that of many American Institutionalists, like Commons or Veblen.

As we shall see in chapters 4 , 5 and 6, these ontological commitments to history (stageism, evolutionism) overlapped, but did not necessarily exhaustively cover all economists' impulse to write history. In many cases, the driving factor was the belief that long-term observation was more useful than just limiting oneself to contemporary facts. Thus, for the sake of our perennial debate framework, the question can usefully be limited to "whether, how and how much to observe" and forms a sub question of the much larger issue of "scientificity" in economics.

The issue of scientificity in economics (what does it take to be a scientific economist?) and the related "observation" question seem to be characterized by the principal features of a perennial debate. They resurfaced on numerous occasions (so far we have encountered at least three episodes in the U.S. – "new" versus "old" economists in the late 19th century, the creation of a separate economic history in 1941 and the cliometric debates – and the thesis will uncover several more); they remained unanswered (unanswerable?) and the truces appeared to be temporary. Given this recurrence, we may want to inquire into the factors that favored mid 20th century debates around these positions.

4.2 Content and Context

If we agree with Novick that changes in science are usefully conceptualized as lasting debates temporarily opened and closed by changes in the broad political context and the demographic and ideological makeover of the profession, we still want to look for a tighter conceptualization of the ways in which these external changes may have acted upon the nature and outcome of the methodological disputes. Otherwise we may hit the same wall that Novick encountered, when he admitted to have written an extremely long book, for the sake of covering every possible issue he found to be relevant in the evolution of the American historical profession.[52] This relates to our earlier point that loose narratives like those produced by Coats and Lamoreaux run the risk of being perceived as selective, since there is no obvious way of ranking all the different potential factors' influence on scientific practice.

Steven Shapin and Simon Schaffer's wonderful study of the emergence of experimental science in 17th century Britain may give us clues for developing a tighter narrative of the relationship between the internal factors (perennial debates) and external features, between knowledge and society. According to them, the explanation of Robert Boyle's "victory" over Thomas Hobbes and the subsequent spread of experimentation within scientific communities lay less in the supposed superiority of Boyle's epistemological position, and more in the better fit between Boyle's model for scientific practice and general 17th century Restoration society.

Shapin and Schaffer carefully showed that Hobbes had legitimate doubts about the laboratory's supposed capacity for creating unambiguous "matters of fact". Having thus established that the Hobbes-Boyle debates opposed two meticulous defenders of logically incompatible visions of science, the authors showed that Boyle's model for scientific practice (specifying how science is organized among its practitioners and its place in society) was more consistent with British Restoration ideals. Indeed the type of knowledge Boyle and his colleagues produced could help the clergy (and thus the king) put an end to religious and civil dissent, as they were willing and able to use experiment to address religious issues. By design, knowledge obtained in a laboratory was

[52] "My own deepest methodological commitment is to the "overdetermination" of all activity, including thought. Therefore, for me, explanation and understanding necessarily involve the exploration of the widest variety of overlapping influences and this book straddles both the internalist versus externalist, and cognitivist versus noncognitivist divisions: explores them all, and does its best to integrate them (…) This is the principal reason why the book is so long" - Novick (1988), 9.

consensual – freely visible and acknowledgeable by all, regardless of education or social status (at least in principle). This contrasted with pre-revolutionary modes of knowledge where the Bible and its interpretation had been accessible only to a few. But it also contrasted with revolutionary beliefs that God's teaching was directly available to all and a source of much civil unrest, as experimentation involved the participation of an experiment designer, an intermediary between "nature" and her "facts" ("disputed knowledge produced civil strife").[53] On the other hand, Hobbes' model for science - where knowledge was the privilege of the very few minds capable of thinking in purely logical terms (like geometry) - was perceived as excessively authoritarian, and would have been a much riskier model to adopt in the newly restored monarchy.[54]

This relationship between epistemologies and different models for the organization of science was the lynchpin between internal features (epistemological debates) and external factors (changes in the political landscape), and Shapin and Schaffer insisted on the two-way nature of the relationship. The ways in which scientists produced and disseminated knowledge were contingent on ways in which society at large set its priorities (in particular in the political sphere). Conversely, the production of scientific knowledge reinforced certain patterns of decision and authority in society. Shapin and Schaffer's strongpoint was to focus on the fit between the division of labor at work within the scientific community and patterns of authority outside of science as the principle motor of this two way process.

This two-way causality has been a favorite theme in recent histories of social science. For example, Theodore Porter and Alain Desrosières have shown that the rise of private and national accounting practices (cost benefit accounting, national income accounting) was triggered by the requirements of international political alliances, yet contributed to alter the world that gave birth to these

[53] Shapin Steven and Simon Schaffer (1985), 283.
[54] Ibid, 323-331.

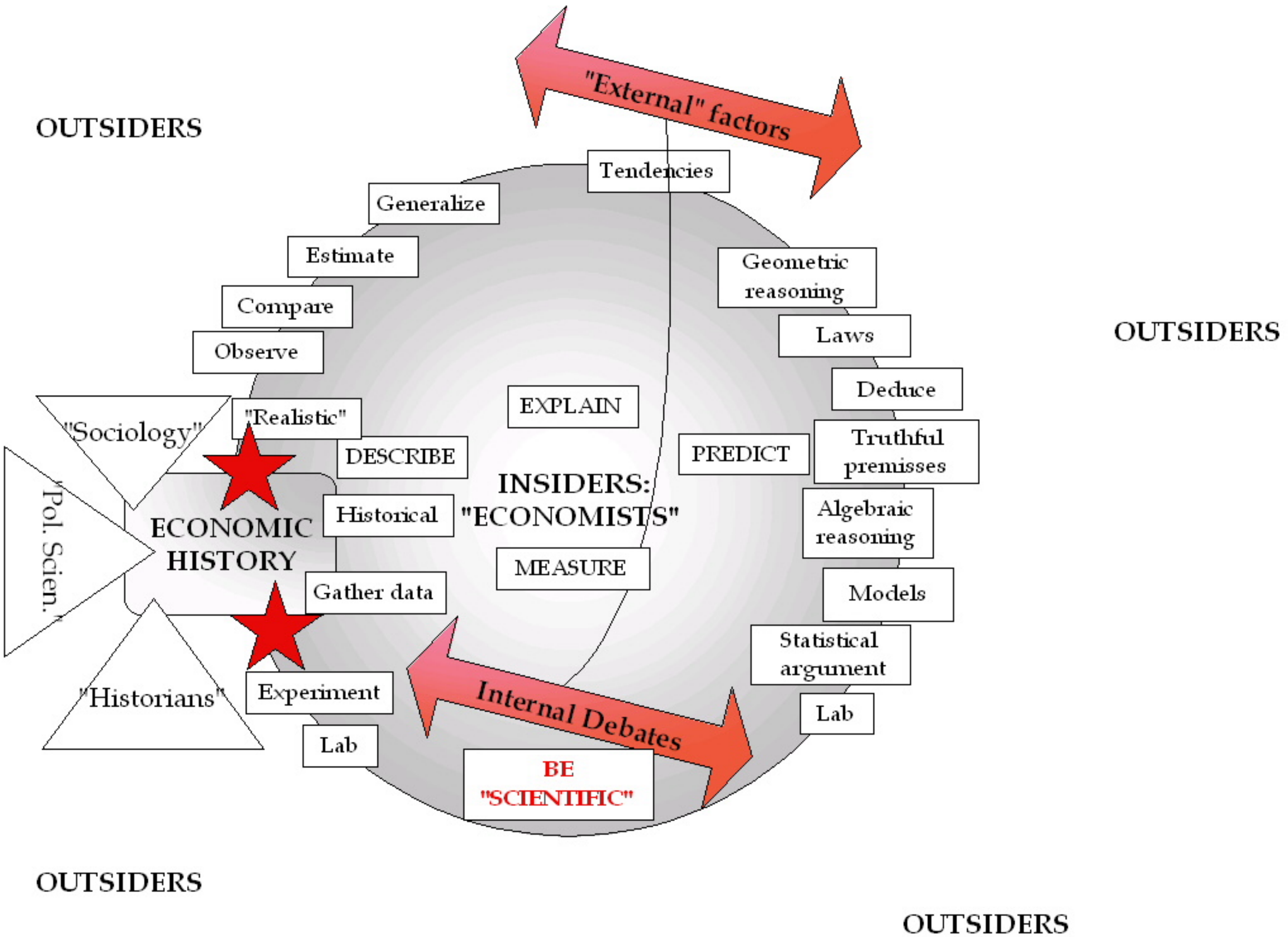
methods: arbitrary concepts invented by economists were appropriated by individuals to define their identity (for example the category "white collar worker" was appropriated by a subset of the French population, "les cadres").[55]

All these elements contain clues for our own investigations. Shapin and Schaffer remind us that epistemological views on good scientific practice have an organizational counterpart, and that this organization may be more or less favored by environmental factors (resources, but also public opinion). They also suggest that certain outside groups, like the clergy, can have high stakes in the production of scientific knowledge. Desrosières and Porter remind us that events in the social scientific realm can have tangible consequences on the real world. The following figure represents an attempt to apply these insights about context and contingency to the forces at play in American economist-history.

As can be seen in Figure 2.3 changes in economics could be broken down into two driving forces, symbolized by the two double-headed arrows. Their net effect resulted in modifications in the emplacement of the economics area within the broader social-science space, and scholars who were once "insiders" could in principle be pushed outside the profession as a result of these changes.

55 Desrosières (1990); Porter (1995); Desrosières (2000).

Figure 2.3: Perennial debates and contingent outcomes in economist-history



By using a cartographic metaphor to describe scientific activity, we acknowledge that much of this activity did consist in erecting borders, frontiers separating acceptable from unacceptable practice, though the porosity of these lines of demarcation and their actual emplacement are seldom as clear as on a map. In other words, it can be very useful to bring to light the efforts that scientists made to define and enforce who was in or out, though it would be simplistic to assume that all their activity was single-mindedly strategic, without acknowledging the contingency linked to the external context these scientists did not control.

Starting from the bottom of Figure 2.3, the first double arrow symbolizes the "scientificity" debate that recurrently involved and opposed economists. While all wanted to be "scientific" they tended to gather around two poles – represented by clusters of words on either side of the circle. These clusters reflect the relative heterogeneity of the men and women who gathered around each pole, reminding us that there never was a meaningful or sharp distinction between "inductivists" and "deductivists" and that the participants were best seen as groups of people who shared one or more epistemological disposition, but not necessarily all (for example some economists might have insisted on "observation" without necessarily thinking that it had to be "historical"). Because of this heterogeneity, the distinction between the clusters was not clearly defined or enforced. Indeed the proximity between terms like "generalize" and "tendencies" suggests that it was not impossible for economists from different poles to collaborate. Importantly some words belonged to both sides, like the term "laboratory" for example. As we shall see in chapter 3, while some 1940s economists believed that history could be a lab for testing theories, or for running controlled experiments, others believed that mathematical models and their econometric manipulation were economists' best chance to have a lab. Thus while some words might have sounded radically opposed they actually corresponded to a relatively non-conflictual reality, while other words that looked identical might have hid a very strong degree of non-compatibility. We should keep this in mind in subsequent chapters, when we sift through the evidence of recurrent methodological debates.

The second double headed arrow symbolizes the "external factors" that were at the origin of the eruptions and quelling of internal debates – Coats' Cold War politics for example. The link between these two arrows, represented by the thin line (barely visible) suggests that there existed a relationship between them, that this relationship could potentially go both ways, but that there is no easy way to conceptualize this link. In Shapin and Schaffer's study this link was the fit between organization within science and external factors. Throughout the thesis we shall see that time – i.e. how much time an economist or group of economists should spend collecting data – played an important role in these debates. Chapters 3, 5 and 6 will show that officers from philanthropic foundations were instrumental in configuring and changing economist-historians' time horizons, thus privileging certain points of consensus, though we will not identify a strong group, like Shapin and Schaffer's clergy, or a clear political agenda.

Instead we shall study economists' mid 20th century debates around history to get a better grip on the external and internal factors that shaped post-war economics. In Figure 2.3. economic history is depicted as a borderland – a place that is both inside and outside of economics. This can be interpreted in two ways. Either we focus on the insiders: *economist*-historians who were recognized by other economists as members of the profession, yet held a special place as their area of specialization led them to interact with representatives of other disciplines, in particular history, but also sociology and political science. In this case, the discussions between *economist*-historians and other economic historians can yield information about the bounds of permissible behavior that defined who was in and who was out of economics. The other interpretation is to think of economic historians as a group of scholars who were not recognized by other economists as "insiders", yet managed to stake a claim on economics via their area of study. In this case, an examination of the motivations that pushed these outsiders into trying to colonize economics, and of their interactions with insiders also yields information about the nature of economics in a particular point in time.

As we shall see in subsequent chapters, both interpretations are useful: cliometricians' controversies with other economic historians said something about the education and socialization of young economists in the 1950s, just as French *Annalistes*' claims on economic history and on social science at large can help us understand the ways in which mainstream economics was becoming increasingly orthogonal to other visions for social science. Thus we will use changes in economist-history as a window into certain key features of the mid 20th century transformation of American economics.[56]

Now that we have developed a framework that will hopefully aid the reader in thinking about the factors that drove economists' interest in history, and periodical changes in the nature and form of this interest, we are faced with the challenging task of applying it. To put flesh on our conceptual framework we can rely on two frequent aids used by historians of science: controversy and comparison.

4.3 Controversy and Comparison

As the reader will soon discover, this thesis revolves around scholars and their disagreements. On the American scene, you will read about Arthur Cole versus Simon Kuznets; Simon Kuznets versus Walt Rostow; Alexander Gerschenkon versus David Landes; Robert Fogel versus Walt Rostow; Louis Hacker versus Robert Fogel etc. In France, you will read about Jan Marczewski versus Pierre Chaunu and Pierre Vilar; Ernest Labrousse versus François

[56] We alluded to this transformation earlier, when discussing Schabas' article; and it will be further developed in chapters 6 and 7. For an excellent collection of essays that grapple with the nature and timing of this transformation, see Morgan Mary and Malcolm Rutherford, Ed. (1998).

Perroux; François Perroux versus André Piatier etc. There is a strong tradition in the history and philosophy of science, following Karl Popper and Thomas Kuhn to consider controversy as the driving force in scientific change and progress. For Kuhn "anomalies" and the conflicting interpretations they invited were the trigger point for paradigm shift. For Popper, good scientists were those who constantly questioned their own and others' hypotheses, making sure to leave no rival interpretation unturned. [57]

Recent scholars have offered slightly adjusted interpretations of the role controversies actually do play in scientific activity. Marcello Dascal for example has argued that controversies are times when proponents of rival ontological views get a glimpse of the distance that separates them from others. This does not result in the victory of one paradigm over another, but in a therapeutic recognition of the conditions and limits of one's own scientific thinking. According to Dascal, this breech into a world that is habitually left unspoken by scientists, and often causes them to talk *past* each other rather than *to* each other, is fundamental to scientific progress. It creates an opening for new ways of thinking. In common parlance, it's a time when the scholars engaged in the controversy can genuinely "think out of the box".[58] For historians, controversies are immensely valuable as they shed light on what scholars have "taken for granted" at a particular period of time.

The expression "taken for granted" is borrowed from Shapin and Schaffer. According to them controversies are windows into "the taken for granted quality of [each man's] preferred beliefs and practices" and "display the artifactual and conventional status of those beliefs and practices".[59] While Dascal, Shapin and Schaffer all agree that certain types of scientific disagreement shed light on the things that are often kept "below water level", they differ on what the historian

[57] Kuhn (1962); Popper (1963).
[58] Dascal (1998); Sergio Cremaschi and Marcelo Dascal (1998).
[59] Shapin and Schaffer (1985), 6.

might want to bring up to the surface. For Dascal, the important features are discursive: the historian must focus on what each opponent said, look at the rhetorical spin, and the ontological underpinnings, and help the reader see the deep epistemological differences that separate the scholars (which in this thesis, we might call the perennial debate elements).[60] For example, in Cremaschi and Dascal's study of debates between Malthus and Ricardo on corn laws, they brought to light these economists' deep divide along whether economics should be "formalist and abstract" or "realist and complex".[61]

In light of our earlier discussion on Mill and the ensuing perennial debate in economics, we may be tempted to say that controversies in economics are always spurred by the same deep factors (disputes about what constitutes good scientific practice), regardless of what the issue appears to be (corn laws, free trade or slavery for example). Yet Shapin and Schaffer cast a broader net: they want to use controversy to revive the thickness of scientific life, its existence in a strongly influential social context, and thus document the many other factors (not just questions of methods) that make scientific life. In this thesis, we will encounter many factors that spurred controversy: financing from philanthropic foundations, the experience of Anglo-Saxon economists during WWII, the Cold War and ensuing demand for economic development theories and rhetoric. Economic historians opportunistically used some of these factors to mobilize resources and deny them to others, and these factors shaped their scientific choices.

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[60] Dascal and Cremaschi were looking for controversies in which the historian could unearth each scholars' "style": "a constellation of positive doctrines, policies, philosophical assumptions, explicitely formulated methodological theses, theological underpinnings, political outlooks, and argumentative preferences and a cluster of basic metaphors." Cremaschi Sergio and Marcelo Dascal (1998), 248.

[61] According to Cremaschi and Dascal, this is "an opposition that social science seems to be unable to escape from "Ibid, 230. This seems to confirm the notion of recurrence and perenniality of certain debates.

While the study of controversy can help paint a multidimensional picture of scientific activity, the process of selecting the dimensions that promoted change or enforced the status quo can be aided by comparative analysis – a comparison with France and the U.S. as Lamoreaux did. Indeed, events in these countries seemed to have been diametrically opposed: while the U.S. had several generations of economist-historians, France seemed to have very few economists who wrote history; while America had a "cliometric revolution", there was none in France. If we can establish that the basic features of the perennial debate were active in both countries in the mid 20th century, the difference in outcome can help us identify the external factors that played a crucial role in the U.S.

The reasons for the non-emergence of economist-historians in France are not clear, and remain quite controversial. Recent accounts produced by cliometricians, or economic historians who did not identify with the *Annales* movement have tended to emphasize institutional barriers that supposedly isolated economists from historians and incited historians to attack economists who entered their territory. George Grantham's 1997 survey of French cliometric work started with French economists' attempts to contribute to Simon Kuznets' retrospective national accounts initiative. According to Grantham, their foray into economic history was halted in the early 1960s, when hegemonic *Annalistes* severely reprimanded the economist-historians, largely because they did not understand what the economists were doing as they were not equipped with the concepts to follow their computations. As Grantham wrote:

> "Institutionally economic history belonged to history rather than to economics, which meant that candidates for the national examination selecting candidates for posts in the national teaching and research establishment did not have to study economics."

He also blamed very different ideological backgrounds:

> " The left had also appropriated the right to define the history of post-Revolutionary France, and it placed a socialist narrative on that history which had little in common with questions about economic growth addressed by Kuznets."[62]

These were strong claims and they have been re-iterated since.[63]

Historians of the *Annales* movement would generally disagree with this diagnostic.[64] As might be expected, there is a colossal literature in the history of *Annales*, and most histories explored the movements' relationship to social sciences, including economics.[65] They reminded us that many *Annalistes* grounded their historical work on the conviction that man and society could only be studied in time, and they considered themselves to be partaking in the same knowledge quest as other social scientists. As a result, *Annalistes* paid attention to developments in these neighboring sciences, and while they may not have been masters in econometric techniques, they were certainly not ignorant of basic statistical devices, and were thus equipped to make sense of most retrospective accounting work (as we shall see in chapter 5).

As Gemelli (1995) and Mazon (1988) have shown, *Annalistes*' social scientific aspirations were real enough to attract sizeable American funding in the social sciences. For American funders, *Annalistes* were not "historians" in any antiquarian sense of the term. Mazon cited a telling anecdote from one Rockefeller Foundation officer's diary:

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[62] Grantham (1997).
[63] Crouzet Francois and Isabelle Lescent-Giles (1998).
[64] Coutau–Bégarie (1983) used the word "phenomenon" rather than "movement" to denote the heterogeneity of this school – see chapter 5.
[65] Coutau-Bégarie (1983).

> "[He] told me about an association for the history of civilization he created which I did not pay attention to before he mentioned what it was about. Behind a name he chose in order to attract traditional scholars, as he told me with an amused look, lies a body of social science researchers whom he encourages to move beyond a purely philosophical tradition towards more empirical study."[66]

Such views encourage us to move away from interpreting French economic history as a field controlled by "historians" and closed to "economists", and to focus rather on the features that defined both these groups, realizing that the disputes involved different models of scientificity. Could we think of *Annales* historians as social scientists wanting to penetrate economics from the left side of the circle (see Figure 2.3)? If we can, how do we interpret the fact that they controlled economic history in 1960s France, at the same time that their counterparts were losing *economist*-history to the cliometricians in the U.S.? We can focus on the differences, in part ideological, in part ideational, in part institutional to suggest factors that played the greatest role in creating different outcomes in each country. In general, this thesis will rely on cross-temporal and cross-spatial comparisons to bring to light both the perenniality of the scientificity debates and the contingency of their outcomes.

5 Conclusion

In chapter 1 the thesis question was presented as: why did mid 20th century American economists' display such interest in history, and why did the nature and shape of this interest radically change in the early 1960s? As we progressed through chapter 2 we operationalized this question into two complementary steps. The first was whether we could trace lines of recurrence

[66] Mazon (1988), 89.

and similarity in the numerous debates around economist-history (perennial debate). The second was whether we could tie lasting consensus and moments of eruption to features and changes in society at large (external factors). Each of the subsequent chapters is built around these two questions. For example, chapter 3 brings us back to the 1940s and shows that the creation of separate institutions for economic history in the U.S was spurred by economists with a strong commitment to "observation", a critical view of most abstract work in the field and that their efforts were greatly facilitated by Rockefeller Foundation officers' conviction that America needed a scientific explanation of its economic success as part of their country's ideological defense against mounting totalitarianisms in Europe.

CHAPTER 3.

ESTABLISHING INSTITUTIONS FOR ECONOMIC HISTORY IN THE U.S.

1 Introduction

This chapter investigates the actions of a small yet influential group of American economists who, in the 1940s and early 1950s, sought to claim economic history for themselves and use it as a springboard to launch a wider transformation of economics. These events have not, until now, been carefully studied, in part due to the post 1960s popularization of the view that earlier work in economic history was obsolete – as reflected in the common use of the expression "old economic history" to describe it, as opposed to "new" or "cliometric" studies.[1] This justified an almost complete lack of interest in both the work and the motivations of pre-cliometric economic historians.

This neglect was particularly strong when it concerned "old" economist-historians – i.e. scholars whose identity was intimately tied to the economics profession, yet chose to write history.[2] Once one recognizes that these "old"

[1] For further analysis of the origin and dissemination of this label, see chapter 2.

[2] An example of this profession wide neglect appears in a 1968 survey of economic historians' contributions to the study of American economic growth, published by the "new" economic historian William Parker (1919-2000), in which he highlighted the work of Frederick Jackson Turner and Charles Beard, with no mention of the economists that feature in this chapter. His 1st footnote mentioned that some of his readers had expressed concern about this omission: "The most important of the criticisms concerns

economist-historians were the founders of the Economic History Association, the *Journal of Economic History* and *Explorations in Entrepreneurial History* (subsequently renamed *Explorations in Economic History)*, in other words, the founders of the institutions that still support American economic history today, one is entitled to wonder about the reasons of this neglect and whether the context of creation of these institutions did not have a lasting impact on the evolution of American economic history.

Historians of economics have recently portrayed the interwar period in the U.S. as one of pluralism in economics. Yet they have also highlighted the growing polarization of the discipline in those years and the emergence of separate groups (institutionalists, neoclassicals, econometricians for example) and their battles for control of the field. They also pointed to the crucial role played by patrons of the discipline in deciding the fates of these competing claims.[3] In this context, one can ask why the Rockefeller Foundation (RF) – then America's largest patron of economics – supported a large economic history initiative? Indeed, in 1941, the RF's Social Science Division started financing economic history and continued to do so for fifteen years, a period that coincided perfectly with Joseph Willits' tenure as Social Science Director. Why did economic history get "created" by the RF as an independent field in the 1940s? How does this episode relate to conflicting beliefs about "scientific" economics? If historical economists had been present in the U.S. since the late 19th century, why did they feel the need to define a new space for economic history at the eve of WWII, and not earlier?

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the roles of Beard and Veblen, and my neglect of the empirical tradition stemming from E.F. Gay". He judged that "the neglect of [Gay's student's work] is a serious omission in my paper", though did not do much to correct it. The timing of these remarks (1968) indicated either that the collective memory had already erased the actions of the founders of the Economic History Association, or that the battle for erasing was still raging, and Parker was more or less consciously taking part in it.

[3] See the collection of essays in Morgan and Rutherford, Eds. (1998).

Following an account of the place of economic history in the U.S. before 1941 (section 2), section 3 examines the role and beliefs of the RF officers who sponsored economic history (Anne Bezanson and Joseph Willits). Section 4 examines the grant's results, namely the research and institutions it supported. The last section (section 5) investigates the advent of entrepreneurial history and considers implications of this episode for our understanding of change and mobilization in mid 20th century American economics. Throughout, this chapter argues that the creation of a splinter group inside economics, self-consciously opposed to the increasing formalization of economics, was made possible by an opportune connection with officers of the RF, but was in turn shaped by this connection with private American patrons.

2 American Economic History before 1941

2.1 Early American economics, "historicism" and "economic history"

As shown in chapter 2, economic history first emerged as a separate activity in late 19th century Europe, in the wake of the German *Methodenstreit*. However, its genesis cannot be reduced to an inevitable division along supposedly irreducible methodological lines (so-called inductivism versus deductivism), but must be understood with reference to the professionalization of economics in those years. As Alon Kadish showed for Britain, for example, economic history emerged as a separate activity from economics due, in large part, to Cambridge department rivalries.

This is an important hint to understand the emergence of a separate area of economic history in the U.S. several decades after Germany, France, or the U.K. Part of this difference in timing was due to the much more favorable status enjoyed by historical economists in early 20th century U.S. The issue of "historicism" in early American economics has been the object of numerous studies, many of which overlap with investigations of German "influence" on American academia. Without a doubt a very large fraction of late 19th and early

20th century American social scientists studied in Germany, and many were exposed to historicism.[4] However, what they brought home and how they adapted it to the American context is a matter of much debate.[5] One prominent obstacle to drawing any generalization about the impact of the German Historical School onto American social science was the fact that Schmoller-inspired men came home to be historians, sociologists, political scientists, and economists, rather than members of one neat disciplinary group.[6] This meant that methodological stances traversed many disciplines, creating stronger ties across fields than among members within a field, thus rendering problematic any strict definition of "historical economics" in early 20th century U.S.

That said, the carriers of a piecemeal appropriation of historical precepts did contribute to spread the view that proper research in the social sciences, including economics, needed to rest on an empirical basis, an examination of things as they were or had been. This assertion encouraged economists to think deeply about the conditions in which observation was possible, and quantification and statistics flourished as a result of this reflection.[7] The German experience also contributed to reinforce a strong sense of advocacy and the conviction that social scientists had a duty to formulate "economic and social policies and reforms." For example, a large number of historical economists studied labour conditions (Edwin Seligman, John R. Commons). Finally, the historical current in America reinforced interdisciplinary dialogue and defended a multi-layered view of social phenomena involving interaction between cultural, political, and economic variables.

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[4] Herbst (1965), 1-8; Diehl (1978), 148 and 130-1.

[5] Carl Diehl used an interesting expression to denote the ambiguity of the effect German historicism may have had on these young scholars: "the anxiety of influence" - Diehl (1978).

[6] Herbst (1965), ix.

[7] Novick showed that many American historians doubted whether they could really show reality as it had been - Novick (1988). Not all social scientists were so perplexed by the act of observation - Herbst (1965), 141. For a statement of the virtues of quantification for observation, see Usher (1932).

In the 1880s and 1890s these “new” economists often derided the methodological precepts of those they considered to be “old”. Their main laughing stock was Simon Newcomb, whose training in astronomy had convinced him that economics could only be a science of immutable laws and eternal principles – to which he added acute political and social conservatism.[8] Though the debate was often needlessly polarized – i.e. forced many historical economists to issue much harsher judgments about analysis and abstraction than they otherwise would have – it did contribute to spread the historical view of “scientificity” in economics.[9] Historical economists were the founders of the American Economic Association (AEA) in 1885 – under Richard T. Ely’s radical leadership - and their general scientific outlook progressively blended into a widespread movement that would officially be recognized as American Institutionalism after 1918.[10]

However, this should not be read to mean that American economics was neatly divided along one methodological line, with historicists advocating a strictly homogeneous methodology and recipe for professional behavior. Actually, many prominent American economists engaged in both sustained historical enquiry and theoretical reflection (for example John Clark, Commons, Frank Taussig, and Seligman), and differed strongly in their political stances.[11] The difference between the Harvard economist Taussig and the Johns Hopkins (then Wisconsin) economist Ely was a good example of the many shapes and sizes of this historical blend. Though Taussig contributed to historical

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[8] Furner (1975), 40-42.

[9] Ibid; Coats, Ed. (1992), chapter 18.

[10] Schumpeter asserted that American Institutionalism could be understood as an offshoot of German historicism, though he never wrote the pages on Instititutionalism that would have figured in his section on “German Historical School Influence Outside Germany” - Schumpeter (1954), 819-20. The relationship between historicism and institutionalism was quite complex, largely due to the many faces of American Institutionalism. However, Rutherford has highlighted American Institutionalists’ consistent interest in history (of events and ideas), see for example Rutherford (2004b).

[11] Furner (1975), 60-80 and 97-9.

investigations of the state's involvement in international trade, he accepted the basic principles of classicism and marginalism, and believed that economists should only interfere with public life on technical matters.[12] Ely on the other hand was a fervent reformer and constantly derided any attempt in abstract economic theorizing, though he too had marginal analysis in his textbook.[13] And at one time or another both men bore the label "historical".

Given this late 19th century continuous spectrum stretching from "old" Newcombian to "new" Ely–type economics, one might wonder whether the label "economic historian" was ever introduced. It was, and it was not. William J. Ashley, a prominent British historical economist, appointed to the first chair of economic history in the Anglo-Saxon world, at Harvard, inaugurated "economic history" as a label in America in 1893. In his acceptance speech before the Harvard faculty he drew the broad outlines of the *Methodenstreit*, calling for a truce to let each side pursue its program for 20 years, and asked that results be compared after that time only. He had no doubt that the historical branch, whose work for the next decades would be in "economic history" of a monographic and ultimately synthetic kind, would emerge victorious.[14] However, Ashley's references to the rivalries opposing historical and deductive economists were more a reflection of his British rhetoric than a correct analysis of the situation in America. After all, Harvard economists had invited him to join their faculty, and historical work was far from being marginalized in the rest of the country. If anything, it was on the ascent. This begins to explain why no American economist was attempting to claim "economic history" as a separate space for historical economists; they would rather have "economics"!

Ashley stayed at Harvard until 1901, when he was called back to Britain to start the first department in Commerce in the U.K. (Birmingham). The following

[12] Cole (1974); Furner (1975), 98-100, 138, 190.
[13] Furner (1975), 84-6, 94-6.
[14] Ashley (1893).

year he was replaced by Edwin F. Gay who had just returned from a decade spent studying in Europe, principally in Germany. The circumstances that led to Gay's appointment were indicative of economic history's general status in the U.S. in the early 20th century, and the many lines of division (not just methodological) that separated American economists. While Harvard economists expressed no doubt about the need to fill Ashley's position, they found it very difficult to find someone to take his place. The problem could not have been a lack of American economists with an interest in the past (Commons had been training many young scholars in this vein at the University of Wisconsin for example).[15] Something else seemed to thwart Harvard economists in their quest for a historical colleague.

Furner has identified the period from 1880 to 1905 as the time that set the parameters for acceptable professional behavior in American economics. One of the main factors of debate and division was the appropriate degree of public involvement and advocacy. According to Furner, Harvard was always on the more conservative end of the scale, favoring discrete and remote economic professionals, rather than scholars involved in generating interpretations for concrete political questions (in particular involving labour). This general conservatism kept all Harvard economists from participating in the early AEA. While the historicism of the new association appealed to some of them (like Taussig for example who was tempted to join), the leftist rhetoric ("socialist") of Richard T. Ely and his followers frightened them.[16] Thus, when it came time to find Ashley's replacement, the issue was not the dearth of potential economist-historians in the U.S., but finding one that wasn't an advocate, and certainly not a leftist advocate! This explained their choice of Edwin F. Gay, in spite of his lack of publication record and absence of scientific recognition in the U.S.

[15] Coats, Ed. (1992), chapter 18.
[16] Furner (1975), 77.

2.2 Edwin F. Gay (1867-1946)

Before Edwin Gay heard Gustav Schmoller lecture in Berlin in 1893 on political economy and economic history, he was on track to becoming a religious historian. Raised in Michigan, Edwin Gay had displayed precocious interest in two fields: botany and religion. The practices he learned in botany (observing and cataloguing) appeared to have set a precedent for his notions of good scientific procedure. His protestant upbringing convinced him that he had a general duty to improve society. He studied history, philosophy (with John Dewey) and literature in college, but in his early 20s he was still hesitating between becoming a doctor or a teacher of history. He settled on the latter and his professors told him to go to Europe for graduate training, as was habitual for American academics of his generation.[17]

Gay's choice came from his conviction that man in the present could not be understood without reference to humanity in the past. He chose to write a dissertation on religious ideas, as he wanted to refute Marx's views that the basic infrastructure of society was economic, and argue instead that it was political and moral.[18] However, his encounter with Schmoller led him to reconsider this. Schmoller first appealed to him on a general level. As Gay's biographer, Herbert Heaton, wrote:

> "[Schmoller represented] the things [Gay] believed in: the devotion to history as the key that might unlock many doors (….) ; [the attempt to grasp] the interaction of all manifestations of the human spirit, economic, legal, political, social and intellectual; the rigorous criticism of evidence

[17] Heaton (1952), 21-23.
[18] Ibid, 33.

> [and] the hope that from it all would emerge a science of economics which would serve as a sure guide for policies of social betterment."[19]

But Schmoller also convinced Gay on a more specific level. He converted him to the belief that economic factors were the foundation of all human events and to the epistemological view that all knowledge required preliminary observation.

Gay chose to write his thesis on the enclosure movement in Tudor England. He spent many years on the problem, accumulating masses of quantitative data to establish whether variations in agricultural productivity could be conclusively linked to levels of tenancy and to the pace of the enclosure movement. His goal was to put Tudor opinion under critical scrutiny (in particular repeated assertions that enclosures had had dramatic consequences for the overall countryside) by recreating a tableau "county by county, of the acreage enclosed and converted from arable land to pastoral use, the percentage of each county affected, and the number of houses of husbandry that had decayed, of persons displaced, of plows rendered idle".[20] Gay completed his Ph.D. in 1902 and returned to the U.S. to take his first job. He entered Harvard as an instructor in economic history at the age of 35. During the next decade, he taught medieval economic history, modern economic history of Europe, American economic and financial history (which he took over from Taussig in 1906) and 19th century German economic thought. By 1907 he was full Professor and chair of the economics department (which only had 7 professors). In 1908, Harvard's president asked Gay to become Dean of the newly created Graduate Business School.

With the outbreak of WWI, he left the university. He spent the war helping provide relevant statistics for planning (restricting imports, reducing

[19] Ibid, 39.
[20] Ibid, 55. "Husbandry" means employment in crop cultivation. For a sample of his statistical tables, see Gay (1903).

exports, using ships more economically etc.) and his success in the war administration led to several job offers in peacetime. He chose to run the *New York Evening Post* from 1919 to 1924. These years also saw him help create the National Bureau for Economic Research (NBER), and he remained an active fund-raiser for the NBER for several decades. In 1921, he created the Council on Foreign Relations; in 1923 he helped set up the Social Science Research Council (SSRC). When the newspaper went bankrupt (it had been losing readers when Gay took it on, but he was unable to turn it around), he went back to Harvard, as Professor of Economic History. He taught and directed graduate students continuously from 1924 to 1936.[21]

Figure 3.1: Edwin F. Gay[22]

Fabian Bachrach

EDWIN F. GAY

[21] Heaton (1952), 186-225.
[22] Scanned from Ibid.

Heaton argued that, while Gay had written very little, his administrative skills and his students were indications of his overwhelming presence in American economics. According to Heaton, Gay created the economic history legacy at Harvard: "the result [of his presence] was that Harvard became one of two or three places in the world for the systematic study of economic history and the training of economic historians."[23] Gay certainly had a great many students. His *Festschrift* contained an impressive number of contributions – 24 scholars who claimed to have learned from him, and his actual list of students was even greater.[24] Among the contributors were people who taught in economics (J.S. Davis, Arthur Cole, Chester Wright, Abbot P. Usher, Earl J. Hamilton), English constitutional history (W.E. Lunt), history (H.L Gray, Arthur L. Dunham, Louis C. Hunter), European history (C. Perkins), and business schools (C.O. Ruggles, N.S.B Gras, M.T. Copeland).

The *Festchrift* contained many clues about the message that Gay gave his students. First, the sheer number of them was indicative of Gay's belief that scientific economics would be a long process, to be passed on from generation to generation of researchers. As Heaton wrote:

> "[Gay] was confident that the great laws of social life, of historical development and of economic behavior could (…) become known if a sufficient number of scholars worked at the job hard enough for many generations."[25]

This long term, large-scale strategy was consistent with many of his other projects for social science like the NBER. Gay was building legacies and institutions for his kind of scientific economics. Second, his diverse students were

[23] Ibid, 2.
[24] Cole, Dunham and Gras, Eds. (1932).
[25] Heaton (1952), 8-9.

indications of his multidisciplinary views. Gay read widely, incorporating evidence from many fields; one of his students remembered him as "an exceedingly important part of the bridge between the social sciences and the humanities".[26] Finally, he inspired many of them to do historical work, having convinced them that the past was key to understanding the present.[27]

2.3 The International Committee on Price History

Among the students he trained, Gay seemed to build privileged relations with a subset of them, and their repeated collaboration gradually developed into a sturdy network that enabled the creation of the Economic History Association in 1941 (see section 3 below). An important moment in this network creation was their collaboration on the International Committee for the History of Prices coordinated by the NBER. It was a historical initiative to gather data on prices and wages in as many countries and over as many years as possible. Jointly sponsored by Gay in the U.S. and William Beveridge in the U.K., this quantitative empirical project involved Anne Bezanson (who worked on prices in Philadelphia), Arthur Cole (who served as the group secretary and participated

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[26] Ibid.

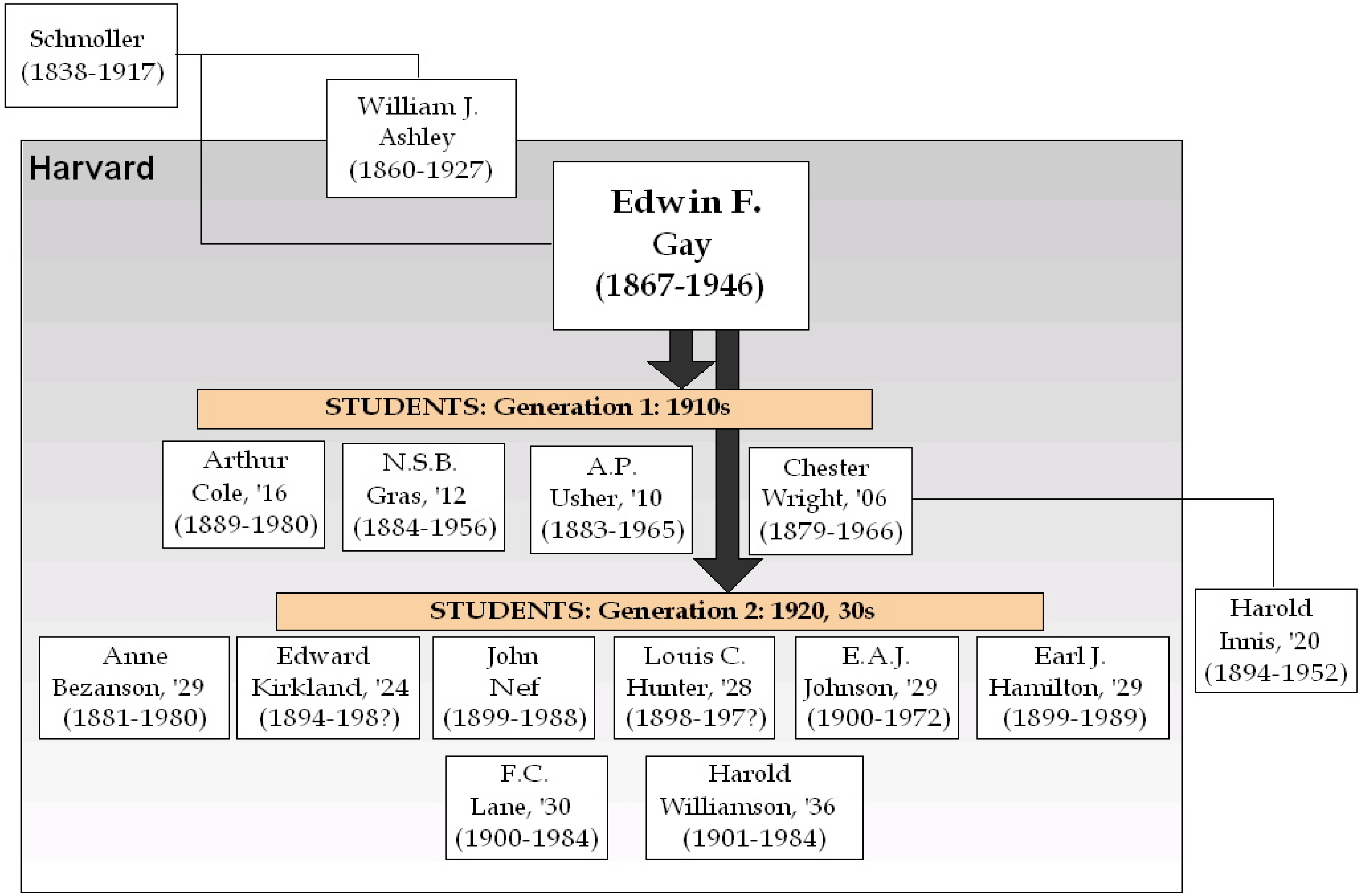
[27] One of the more interesting outgrowths of Gay's commitment to history was the "case study" in Business education. During his deanship at the Harvard Graduate School of Business Administration, he pioneered this instruction method based on a detailed examination of real life situations that students could "learn" from. As one of Gay's students would later comment: "It is not difficult to trace the effects of [the German historical school] upon the present methods of teaching and of analyzing economic problems. In many of our business schools, the use of the 'case system' has been recognized as one of the latest and best developments. Under this system specific examples are taken from the experience of business men (…) Letting his class see the flow of cause and effect from the past, through the present, to the future, in the case in hand, the instructor then leads his students to form their own conclusions as to what should be done under these particular circumstances (…) The thing to be sought through such studies is, not the accumulation of a series of precedents to guide future action under similar conditions, but the ability to think through the problem with a balance and sanity of judgment which arise from taking into account basic principles as they have operated through the recent past and are likely to operate in the immediate future"- Jackman (1932), 5.

in the investigation of American prices), and Earl Hamilton (who did the price series for Spain). As seen in Figure 3.2, all three were Gay's students, having studied economic history with him at Harvard. Other participants in the Price History Project included Ruth Crandall, George R. Taylor, Thoman Berry, George Warren and G.A. Pearson in the U.S., Henri Hauser for France, Bujak for Poland, Pribram for Austria, Ernst Wageman and Moritz Elsas for Germany, Posthumus for the Netherlands – nearly 20 in all.[28]

In recalling his responsibilities as secretary, Arthur Cole mentioned that most communication was done in German, reflecting young American scholars' lasting commitment to German scholarship, though they were the first generation not to need to travel to Europe for their studies. The project was financed by the RF, and lasted 5 years: 1929 to 1933. The RF awarded $50,000 a year to be spent recruiting collaborators in various countries, and had to make an emergency grant in 1933 to cover the International Committee's debts.[29] Research was much more costly and progressed less quickly than expected. When the initiative was shut down in 1933, the work had not been completed – the only result was the publication of individual monographs on various countries' price history (which took several more years to appear in print) and not the comparative study that had been hoped for.

[28] Mills (1936), 289-91; Heaton (1952), 213-5; Cole and Crandall (1964); Dumoulin (1990).
[29] The total outlays were thus $250,000 + $75,000 = $325,000 - Cole and Crandall (1964).

Figure 3.2. Edwin F. Gay's Lineage at Harvard and in American economic history



In a 1964 article, Arthur Cole and Ruth Crandall reflected on the reasons for the project's failure, and identified two central obstacles. The first was "ideational": certain scholars wanted meticulous comparisons of standards of living (which required additional series) - he cited the Austrian economist Pribram as an example - while others "chose to deal rather broadly with cause and consequence" – he cited Hamilton's work on Spain as an example.[30] Hamilton had developed an argument about the relationship between price levels and gold and silver supply in 16th and 17th century Spain.[31] Hamilton's outlook seemed to have been common to many of Gay's students. This was certainly the nature of Anne Bezanson and her colleagues' work on Philadelphia prices. They built indices of prices from 1720 to the mid 19th century, to bring out the general cycles and trends in the time series. Having identified a periodicity in the cycles and a secular break in the trend, they suggested a list of causes (based on correlation), but made no reference to an overall theory of the price system, citing only one theorist of prices, who incidentally was an NBER economist (Frederick C. Mills).[32]

The second reason for failure cited by Cole and Crandall was, on the surface, logistical. Individual researchers seemed to hit one of two walls: either they had not expected the overwhelming masses of data they found in archives and had no rule of thumb to identify the more useful or relevant prices; or they found that the data were very scarce and irregular, and that they had no means of bridging series. Yet this logistical problem was fundamentally a methodological one. Gay and Beveridge's researchers were discovering that empirical work without much of a pre-established framework (theoretical or conceptual) ran the risk of spiraling out of control, or of ending with relatively inconclusive statements. At least one contemporary economist made this point.

[30] Ibid, 383.
[31] Hamilton (1934).
[32] Bezanson, Gray and Hussey (1935); Bezanson, Gray and Hussey (1936).

In a 1936 article in *Econometrica*, F. C. Mills, surveyed progress in price data and theory, showing that a better theoretical understanding of the price system was a prerequisite to running useful empirical studies which, until now, had yielded inadequate generalizations. His main point was that the price system was not homogeneous (through time, space or category of commodities) – hence conclusions drawn from various samples were unlikely to apply to the entire system. According to Mills, the problem of devising more representative samples was essentially a theoretical one.[33]

Yet, this general experience did not seem to discourage Gay or his students. By the time he retired, Gay had grown optimistic about the distance travelled in American economics, thanks to the initiatives he had helped establish. When his students elected him as first president of the Economic History Association and invited him to contribute to the first "Tasks" issue of the *Journal of Economic History* (see section 3 below), he wrote that their mission as economic historians was to further the empirical, factual work that the German historical economists had once called for. He also reminded his pupils that the controversy launched by their forebears was no longer timely, given the tremendous "progress" economists had made:

> "As the nineteenth century has moved on to the twentieth, economics has increased the range and depth of its contemporary observation; its use of the deductive method has become more guarded, its analysis more subtle and in the hands of such masters as Wesley Mitchell, inductive research has notably developed."[34]

Thus, the slow pace and difficulties of empirical, historical research did not dissuade Gay or his students from the necessity of their mission. As seen in

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[33] Mills (1936).
[34] Gay (1941), 14.

their price history work, this mission consisted in extending the horizon of observation beyond their personal experience and using quantification to extract general trends that typified the system at work. They could then issue descriptive and sometimes causal statements. To establish causality they relied principally on correlation - a practice that was becoming increasingly problematic, as new voices were rising in economics to argue for a different view of science and knowledge. The loudest voice came from the econometricians, who created their society in 1930 and journal in 1933 and were beginning to make critical statements about the statistical work done at the NBER, and attempting to bridge the data-theory gap in a way that did not entail the complete rejection of *ex-ante* theory.[35]

This is the context in which Gay 's students (depicted in Figure 3.2) moved to organize economic history as a recognizable, relatively independent activity – in contrast with the « blended » existence it had enjoyed since the founding of the AEA (at least within economics). As early as 1937, Hamilton appealed to other students of Gay's. He wrote to Gay, Gras, Cole and Bezanson, to share his fear that economic history would soon be written off the AEA agenda and to enjoin his colleagues to create their own separate association. To Anne Bezanson, he wrote: « I hope that the idea of the society will appeal to you. As one of the leaders in the field, you will be able to do a great deal toward the realization of this 'plan' » .[36] However, nothing came of Hamilton's 1937 attempt, evidence that there was very little momentum for a seperate economic history in the U.S at that time. It took Bezanson's appointment to the RF, two years later, to effectively mobilize their network.

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[35] Morgan (1990), 158-9.

[36] Letter from Hamilton to Bezanson, 1937. Hagley Museum and Library, Archives (hereafter Hagley), Accession 1479, Folder 6.

3 Rockefeller Foundation Officers and their priorities for American economics (1939-1954)

3.1 Anne Bezanson and Joseph Willits

Joseph Henry Willits was Director of Social Science at the Rockefeller Foundation (RF) from 1939 to 1954. During this time, he was arguably one of the most influential men in American social science. As the head of a large private philanthropic fund, in an age when economics was not yet sponsored by public appropriations, he had the unmatched ability to push research and priorities in directions he saw fit. [37] Within months of his appointment to the RF, Willits invited Anne Bezanson to join him, as part-time consultant, where she stayed until 1950.

Willits and Bezanson had met many years earlier at Wharton (University of Pennsylvania). In 1921 they had co-founded the Industrial Research Department (IRD), an institute devoted to understanding industrial labour problems. Willits had earned his Ph.D. (1916) in economics from the University of Pennsylvania. His thesis was on unemployment in Philadelphia and on the parameters for establishing a city unemployment office. He had drawn a grid of the entire city, calculating the average distance that separated employers and employees from unemployment offices (by walking these distances), and argued that higher frequency and better distribution of offices would decrease the duration of unemployment. He took his responsibility as a labour and industrial expert seriously. From 1913 to 1921 he served as the vice president of the Philadelphia Association for the Discussion of Employment Problems. [38] His

[37] For a brief overview of the main sources of funding for American economists in the first half of the 20th century, see Rutherford (2005). See also "How much was that?" interlude before chapter 8.

[38] See documents in Biography files: "Joseph Willits" at Rockefeller Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, New York (hereafter RAC-RF). Bezanson's Ph.D. was on "Earnings and Working Opportunities in the Upholstery Weaver's Trade in 25 Plants in Philadelphia". She completed it in 1929, and earned her degree from Radcliff; see hand written notes in a the form for her *Who's Who* entry, 1940s. Anne Bezanson Papers, held by her niece, Doris Souza, Boston, Massachusetts.

subsequent career moves were consistent with this commitment to social reform and empirical work: he became Professor of Industrial Economics at the Wharton School of Business in 1921; from 1933 to 1939 he was Dean of the Wharton School, and president then Executive Director of the NBER.

Bezanson directed the IRD's first project: a survey of employee turnover in Philadelphia. To do this, she built a sample of measures of labour turnover for 1921 and 1922, using data from dozens of Philadelphia firms, big and small, across various sectors. She wanted to see if there were common trends and if she could find their causes. By building monthly indices of turnover, she established that the ups and downs were very similar across firms, in spite of different industry, size and management practices. The similarity was confirmed by graphical representations and letting the naked eye observe correlations. From these correlations, Bezanson deduced that turnover was not the result of individual management practices but of larger trends in local markets. She listed several features of these markets (for example a boom in the building trade) and used more indices to establish their correlation with turnover rates, but she did not stipulate the relative order of importance of these causes, nor any overall theoretical relationship among them. Her conclusions were directed at business leaders, who were encouraged to discriminate between different types of turnover, to establish the limits of what they could personally control and improve.[39]

Overall, these projects were indicative of Willits and Bezanson's vision of economists' role in society: to examine, understand and help alleviate the main causes of discontent and instability. In a 1922 interview, Bezanson emphasized her belief that solutions to the U.S.'s labour problems could be found via systematic scientific research.[40] Provided all parties could agree on the causes of

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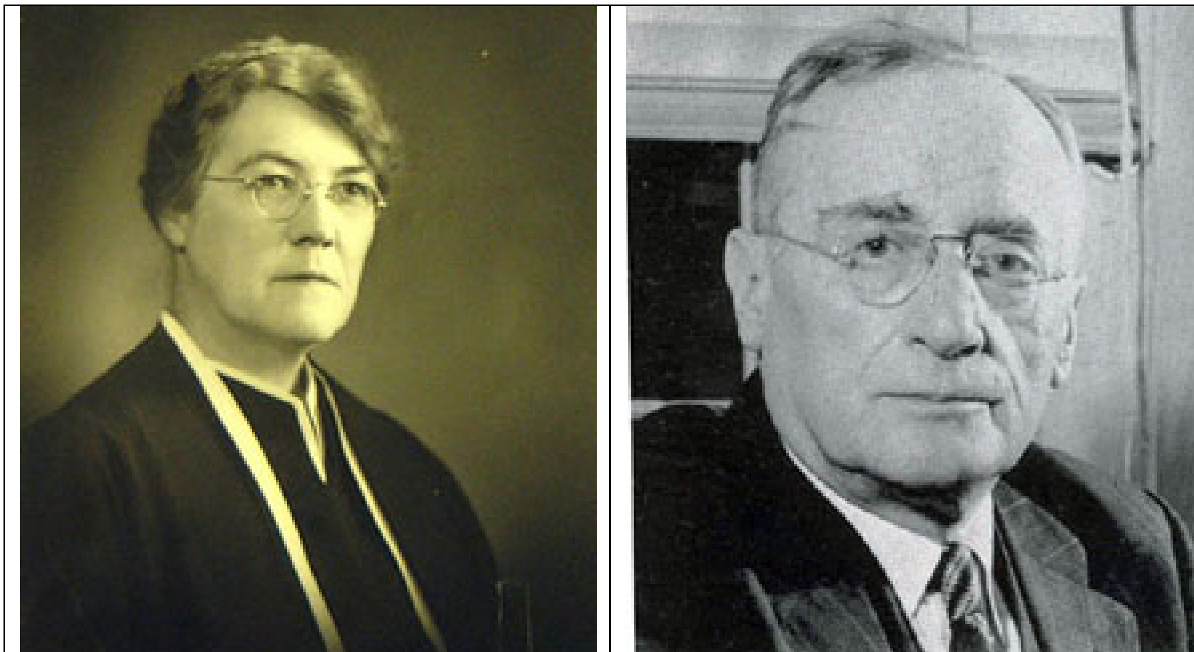
39 Bezanson (1923).

40 *The North American*: "University of Pennsylvania investigation to stabilize labour conditions: a project to improve employment conditions". University of Pennsylvania Archives and Records Center (hereafter U. Penn Archives), UPB5.9 IR, Box 1.

labour instability, a solution acceptable to all could be found. Indeed, the purpose of such scientific preliminaries was to generate "facts" everyone could presumably accept. Their faith in science rested on the premise that certain facts would be recognized by all, and would be a prerequisite for agreeing about the best solution.

Figure 3.3. Anne Bezanson and Joseph Willits



Anne Bezanson (1881-1980) [41]	Joseph Willits (1889-1979) [42]

3.2 The Willits years at RF – moral and philosophical dimensions

By the time they joined the RF in 1939, Willits and Bezanson's strong expectations for social science had been reinforced by the experience of the Great Depression and its political consequences in the U.S. and in Europe. This "historical crisis" as they called it demanded more from social scientists, who now needed to partake in the grand battle of ideals that America began fighting

[41] Rare Book, Manuscript, and Special Collections Library, Duke University (hereafter Duke Archives), American Economic Association Records, Box 1129.

[42] Joseph H. Willits, Biography File. RAC-RF.

in WWII. The issue was now much bigger than alleviating industrial tensions; it was about finding a scientific justification - one based on fact, not fiction - for defending what they saw as the few remaining liberal societies.[43]

Such heavy responsibilities required financial support, as the economist's task was far from easy. Willits and Bezanson had always stressed the complexity of social reality and the consequent difficulty of apprehending it scientifically. To them, the social scientist was in a perpetual "race against complexity".[44] Unfortunately as the complexity grew, so did the tendency to subsume it under some simplistic scheme, rather than try to struggle with it and comprehend it. According to them, many "ideologies" exploited this urge for simplicity and trivialized the search for cause and effect. As early as 1922, Willits had written Bezanson to decry the blindness of those who followed the communist creed:

> "What a comfortable philosophy it is to be able to believe that all our troubles come from a few "bad" people - other people of course. Then, every time trouble arises it is so easy to point out their sins and flaws as the cause of it all. It, by inference, makes us seem so holy, it makes the problem so simple. All we have to do is to snatch 'Control' away from their hands, and the problem is solved. It's a *quitter's* philosophy [his emphasis]. "[45]

As the international situation worsened in the late 1930s and early 40s, Willits and Bezanson reinforced their commitment to social science as a weapon against dogma. They were consistent in this fight against simplistic ideologies as they discarded all types of easy rhetoric, including more conservative ones. Thus

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[43] Willits to the Board of Trustees, 1940. RAC-RF, RG 1.1, Series 200 S, Box 396, Folder 4700.

[44] Letter from Willits to Warren, April 24th 1941. RAC-RF, RG 3, Series 910, Box 3, Folder 17.

[45] Letter from Willits to Bezanson, June, 28th 1922. U. Penn Archives, UPB5.9, IR, Box 1, Folder "administration / IR history file, 1922-24".

Willits also spoke against those who argued that society should be left to the spontaneous coordination of self-interested individuals, calling them: "a public that desires special pleading, propaganda, and a veneer of scientific rationalization for selfish ends".[46] It seemed that Willits' appreciation of various social theories was not so much grounded in his general political disposition (though he was fervently anti-communist, a trait he manifested early and embraced more fully with age), but in the conviction that society was complex, hence difficult to comprehend.[47] To counter this tendency to simplify, Willits and Bezanson were determined to help social scientists develop a "philosophy" (a word they used to contrast with "ideology", philosophy being implicitly more subtle).[48]

3.3 The Willits years at RF – epistemological dimensions

Joseph Willits was the third Director of Social Sciences at the RF. As emphasized by most studies of his directorship, he continued his two predecessors' (Beardsley Ruml and Edmund Day) vision.[49] The agenda had been roughly set by Ruml who had favored social science aimed at producing knowledge that could be "used for social improvement". Ruml's conception of useful knowledge was intimately tied with ideas of proper scientific method: to be useful, social science had to follow the "natural science mode" – be empirical,

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[46] Letter from Willits to Warren, August 24th 1942. RAC-RF, RG 3, Series 910, Box 3, Folder 17.

[47] In 1946 he spent several weeks in Paris. His diary for that period revealed his general interest in the level of influence Communists had in French politics and society and his total distrust of "Party men". See RAC-RF, RG 12.1, Diaries, Box 70, Joseph Willits, 5 volumes.

[48] In the spring of 1941 Willits exchanged numerous letters with Robert Warren, in which they shared their angst about the direction economics was taking, in particular the way the profession attracted technicians with no "philosophy". They also worried that mistaken ideologies would lead nations down a miserable road. See for example, Letter from Warren to Willits, February 8th 1941. RAC-RF, RG 3, Series 910, Box 3, Folder 17.

[49] Craver (1986); Fisher (1993); Rutherford (2005).

objective, and realistic. The first step in Ruml's makeover of social science required extensive basic, empirical research. RF's long-term commitment to the NBER fit within this "scientific" vision.

However, halfway through Day's tenure (1929-1937) this emphasis on basic work had become increasingly difficult to defend. Indeed, the urgent social and political problems of the 1930s had led the RF board of directors (under Raymond Fosdick) to request a more concrete, problem-based strategy. The Social Science Division's status became even more precarious in 1936, when Fosdick was appointed President of the RF and began voicing doubts about the overall merits of supporting social science. Day resigned in 1937. In spite of this potential crisis for social science at RF, Day's chosen successor (Willits) did not mark a departure from the vision pushed by his predecessors. Thus the Willits years have been interpreted as a precarious time for social science at the RF, but also as a last chance for "humanist" economics.

In describing Willits' years at the RF, Rutherford (2005) emphasized the latter's commitment to a certain type of economics - which Willits contrasted with an increasingly popular "bad" type. Quoting from Willits, Rutherford described this "bad" type as a tendency to:

> "(…) 'retreat from science', to 'retreat from reality' and to 'retreat from humanism'. The 'retreat from science' he illustrates by the work of Keynes' more ardent disciples and their 'tendency to substitute a new dogma for an old with neither based on systematic verification nor observations'. The 'retreat from reality' he illustrates with the work of the econometricians who focus too much on the building of mathematical models and too little on 'the study of actual situations and the motivations essential to real understanding'. The 'retreat from humanism' he sees as

> coming from a loss of historical perspective and the substitution of mathematics for an understanding of the broader institutional setting."[50]

Though the grant papers Rutherford relied on for this analysis dated from 1947, Willits' ideas about good and bad economics were not noticeably different in the early 1940s. In a 1942 letter he had described the projects he would prefer to fund:

> "(…) research preferably that does not consist merely of jiu-jitsu with symbols of symbols of reality, but has such relation to a modest reality that the results of research may always be checked against it."[51]

Thus Willits purposely limited funding to the "jiu-jitsu" types (the most renowned being the econometricians at Cowles) and focused on scholars who shared his beliefs.[52]

Rutherford (2005) showed how this conjunction in belief between Willits and economists such as Mitchell, Kuznets and Arthur Burns accounted for the privileged status held by the NBER within the profession until the end of WWII. He even argued that the history of the NBER almost perfectly followed the chronology of its relationship with various funding agencies. This conclusion may also hold for the fate of economic history in the 1940s and 50s. Just as RF officers' relationships with NBER men served to secure a status for this

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[50] Rutherford (2005).

[51] Letter from Willits to Warren, August 24th 1942. RAC-RF, RG 2, Series 910, Box 3, Folder 17.

[52] Several recent studies have stressed Willits' overall disapproval of work done at Cowles. The RF funded the Cowles Commission for a few years, but these were small amounts - Mirowski (2002), 217-9. Rutherford (2005) also emphasized Willits' dislike of Cowles. To explain the RF's regular, but small contribution to Cowles, he cited a 1951 letter from Willits to Sir Henry Clay: "I have brought myself to recommend grants for [men who are not close to what really happens but engage in a most adroit game of formal logic or higher mathematics] because I do not feel that the RF should be limited to my narrow prejudices, but I must confess that I find it harder and harder to do so".

organization, so did their relationship with economic historians allow for the emergence and entrenchment of economic history in the U.S.

3.4 RF 1940 roundtable for economic history and subsequent grant

One of the first projects Willits and Bezanson launched at the RF was a survey of economic history. Within weeks of her appointment Bezanson was busy organizing a round table on the state and future of the field: the "first ever" meeting of economic historians in the U.S. [53] As can be seen in Figure 3.4 (which draws on elements from Figure 3.2), among the twelve people present at the September 1940 meeting, seven were directly linked to Gay - Bezanson, Cole, Gay, Hamilton, Harold A. Innis, E.A.J. Johnson and John Nef- one was Willits and Bezanson's colleague at U. Penn and the NBER - Simon Kuznets; one was an economic historian who had emigrated from Britain and become so close to Gay that we would ultimately write his biography - Herbert Heaton; and three were silent staff of the RF - Willits, his assistant Stevens, and Walter Stewart (Chairman of the Board of Trustees, but also a student of the American "institutionalist" Walton Hamilton and a firm believer in empirical economics).[54] The connection with Gay depicted in Figure 3.4 hinted at a likely communion of beliefs, in particular as it pertained to scientific method in economics (empirical, historical, principally quantitative). Earlier sections of this chapter brought to light the other forums in which these scholars had interacted (NBER, price history) and the opportunity for deeper ties among them. In general, they seemed to share a view of their responsibility in society, not unrelated to their protestant progressivism.

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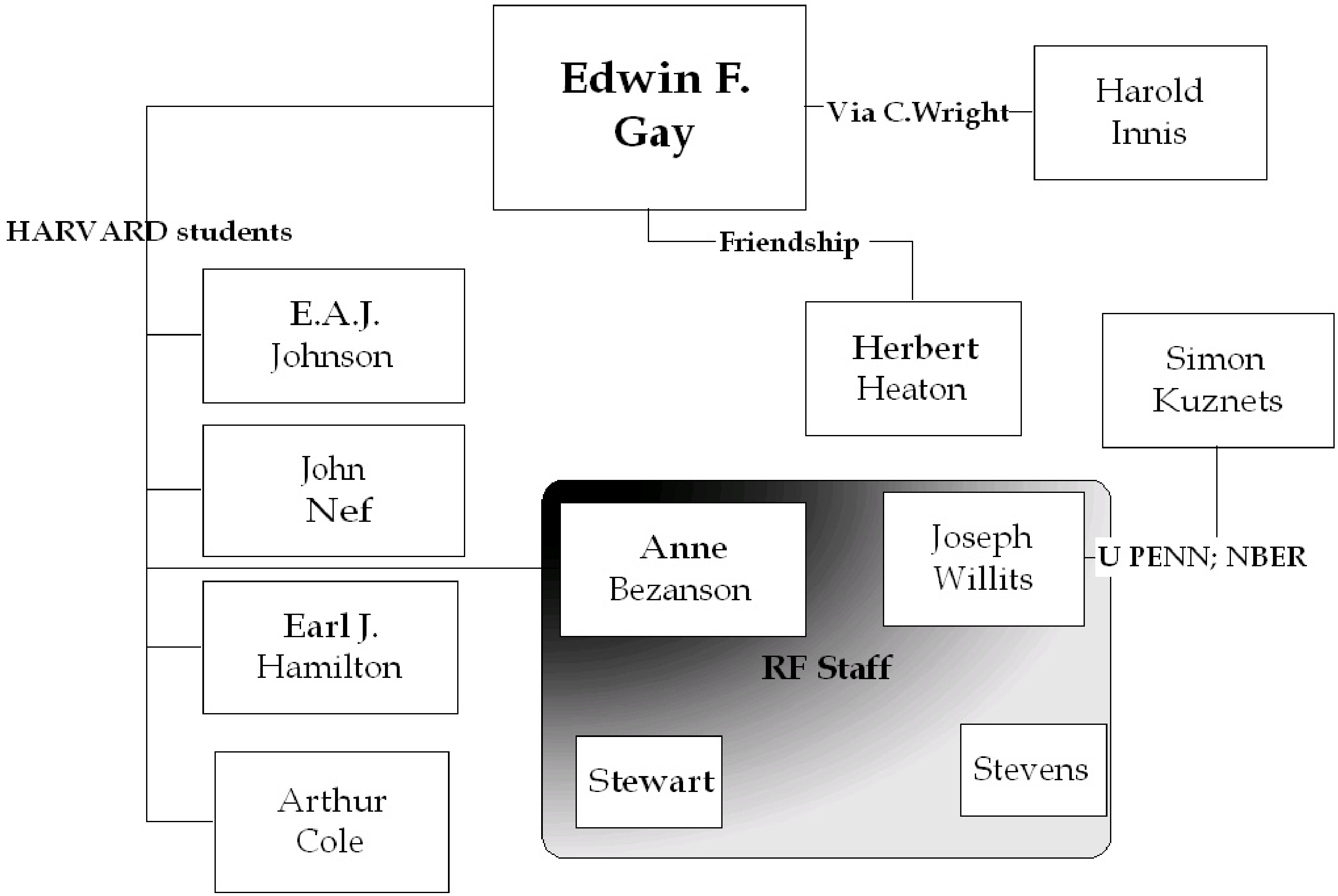
[53] See Grant Application, October 1940. RAC-RF, RG 1.1, Series 200, Box 396, Folder 4700.
[54] Minutes of the meeting, September 1940. RAC-RF, RG3, Series 910, Box 5, Folder 42; for a presentation of Walton Hamilton and his students see Rutherford (2004b).

Figure 3.4: Participants at the 1940 RF roundtable and their connection to Edwin Gay



Gay was raised along Methodists and Unitarian principles, Willits was a Quaker, Innis and Bezanson were raised in a Canadian protestant rural environment.[55]

Yet, when asked to define a concrete, specific agenda for economic history in the U.S. scholars present at the roundtable seemed at a loss. Neither the preliminary report commissioned by the RF (and delivered by Herbert Heaton) nor the minutes of the September 1940 meeting contained any mention of what economic history was or why one might wish to study it.[56] Even when Simon Kuznets raised questions during the meeting, calling for a more specific definition and plan, this did not trigger a general discussion of purpose or strategy.[57] From their answers to his question one sensed that the economic historians were torn (as a group, but also individually) between being primarily "fact collectors" and moving towards a more interpretative, possibly theoretical role. Gay repeated the need for more "facts", by which he meant work like the price history project, with emphasis on more qualitative material that could help contextualise the data and establish causal claims. He never addressed Kuznets' general point about the need for a pre-defined framework - a point that echoed F.C Mills' 1936 concern about the methodology of the international price project. At the other end of the spectrum, Johnson wanted to promote the economic historian as the ideal government advisor (hence an evaluator/interpreter). He seemed to think that it was time for economic history to get involved in the

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[55] See Heaton (1952); Evans and Elderton (Undated). Conversation with Doris Souza, Anne Bezanson's niece.
[56] Minutes to meeting, September 1940. RAC-RF, RG3, Series 910, Box 5, Folder 42.
[57] Though Simon Kuznets voiced his doubts at the September 1940 meeting and subsequently in a long letter to Willits, his objections were ignored. Overall Kuznets shared Willits' outlook for "good" economics and was one of Willits' most trusted advisors. Willits asked him to sit on the Committee for Research in Economic History, which he reluctantly did - though he asked to resign as early as 1942. See chapter 4 for a detailed narrative of Kuznets' agenda for economic history and the roots of his disagreement with Edwin Gay's students.

political decision making process. He personally made this jump a few years later, when he joined the Foreign Service in 1943.

In spite of Johnson's call for a more activist economic history, the majority of participants opted for Gay's agenda, indication of his stature among them and of their shared view that the groundwork still needed to be laid. They settled upon "any attempt at coordinated work in economic history should be designed to promote and encourage monographic research".[58] Hence they concluded on a series of projects (listed as examples) for economic historians to study: history of banking, of enterprise, of economic ideas, of specific industries, of the relationship between state and economic activity etc. They did not specify the exact relationship between these different themes. This apparent failure of a priority to emerge did not discourage the economic historians from applying for funds. Not surprisingly their application contained no solid statement of purpose aside from a vague feeling that the economic history of the U.S. could be used to uncover the roots of economic change - a process that could only be understood historically, i.e. over time.[59]

In spite of this overall vagueness, the application was successful. On December 3rd 1940, the RF awarded the newly created Committee on Research in Economic History (CREH) $300,000 to be spent in a period of four and a half years (from February 1st 1941 to June 30, 1945). The money was officially transferred to the Social Science Research Council (SSRC), as the economic historians were expected to organize under this existing institution's tutelage. This was a relatively standard procedure for the RF – who awarded large grants to committees rather than individuals, and preferred to sponsor inter-university initiatives. The SSRC – which Ruml had created with Rockefeller money in the

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[58] RAC-RF, RG 3, Series 910, Box 5, Folder 42. The word "monographic" referred to studies that did in-depth investigations into one specific facet of economic activity – for example, carpet manufacturing in North-Eastern U.S. in the late 19th century. This was the type of work Gay's students had learned to do under his guidance – see for example, Cole (1928).

[59] Minutes of meeting, September 1940. RAC-RF, RG 1.1, Series 200, Box 396, Folder 4700.

early 1920s – was thus a frequent home for their grantees.[60] This was a large grant – in the same range as Gay and Beveridge's earlier international initiative. However, it was for research in the U.S. only, thus representing an even more generous appropriation. In 1940, Willits' division had awarded $1.5 million (approximately 15% of the RF's total grants for the year) and the CREH's grant represented 20% of this expenditure.[61] Thus, while this grant may have been awarded to work that could seem eclectic to readers today (and certainly was perceived as such by people like Kuznets), its size suggested that it was an integral component of Willits and Bezanson's vision for good economics.

3.5 Economic history and "good" economics

One of the reasons that participants in the 1940 roundtable did not push for a common definition of economic history was that the forces that united them were mostly built around what they considered to be "bad" practice in social science. Most of them had a very clear vision of what economics ought *not* to be. This vision could be gleaned from the casual remarks they made at the meeting and in subsequent letters and reports. In general they shared a suspicion of theoretical, abstract economics. For example, Harold Innis (1894-1952) who had studied at the University of Chicago with Chester Wright – one of Gay's first students - and written a thesis on the Canadian railroad, wrote to Joseph Willits in 1941:

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[60] For a discussion of the hidden control exercised by RF officers on American social science via the SSRC, see Fisher (1993), chapters 5 and 6.

[61] This money was transferred in one lump sum to the SSRC- whose director was responsible for dispensing it to the *Committee*, on a yearly basis. The above calculation may be misleading, as the $300,000 were not apportioned for one year- but for 4 years. Hence it might be more relevant to compare it to the total outlay for 4 years. The verdict of "importance" still holds, as the economic history grant was singled out as one of the noticeable ones for the 1941 RF *Annual Report*. For a more general reflection on the buying power of this grant, see "How much was that?" interlude.

> “It was good of you to write to say that funds have been made available to support this project we had in mind, you are to be congratulated in bringing to a successful conclusion a most promising venture. It raises ones’ hopes that a sense of balance can yet be given to the social sciences and that a corrective can be introduced in the bias of mathematics which has begun to blight the subject”.[62]

In alluding to the “bias of mathematics” Innis was referring to the complaint that united RF officers and all members of the CREH (from Kuznets to Cole): that mathematical, technical economists would take over the discipline. They worried about multiple features of this “bad” economics, but the one they singled out as theirs to fight was a lack of “perspective”, an incapacity to set current problems in their historical context.[63] Combined with other outlets for “good” economics like the NBER, the economic historians’ work could contribute to redefine the economics profession’s goals and methods, tilting the balance in favor of Willits and Bezanson’s vision. Thus, economic history was not envisaged as the only form of good practice, but a potentially very promising one, as it was worthy of the complexity of society. It earned this advantage from its combination of empiricism and multi-disciplinarity.

Economic history was empirical insofar as it dealt with facts of the past. These facts could be quantitative or qualitative, preferably both. Indeed, to interpret data series, one needed to set them in context, using facts of a more qualitative nature: description of beliefs, laws, policies, technologies, international events and their evolution over time. This recognition that causal statements could only be developed within such detailed pictures may have been a result of Gay’s students’ experience in price studies. It may have convinced

[62] Letter from Harold Innis to Joseph Willits, December 19th 1940. RAC-RF, RG 1.1, Series 200 S, Box 396, Folder 4700.
[63] RAC-RF, RG 1.1, Series 200 S, Box 396, Folder 4700.

them that there was a missing element in the general division of scientific labour: putting flesh on the statistical skeleton. As Bezanson wrote in 1945:

> "the NBER in pushing backward their studies of fluctuations, prices and financial series felt that the time had come for supplementing the methods of inductive research with those of historical research."[64]

However economic history was empirical in another sense as well: it wasn't just about collecting information, it was also about providing a forum for experiment. For economic historians, the past was a laboratory where one could test hypotheses: "since in the social field we can't try laboratory experiments, and must test our ideas against the backdrop of experience".[65] This association of the past with a lab was probably not an innocent one. Indeed, in the early 1940s, Cowles econometricians were presenting their work as a form of experimentation for economics - a forum that Willits et al. disapproved of (remember the quote on jiu-jitsu).[66] Thus in their opinion, the economic historians were proposing to use a better (more realistic) laboratory.

If economic historians were to be testers of hypotheses, they would need a source for these hypotheses. As mentioned earlier, Willits et al. were quite suspicious of those formulated *ex-nihilo*, in some abstract, speculative way. Rather, the type of hypotheses they were looking for were generated from a careful examination of the quantitative and qualitative facts that the first stage of empirical investigation had yielded. Recall Bezanson's Philadelphia price study. She had made no mention of existing theories of prices, preferring rather to explain the observed cycles and trends with factors she found in the historical

[64] Review of the 4-year performance of the economic history grant, RAC-RF, RG 1.1, Series 200. Box 396, Folder 4705.
[65] Letter from Willits to Warren: February 27th, 1942; RAC-RF, RG 3, Series 910, Box 3, Folder 17.
[66] See Morgan (1990), 251-3.

record – many of which had been proposed by contemporaries (for example many local merchants were convinced that the number of ships loading at a given time would affect the price of bread, and she suggested this was a reasonable explanation). Economic history would generate, test and refine its own hypotheses - using "facts" derived in the first step of research. Such a process was empirically based at all stages, and thus, in their minds, scientific.

The superiority of economic historians' hypotheses lay in their multidisciplinary nature. Economic historians trained by Gay seemed to share Willits' concern for complexity. Gay reminded them that this was a lesson they had learned from the German historical economist, Schmoller, who used to tell his pupils: "*Es ist alles so unendlich compliziert*".[67] To account for this complexity, hypotheses had to reflect several levels of analysis: social, political, international, and psychological - not just economic. Only then could the interpretations and generalizations be of any use. Their conviction that useful generalizations would involve these many dimensions was reflected in the choice of Committee members: for example, when time came to appoint a new member after WWII they chose the sociologist Leland Jenks.

4 Economic history: 1941-1950

4.1 Forging Institutions for Economic History in the U.S.

As a result of the grant, economic history found a place and a status in American academia. By early 1941, a few months after the roundtable meeting, economic history had an entirely new infrastructure: a research committee, an association, and a journal. The relationship between RF's actions and the simultaneous formation of the Economic History Association (EHA) and *Journal of Economic History (JEH)* deserves to be highlighted, as it is little known. As can be seen in Figure 3.5, the EHA was officially incorporated in 1941. A few years earlier, in 1939, Anne Bezanson had helped set up a Steering Committee of 26

[67] "Everything is so infinitely complicated", Gay (1941).

delegates whose job was to define the prospects for an association of economic historians (presumably in response to Hamilton's 1937 letter). The heart of this prospecting committee was made up of Edwin Gay's students (see Figure 3.2), but it also contained representatives of other existing professional associations: the Business History Society (N.S.B. Gras), The Industrial History Society, the Agricultural History Association, and the AEA (F.C. Mills), to make sure that this initiative was not seen as an attempt to encroach onto already defined territory. Negotiations within the Steering Committee that occurred before the RF roundtable suggested that the idea of an EHA was not congenial to many of these representatives.[68] However, by December 1940, when the time had been set to hold votes and draw pledges from potential members, it seemed that the tides had turned and were now in favor of the soon to be RF grantees.

In December 1940, Gay's students brought the idea of an EHA to the annual meetings of the AEA and of the American Historical Association (AHA). While Bezanson and Hamilton were sent to New Orleans to speak to economists and gather their votes, Cole and Heaton were in New York doing the same with historians of the AHA.[69] Timing was crucial: the December votes happened three months after the roundtable, one month after the application, and a few days after the informal notification of acceptance (the importance of timing appears as a clutter of events in Figure 3.5 around 1940). Thus, the prospects of a large grant gave the Gay network the self-confidence to rally other economists and historians to their cause. It also turned them into the controlling nucleus of economic history institutions for years to come.

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[68] See responses to Arthur Cole, April 1940. Hagley, Accesion 1479, Folder 9. For example, letter from Bezanson to Gay, June 19th 1940: "for a time it did look as though we would be confronted with some very serious animosities and either have to abandon our plans or get a number of ineffective specialty groups organized in economic history."

[69] See minutes of the New Orleans December 30th meeting, and New York December 29th meeting. Hagley, Accession 1479, Folder 18.

Figure 3.5: Forging institutions for American Economic History

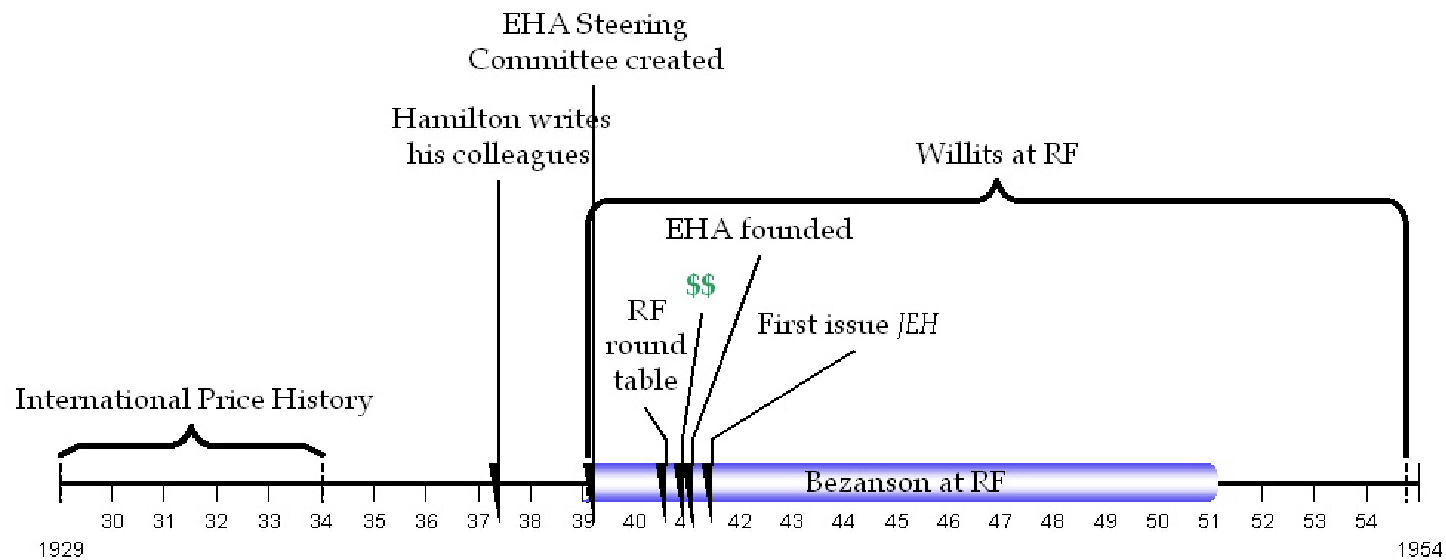

Figure 3.6 illustrates their controlling position in the field of American economic history until the early 1960s. First, notice the overlap between members of the Steering Committee and those invited to the RF roundtable: nearly 1 in 3 members of the Steering Committee attended the roundtable meeting (7 out of 26). But also notice the overlap between membership in the Steering Committee and membership in the CREH (i.e. right to vote on projects that would get funding). Of the 17 people who served the CREH, 11 of them had helped found the EHA. But the most striking connection was the overlap between attendance at the RF roundtable and presidency of the EHA. The first 6 presidents were all RF roundtable attendees: Gay (1940-42), Innis (1942-44), Cole (1944-46), Bezanson (1946-48), Heaton (1948-50) and Hamilton (1950-52). Among the next 5 presidents, 3 were Edwin Gay's students: Kirkland (1952-54), F.C. Lane (1956-58) and Johnson (1960-62).[70] Also note that many of *JEH* editors were members of the CREH: this was the case of Cochran (from 1943 to 1957) and F.C. Lane (from 1943 to 1951).

Hence the RF grant operating via the CREH had important side effects, as it permitted and sustained the existence of crucial professional institutions. This was certainly the opinion of the people involved in the process. In the early 1950s, Harold Innis wrote to Arthur Cole:

> "It can be argued that the expenditure of a large sum under the conditions of the grant was the most effective way of securing important, intangible results. The establishment of an association, improvement of textbooks in the field and more general recognition of the significance of work in economic history can be attributed to the grant." [71]

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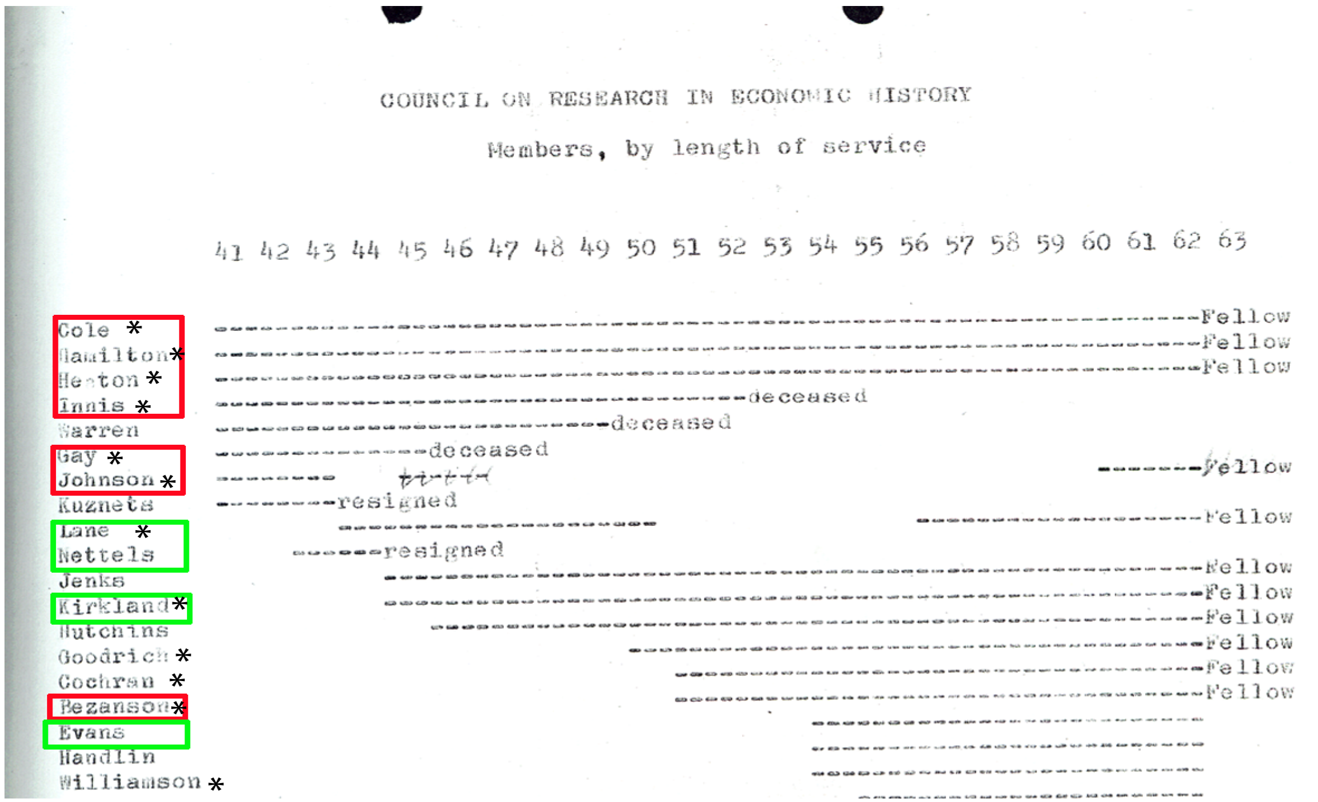
[70] The two others were Carter Goodrich (1954-56) and Thomas Cochran (1958-60).
[71] Harold Innis to Arthur Cole, Confidential Memorandum on operations of the Economic History Committee, 1952, p. 1-2. Harvard University Archives, Arthur Cole Papers, Folder "Correspondance: a-c".

Figure 3.6: Overlap between CREH members and EHA founding members[72]



Red (dark) is for people who attended the 1940 RF roundtable and were members of the 1939 EHA Steering Committee. Green (pale) is for members of the EHA Steering Committee only. Stars indicate presidency of the EHA.

[72] Undated document (1963?). Hagley, Accession 1479, Folder 7. After 1960 the name changed from Committee to Council on Research in Economic History.

One should not interpret Innis' words to mean that RF money went to finance the EHA. Actually, no RF money was ever used for the association or for the *JEH*. From its first year, the EHA ran a balanced budget, and paid for the journal in full from membership fees – thus publishing only as many issues as it could afford. The *JEH* only became quarterly in 1951.[73] In addition no CREH member ever received a salary for sitting on the Committee.[74] Instead, Innis' statement and the evidence of this overlap could be interpreted in one of two ways. It was consistent with the view that Gay's students were united in a strong and tight network. But it also fit with a picture of an overall weak economic history in the U.S., with very few scholars to come and challenge the institutions, resources and honors Gay's students had created. Actually, both scenarios were at play.

4.2 The CREH's activities

The CREH found it very difficult to identify promising economic historians. In the same memo Innis sent to Cole in 1952, he mentioned his overall disappointment with the CREH's actions and results. According to him, the reason for the mediocre performance was "too much money yet no established field", hence the pressure to spend money relatively quickly (the RF usually requested that all funds be spent within the agreed time period) had led to large grants to graduate students! It's not clear why CREH members should have been surprised about this dearth of candidates. They certainly were aware that only a few centers trained economic-historians, and certainly *economist*-historians in the U.S. Already in 1929, the Harvard economist-historian A.P. Usher (one of Gay's first students) had contributed a short article for one of the first issues of the French journal *Annales*, in which he had presented the economic history landscape in the U.S. He had mentioned that there were less than a dozen professors in the field - and only 3 were actual professors of *economic history*, the

[73] *JEH* accounts. Hagley, Accession 1479, Folder 6.
[74] Cole (1970).

others holding chairs in history, economics or political economy.[75] He lamented that in other institutions (he numbered 44 institutions of higher education in the U.S.), economic history was at best a "side job" (and quite often, totally inexistent).[76] A decade later, Heaton's report to the RF roundtable had not struck an appreciably different note. He had painted an ailing field, where there was "less than a dozen teachers of senior status giving their whole time to economic history".[77] Heaton had emphasized that no new chairs in economic history had recently been created, while some were vanishing for lack of adequate replacement.[78] In other words, the CREH should have been prepared for a small number of recruits.

By 1952, a decade after the RF grant was awarded, it was obvious to most members of the CREH that their actions had not resulted in any radical increase in the quantity or quality of work in economic history. The CREH had spent its money on a collection of relatively ad hoc grants, which fit under their original categories (history of banking, government, economic ideas, enterprise) but had no links among them. CREH members themselves conducted a few studies, for example Edward Kirkland's work on transportation. [79] But the bulk of the money

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[75] Usher listed three professorships: two in economics (Harvard and Columbia) and one in History (Minnesota). Though he did not give names, the Harvard economist-historians were Edwin Gay as professor and A.P. Usher as associate professor. The Columbia professorship was in European Economic History held by Simkhovitch until 1944, and in 1930 Carter Goodrich was hired to develop American economic history. Usher also listed three professors who did economic history without holding chairs in economic history: Illinois (economics), Mount Holyoke (history) and Yale (political economy). Interestingly he did not mention Chicago, where Chester Wright (also Gay's student) had been teaching economic history in the 1920s, and John Nef (also Gay's student) had just been hired in 1929. The other institutions he mentioned where economists did serious history were Berkeley, University of Illinois and Northwestern. For information on economic history at Columbia and Chicago see Rutherford (2004a); Rutherford (Forthcoming).
[76] Usher (1929).
[77] Minutes of the meeting, September 1940. RAC-RF, RG3, Series 910, Box 5, Folder 42.
[78] Heaton's picture was not contradicted by any scholar in the room. Many seemed disappointed with the state of existing research in economic history.
[79] Kirkland (1948).

was distributed to relatively young and often completely unknown scholars. Among the more promising grantees were Oscar and Mary Handlin who worked on the role of government, Louis Harz, who worked on the impact of economic and democratic ideas, and Louis Hunter who worked on steamboats.[80] The CREH financed several initiatives in the study of individual businesses or industries; for example Warren Scoville's work on glass manufacturing and a study of the Brown family of Rhode Island.[81] In total they provided financing for 19 book-length studies, though not a single synthetic piece that might have attempted to summarize and link these various findings.[82]

However, in spite of this disappointment, they remained somewhat optimistic. Innis' 1952 memo did indeed end with the hope that some "failures" might turn out to be successes, but that this would require time – as, in his opinion, economic history was a field made up of lone scholars working for a decade or more. Many other CREH members agreed that the fruit of their investment would only appear with time. In their minds, the generalizations that economic historians promised to deliver were well worth the wait. As Arthur Cole wrote in 1948:

> "Amongst the social scientists, only the economic historian is acutely conscious of the time element (…) Economists prefer to think - or unconsciously assume - the world, its institutions, social classes and patterns of thought to be static and unchanging. The economic historian, however, imbibes from all his study the sense of change, a consciousness of time. (…) Herein lie the potentialities of breaking new ground in the social sciences (…) Perhaps the name and surely the connotation of the term 'economic history' will slough away as did 'natural philosophy' a

[80] Handlin (1947); Hartz (1948); Hunter (1949).
[81] Scoville (1949); Hedges (1952).
[82] For a list of all CREH sponsored works, see Cole (1970), 738-9.

> few decades ago. At least the content of the awakened and organized subject would be better suggested by the phrase ‘economic dynamics’.”[83]

This big picture was at the root of Willits and Bezanson’s continued support to the economist-historians, in spite of disappointing results.

4.3 RF’s continued support

Willits and Bezanson had to work hard to keep funding economic history. They had to cover up for the fact that members of the CREH did not have a unifying plan according to which they sought and selected research projects. This lack of a road map was apparent both in minutes of the CREH meetings and early publications of the *Journal of Economic History* (in particular the annual “Tasks of economic history” supplements): each scholar seemed to be pushing for his own hobby-horse.[84] Bezanson took it upon herself to be the CREH’s spokesperson and shadow defendant. Though she was not a member of the CREH, she attended every meeting and reported back to Willits.[85] When criticisms of the CREH’s activities reached Willits’ desk he would forward them to her, and rely on her opinion. Though, at times, she agreed with the specifics of the critique, she always placed it in a larger framework, thus minimizing it. For example, in 1944, she countered criticisms he had received from Robert Crane (director of the SSRC) and Robert Warren (a Princeton economist sitting on the

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[83] Arthur Cole in “A report on economic history”, April 1948. RAC-RF, RG 1.1, Series 200, Box 397, Folder 4708.

[84] See *Journal of Economic History*, Vol. 1 and 2 supplementary issues “Tasks of economic history”. Neither spells out the overall purpose of economic history in the division of social scientific labour.

[85] For an example of her reports, see March 1945. RAC-RF, RG 1.1, Series 200 S, Box 397, Folder 4705.

CREH), both of whom wondered if the CREH was really producing anything worthwhile.[86] In defense of her protégés, Bezanson wrote:

> "Unless one starts with a concept of what the Economic Historians are trying to achieve I do not see a way of judging their work. Their problem is (…) [to find] better ways of framing the questions to be answered and an understanding of the processes in North American development and its peculiar contribution to the development of Western Civilization."[87]

From this perspective, she could weave their apparently eclectic portfolio of research topics into a well thought out strategy.[88] Thus she told Willits and the RF that the economic historians' plan consisted in understanding American economic development, but that before they could look at the specifics of growth, they needed to survey the various types of ideas and policies introduced in America's early years - and trace their impact on every day economic activity. Once the nature and influence of these beliefs had been understood, they would look at the actual mechanics of growth. She was making a case for a building block vision of empirical research that would take time to bear fruit. Her actions and support in 1945 directly resulted in a 5-year extension of the initial grant - which had been scheduled to expire in 1946, but was now extended to the early 1950s.[89] Willits trusted Bezanson's judgment and implemented her recommendations, as he shared her general impression that it would take time before any tangible results could be obtained.

The notion that proper research in the social sciences would require time and money was a constant feature of Willits' administration. In his old age, in

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[86] Letter from Warren to Willits, May 7th 1945. RAC-RF, RG 1.1, Series 200 S, Box 397, Folder 4705.
[87]RAC-RF, RG 1.1, Series 200 S, Box 397, Folder 4704.
[88]RAC-RF, RG 1.1, Series 200 S, Box 397, Folder 4704.
[89] RAC-RF, RG 1.1, Series 200 S, Box 396, Folder 4700.

one of the many letters he wrote to an even older Anne Bezanson, he contrasted the work being done by the RF in the 1970s to the foundation he once knew. He lamented what he considered to be current directives – "subjects will be selected that promise quick show" – comparing it to:

> "Walter Stewart's wise statement to the board: RF does not need to be in a hurry. Nearly everybody else is. But we don't need to be. We should act with the conviction that if we do what we should do now, twenty five years from now something will happen that would not otherwise have happened and we could not have foreseen."[90]

That said, it would be inaccurate to say that Willits and Bezanson were not under any pressure to show results. As mentioned earlier, social sciences were in a delicate position at the RF. During the period of the CREH grant, they found a compromise by moving their support away from economic history in general towards entrepreneurial history in particular.

5 Entrepreneurial History

5.1 The switch to entrepreneurial history

As mentioned earlier, Willits and Bezanson had arrived at the RF in 1939 with the intention of helping social scientists develop a "philosophy" for the western world. Willits became convinced that the origin of this philosophy could be found in the meticulous examination of the U.S.'s success in becoming and remaining a free and prosperous society:

> "If the United States is to be the center of the liberal democratic culture and development, it would seem to be important to know thoroughly and

[90] Letter from Willits to Bezanson, January 16th 1974. Anne Bezanson Papers – held by her niece Doris Souza, Boston Massachussets.

> realistically the story of our own economic development - the development of the most modern industrial nation in the world and the one which exerts the greatest economic force of all."[91]

The study of *American* economic history thus appealed to him as it could generate the "story" the U.S. so urgently needed for itself and to explain its position in an increasingly bi-polar world. Of course, the type of story Willits was looking for was not any story- it was one that squared with his essentially liberal beliefs.[92]

Bezanson had similar aspirations for *American* economic history and she focused on one of the CREH's projects as the most promising theme for the philosophy she and Willits sought. In the mid 1940s she turned her attention to studies of the "entrepreneur" and his role in bringing about freedom and prosperity. Within the CREH, Arthur Cole (1889-1980) had introduced the theme. Cole was one of Gay's first students (Ph.D, 1916) and a professor of economic history in the Harvard economics department until the 1930s when he moved to the Business School (to become librarian of the Baker Library). In a 1942 contribution to the *JEH*, Cole called for a systematic study of entrepreneurship. Bezanson immediately grasped the potential of this research area. In 1943, she wrote Cole:

> "You are confronted with an economic development, especially an industrial and economic development, which was the marvel of the known world. Was it made possible by reason of lack of restrictions both

[91] RAC-RF, RG 1.1, Series 200 S. Box 396, Folder 4700.

[92] Willits' beliefs are well described in the following quote: "I believe that the necessary degree of effective economic interdependence (I dislike the word unity, it implies in advance a totalitarian unity) can be achieved without bondage being necessary or preferable to the individual. My prejudice is in favor of any organization - family, state or church - which distributes responsibility as widely as possible." Letter from Willits to Warren, August 24th 1942. RAC-RF, RG 3, Series 910, Box 3, Folder 17.

> governmental and customary or was there any special stimulus to creativity evident in our entrepreneurial group? (…)
> Somewhere in the development we hit upon a different division of labour than they used in Britain. Surely such an innovation can be explained."[93]

In her hastily drafted comments, one read the excitement at having identified both a fruitful research avenue and one that corresponded nicely to the *American creed* she and Willits were searching for. Willits realized its potential to pacify the board. In a 1948 memo to the RF president – Fosdick - he wrote:

> "I think that one of the most significant things that has come out of the Committee on Economic History has been the emphasis upon and approach to the study of entrepreneurship which is unique. The thought that appears over and over again in one form or another is the lack of a philosophy about our society as a whole - political, economic and social - which characterizes our people. We have no clearly thought-out and articulate scale of values that fits our society, although the Fascists, the Communists, the Nazis and the Socialists all pretend to have one. It's much more complicated than just sitting down and writing a statement about the American way".[94]

Thus Willits was weaving together his favorites themes: a complex society, the battle against simplistic ideologies and an American creed to be constructed from scientific study. In addition, the emphasis on the entrepreneurial spirit was surely meant to appeal to the RF Board. As Fosdick and the Board were not naturally inclined to view empirical, humanistic social

[93] Letter from Bezanson to Cole, July 26th 1943. RAC-RF, RG 1.1, Series 200, Box 396, Folder 4702.
[94] RAC-RF, RG 11, Series 200 S, Box 397, Folder 4708.

science as an end in-itself, Willits tended to emphasize other arguments that he thought would appeal to his constituency, thus interacting with them on a "big picture" level they could understand. This was a skill he had previously honed, having served on the NBER executive board (as a fund raiser) and as Dean of the Wharton School of Business. If Willits' objective was to insure the survival of his type of economics he would have been keen to keep private funders on his side, an increasingly important ally given the post-war advent of new sources of financing for the "other" type of economics via the Military and State.[95] Thus the focus on entrepreneurial history could well be interpreted both as an outgrowth of the CREH' s relatively eclectic brainstorming and as the element deliberately chosen from that experience by Willits and Bezanson to appeal to their funders.

Bezanson encouraged Cole to use the CREH as a springboard for his chosen entrepreneurial theme. When it became clear that this was not an efficient forum for such a project, she backed his decision to create an independent Research Center for Entrepreneurial History (RCEntrepH) at Harvard.[96] Cole managed to gather enough interest to set up a temporary center in 1948 and started soliciting RF help shortly thereafter.[97] Bezanson and Willits managed to obtain temporary grants to pay for salaries of key researchers at the RCEntrepH, and in 1952 the RCEntrepH was awarded $30,000 a year for 5 years. By the end of 1956, RF had awarded $219,000 to Cole's Center.[98] This represented a shift in Willits and Bezanson's alliances from the CREH to the RCEntrepH. Indeed, in 1950, the Foundation had declined the CREH' s application for another 5-year

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95 Mirowski (2002).
96 Principally due to the fact that not all CREH members were equally enthused by the theme's potential. See Letter from Cole to Committee members, August 5th 1943, RAC-RF, RG 1.1, Series 200, Box 396, Folder 4702.
97 According to Cole, the Center was informally started as the "East Coast Institute of Entrepreneurial History", after his 1947 Presidential Address to the EHA. It was then officially chartered in September 1948, see Cole (1970).
98 "Harvard University Research Center in Entrepreneurial History, 1948-1956". RAC-RF, RG 1.2, Series 200S, Box 514, Folder 4386.

term.[99] The field of promising economic history had just shifted from American economic history at large to entrepreneurial history at Harvard. This switch revealed what Willits and Bezanson wanted economist-historians to accomplish – though the focus on historicism and the desire to better understand the process of American economic growth were still there, the emphasis on "facts", in particular of a quantitative nature, had given way to a much vaguer feeling that the entrepreneurial system was the key to their philosophy.

5.2 Business History at Harvard

By creating a center for historical studies of entrepreneurship at Harvard, Cole was certainly not entering virgin territory. Harvard had an existing body of business history, inaugurated before WWI by Gay at the Business School. This tradition was officially entrenched in 1926, with the creation of the Business History Society, and a short-lived journal: *The Journal of Economic and Business History* (which ceased publication at the onset of the Depression). N.S.B. Gras, also one of Gay's students, was one of the founders of the society and the first president. He benefited from the support of Gay's successor at the Harvard Business School – Dean Wallace B. Donham – who helped him create the society, raised funds to endow a chair in Business History, and encouraged Baker Library to build a historical collection.[100]

The founders of the EHA had attempted to rally Gras to their cause, in the late 1930s (recall that he was on the Steering Committee), but he never got involved in any of their activities.[101] This was certainly due to their rather unfavorable judgment of his work, which they considered to be un-analytical.[102] Yet, when Cole started the RCEntrepH he solicited Gras again and some

[99] This did not spell the death of the CREH, which continued to exist (albeit not under SSRC guidance but under the auspices of the EHA) for several decades. However, its lack of funds turned it into a much smaller player on the economic history scene.
[100] Cole (1974).
[101] Hagley, Accession 1479, Folder 9.
[102] See Cole (1970), 728.

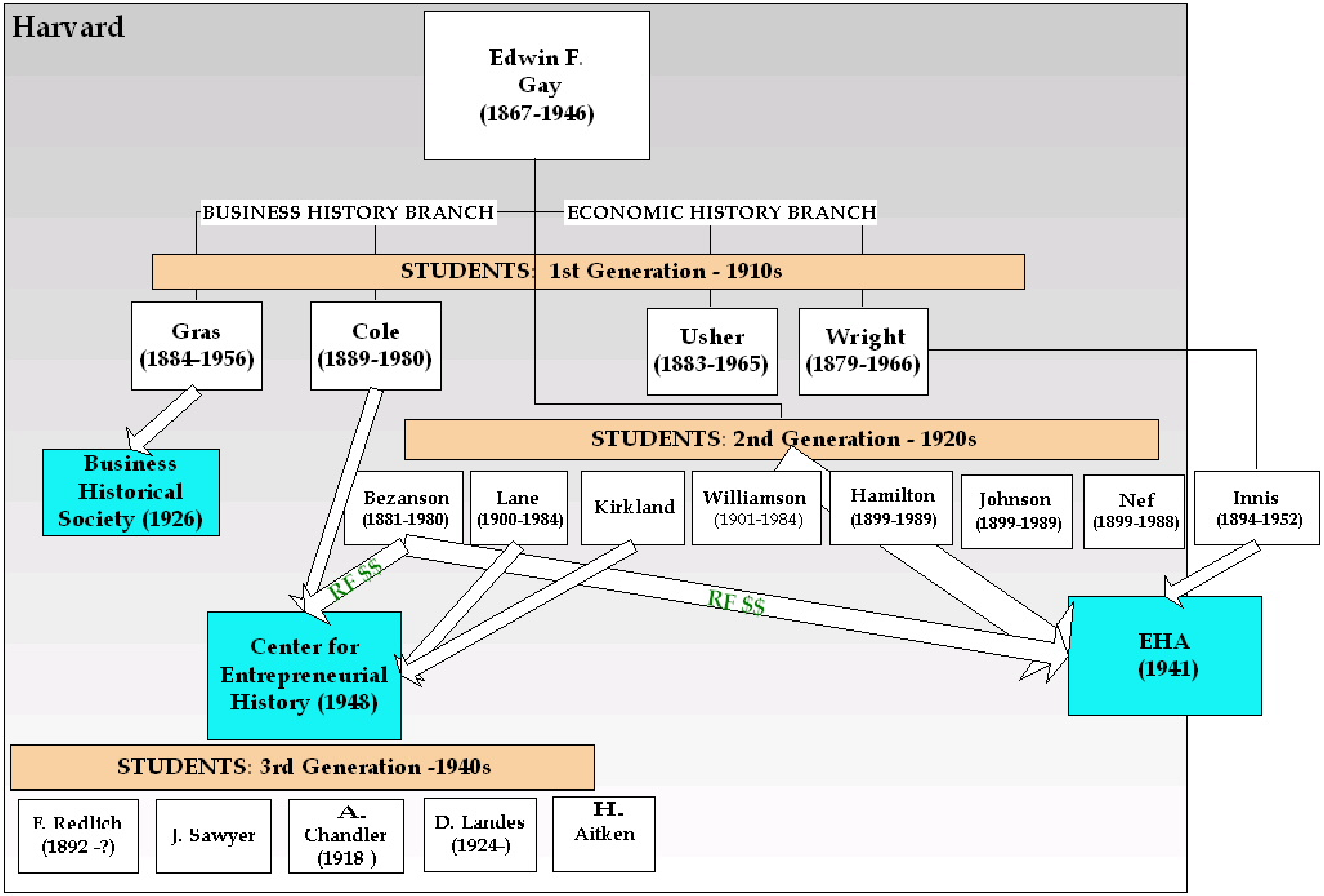
communication occurred via their students. The other, more recent influence for entrepreneurial studies at Harvard was Joseph Schumpeter (1883-1950), who had arrived from Austria in 1932. Schumpeter had written a favorable report for Cole's application to the RF in the late 1940s, and he participated in RCEntrepH activities until his death (i.e. he really only participated for 1 or 2 years).[103]

Thus the RCEntrepH was able to graft itself onto these Harvard traditions, but considerably amplified them, as these earlier currents had no official funding. RF money allowed for the creation of a research center (a physical meeting place), a journal (*Explorations in Entrepreneurial History*) and funds. This resulted in the temporary reunion of two branches of economist-history: economic history and business history. Until then, they had led relatively separate existences and they would diverge again in the 1960s. Figure 3.7 illustrates these two lineages stemming from Gay's presence at Harvard – note that generation 3 students were not Gay's students, but came to business history via Cole and his colleagues at the RCEntrepH.

As seen in Figure 3.7 the key protagonist of the temporary unification of business and economic history was Arthur Cole, though other members of Gay's second-generation students also took part (notably Bezanson, Lane and Kirkland). Their enthusiastic participation in the research life of the RCEntrepH attracted young talent. Among the contributors to *Explorations,* one found many of their colleagues (Nef for example), their EHA and CREH friends (Shephard Clough, Leland Jenks and Thomas Cochran), their students and protégés (Oscar and Mary Handlin, Louis Hartz, Hugh Aitken, David Landes, Fritz Redlich, John Sawyer)

[103] Ibid.

Figure 3.7: Edwin Gay's lineage in economic and business history



but also the prominent sociologist Talcott Parsons, reputed foreign scholars (Peter Mathias, H.J. Habakkuk, Alain Touraine), and economists whose names would subsequently be associated with the cliometric movement (Alexander Gerschenkron, Henry Rosovsky, William Parker, Doug North, John Meyer, Lance Davis for example).[104]

5.3 Entrepreneurial history = economic history = economics

If one takes participation and activity as indicators of a research center's popularity and performance, the Harvard RCEntrepH's first decade was a success. In 1949 the first issue of *Explorations in Entrepreneurial History* was published and it garnered enough interest and submissions to come out nearly monthly. The bulk of the articles were monographs on particular businesses, industries or entrepreneurs, but there was also a fair amount of methodological pieces and several attempts at the sociology of entrepreneurship, identifying trends and features of the general "population" of business entrepreneurs (for example the contributions of Jenks, Parsons and Redlich cited above). The driving theme was the relationship between entrepreneurial activity and economic change and growth.

As Cole envisaged it, the study of the entrepreneur was not just one of many themes one could investigate, it was *the* unifying, umbrella theme, under which issues of change, growth, development and economic order could all be subsumed. Ultimately, it would be a "theory", in the sense of a coherent, comprehensive way of understanding and perhaps influencing the economic world. He made this explicit in the late 1950s:

[104]See for example: Bezanson (1952); Redlich (1952); Gerschenkron (1953); Parker (1954); Rosovsky (1954); Sawyer (1954); Meyer (1955); Aitken (1956); Handlin (1956); Parsons and Smelser (1956); Davis (1957); Jenks (1957); Parker (1957).

> "Economic historians [must achieve] somehow the reputation of being theorists. I am sticking out my own neck in my new book; the final chapter is an attempt to state a theory of economic development in terms of entrepreneurial capacities."[105]

This "theory" was Cole's attempt to synthesize the growing number of biographies, company histories and studies of foreign entrepreneurship. One example of this synthetic work could be found in his 1959 *Business Enterprise in its Social Setting*, where he argued that the chief explanation for the varying wealth of nations was their differing stages of entrepreneurial sophistication. He was not so much concerned with individual entrepreneurs as he was with the "entrepreneurial system": a set of rules of behavior and tools available to the risk-taking individual. According to Cole, mid 20th century U.S. stood out from other countries in so far as entrepreneurship was not the work of one man, but of an executive team assisted by a large number of consultants (legal advisers, strategy advisers, accountants, advertising agents etc…) and knowledge producers (business schools and business scholars!). To substantiate these claims, he used references to sociological studies of the relationship between individual and environment (often citing Talcott Parsons) as well as anecdotal references to concrete cases (drawn from the scholarship of Gras' group and of CREH grantees, but principally by the researchers affiliated to the RCEntrepH).[106]

Having argued that this evolutionary force from crude to sophisticated entrepreneurship was the main motor of growth, Cole went on to examine the forces that favored or inhibited this evolution, pointing to various directions for future research. For example, he wondered how specific historical economic opportunities (Italian exploration of the Mediterranean basin, the discovery of

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105 Arthur Cole in a memo to the Sub-Committee on Future Policies to the Trustees of the Committee on Research in Economic History, 1959. Hagley, Accession 1479, Folder 99.
106 Cole (1959), chapter 3.

the new world) affected the supply of entrepreneurs and the entrepreneurial system. He also asked about the geographical patterns of entrepreneurship and their relation to immigration patterns. In defining such questions, Cole was setting out a research agenda that put entrepreneurial history at the heart of economics – in line with his definition of economics: "[to be a social science, economics must be] concerned with the impact of cultural forces upon the formation and performance of social groups out of which flow the goods and services desired by society."[107] His tight association between entrepreneurial studies and economics at large led him to make extreme claims. For example, as he told *Business Week* in 1952: "if you could transplant some of the thinking of American businessmen, you might be able to speed the development of, say, Brazil or Indonesia."[108]

Critics started pointing to the very speculative nature of the work that came out of the Center. In 1954, Willits' successor (DeVinney) interviewed a young British graduate student, who had spent a year with Cole at Harvard. The latter said he thought that the scholars were too willing to play around with ideas, and not enough facts:

> "He illustrate[d] his point by referring to an entertaining paper presented by a young historian from Yale that economic development in the U.S. during the past century depended in large degree on the over-optimism of the entrepreneurs… It turned out that this was merely an idea that this young man dreamed up and a very lively discussion went on for a considerable period of time entirely on theoretical and hypothetical

[107] Ibid, chapter 4. This is a far cry from Lionel Robbins's 1932 definition: "Economics is the science which studies human behavior as a relationship between given ends and scarce means which have alternative uses".
[108] Business Week (April 12, 1952).

> grounds. The student contrasted this with the situation in Britain, where people did not stray so far away from evidence."[109]

If one recalls the RF's insistence on funding studies based on extensive observation, it might not come as a huge surprise that DeVinney, the new social sciences director at RF, did not renew the RCEntrepH grant. In 1956, he turned down an application for a 6-year renewal.[110] In 1958 the center ceased operations and shut down *Explorations.* Some of its research migrated to Columbia, where Carter Goodrich had been running a research center on "state governments and economic development in the U.S." since the mid 1950s.[111] Recall that the influence of state policies on economic development had been one the themes proposed in the early brainstorming sessions of the CREH, and Goodrich's work in this area was in line with studies conducted by the Handlins for example.[112] The specific focus on entrepreneurship and the stronghold of Gay's network were not upheld at Columbia.

At Harvard, generation 3 students (depicted in Figure 3.7) held on to their interests in entrepreneurial themes, but soon moved to other institutions to take their first academic jobs (Sawyer went to Yale, Chandler went to MIT for example). Business history continued to exist within the business school, and would provide the seat for a renewal of business research in the mid 1960s, when separate institutions for business history were resurrected and reinforced, as we shall see in chapter 7.[113] It thus took a change in the directorship of Social Sciences at the RF to bring an end to the continuous funding of Gay's lineage. This abrupt change confirmed both Willits' power in choosing his grantees and his growing fear that his type of economics would not survive in the long term.

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[109] From DeVinney diary, December 9th 1954. RAC-RF, RG 1.2, Series 200S, Box 514, Folder 4386. The graduate student was not named.
[110] RAC-RF, RG 1.2, Series 200S, Box 514, Folder 4387.
[111] Cole (1970).
[112] Cole (1974).
[113] Chandler (2004).

6 Conclusion

In 1958, when the RCEntrepH closed down, American economic historians were in an unstable situation. On the one hand, they had lost their connection to direct sources of funding, but on the other they had forged what appeared to be lasting institutions. In 1958, the EHA had a little over 1400 members; at least 1000 had an American address, up from approximately 300 in 1941. This can be compared to the AEA, whose domestic membership tripled from about 3,000 in 1941 to a little over 9,000 in 1958.[114] Both the *JEH* and the *EEH* were regular periodicals, and the *JEH* seemed to have an increasingly healthy rejection rate: George Rogers Taylor (editor from 1955 to 1961) remembered accepting 40% to 50% of the articles, which was more selective than what Cochran and E.A.J. Johnson recalled of their 1940s editorships (the war was a particularly dim time for journals).[115]

These scholars' ability to put economic history on the American academic map was the result of both need and opportunity. The need seemed to emerge from a change in the interwar economics landscape, leading many social scientists to worry about the direction economics was taking, away from humanism and their type of empiricism. The opportunity came from the relative strength and cohesion of their network, based on a common lineage, and shared philosophical and epistemological views. Anne Bezanson's strategic placement as consultant at the RF gave them resources to act. Their dependence on the RF lifeline tended to push this self-generated demand towards more American, more conservative themes. Entrepreneurial history gradually emerged as an area that could combine their thirst for empiricism, multi-disciplinarity and

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[114] For EHA membership, see Treasurer's Reports, Hagley, Accession 1479, Folder 23-27. AEA numbers can be found in Siegfried (1998).

[115] See Parker Questionnaire and Answers, in Hagley, Accession 1479, Carboard Box, Folder "JEH Parker".

commitment to long term research with the RF's growing suspicion that social sciences were not in any shape to yield concrete, usable knowledge.

Though they did not succeed in checking the tide of tool-based economics, they did manage to create a quasi independent "home" for economic history, with its own institutions and enough visibility to become worthy of consideration by the next big patron: the Ford Foundation.[116] This had important implications for the future of the discipline. For one thing, it gave the cliometricians something to take over. For another, this institutional inertia seemed to have set the themes and questions that ensuing generations of American economic historians would continue to tackle, thus effectively defining patterns for the field's evolution. This occurred in spite of the subsequent "revolution" in methods, changes that were creeping in from many directions, as we shall see in chapters 4 and 7. American economic historians held on to a fascination for banking, railroads, key industries, and other motors of American economic growth, even if they presented their work as essentially revisionist of these "old" interpretations. In addition, they rarely broke free from the view that economic history was principally an activity for economists interested in growth and the successful development of nations. As we shall see in chapter 5, this contrasted sharply with the situation in other countries, France for example, where economic history never stood apart from social history and investigations were seldom related to economic growth.

[116] In 1957, the Ford Foundation organized a round table on economic history to take over from RF – see chapter 6.

CHAPTER 4.

MEASURING THE PAST THROUGH NATIONAL INCOME ACCOUNTS

1 Introduction

One of the most striking differences between economic history as it was practiced by CREH scholars (see chapter 3) and post-1960 cliometricians was the evolution of the meaning of "empirical". For the founders of the EHA, the word covered a wide range of evidence, quantitative or not, whereas for 1960s cliometricians, to be empirical was to be quantitative. [1] It has sometimes been assumed that this increase in the use of quantification in economic history was the result of the application of neoclassical economic theory to historical questions, as mathematical expression presumably forced cliometricians to look out for numerical counterparts in their body of evidence.[2] Yet, such

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[1] In 2003-2004, I conducted interviews with a dozen scholars who were either graduate students or rising academics in the 1960s and early 1970s (the height of the "cliometric revolution"). I asked them to comment on the word "empirical" for this time period; they all associated it with "quantitative" (including Paul David, Lance Davis, Richard Easterlin, Robert Fogel, Claudia Goldin, Deidre McCloskey, Hugh Rockoff, Peter Temin and Gavin Wright). A notable exception was Douglass North who was the only one to associate "empirical" with quantitative and qualitative information. Most respondents indicated that their views had since changed, and that they now willingly and explicitely relied on numerous types of evidence . See Easterlin's response as an example: "subsequently, I've come to realize that this sort of micro level evidence, diaries and, you know, various historical records are an important empirical source."

[2] For a discussion of this assumption, see Whaples (1991).

interpretations are inconsistent with two features of the historical record. First, there was much quantification before the advent of theoretically driven economist-history (recall Gay, Hamilton and Cole's work mentioned in chapter 3). Second, the spread of quantification seemed to have antedated the spread of mathematization. Robert Whaples' has studied the evolution of articles published in the *JEH* and shown that the number of tables printed per page – his indicator for quantification - increased tenfold between 1941-45 and 1966-1970 – the big discontinuity occurring in the late 1950s, early 1960s, *before* cliometricians had gathered momentum as a self-identified revolutionary group, whereas mathematization of the *JEH* happened five to ten years later.[3] This suggests that the history of quantification in American economic history does not quite overlap with the history of the cliometric movement and that we may benefit from a closer examination of the types of quantification in economic history, and of their evolution over time.

Quantities and quantification have been the object of much study among historians and philosophers of social science.[4] They have examined the role numbers played in the edification of social scientific knowledge and they have brought to our attention the existence of many different types of numbers – whose descriptive, representational, rhetorical and analytical power vary. According to them, not all numbers carry equal weight and much of their power depends on the method by which they were obtained. Theodore Porter for example has shown that the types of numbers that most increased social scientists' legitimacy in the 19th and 20th centuries were the ones generated by "standardized quantitative rules". According to Porter, numbers defined by fixed rules (and supported by bureaucracies that could monitor and enforce these rules) became an increasingly important feature of public and scientific life. Cost-benefit accounting, for example, was rapidly adopted in public

[3] Ibid, 293.
[4] Morgan (1990); Porter (1995); Desrosières (2000); Porter (2001).

administrations to monitor and distribute tax revenues, thus reshaping lobbyists and legislators' interactions.[5]

The general field of accounting, and national accounting in particular, has often been used as an example of the complexities and rewards of building these "standardized quantitative rules", or "measuring instruments", as some historians prefer to call them.[6] Though these histories do not specifically consider retrospective accounting (building national accounts for the past) in their analysis, there is a case for examining the impact on economic history of a national accounting view. At the heart of this connection lay Simon Kuznets, whose work inspired generations of economist-historians.[7] The contrast between the rapid dissemination of his retrospective accounting project (rewarded with a Nobel prize in 1971) and the sudden end of Gay and Beveridge's International History of Prices initiative invites us to ask: what were the differences between national accounting quantifications and other numbers, like the prices and price indices used in the International Price History Project? Did the former have any conceptual or organizational advantages over the latter? Did Simon Kuznets' influence entail a significant change in the meaning of evidence for American economic historians and did this alter the terms of the perennial debate around the proper status of observation in economic science?

This chapter begins with an investigation of the roots and course of Simon Kuznets' disagreement with the "old" economic historians. Section 3 then delves into the conceptual and organizational factors that permitted the rise and spread of retrospective accounting. Section 4 uses a disagreement between Walt Rostow

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5 Porter (1996), 39-43.

6 Morgan (2003).

7 Easterlin explicitely made the connection between "empirical = quantitative" and Simon Kuznets. To the question: "what did the word "empirical" mean to you in the 1960s?" he replied "well, I think the way I would have interpreted it and the way Kuznets interpreted it had to do with the construction of time series estimates of income and labor force and population and all kinds of things of that sort." Richard Easterlin, Interviewed by Cristel de Rouvray, San Diego, January 2004.

and Simon Kuznets to spell out the implications of this accounting view for American economic history, in particular the ways in which it changed the notion of reliable empirical evidence. Throughout, the chapter argues that Simon Kuznets' efforts to frame the past in a national accounting framework were greatly enhanced by the situation in post-WWII U.S., as he was able to leverage the demand for growth and development knowledge and piggy-back on the establishment of national accounting offices in numerous countries. This successful combination of conceptual (though not necessarily theoretical) and organizational features contributed to marginalize economist-historians who relied on more traditional forms of evidence (quantitative or not) to derive arguments about the origin and dynamics of economic growth.

2 Kuznets and the "old" economic historians

2.1 Kuznets' reasons for attending the 1940 Rockefeller Foundation (RF) round-table meeting

As mentioned in chapter 3, Kuznets was the only participant at the September 1940 economic history roundtable organized by RF not to belong to Edwin Gay's network of students and colleagues (recall Figure 3.4). Born in Ukraine in 1901, Simon Kuznets had emigrated to the U.S. in the early 1920s and began an academic career under Wesley Mitchell's guidance at Columbia University. He had earned his degrees with impressive speed (BA-1923, MA-1924, Ph.D.–1926), indication that he already was a fine social scientist when he landed on American soil. Before leaving the Soviet Union he had worked in the Ukranian bureau of labor statistics, where he had acquired solid foundations in empirical and statistical work.[8] Kuznets' family also seemed to be well versed in

[8] Easterlin (1971) believed that Kuznets' methodological dispositions were developed prior to his arrival in the U.S. Kapuria-Foreman and Perlman (1995) cited more specific evidence for this claim: they mentioned two papers that he wrote as an economics student at the University of Kharkov. The first was on money wages of factory workers

the general area of social science – his older brother Solomon was on track to becoming a well-known statistician, and the Russian economist Kondratieff was a "family friend".[9]

When Kuznets graduated from Columbia, Mitchell invited him to join the staff of the National Bureau of Economic Research (NBER), where he began a long career (1927-1961) in the study of business cycles, national income and capital accumulation. The NBER had only been founded a few years earlier (1922) and Kuznets soon became recognized as one of the most accomplished scholars in Mitchell's team of economic measurers. As most economists affiliated to the Bureau, he worked on aspects of the business cycle, and his first major contribution to scholarship was the identification of long term swings (20-25 years) which he uncovered from a historical study of the American economy – these subsequently came to be known as "Kuznets cycles".[10] Though the bulk of his work was oriented to empirics and measurement, he showed some willingness to generalize from the evidence – for example, he established that the consumption to investment ratio was stable throughout time, thus refuting the theoretical proposition that the aggregate marginal propensity to consume declined as national wealth increased.[11] In 1930 he obtained his first academic post and became Professor of Statistics at the University of Pennsylvania, where

in Khrakov; the second was on Schumpeter's theory of innovation. Both the quantitative empirical work in the first paper and the interest in Schumpeter's ideas seemed to stick to Kuznets throughout his lifetime. The authors stated that Kuznets' main argument in the Schumpeter essay was that the later had great insights but untestable propositions. Already the importance of measurement as a pre-requisite for testing and proper scientific work was a part of Kuznets' method; as we shall see in his debates with Rostow.

[9] Angus Maddison recalled that Kuznets told him Kondratieff was a "family friend" and that the Kuznets family had tried to bring the great Russian economist to the U.S.; Angus Maddison, Interviewed by Cristel de Rouvray, Compiègne, France, March 2004.

[10] Easterlin (1979); Fogel (2000).

[11] Kapuria-Foreman and Perlman (1995).

he remained until 1954, when Johns Hopkins awarded him a full professorship in Economics.[12]

Among his U. Penn colleagues, Joseph Willits and Anne Bezanson appear to have been two of his closest acquaintances.[13] Kuznets' presence at the 1940 RF roundtable was a testimony of this friendship and collegiality. Anne Bezanson's subsequent home visits to Kuznets to convince him to join the Committee for Research in Economic History (CREH), and his acceptance in spite of his objections to their general program, further reflected their mutual respect and support.[14] While Kuznets seemed to have a more personal link to Bezanson, he certainly was not unfriendly with Joseph Willits, who held the Russian émigré in high esteem and consistently solicited his scientific opinion on many initiatives the RF was considering funding. In 1940 Willits wrote the RF president that: "Kuznets is the man who has contributed more in economic research in the last ten years than any other economist."[15]

Kuznets' presence at the 1940 RF roundtable was both an indication of his closeness to Willits and Bezanson, and of his general interest in the broad

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[12] Some of Kuznets' biographers have suggested that his U. Penn appointment was not as prestigious a post as a professorship in economics, and that Kuznets' Judaism may have been the reason he was not invited to join the U. Penn Economics Department - Ibid, 1534-7. They added that Kuznets' subsequent appointment at Johns Hopkins was slowed down by a notoriously anti-semitic Dean. Mark Perlman, Interviewed by Cristel de Rouvray, Chapel Hill, North Carolina, June 2003. The general impression that American academia was not congenial to outsiders like himself may have been one of the reasons Kuznets so energetically helped the State of Israel develop its own academic landscape after WWII.

[13] Recall from chapter 3 that both Willits and Bezanson were also at the University of Pennsylvania in the 1930s. Willits was Dean of the Wharton Business School and Bezanson was director of the Industrial Research Department at Wharton from the late 1930s to the mid 1950s.

[14] Both may have felt like outsiders at U.Penn; Kuznets because he was Jewish, Bezanson because she was a woman. Bezanson told her niece that she had to take up smoking, to encourage her male colleagues to drop by her office!

[15] Memo from Willits to Fosdick, October 25th 1940. RAC-RF, RG 3, Series 910, Box 10, Folder 86.

initiative they were considering – namely a grant in economic history. As he would tell Mitchell in 1943:

> "As you may remember, interest in [the comprehensive analysis of the longer term economic trends] was one of the main reasons why I was eager to serve on the Committee on Research in Economic History (where my views had a thorough airing and negligible, if any, effect)."[16]

Kuznets' study of history was tied to his interest in long-term phenomena and to their "comprehensive" observation, by which he meant a description and understanding of the general aspects of economic growth. Kuznets' commitment to history and to the long term were the cornerstones of the work he would produce in the decades following WWII, and they were most cogently expressed in the crowning synthesis of decades of research, namely his 1966 *Modern Economic Growth: Rate, Structure and Spread.* As he explained in the first chapter, he wanted to highlight the features and dynamics of modern economic growth, a phenomenon he deemed historically distinct from earlier periods of economic activity, even from earlier periods of capitalism (such as medieval merchant capitalism).[17] Establishing the distinctiveness of this new stage of economic activity was one reason for studying the history of developed nations. But modern economic growth was not an exclusively mid 20th century phenomenon. According to Kuznets, it dated at least as far back as the late 18th century, and the context of its emergence and the dynamics of its evolution were crucial to understanding its present shape (in particular if one was concerned with the fate of less developed nations).

[16] Kuznets to Mitchell, September 24th 1943. Harvard University Archives, Accession 88.10, Box 2, Folder: "Comparative Economic Growth: Conception and Exploration".
[17] Kuznets (1966), 7.

In other words, in order to describe and understand economic growth appropriately, one needed to apprehend it over the long term – only then would the generalizations derived from empirical study stand any chance of explaining it. Kuznets presented this argument in statistical terms: the sample needed to be big enough (across space and time) to generate enough variability to establish reliable causal knowledge.[18] Those who did not embrace a sufficient time span produced inadequate conclusions.[19] As he reminded his colleagues in his 1955 presidential address to the AEA, economists had a "natural tendency (…) to generalize from what little experience is available - most often the short stretch of historical experience within the horizon of the interested scholar". He tied this dubious instinct to legacies from the Classical School (generalizations from British historical record which were limited by "the brevity and exceptional character of that period and place") and Marxism ("overgeneralization of imperfectly understood trends in England"). For Kuznets, the only check to these tendencies was "the observation of a greater variety of historical experience and a recognition that any body of generalizations tends to reflect some limited stretch of historical experience".[20] The recognition that previous generations of economists had failed to cast a wide enough net in their investigations only served to strengthen his conviction that economic growth needed to be urgently studied, defined and explained, given the increasing popularity of the concept in the post war world.

2.2 Kuznets attempts to convince the CREH economic historians to adopt his approach to historical study

While the participants of the 1940 meeting all agreed that economic history was an indispensable area of study for the economist, this did not entail

[18] Ibid, 487-90.
[19] Ibid, 9.
[20] Kuznets (1955).

any agreement on the framework and method of study. Kuznets offered his critique of the direction Gay and his students were taking in a lengthy report he sent to Willits a few weeks after the 1940 meeting.[21] In this report, he stated that the proposal Cole, Gay and Johnson had put forth lacked "a basic framework within which [individual] studies will be evaluated and cumulated into a significant story of the historical development of this country's economy". This worried Kuznets because although he agreed with the basic motivation of the grant - an explanation of "economic change in its chronological succession… to reveal the factors that govern [it]"- he had strong beliefs about the means that would make such an investigation successful. For Kuznets, this project had to be comprehensive, comparative and precise.

A comprehensive view – According to Kuznets, the first step was to draw "a cogent (…) picture of the economic development of the whole country", rather than immediately zoom in on factors that were deemed important ex ante:

> "The crux of understanding secular or long term changes is in visualizing their continuous interplay in the economic system (that is why we call the economy an economic system) (…) The breaking up of the area among industries or separate aspects of the economy runs the danger of missing the whole in the study of parts."[22]

Indeed the CREH economic historians had no plans to deliver an overall view of the system: having already established the areas (banking, legislation) and actors (entrepreneurs, government) they wanted to examine, without any stipulation about the ways in which these areas specifically related to each other.

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[21] Kuznets' Comments on the Report on Projects of Research in American Economic History, October 1940. RAC-RF, RG 3, Series 910, Box 5, Folder 42.
[22] Comments on the Report on Projects of Research in American Economic History (p.12), October 1940. RAC-RF, RG 3, Series 910, Box 5, Folder 42.

A comparative view – In his 1940 memo, Kuznets reminded Willits that one could not generalize about the U.S. without a reference point:

> "I agree (…) that the emphasis is to be placed upon the study of the American economy (…) It is of prime importance to study the economic development of this country (…) we should also try to see whether the pattern of temporal changes which this country revealed in its historical past is not similar to the pattern of temporal changes in other countries of similar economic structure (i.e., countries of developed industrial capitalism, such as Great Britain or Germany)." [23]

In contrast, the economic historians were committed to an in-depth exploration of American economic history, and had not presented any rationale for carefully or systematically comparing it to other countries.

A precise view – According to Kuznets, one could not assess the relative importance of various economic factors without measuring them. Though Kuznets acknowledged that not all meaningful data could be quantified, he insisted that the first step was necessarily quantitative:

> "Above all the framework needed should comprehend a quantitative and analytical study of the character of these temporal changes of various description that would permit us to distinguish the groups of factors of primary importance to each." [24]

Kuznets' commitment to measurement and quantification had been well honed at the NBER. Though NBER economists gathered numerous types of information

[23] Comments on the Report on Projects of Research in American Economic History (p.8), October 1940. RAC-RF, RG 3, Series 910, Box 5, Folder 42.
[24] Comments on the Report on Projects of Research in American Economic History (p.3) RAC-RF, RG 3, Series 910, Box 5, Folder 42.

(qualitative and quantitative) they synthesized it all into numbers. Importantly, all final analysis was grounded in tables and charts, which were the heart of Wesley Mitchell's "rigorous" work (and he insisted that most other work in economics was unscientific and speculative).[25]

Gay's students were certainly not newcomers to data collection and quantification (recall that Gay had been one of the original visionaries for the NBER) but they did not have Kuznets' training in measurement nor did they have his statistical knowledge. The disagreement between them seemed not to be "whether or not to quantify" – but rather "how" and "why" one should quantify. If one compared Bezanson's 1930s work on prices in the Philadelphia region to Kuznets' proposed plan for a comprehensive, precise and comparative view of the past one began to sense the difference between these strategies for quantification.[26] Bezanson's prices of individual commodities, and even her price indices (compounds that were supposed to reflect the overall evolution of all prices) constituted a string of available facts with very few clues as to where these facts fit in the broader context of American economic development. Even if she had further developed her causal analysis of the origins of price trends and fluctuations, her conclusions would have been hard to relate to questions about industrialization, productivity, sectoral growth: questions which were beginning to emerge as focal points in the American economics profession. Kuznets on the other hand wished to build a long-term view that could be used to answer such questions.

In essence, Kuznets laid out a much more structured view of the research project he thought the RF should finance, and cautioned against ad hoc apprehensions of the past. His recommendations were not taken into account when the RF allocated funds to create the CREH. As is evident in the studies they commissioned, their notion of useful research had a very different focus than

[25] Morgan (1990).
[26] Bezanson, Gray and Hussey (1935). See chapter 3 for a description of this work.

Kuznets'. They funded studies of micro and macro events (for example studies of one north-eastern banking system as well as a study of the first federal bank and the national capital market) and of a qualitative and quantitative nature (for example a study of the Brown dynasty in Rhode Island as well as a study of bank reserves and interest rates). In general they had no stipulations as to which type of empirical evidence came first, and their standards of reliability rested on the critical examination of the sources rather than on a reflection on how useful any particular piece of information would be in the general picture. Kuznets and the "old" economic historians did not specifically confront each other on the matter of what and how to observe, yet they certainly held different views on what constituted reliable and useful empirical data.

Kuznets repeated his dissenting opinion on numerous occasions, yet was not listened to.[27] He resigned from the CREH in 1943 – though RF officers had enjoined him to stay until the end of the war. During his two-year tenure, he was a reluctant participant and minutes of meetings show his absence and lack of influence.[28] This disagreement would last for years. For example, in a 1951 paper in the *Economic History Review* Kuznets defended his rationale for using aggregate measures to capture historical change. He contrasted the potential completeness of numerical data to what he deemed to be the necessarily limited scope of qualitative analysis:

> "To illustrate: total production of cotton cloth by the textile industry in Great Britain in 1925, or over a period extending from 1860 to 1938 can be measured quite readily. Contrast this with the difficulty of giving a complete account of 'entrepreneurial activity' (…) in the same industry during that same period."[29]

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[27] See for example, Kuznets (1941b).
[28] See for example letter from Cole to Willits, September 9th 1942. RAC-RF, RG 1.1, Series 200, Box 397, Folder 4701.
[29] Kuznets (1951), 267.

The reference to the entrepreneur was probably not entirely innocent. It reflected Kuznets' disagreement with the agenda Arthur Cole and his colleagues had put forth.

3 Building the measuring instrument

3.1 The conceptual basis

Though Kuznets did not describe a specific framework in his 1940 report to Joseph Willits (where he wrote in very general terms), there was much evidence that he was already organizing his own view of the past within a national accounts perspective. National accounting had a long history (dating as far back as the work of French and British civil servants in the 17th century), yet the mid 20th century witnessed an acceleration of the development and deployment of national accounts worldwide.[30] Kuznets was one of the main actors in this process. In the early 1930s he had headed the construction of the first official accounts for the U.S. (estimates for 1929, 1930 and 1931). Prior and subsequent to this involvement with the Department of Commerce he had worked on the theory and measurement of national income accounts, having taken the lead of the NBER's project on U.S. National Income – a responsibility he held for 15 years (1931-1946).[31] In 1933, he had contributed an article on national income for the *Encyclopedia of the Social Sciences*, which served as a theoretical guide for decades.[32]

As part of the NBER initiative, Kuznets built accounts for past periods - pushing American series back to 1919 and then to 1869.[33] In doing this he was not breaking new conceptual grounds, but improving estimates made by W.I.

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[30] Studenski (1958).
[31] Kapuria-Foreman and Perlman (1995), 1531. This article had been commissioned by Kuznets brother, Solomon, who advised the editor of the Encyclopedia.
[32] Fogel (2000), 8-9.
[33] Kuznets (1941a); Kuznets (1946).

King and pushing them as far back as the civil war. The idea of apprehending the past via national income was not new. However, the idea of creating a consistent set of estimates, pushed as far back as possible, and leveraging the power of the accounting balances to fill in the missing gaps was arguably quite innovative, and Kuznets certainly was one of the first to do it at such a scale. [34]

Kuznets was not only calculating an aggregate national income statistic for each year or each period; but he was building national accounting tables, bringing to light all the different parts of the macro-economic system. His favored metaphor was the human body: national accounts were the anatomical view of the economic system; the first step in making sense of the dynamics and causal relationships he wished to uncover.[35] Thanks to this systemic view that related all variables to each other in an unambiguous way (the accounting equations), Kuznets had a means to cross-check the consistency of his data (and consequently its validity) and impute missing variables where he could not find the relevant micro-economic data (because it did not exist in archives or almanacs). For these reasons it may be useful to think of retrospective accounts as a measuring instrument, as the framework could be used to generate new data that had not been found in any existing record (the instrument created numbers both by aggregation - from the sum of different inputs - but also by imputation).

For example to create the consumption accounts, and break them down according to category of goods (there were four categories: perishable, semi-durable, durable and "all other goods" which included services) Kuznets could start from the highest level of aggregation (total consumption could be established from the national income figure obtained via the production accounts due to the accounting equations that related income to production and

[34] At about the same time, Colin Clark was attempting a similar exercise in the U.K.- published as Clark (1940). However his series did not go as far back as Kuznets' did, and as we shall see in section 3.2 he did not have Kuznets' phenomenal organizational talents that insured the comparability of international data.

[35] Kuznets (1955).

consumption to income); then he could individually estimate each of the three large consumer categories where data was available (perishable, semi durable and durable); to finally "derive" a figure for all other consumer goods. This last figure was obtained by computing the "residual" ("subtracting from national product independently estimated final product categories except services").[36] As we see from this example, the national income framework was conceptually strong (the researcher had to start out with fixed equations relating one variable to another) but theoretically weak (there did not need to be any presuppositions about the ways in which one factor affected another via the agency of individuals or institutions). Significantly, Kuznets was not a Keynesian.

By 1940, when Kuznets attended the RF roundtable he had been working on U.S. retrospective accounts for nearly a decade and seemed to have developed the conviction that this project could significantly add to economic knowledge if it could be turned into an international, comparative study of growth. His decision to attend the roundtable meeting was prompted by his desire to find a sponsor, and he was simultaneously probing the CREH and the NBER as potential donors. Both turned him down. It is unclear why Mitchell did so.[37] He was nearing the end of his career (he would retire in 1946) and Arthur Burns had been designated as his chosen successor. There was known rivalry between Burns and Kuznets (who had been the other possible candidate for succession), and this may have been a reason why the NBER decided not to house Kuznets' long-term study.[38] Yet, there may also have been more substantive reasons for the rejection.

The first such reason may have been was different degrees of eagerness to generalize from empirical work, a difference prompted by differing degrees of pragmatism and sensitivity to urgency. When comparing Kuznets to Mitchell,

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36 Kuznets (1946), 77.
37 Kuznets to Mitchell, September 24th 1943, Harvard University Archives, Accession 88.10, Box 2, Folder: "Comparative Economic Growth: Conception and Exploration".
38 Kapuria-Foreman and Perlman (1995).

some biographers have highlighted that the former's "thrust towards theoretical generalization was much stronger than Mitchell's".[39] By the 1940s this divergence may have become increasingly difficult to accommodate, as the NBER was under growing pressure to defend its "empirical only" commitment. It was harder for them to continue claiming that economics needed a sound empirical basis before any generalizations could be risked, as they had been accumulating evidence for two decades, with very little general propositions to show.[40] Thus Kuznets' greater willingness to take the generalization step may have been perceived as a slight threat, or at least a counterculture move. This reminds us to be careful in indiscriminately labeling economists as "empiricists", when this apparently homogeneous term masked acrimonious differences. This may be one way of interpreting a confidential letter Kuznets sent to Willits when his project was turned down:

> "My own feeling is that the National Bureau is becoming all together too narrow minded, and that it would have been well to experiment with the [growth project] as a departure that might open way to types of research different from the kind pursued at the Bureau now."[41]

Kuznets' greater willingness to use national accounts to develop a theory of growth went hand in hand with a certain degree of pragmatism regarding available data and its quality. His students and colleagues were always encouraged to make the best with what they had, rather than shun the task altogether because the data was too patchy or too rudimentary. He reminded them to learn about the ways in which the data had been produced (i.e. about the

[39] Fogel (2000), 6.
[40] Mitchell himself acknowledged and somewhat regretted the delays in producing his second masterpiece on business cycles; see Morgan (1990).
[41] Kuznets to "Joe", undated. Harvard University Archives, Accession 88.45, Restricted Letters.

past institutions that collected and preserved it) to extract meaningful information from the numbers, in spite of their obvious flaws. The knowledge set needed to make such imputations was another reason for encouraging his students to study economic history.[42] This disposition towards data quality definitely clashed with Mitchell and Burns' general philosophy on useful empirical evidence, which may have been a second explanation for the NBER's rejection. According to Burns, retrospective data in the U.S. (and perforce in every other country) was simply not reliable enough to be used in any meaningful way.[43]

Kuznets' answer to this conservatism was to underline the importance of the study's final goal. To those who had a "tendency to shrink from long-term estimates [and rationalize it] by references to an increasing inadequacy of the data as one goes back in time", he replied: "we patently need tested information about our past, and hence about the conditions that govern our choices today".[44] According to him, there always were ways around poor data. Sometimes it simply meant that one had to change the order of magnitude – data that was insufficient to depict a country's evolution on a year-to-year basis, could still be useful for a decade-by-decade analysis. In Kuznets' view, the latter was much better than nothing, as the economist had a responsibility to decision makers around him.[45]

Kuznets' personal experience measuring U.S. income for the 19th and early 20th centuries armed him with the conviction that this was a potentially rewarding project, but that it would require sustained work on the conceptual and organizational level to carry out satisfactorily. On the conceptual level he reflected on the representational value (i.e. the "realisticness") of each accounting

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42 Fogel (2000), 18-20.
43 Burns would sometimes accuse Kuznets of being a sloppy data collector as recalled by Mark Perlman (interview, June 2003).
44 Kuznets (1952).
45 Ibid.

item. But Kuznets soon came to realize that these conceptual issues were less crucial than the necessity to establish reference points from which to draw conclusions – hence the need to study more than one country and to do so in a standardized way. To some extent it was more important to gather comparable data from one country to the next than to strive for representational accounts of each place (which may be difficult to compare, as each nation had its institutional idiosyncrasies). In other words, his project required a large degree of organization and coordination across multiple countries – a goal that would either require lots of money or some money and a favorable international situation.

This invites us to reflect on the historical and philosophical literature on measuring instruments. Morgan has agreed with Porter that the most powerful quantifications are those produced with relatively standardized procedures, yet she insisted that this was only a necessary and not a sufficient condition. According to her, they must also have representational value, i.e. have a counterpart in human beings' experience. She compared business cycle measurements to national income measurements, to show that lack of conceptual clarity and representational fit in business cycle indicators accounted for their relatively small popularity and dissemination, certainly compared to national income accounting concepts like GDP – which were completely taken for granted within a few years of their introduction.[46]

Seen from this perspective, Kuznets' advantage should have principally come from his instrument's conceptual fit with reality. Yet his failed attempts to convince either the "old" economic historians or NBER economists to go along with his vision invites us to go beyond the representational power of national income accounting and inquire into the more tangible factors that permitted the collection of standardized, comparable data. We must consider the context in which Kuznets managed to build an organization, a network and a pattern of

[46] Morgan (2003).

division of labor. In other words, as Shapin and Schaffer showed in their analysis of the air pump and the emergence of experimental culture, one cannot understand the power of a measuring instrument without attention to the culture and the organizational model it required and promoted.[47]

3.2 The Organizational Basis

As can be gleaned from the time line depicted in Figure 4.1, WWII temporarily interrupted Kuznets' work on retrospective accounting. Kuznets' war-time experience appeared to strengthen his conviction that the accounting framework was a particularly useful way of depicting the world. In 1942 he was recruited to the War Production Board by one of his former students, Robert Nathan.[48] Nathan and Kuznets' central contribution was to set up material procurement in a national accounting framework.[49] In doing so they established the possibilities and sources for increasing and streamlining production. This systematic approach led Kuznets to re-evaluate the Military's post Pearl Harbor plan and suggest ways in which they could avoid exerting undue pressure on the civilian economy.[50]

Use of national accounts at the War Production Board and in its British counterpart was one of several events that turned national income accounting into an indispensable tool for economic organization and planning in North America and Europe after WWII.

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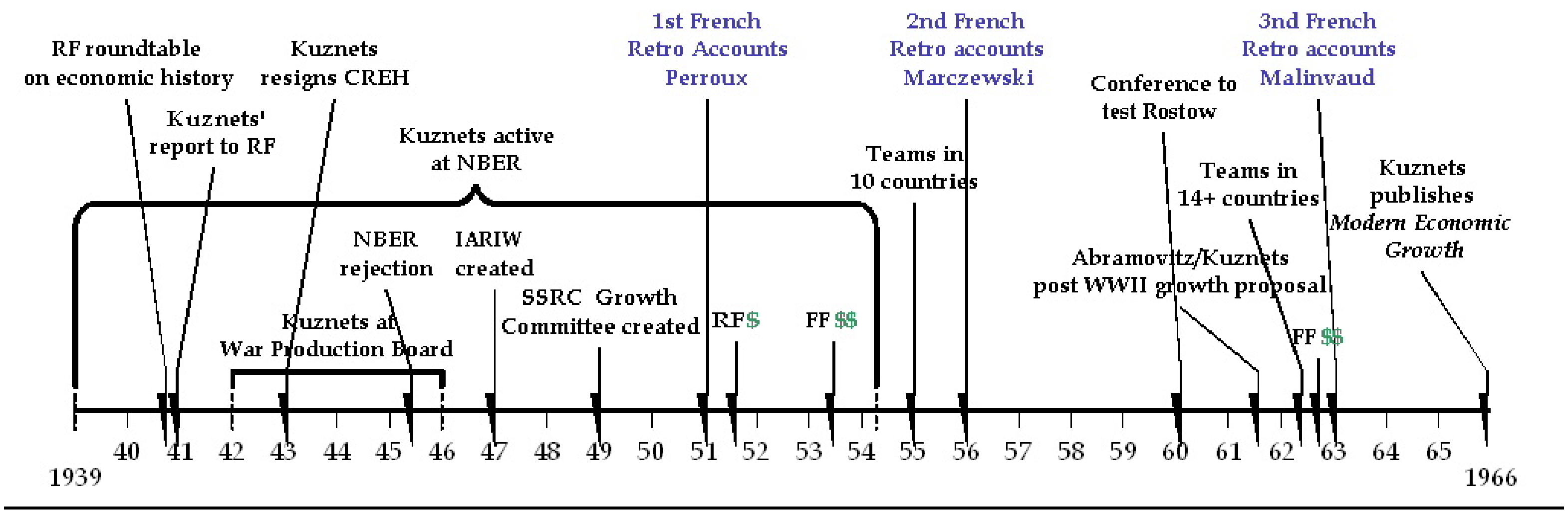
[47] See chapter 2 for a discussion of Shapin and Schaffer's work.
[48] Robert Nathan had been appointed head of the Military Requirements and Industries Studies in the Defense Commission in 1940 – this group later changed its name to War Production Board; Fogel (2000), 11.
[49] Studenski (1958), 149-54.
[50] Fogel (2000), 11.

Figure 4.1: Time line depicting Kuznets' interest in economic history and retrospective accounting



The history of the development and spread of national accounting is a multifaceted and fascinating tale that weaves both theoretical and operational strands: the growing popularity of Keynes' ideas, the success of national accounts for solving war problems, its adoption by the United Nations as a universal communication tool, and its association with the Marshall Plan as an obligatory vehicle for accountability are but a few of the tales that have been extensively investigated in this rich history.[51]

In the late 1940s economists and politicians who wanted to organize economic understanding and cooperation between nations on a standardized basis pushed for national income accounts. This international standardization was principally conducted by American and British economists - Richard Stone earned a Nobel prize for his work in this area.[52] They succeeded in turning this framework into a universally accepted depiction of reality, in a relatively short time span.[53] This was all the more impressive that there was quite a bit of discussion and disagreement about the actual set of concepts that best depicted the national economy (their representational value). For example, they held protracted discussions on what should and should not be included in "income".[54] Yet the main scholars and politicians involved in these discussions agreed that the debates needed to be settled and that the standardization of accounts was the most pressing issue. With this in mind, they created the International Association for Research in Income and Wealth (IARIW) in 1947. The IARIW was meant to "bring active scholars working in the field of national income and social accounting analysis into organized contact with each other."[55]

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[51] Some of the major contributions are Studenski (1958); Desrosières (2000); Vanoli (2002).
[52] Studenski (1958), 160.
[53] Fourquet (1980); Bogaard (1998); Suzuki (2003).
[54] For example, should non-remunerated household work be included in income?
[55] Lundberg, Ed. (1951), v.

Kuznets participated in these general conversations on accounting concepts and methodology.[56] Yet, his main interest seemed to be slightly tangential to the stated goals of the association. He wanted to use this organization to do comparative long term, retrospective accounting:

> "(…) assembly, review and analysis of estimates of national income, wealth and their components for countries for which adequate data extend over at least half a century and thus permit observation of longer-term trends."[57]

His stated purpose was to understand "why and how did the different national units grow at different rates during the last century".[58] Notice that this was not a historical question, for the sake of an historical answer – but a necessary historical investigation to understand the present day.

As can be seen on the time line (Figure 4.1), the IARIW marked the beginning of the retrospective accounting initiative. In 1949, Kuznets helped create a new SSRC Committee: the Committee on Economic Growth (other members included Hoselitz, Spengler and Webbink).[59] He was Chairman of this Committee into the late 1960s, and used it along with the IARIW to coordinate his international project. He also used the SSRC Committee to raise funds. In 1953 he obtained a relatively small grant from RF ($7000 per year, for 5 years) to help finance research outside the U.S.[60] That same year, the Committee on Growth also obtained more sizeable funds from the Ford Foundation (FF) who

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[56] For example he held strong opinions against including government expenditure in national income, arguing that this would amount to double counting – see Vanoli (2002).
[57] Kuznets (1952).
[58] Memo written by Kuznets, April 10th 1945. Harvard University Archives, Accession 88.10, Box 2, Folder "Comparative Economic Growth: Conception and Exploration".
[59] Minutes from October 8th 1955 meeting. RAC- SSRC, Special collection, Box 143, Folder 791.
[60] RAC-RF, RG 1.2, Series 200S, Box 554, Folder 4741.

kept financing it until 1965.[61] In the early 1960s, Kuznets and Moses Abramovitz put forward a request for a post-war comparative growth study: here again framed in national accounts.[62] The FF granted their request, awarding $300,000 for 3 years - ultimately extended to the late 1960s.[63] In 1960 Kuznets helped draft a proposal (with other notable national accountants and economists) for a large research center on the quantitative aspects of economic growth. The center was established at Yale and FF gave it $2 million for ten years. The center sponsored a larger version of the IARIW project, including retrospective and current accounts for developing nations.[64]

In the period 1950-1958, the SSRC Committee received nearly $370,000 from Ford and $70,000 from Rockefeller.[65] With the additional $300,000 for the Abramovitz post-war economic growth project, this amounted to $740,000 dollars – though this second wave of studies was arguably less historical than the first (since it relied mostly on income figures that had been computed by statistical offices since WWII). As we shall see in the "How much was that?" interlude Kuznets' pre-1963 funding was smaller than earlier economist-history initiatives. In 2003 dollars, it represented approximately $2.9 million, which amounted to about $175,000 per year. Compare this to Gay's International Committee for Price History that had received $4 million in 2003 terms – i.e. $780,000 per year. Kuznets' grant was also smaller than the CREH grant, and not much bigger than the money given to run the RCEntrepH.[66]

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[61] Ford Foundation officers had as high an opinion of Kuznets as their Rockefeller predecessors: "give Kuznets carte blanche for any work he chooses to do in this field, within reasonable dollar limits, of course. I think he is a great man"; Memo from Harkavy to Carroll, July 16th 1954. FF, PA 55-28.

[62] Report stating mission of EDA (1962). FF, PA 1429.

[63] This was an important grant and the largest for 1963 (In the annual report they listed it as one of 4 most important grants in 1963 – 2 of them were less than $150,000 and one other was $300,000).

[64] Yale University Grant for Growth Center, FF, PA 61-42.

[65] Letter from Kuznets to Neil Chamberlain, July 1st 1958. FF, PA 55-28.

[66] For details to these calculations, see the Interlude before chapter 8 in this thesis.

Considering the size of Kuznets' project, and the number of people / teams he coordinated with worldwide, this was an impressively efficient use of resources. It also revealed the fact that Kuznets was benefiting from the IARIW, and a growing number of publicly funded statistical offices – piggy backing on their resources. For example, Edmond Malinvaud, who was solicited by Kuznets and Abramovitz in 1963 to analyze post-WWII French accounts, recalled that the bulk of the costs were carried by l'INSEE (the French national statistical office).[67]

This was all the more important given that such data collection exercises could be either very expensive, or very time consuming. This had been demonstrated in an earlier retrospective initiative in France. As we can see on Figure 4.1, Kuznets made three seperate attempts to find the right French project manager. In the early 1950s, he had established contact with François Perroux who ran an independent research institute in Paris (Institut de Sciences Economiques Appliquées - ISEA). Kuznets had commissioned a survey of all national accounting data available in France, with comments on its validity and reliability.[68] Perroux had presented these results at a 1951 IARIW conference. The report had concluded that 18th and 19th century secondary sources could not be trusted, so numbers needed to be computed from disaggregated data. This was a large endeavor – which began in 1956, when Kuznets found both the funds and a full time project manager, Jan Marczewski. Marczewksi was given enough money to hire a couple of junior scholars to help with data collection and assembly. They started with production accounts: agriculture (published in 1961), then industry, and finally services. SSRC funding ran out in the late 1950s,

[67] His work was published as Carré, Dubois and Malinvaud (1972).; Edmond Malinvaud, Interviewed by Cristel de Rouvray, Paris, January 2004.

[68] Perroux and Kuznets had probably been introduced by Richard Stone, whom Perroux visited in the late 1940s, and to whom he sent one of his junior collaborators, Jan Marczewski; Interviews with Jean Claude Toutain, March 2003; Jacques Mayer, October 2003 and Andre Vanoli, October 2003.

and the partial funding they obtained from the ISEA was not sufficient to forge ahead rapidly. The final accounts were only published in the late 1980s![69]

The SSRC could only spare relatively small amounts of money per country, as Kuznets was recruiting scholars in a dozen separate places. By the mid 1950s he had collaborators (or concrete prospects of collaborators) in ten countries: the U.S. (Kuznets, Robert Gallman), U.K. (Phillys Deane, H.J. Habbakuk), France (Perroux, Marczewski), Italy (Barberi), Japan (Henry Rosovsky), Germany (Mueller, Walter Hoffman), the Netherlands (Jan Tinbergen), Sweden (Erik Lindahl), Norway (Central bureau of statistics) and Denmark (J. Bjerke). [70] By 1966, when he published his overall results, he had data from ten countries in Europe, "four or five overseas offshoots of Europe" and Japan.[71]

Kuznets chose his collaborators carefully, as the task was not only one of measurement (technical) but of organization and interpretation. On the organizational side, Kuznets used most of his funds to put together regular meetings. One of the regular venues was the annual IARIW conference, but he also organized separate events:

> "Much of the analysis would depend for its relevance and validity upon the comparative aspects of the study (in the sense that finding the factor X responsible for economic growth in country A could be checked to see whether that factor was also present in county B and did not produce growth). It is therefore important that at certain phases in the inquiry, particularly in the early stages of planning, in the formulation of explanatory hypotheses, in devising of tests for the latter, in the evaluation

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[69] Chapter 5 will show that this delay was not principally driven by missing resources.
[70] Minutes of October 8th 1955 meeting of the Committee on economic growth. RAC-Special, RG SSRC, Box 143, Folder 791.
[71] Kuznets (1966), 505.

> of the future bearing of the factors there be adequate communication among scholars in the several countries."[72]

On the interpretational side, he required his scholars to be "imaginative" and well read:

> "The assembly, review and interpretation [of the data] require familiarity not only with the basic economic and other statistics of the country, but also with… its economic history, which is the background against which inferences suggested by the imperfect data can be checked."[73]

This need to rely on qualitative information to understand sources and interpret the data was well recognized by all national accountants, and sometimes led to pessimistic scenarios about the value of retrospective accounting. As Jacques Mayer, a mathematician and active participant in France's early national accounts said when reflecting on the feasibility of retrospective study:

> "You know a great many things that are not in the accounts, and that give these accounts a great depth, and that enable you – though you use and cite only numbers – to speak sentences that the straightforward reading of numbers would not have allowed. A few years later- very few – all this disappears… which shows how difficult and misleading it is to reconstruct a past epoch using numbers, and how limited our interpretations should be even if we have many precise numbers. There is a terrible loss of information. This was obvious to someone like me, who literally lived with numbers."[74]

[72] General outline of study, Kuznets, March 6th 1963. FF, PA 1429.
[73] Kuznets (1952), 9.
[74] "Vous savez énormément de choses qui ne sont pas dans les comptes, qui donnent à ces comptes une très grande richesse, et qui - tout en vous appuyant sur les chiffres, en

For scholars like Mayer, the reason present-day accounts were useful was because they were implicitly supplemented by tacit knowledge about the economy. The accountant did not only construct aggregates; he also partook in the general life of his nation, and used this extra information when interpreting numbers. There was a crucial hidden component to an accounting representation of the world – which was a core part of the accountant's activity.

Yet recognizing the need to put flesh on the numbers did not entail much collaboration with local economic historians. If anything, Kuznets' collaborators seemed to be suspicious of existing historical accounts of economic growth. In France, Perroux, then Marczewski worked in complete isolation from the growing number of *Annales* economic historians. And in the U.K. Deane seemed to be avoiding the work of British scholars:

> "Phyllis Deane of the Department [of applied economics at Cambridge] is the British member of the Kuznets empire… she says she has gotten very little help from the standard economic history materials, finding that economic historians either misused or ignored the available statistics."[75]

As we shall see in section 4 of this chapter, this lack of collaboration and general disregard of much of the work produced by other economist / economic-historians was an indication of how much they differed on their appreciation of reliable evidence.

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ne citant pas d'autres chiffres que ceux qui sont là-dedans - vous permettent de dire des phrases que la simple lecture de ces chiffres seuls ne vous auraient pas permis de dire. Quelques années plus tard – un petit nombre – tout ceci disparaît… ce qui montre à quel point la reconstruction d'une époque à l'aide d'une information chiffrée est trompeuse et difficile, et à quel point il faut se contenter, même si les chiffres sont nombreux et précis, d'interprétations assez générales. Il y a une perte d'information terrible. Pour moi qui, vraiment, vivais au milieu de ces chiffres, ça a été frappant." Fourquet (1980), 365-7.

[75] From Kermit Gordon's report to the Ford Foundation (p. 17), visit to Europe, March-May 1957. FF- unprocessed accession, Box 14501.

4 The impact of a National Accounting view on economic history

4.1 The Perennial debate at work: Rostow v. Kuznets

One way to understand the disagreements between Kuznets and other economic historians is to relate them to a long lasting debate about the proper nature of observation in economics. This connection clearly came out at a 1960 meeting in Konstanz, Germany, that regrouped numerous economists interested in historical examinations of growth. As we shall see in an analysis of the debates, though there was some discussion around whether and at what stage one should use "evidence" (i.e. discussion between sides of the circle depicted in Figure 2.3), the most bitter disputes occurred among scholars who could all be placed on the left side of the figure.

Kuznets organized the conference with the stated purpose to discuss Walt Rostow's recently published book *The Stages of Economic Growth: a Non-Communist Manifesto.* Rostow argued that economies transitioned from traditional to modern stages via a twenty-year growth spurt, which he called the "take-off". These decades were crucial, as a country could fail to use this momentum to launch sustainable growth. To reach this desirable final stage, the country's population needed to be psychologically predisposed to take advantage of risk and opportunity and follow the lead of the most dynamic sectors.

Rostow had been developing his theory for nearly a decade and Kuznets had been arguing with him for as many years. For Kuznets, Rostow's ideas were the epitome of unfounded generalizations based on fuzzy concepts and inadequate empirical investigation. In 1951 they had exchanged several letters reflecting this disagreement. Kuznets acknowledged their shared interests, but treated Rostow much as he had viewed the CREH historians: good intentions did not suffice and were potentially more dangerous than no historical work at all. He expressed this in a 1951 letter to Rostow:

Figure 4.2. Walt Whitman Rostow (1916-2003)[76]

"I am in full sympathy with the attempt that you are undertaking to fuse economic analysis with economic history. But it is perhaps because of such sympathy and keen interest in the task, which I also have been

[76] Walt Whitman Rostow was born in NYC in 1916, from Jewish immigrant parents. He went to Yale, Oxford (Rhodes Scholar) and back to Yale for his Ph.D. He served during the war (at the Office of Special Services, which later became the CIA) then taught at Oxford and Cambridge (with an interruption to work for the Marshall plan) before returning to the U.S., where he held a chair at MIT. He joined the Kennedy administration in 1960, and was not invited to resume teaching duties at MIT in 1968, when he left politics. Picture from http://texana.texascooking.com/gifs/news/rostow.gif

> attempting intermittently, that my feeling of the shortness of the distance that the present discussion succeeds in advancing is so strong."[77]

Kuznets was acknowledging their similar commitment to empirical work leading to a theory of growth and this made him all the more critical of Rostow's hasty generalizations. Kuznets found most of Rostow's concepts (for example the notion of "propensities to innovate, to emulate, to take risks" and certainly his concepts of different stages like the take-off, or the mass-consumption nirvana) very vague, hence un-observable (by which he meant un-measurable). In response, Rostow asserted that his theory of propensities and stages arose "directly from such knowledge as I have of history" – thus claiming that his theory was empirically based.[78] Kuznets found this empiricism to be no better than Classical or Marxist hasty generalizations. Yet their epistolary exchanges did very little to convince either man to change course. Rostow stuck to his theory (which he published in 1960), and Kuznets continued – in a very NBER fashion – to live in hope of theories that would appear from the evidence, provided it were gathered systematically.

Rostow's work obviously continued to irk Kuznets, and he displayed this annoyance fully at the 1960 meeting. Thirty-seven scholars were in attendance. The minutes of their discussions were transcribed at the end of the conference proceedings, edited by Rostow in 1963.[79] Among the participants, Kuznets was the most vehement anti-Rostowian. He had urged his retrospective accounting teams in at least 5 separate countries to bring the adequate statistical material to prove that there had been no such thing as "take-off". Of the sixteen scholars who presented papers at the 1960 conference, six were directly linked to his

[77] Kuznets to Rostow, June 25th 1951. Harvard University Archives, Accession 88.10, Box1, Folder "Correspondence 1940s-1950s".
[78] Rostow to Kuznets, June 18th 1951. Harvard University Archives, Accession 88.10, Box1, Folder "Correspondence 1940s-1950s".
[79] Rostow, Ed. (1963).

"empire" (Kuznets, Habakkuk, Deane, Cairncross, Hoffman and Marczewski).[80] They seemed to be responding to a shared task - to confirm that they could not find a relatively brief period of time during which the rate of investment had doubled and during which national income had grown at an accelerated rate (according to Kuznets, these were the only precise and thus testable features of "take- off" in Rostow's text).[81]

The statistical data presented by Marczewski (for France), Deane (for the U.K.), Hoffman (for Germany) and Cairncross (for Canada) were not consistent with Rostow's definition nor with his dating of the take-off in each of these countries. Instead their data displayed a rather continuous and gradual increase in capital formation throughout the period of study. Kuznets' paper presented a multi country synthesis relying on evidence from additional countries - Sweden (where his collaborator Johansson's data showed that the ratio of investment to income doubled in eight decades, not just two or three), Japan (where Rosovsky's data showed that it took four or five decades of erratic movements to double the ratio) and the U.S. (where he cited his student Gallman's work) - and promised that he would have extensive data on at least 12 countries in the near future.[82] Having tested what he considered to be the only testable features of Rostow's hypothesis on so many countries he concluded "that the available evidence lends no support to Professor Rostow's suggestions".[83]

Kuznets enjoined the members of the conference to go back to the drawing board and locate the key decades of modern economic growth for each country. They could then focus on these crucial periods to bring to light the causes of long-lasting economic success, without any a-priori assumptions about

80 Habbakuk, Deane, Hoffman and Marczewski were mentioned in section 3. The Scottish economist Alec Cairncross was a member of the IARIW who took interest in the retrospective project. Though he was not financed by the SSRC, he partook in many of the IARIW retrospective discussions.

81 Rostow, Ed. (1963), 30-35.

82 Ibid, 34.

83 Ibid, 35.

what to look for.[84] In other words, Kuznets discarded all of Rostow's work as unscientific; though the energy he consecrated to doing this – compared to his rather hands-off disregard of the CREH's work – is an indication of how much more threatening Rostow's work was to him.

Kuznets was not the only person at the 1960 meeting to call Rostow's work unscientific. Robert Solow, a rising star in growth economics was also dubious of the foundations of Rostow's theory.[85] Unlike Kuznets, he was not so much concerned with the empirical foundations of the theory, as he was with its logical underpinnings. He followed Popperian lines, arguing that Rostow's hypothesis was un-testable.[86] He found that Rostow's tendency to evade Kuznets' specific counter-tests, by referring to untestable psychological dispositions of the population at large, and the inspirational impact of "leading" industrial sectors for example) was evidence that Rostow's concepts were too flexible, hence unscientific. Several decades later, Solow would recall his participation at the meeting as having been motivated by uneasiness:

> "I had great doubts that what Walt [Rostow] was doing was unambiguous enough to be a basis for theorizing about economic growth. I thought that his concepts were too elastic, too capable of being redefined to fit any set of facts. That is not a good way to do theory. What you need are ideas that are refutable, that you could say these are inconsistent with the data."[87]

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[84] Ibid, 35-7.

[85] Robert Solow was born in 1924 – he earned the 1987 Nobel Prize for "his contributions to the theory of economic growth". By 1960 he was well known for his groundbreaking theoretical article on the sources of economic growth - Solow (1956).

[86] Solow argued in favor of a Popperian criterion of scientificity: if there was no event that was inconsistent with Rostow's outlook – as this conference seemed to have shown - then it was not a scientific theory; see "Final Session" in Rostow, Ed. (1963).

[87] Robert and Barbara Solow, Interviewed by Cristel de Rouvray, Boston, June 2004.

Though Solow and Kuznets both had doubts about the scientificity of Rostow's work they disagreed about the diagnosis and the remedy. Kuznets disputed Rostow for doing inadequate empirical work. The remedy was more careful and systematic empirical work. Solow on the other hand warned Rostow against vague, un-testable generalizations. The remedy was more rigorous abstract, logical work. As he recalled in a 2004 interview:

> " I don't think that my take on all that was like Simon's [Kuznets] (…) Simon wanted to say, well here is this punctilio, and that punctilio and Rostow does not get that right. But we are not to be critical of each punctilio, it's the main design. Rostow's general point did not seem to have any logical basis and did not correspond to the broad outline of growth." [88]

Solow's own work on growth theory was indication of his views on proper scientific method: he developed macro-economic models (expressed in mathematical terms) and tested them on available data. For this second step, he was immensely grateful to Simon Kuznets and his students - "if you are interested in long term growth, then you need long runs of data" – but he did not expect the "reasonable" and "logical" theories to organically emerge from the data. He further developed this point by suggesting that Rostow and Kuznets were perhaps more alike than Kuznets had been willing to acknowledge:

> "The interesting thing about Simon of course, was that he too was capable of making broad generalizations that were not accurate in every detail. Take for example the Kuznets environmental curve - in the early stages of development societies paid a lot of attention to the environment, then in the course of industrialization, they diminished this attention but paid

[88] Robert and Barbara Solow, Interviewed by Cristel de Rouvray, Boston, June 2004.

> more attention as they got richer (…) So he did a lot of generalizing at a very high level from just a selection of facts, without asking really for a logical basis, without asking why a reasonable person would believe this to be a true statement."[89]

Thus Solow was confirming the fact that "scientificity" debates could rage as hard within the "empirical" side of the circle, as between so-called empiricists and more deductive, logic driven economists.

These debates may have become more acerbic as a result of this proximity, yet their existence seemed to be tied to stakes that went beyond the community of economist-historians. Indeed, the fact that Rostow's book could have spurred such disagreement and effort (it had cost Kuznets' teams time and money to test Rostow's hypothesis; and these resources could have been used to pursue the comparative work that was supposed to generate knowledge on the causes of growth) was an indication of the growing popularity of development economics and growth studies after WWII. Participants at the meeting included representatives of development agencies, like the U.N. or Brazilian Institute of Economics – a sign that economic historians were partaking in a high stakes conversation and that their knowledge and conclusions were being solicited and used.

Growth and development themes had quickly become the *raison d'être* of economic history. As early as 1947, for example, the EHA's annual meetings and *JEH Tasks* issue was built around a "growth" theme.[90] In some ways, economic historians were jumping on a popular bandwagon, as student demand for classes in these areas created opportunities that savvy job seekers would not have failed to recognize, and by 1960, young economic historians were being hired to teach

[89] Robert and Barbara Solow, Interviewed by Cristel de Rouvray, Boston, June 2004.
[90] Economic History Association (1947). The contributors included Schumpeter, Spengler, Kuznets, Usher and Gerschenkron.

development economics.[91] Yet the connection was not just opportunistic - there was a scientific justification for studying growth through time, thus through history. This historical material could be used in various ways. For some researchers, past economic events were used as a laboratory in which one could establish the relative role of various factors for economic growth.[92] For others, successful nations (namely Britain and the U.S.) could reveal the secret recipe for industrialization.[93] For others, failures or delays to industrialize could also be used to identify crucial factors.[94]

Foundation officers recognized the connection between growth and economic history. As a Ford Foundation officer said in 1960:

> "[I was sent] to visit a number of universities in the U.S. to get some idea of what work was being done in the field of economic development. After staff discussions the feeling was unanimous that (…) the basic research in economic development seems more and more to be research in economic history. I should say, indeed, that work in economic history has high priority in the social sciences program and that this has been true for at least three years."[95]

In recognizing it, they contributed to spread this connection between economic history and development economics, as we shall see in chapter 6. As the stakes

[91] Paul David recalled having been hired at Stanford in the early 1960s for this reason. Paul David, Interviewed by Cristel de Rouvray, Palo Alto, January 2004.
[92] Recall from section 3.2 that Kuznets' project was supposed to ultimately approximate controlled experiments by comparing crucial factors from one country to the next and eliminating those that did not really matter.
[93] The « old » economic historians were operating under the hypothesis that there was something special about American economic history – see chapter 3.
[94] Alexander Gerschenkon made a huge impact on the discipline by studying nations that had undergone late or incomplete industrialization and suggesting that governments could make up for "missing pre-requisites" – see chapter 6.
[95] Letter from EWM to Landes, March 1st 1960. RAC-RF, RG 1.2, Series 200, Box 566, Folder 4846.

grew higher, various schools of economist-historians were pushed into disputing each others' findings and methods – this may be one of the "outside factors" that explained Kuznets' eagerness to discredit Rostow and the focus on scientificity debates at the 1960 meeting.

4.2 Kuznets' legacy to American and world-wide economist-history

With hindsight, Kuznets' agenda for economist-history lasted much longer than Rostow's. In 1960, Kuznets' teams had already been active for a decade and they would be going strong for several more. In 1971, Kuznets was awarded the Nobel Prize for his "empirically founded interpretation of economic growth". Yet the most telling example of the retrospective accounting initiative's success was its transformation into a research program that went well beyond Kuznets' direct involvement. The tasks he had defined were taken up by generations of subsequent economists. In Lakatosian terms, Kuznets had defined the methodological "hard core" (overall, quantitative and comparative work), but the "periphery" was subject to constant renewal and slight adjustments.[96] There were new countries to add from the developed and underdeveloped world. Also, researchers could always go further back in time. When Purchasing Power Parity calculations were developed in the 1960s, there were opportunities to make international data more comparable etc… The size of this program can be gleaned from a quick review of the bibliography in Angus Maddison's latest volume of historical statistics. He has seven pages of bibliography just for studies of national income and population in Western Europe. For the GDP series, there are more than 40 separate sources of historical quantitative work produced between 1949 and 2001, with an average of 3 per country.[97] In other words, Kuznets' project gave a lot of people a lot of work!

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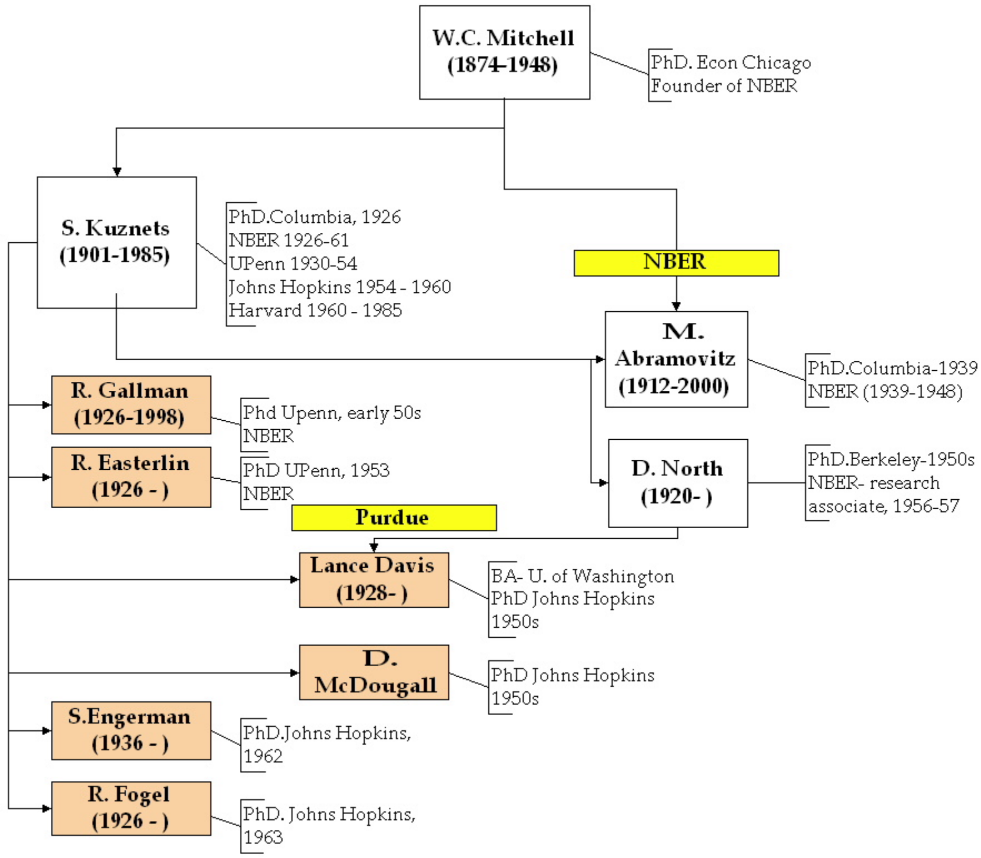
[96] Lakatos (1983).
[97] Maddison (2003).

Kuznets also impacted American economic-history through his students. Figure 4.3 not only highlights the connection between Kuznets and some of the greatest contributors to American retrospective accounts (Robert Gallman, Richard Easterlin and Moses Abramovitz), but also his connection with many of the most prominent cliometricians – in particular with Robert Fogel and Douglass North, who received the 1993 Nobel Prize for "renewing research in economic history". Fogel was Kuznets' Ph.D. student at Johns Hopkins University; and Douglass North spent two years at the NBER, where he made weekly visits to Kuznets at Johns Hopkins.[98] Also notice the connection with Lance Davis and Duncan McDougall, who organized the first cliometric meetings at Purdue University in the early 1960s, as we shall see in chapter 7.

Though the connection between Kuznets and cliometrics existed at this genealogical level, it is harder to establish it at a more substantive level. Indeed, the cliometricians' greater reliance on theoretical models seemed to be at odds with Kuznets' general suspicion of abstract reasoning. Chapter 7 will provide a closer examination of the differences between Kuznets and the cliometric movement via an analysis of Robert Fogel's work. In this chapter, without fully tackling the relationship between Kuznetsian retrospective accounting and cliometrics we inquire into the immediate ways in which Kuznets' work seemed to have changed work in economic history and analyze the factors that gave his program such power among economist-historians.

[98] Douglass North, Interviewed by Cristel de Rouvray via phone, St Louis, February 2004.

Figure 4.3. Kuznets' Lineage in American economist-history



4.3 Kuznets' impact (1): A systemic view of the past

In practically every conversation that Kuznets had with other economist-historians he highlighted the necessity to apprehend the past in an overall, systemic way, rather than relying on piecemeal pictures that would have to be assembled later. He had done this at the 1940 RF roundtable on economic history and he did it again at the 1960 "take-off" conference. In this respect, Kuznets' retrospective accounts were a real innovation in economic history – and constituted a radical departure from views predicated on index numbers of a particular product or price, or on a constellation of monographs about entrepreneurs, or even on a description of the evolution of the entrepreneurial system.[99] For many of Kuznets' collaborators, there was something very appealing to this macro-economic view of the past, not only because it was aggregative, but also because it helped visualize the relationships among various sectors of the economy.

The conceptual power of this view is illustrated in Figures 4.4 and 4.5 – taken from the first report on French retrospective accounts, published in 1952.[100] These figures represented snapshots of the French economy in 1788 and 1845 – showing the system, and the anatomical breakdown of parts. Representations like those depicted in Figures 4.4 and 4.5 were a powerful descriptive tool. The author fitted the data he had accumulated from various historical sources into a simple accounting model (figures on charts corresponded to millions of pounds, "livres"). In this model, there were three classes of consumers, defined by the source of their income – *salariés* or wage earners, *rentiers* or people with private means and *revenus mixtes*, i.e. all other consumers. There were three sources of production (agriculture, industry and services), and their accounts could be read in the tables preceding these diagrams, though the author had chosen to merge them into one large production box in the drawings. Other sectors included the state (*Etat*) and foreign lands (*Exterieur*), and all Investment (*Inv-sst*) amounted to buying goods from the productive sectors. Arrows between the boxes symbolized flows of money or goods from one category to another. Figure 4.5 had more arrows than Figure 4.4, reflecting changes in the general structure of the French economy. For example, the arrow connecting *salariés* to *Etat* (with the number 50) was meant to reflect wage-earners' savings, a phenomenon that had only reached noticeable scale in the 19th century.

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99 As mentioned earlier Kuznets did not "invent" retrospective accounting, but he certainly was the first to 'diffuse' it in the profession (hence the term innovation, rather than invention).

100 Mayer (1952). This was a revised version of the original report presented by Perroux at the 1951 IARIW meetings.

Figure 4.4 The French economy in 1788[101]

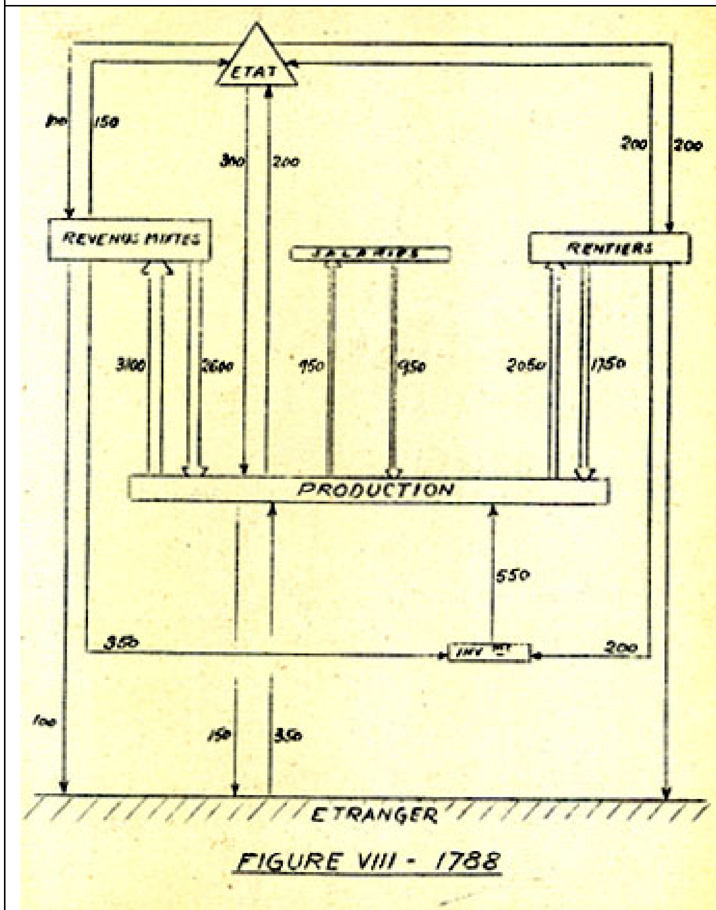
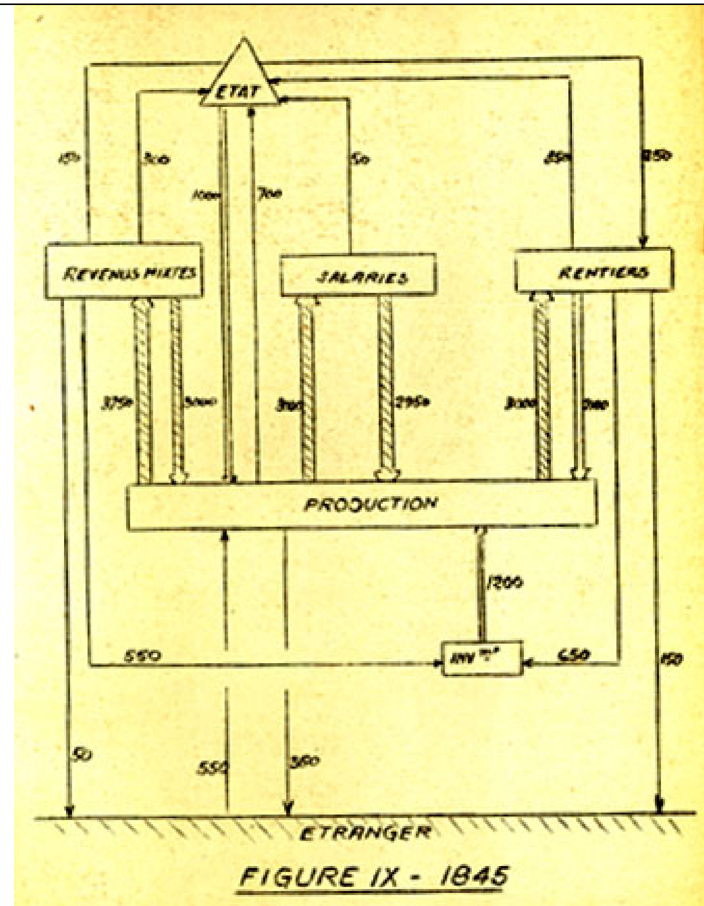


Figure 4.5 The French economy in 1845[102]



[101] Ibid, 121.
[102] Ibid, 123.

These time shots of the French economy at 50-year intervals (there was a third set of accounts for 1890) could yield a quick sense of the growth of the French economy in the interim (the boxes and arrows in each chart were bigger than the previous). It is revealing that the authors felt compelled to add drawings to their tables, as if there were something intrinsically "visible" about the accounts. This may have been a French idiosyncrasy, linked to the legacy of François Quesnay's *Tableau Economique*.[103] As accounting tables became more frequently used, the visual intermediary was abandoned, perhaps because it was less useful (the "view" was now available to them in the tabular form, due to habit), though tables always made it difficult to think in terms of stocks (depicting only annual flows). Kuznets for example only relied on tables, and his 1966 opus magnum (*Modern Economic Growth*) did not have a single chart or diagram.

Though the French model was a simplified version of reality, and contained certain assumptions about what may be most useful for understanding the origins and dynamics of economic growth, it did not carry much causal weight. Indeed, it was not tied to any specific theory of growth. Yet it could easily feed into macro-economic theories of growth and to a great extent called for them. Indeed the comparison between such snapshots invited the question "why": why and how had the French economy grown? Was one of the boxes, or one of the arrows depicted in the chart responsible for this growth? Alone, national accounts could not answer this question. However, they could if they were combined with Keynesian theory (as they were in the 1940s) or macroeconomic growth theory (as they were in the 1950s and 1960s). The power of this overall, systemic view was precisely the fact that it combined a strong descriptive value with theoretical flexibility: economist-historians could use the

[103] The French "physiocrat" Quesnay (1694-1774) produced one of the first visual depictions of flows of income and wealth among different groups and sectors (agricultural, landlords, workers) between 1758 and 1767.

accounting view in tandem with a theory of growth or as a stand-alone means of investigating the past. This latter strategy was Kuznets' preferred method, and he continuously expressed his doubts about the relevance or utility of most theories of economic change.[104] Kuznets preferred to use the accounting view of the past to generate his own generalizations like the environment curve mentioned by Robert Solow, or the famous "inverted U" curve that was meant to describe the evolution of income inequality in developing nations.

Compared to an entrepreneurial view, or even Rostow's leading sector view, the accounting view had a distinct advantage. It was systemic (which the entrepreneurial view was not), though it was conceptually precise, by itself it provided no account of the causes of growth (which Rostow's leading sector view did, as leading sectors were by definition agents of economic growth) and it provided the raw materials to test popular growth and development theories and to generate hypotheses.

4.4 Kuznets' impact (2): Changing the relative value of different types of evidence

The spread of the retrospective accounting mindset entailed changes in economist-historians' notions of reliable evidence. This battle of the evidences surfaced at the 1960 meeting, indicating that there already was a clear distinction between Kuznets type economic history and the work done by other economist-historians (Rostow, or students who had trained at the Harvard Center for

[104] See for example his refutation of the commonly held belief that the structure of human wants necessarily entailed a shift in overall production capacities away from agriculture towards manufactured goods. This hypothesis was based on Engel's "law" that the propensity to consume food-stuffs decreased with income - as individuals, and nations got richer, they spent a decreasing proportion of their revenue on agricultural goods. According to Kuznets, one could find many other explanations for the shift towards industrial goods, not least differential rates of invention and innovation across sectors. In any case, he found no convincing evidence that Engel's law did indeed exist and was at play, Kuznets (1966), 101-4.

Research in Entrepreneurial History like David Landes), even though cliometrics had not yet made a public appearance (though it was about to, as we shall see in chapter 7).

One striking feature of the 1960 Konstanz discussions was the near immediate division of participants into two camps: those who argued in favor of aggregate views of the past and those who argued against it, privileging instead studies of particular sectors (like the textile or transportation industry). This divide overlapped with people's assessment of how to evaluate Rostow's theory: the aggregate people wanted to see statistical evidence of the so-called "take-off" period, while the sectoral people were more interested in detailed studies of a particular "leading sector" and the way in which it may have influenced other sectors up and down the industrial chain. As Rostow clearly put it: "to confine growth analysis to [aggregates] is to play the piano while wearing mittens".[105] For several conference participants, the idea that certain dynamic sectors had changed the way Western economies functioned was very compelling – and even members of Kuznets' "empire" tried to substantiate it. Hoffman, for example, studied the effects of railroad construction on overall growth, tracing its downstream influence to mining – and willingly acknowledged that both had been "leading" sectors for Germany in the mid 19th century.[106]

The macro-micro divide overlapped with notions of quantification. During the conference, Rostow repeatedly alluded to the example of the American railway, stating that its "forward and backward linkages" could not be reduced to calculation.[107] David Landes worried that quantified aggregate views could miss crucial changes. He used Deane and Habbakuk's paper as an

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[105] Rostow, Ed. (1963).
[106] Hoffman (1963).
[107] The choice of railroads was ironic: at that exact time (early 1960s), Kuznets' student Robert Fogel was following the former's lead and measuring the impact of the railroad on the 19th century American economy, an endeavor that would trigger enduring controversy among economic historians. Chapter 7 explores this controversy in more detail.

example, saying that they had paid insufficient attention to technological innovation in late 18th century England:

> "Professor Landes suggested first that it was a mistake, in view of the small place of iron, and even manufacturing industry as a whole, in a largely agricultural economy, to expect changes in one sector, however revolutionary, to have a massive and immediate impact on overall national income. Second he wanted to stress the qualitative importance of these changes, which went well beyond what the authors had said in the paper. For example, in iron there was the whole development of puddling and rolling techniques… indeed one could say that a whole new system of production on the use of machines and mechanical power was being introduced."[108]

Kuznets immediately disagreed with Landes:

> " Professor Kuznets said he would quarrel with Professor Landes about the revolutionary technical changes at the end of the eighteenth century. These were revolutionary in retrospect, but would not have appeared in this light at the time. The crucial question was whether one regarded a change as important when it was first introduced, and when people were impressed by it, or at the date when the change actually became important because of its weight in the economy."[109]

Kuznets was clearly saying that a change that did not register on the national accounts tables was not worthy of the economic historian's attention. This was a strong claim considering that much of the data he and his collaborators relied on

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[108] Rostow, Ed. (1963), 335.
[109] Ibid, 336.

was rather patchy. Compared to the evidence Landes was using (contemporary testimonies from individuals who had witnessed the invention and deployment of new technologies for example) there certainly could have been disagreement about which was more reliable for painting a tableau of late 18th century England. As we shall see in chapter 5 economic historians were disputing the relative merits of “found” (in an archive, in a text) versus “created” (by a measuring instrument) data.

One may be tempted to ascribe the Kuznets-Landes disagreement to differences over the relative merits of quantified v. qualitative evidence. Yet this may miss the central bone of contention. Indeed, Kuznets was equally critical of Rostow’s evidence, which was to a great extent made up of quantities, and tables. When we compared Kuznets’ use of numbers to Anne Bezanson’s prices and price indices earlier, we insisted that the difference was not quantification but rather the systemic nature of Kuznets’ evidence, and the national accounting framework’s ability to create data. It may thus be more useful to think of various types of evidence in these terms, rather than focus solely, or principally on quantified v. qualitative features of the data.

The following table compares different types of evidence used by various groups of economist-historians. When one compares the entries in the table below, the two striking features are the power of the national accounts data to check the validity of each individual piece of evidence (a power that the other groups did not have) and its related ability to create evidence: to make estimates. This double validation and creation power is better understood in the comparison with Rostow. While Kuznets was correct in saying that the CREH, “old” economic historians did not have a systemic view of the past, this critique was incorrect when applied to Rostow. Indeed, as Rostow had himself pointed out:

"when [sectoral analysis] is done systematically the sequence of growth becomes not merely a matter of movement in the aggregates, it becomes a succession of surges, in clustered sectors, linked in turn to the sequence of leading sectors."[110]

Figure 4.6: Different Types of Evidence

	Type of Evidence	**Means of assessing its Validity**
"Old" Economic Historians: eg: Bezanson, Cole, Landes	Piecemeal; Quantitative; Qualitative: eg: prices, index numbers, testimonies from individuals (diaries), business account books	Critical examination of sources; triangulation of evidence
Kuznetsians: eg: Kuznets, Deane, Gallman	Systemic; Quantitative: eg: all initial data aggregated into industrial, sectoral or national figure; averages; estimates	Cross checking data using accounting equations and construction of separate accounts (production, consumption and income)
Rostow	Systemic; piecemeal; quantitative; qualitative eg: market share of leading sector; growth rates of national income; evidence of innovation and business practices being spread from leading sector to other sectors.	Piecemeal data is assessed via critical examination of sources; no way to check validity of the evidence that is supposed to show the relationship from one sector to all others, except by checking its consistency with the general causal account.

Thus Rostow had an overall view of the past, and one that may well have fit into a clearly defined conceptual framework, like a Leontief Input-Output table for example. Had he built Input-Output tables for a sequence of years, on regular intervals, he may well have been able to observe these clusters and movements in a systemic way, and even benefited from the data creation power of such a strong conceptual view. It would also have had the advantage described in section 4.3 of being theoretically weak: there would have been

[110] Ibid, xvii.

opportunity to revise the leading sector view, while retaining a sectoral analysis, as input-output tables were neutral with respect to causality (to build them, you did not need to know which sector had presumably driven the economy).

The contrast between this imaginary input-output view of the past, and the actual retrospective accounting view that Simon Kuznets developed is informative on several levels.[111] First it highlights the power of a conceptually strong, but theoretically neutral framework, both to check and to create data. Second it reminds us of the important resource and organizational elements that underlay these economist-history initiatives. As Robert Solow said in response to a question about the feasibility of retrospective input-output analysis in the 1960s:

> "It was very expensive – in terms of time. I don't think that the median economic historian had those resources. It is not a starter for an economic historian, or a team; way too much number crunching."[112]

Retrospective accounts may have been slightly less resource-heavy, and more easily amenable to time-series analysis.[113] Yet Kuznets' central contribution was to make them feasible - to piggy-back on the IARIW, to leverage the demand for development knowledge, and to use the ever growing number of statistical offices and national income accountants. This organizational element was the key to ensure feasibility and sufficient standardization for the ensuing comparative and causal analysis of growth.

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[111] Some economist-historians had attempted to build input-output views of the past, see for example Meyer (1955). This was not very common, possibly because of the greater effort and data requirements for this type of retrospective (and contemporary) exercise. For a discussion of the relative merits of contemporary NIA and Input-Output views, see Vanoli (2002).

[112] Robert and Barbara Solow, Interviewed by Cristel de Rouvray, Boston, June 2004.

[113] Whereas input-output tables have to evolve with the economy, reflecting the birth of new activity, and the death of old ones – this potentially complicated comparative analysis.

5 Conclusion

The spread of a national accounting view of the past contributed to marginalize types of evidence that did not register on this measuring instrument and promote those that did. This resulted in a wider adoption of systemic (aggregate, macro-economic), quantitative and estimated data, while economic historians who used other types of evidence were pushed into "history", thus erecting previously inexistent barriers inside economic history. As we shall see in chapters 5, 6 and 7, the bearers of different types of evidence (piecemeal, found, whether quantitative or qualitative) clashed against this new generation of Kuznetsian economist-historians, in particular over the issue of estimation, and the configuration of the economic history space was altered to reflect these battles over what constituted legitimate evidence.

The conceptual, but perhaps even more importantly the organizational effort that Simon Kuznets deployed to spread retrospective accounts are paramount for understanding the success of this initiative. It also depended on a favorable international situation that could provide the resources for such a large project. In a century long debate that had pitted observers versus abstract thinkers, Kuznets was able to leverage the long promised empirical-comparative method thanks to an international organization that could support the standard procedures to insure the comparability of the data. Thus, there is no determinism in this story – had it not been for the proliferation of national income accounts in the 1940s and the growing demand for knowledge about economic development Solow's point about the dissuasive cost of input-output tables may well have applied to Kuznets' plan. Though Simon Kuznets' measuring instrument did have the superiority of theoretical flexibility, there were other ways of representing the economy that appealed to economist-historians and buttressed their objections to his project. As we shall see in chapter 5, the question of evidence, and of what constitutes reliable evidence in economic history was not easily closed in France.

Chapter 5.

ENCOUNTERS BETWEEN ECONOMISTS AND HISTORIANS IN FRANCE: 1950s -1960s

1 Introduction

The history of economic history in 20th century France is a vast and complex one, not least because it weaves into the story of *Annales* - an episode in French social science and humanities that defies any easy classification. But the story of mid 20th century French economist-history is not the history of *Annales* or of economic history at large, but of a much smaller set of events, that can be roughly characterized as encounters between « economists » and « *Annales* historians ». Indeed, French economists who sought to do history inevitably ran into this increasingly well-established group of economic, social and cultural historians.

For French economists, the task of setting up an economist-history separate from *Annales* proved to be impossible. The difficulties stemmed from several factors. Among them was the fact that the empiricist and historical vision that had motivated economists like Arthur Cole or Simon Kuznets to create economic history in the U.S. had already been largely appropriated by the *Annales* movement. Though these scholars belonged to history departments, they favored an explanatory history aimed at understanding man in society. In other words, the movement may well have been fulfilling many historically inclined economists' wishes, thus precluding the need for any separate economist-history.

In principle, the fact that certain economists shared *Annalistes*' views should not have hindered collaboration, and did make it possible for some economists to join the *Annales* interdisciplinary bandwagon. Yet, these economists were inevitably led to accept the view that all social phenomena were essentially multifaceted, and that consequently it made little sense to study them in disciplinary isolation. Economic history was not a stand-alone field for *Annalistes* but rather symbiotically tied to social, cultural and to some extent political history. Economists who did try to erect it as a separate activity ran into *Annales*' well-articulated conviction that it was impossible and useless to do so.

Encounters between French *Annalistes* and economists in the decades following WWII provide a glimpse of the ways in which scholars defined who was "in", and who was "out" of their respective fields – and how they adjusted these parameters based on events occurring outside science. This window is particularly useful for understanding French economics after WWII, a time of phenomenal change for the discipline. Economics was gaining emancipation from the curriculum of law departments, mathematicians and engineers' prestige was growing, and traditional, literary economists were loosing grounds and influence. The issues at stake were much broader than the mathematization of the discipline, and included economists' status in the university and in society. By looking at French economists' motivations for writing history and their consequent run-ins with *Annalistes* in the decades following WWII, we can examine the following questions: what did certain French economists have in common with *Annales* scholars? How did this evolve and what processes consolidated the change? Specifically, how did so-called "economists" and "historians" differ in their appreciation of useful and reliable evidence?

To answer these questions this chapter begins with an overview of the *Annales* space since its creation in the late 1920s. Section 3 then explores the factors that turned the movement into an exceptionally dynamic field after WWII. Section 4 looks into several economists' attempts to engage with economic history. Section 5 analyzes these encounters in light of diverging interpretations on what it meant to be « empirical », thus showing that economists were drifting away from *Annales* methodologies in their increased use of estimation. Throughout, the chapter argues that differences between French "historians" and "economists" are more usefully conceptualized as differences between different types of "economists" and that certain scholars' increased willingness to use non –primary source (i.e. estimated) data seemed to reflect an evolution in their sense of time, as if a feeling of urgency encouraged them to work at a faster pace.

2 Economists, History and Historians before WWII

2.1 Social Science and History in early 20th century France

Gustav Schmoller's vision for an empirical social science, based on detailed historical study had a rough counterpart in France, in the work of Auguste Comte (1798-1857). The French philosopher had encouraged social scientists to imitate the experimental method in the natural sciences, and had insisted that social phenomena were all fundamentally sociological, meaning that they could only be explained with reference to groups of human beings, their interactions and mutual constraints. The reliance on groups and institutions gave a special emphasis to diachronic studies, as only a historical perspective could show how institutions came into being.[1] His philosophy came to be known as the "positive and unified" view of social science. This was a broad creed subject to much interpretation, though its role in shaping a demand for economic and social history in France was somewhat narrower and can be usefully grasped in the career and beliefs of the French philosopher, economist and sociologist François Simiand.

In the late 19th and early 20th century, Simiand (1873-1935) contributed a great deal to disseminating the Comtian / Durkheimian view of a positive and unified social science.[2] Imitating what he considered to be a proven method in the natural sciences,

[1] For a general presentation of Comte's philosophy see Berthelot, Ed. (2001), 215-222, 358-363.
[2] Emile Durkheim (1858-1917) is credited by many as being the father of modern sociology. He extended the Comtian proposition that there were such entities as "social facts" – as opposed to individual actions – and brilliantly illustrated it with a study of suicide rates, which he found to

Simiand began with observation. Social phenomena needed to first be described, with as little mediation (from theory or hypotheses) as possible. These phenomena became interesting and useful once they could be identified as social objects, namely repeated facts, whose cause could be extracted from the comparative analysis of similar outcomes occurring in different contexts. This comparative handle would help establish general scientific propositions.[3] He applied this method to his research on wages in France, to develop a theory of wages, prices and money supply.[4] His study of price history was the most extensive, as he collected and analysed commodity price series from the 16th to the 19th centuries.[5]

Economic history held a special place in Simiand's views. In general, he believed in the historical nature of all social phenomena and in the corollary methodological proposition that events could only be studied in time, as this was the only way to observe both their repetition and evolution. Within history, economic history held a special status, as it was more amenable to statistical study (as many economic phenomena were quantifiable). For Simiand, statistics were the prime tool of scientific observation, as they made sure the analyst focused on the common features of a particular event (those that were liable to repetition) rather than getting distracted by peculiarities. However, he warned his contemporaries about giving too much importance to correlation and maintained that causal analysis required careful examination of all possible links, not just the most prominent ones. Thus he argued in favor of the symbiotic relationship between a statistical and more literary economic history.[6]

be constant within a given community. He thus claimed to have shown that, for each social group, there is a specific tendency to suicide that can be explained neither by the psychological disposition of individuals nor by the nature of the physical environment; and is consequently a collective phenomenon, that depends on social causes - Durkheim (1897). He started the journal *Année Sociologique* in 1898.

[3] Cedronio, Ed. (1987), 9.

[4] Simiand (1932a).

[5] Simiand (1932b).

[6] Simiand (1932a), vol.1, 68-79, 96-110, vol. 2, 541-556.

Not surprisingly, Simiand held Schmoller's work in high esteem. Though Simiand rarely had a kind word to say about contemporary social scientists he was quite enthusiastic about the German Historical School.[7] When Schmoller's first volume of the *Grundrisse der allgemeinen Volkswirtschaftslehre* came out in 1900, Simiand made a point of agreeing with the German scholar that:

> "The tasks of a rigorous science are 1) to observe precisely, 2) to define and classify and 3) to find typical forms and general causal explanations." [8]

He specified that this had nothing to do with a schematic division between induction and deduction:

> "This process involves deductive reasoning as much as it does inductive reasoning. The inductive method contains many deductive steps: however it constantly emphasizes the need for verification and confrontation with reality. The deduction that we criticize is the arbitrary one that starts from vague and unfounded premises."[9]

This last sentence reminded his readers of Schmoller's battle with the Austrian economist Karl Menger, and of Simiand's own critique of what he considered to be simplistic, deterministic schemes in political economy. It was also an indication that the "scientificity" debate was at work among French economists.

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[7] Simiand thoroughly critiqued the work of sociologists, economists and historians in his column in Durkheim's journal *l'Année Sociologique*, Cedronio, Ed. (1987).

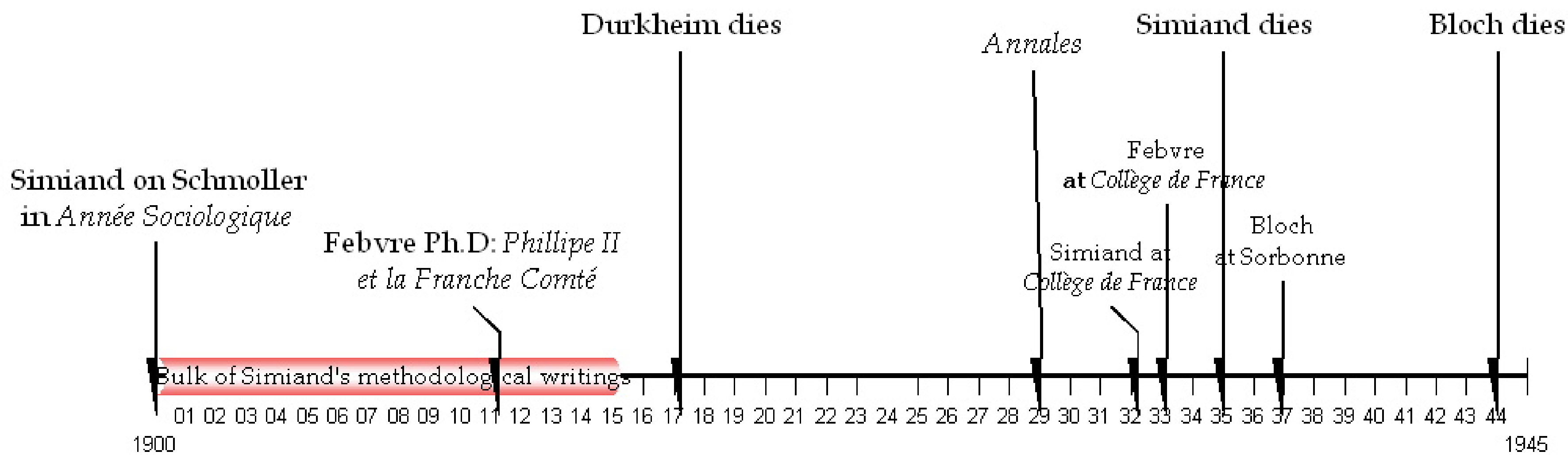
[8] "Les devoirs d'une science rigoureuse sont 1) d'observer exactement, 2) de bien définir et de classifier, 3) de trouver les formes types et d'expliquer causalement.»

[9] "Dans ce travail, le raisonnement déductif a place autant que le raisonnement inductif. La méthode inductive comporte beaucoup d'opérations déductives : seulement le souci de la vérification et de la confrontation avec la réalité y domine toujours. La déduction critiquée est la déduction arbitraire partant de prémices vagues et mal assurées. » F. Simiand, "L'Ecole Historique Allemande", in *Année Sociologique,* 1900, cited in Cedronio, Ed. (1987), 275-6.

Simiand's academic career picked up in the early 1920s. In 1923 he was awarded the chair in political economy at the *Conservatoire National des Arts et Métiers*; in 1924 he began teaching economic history and statistics at the *Ecole Pratique des Hautes Etudes*; and in 1932 he became Professor at the *Collège de France* where he held the chair in Labor History until his death in 1935. These were prestigious posts, but he held them only briefly. When Simiand died he had not achieved any noticeable reorganization of social science in France, which continued to be practiced along various methods and ideological lines while many historians were still committed to political, event-based history.[10] As we see in Figure 5.1, Simiand spent a decade on methodological writings in the early 20th century, but did not resume this task after the first world war.

[10] For biographical information on François Simiand, see Frobert (2000), 5-22.

Figure 5.1 : Timeline of Events before WWII



2.2 Origins of the "Annales" movement [11]

Though French social science in the mid 1930s did not correspond to the positive, historical view envisaged by Comte or Simiand, there had been one noticeable new arrival on the scene, the journal *Annales Histoire Economique et Sociale (AHES)*, which seemed to be pushing in this direction. The journal had been founded by Lucien Febvre (1878-1956) and Marc Bloch (1886-1944) in 1929, when they were teaching at the University of Strasbourg in eastern France (see Figure 5.1). As relatively young scholars, both "normaliens", successful candidates at the "Aggrégation d'Histoire" (the French license to teach at secondary and university levels with the option to start writing a thesis) and authors of monumental *Thèses d'Etat* (a ten-year work of scholarship that would earn the author a full professorship) they were following a typical route to tenure in France, which consisted in taking jobs in provincial universities in the early years of one's career, with the intention of returning to Paris once experience and a good publication record would allow.

Throughout their careers, they had both expressed frustration at the state of French historiography, which they considered to be overly focused on political chronologies and exceptional individuals – an "event" based history, that seemed to ignore the discipline's potential for generating knowledge of a more explanatory nature.[12] For example, Lucien Febvre had written his Ph.D. thesis on "Philippe II et la Franche Comté" – the title suggesting that a geographical region (Franche Comté) could have as much "agency" as a traditional political figure (Philippe II) and that geography placed real constraints on human activity.[13] From the outset, their vision was to usher in a new era for history *and* for the social sciences, along lines

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11 English readers may be more accustomed to seeing the expression "*Annales* School", rather than "movement". However, the long life of *Annales*, and its numerous twists and turns have incited many French scholars to wonder whether there really were core epistemological propositions that united all scholars that were associated with the movement; see for example Coutau-Bégarie (1983), xx-xxviii; Revel (1986).

12 Burke (1990), chapters 1-2.

13 The parallels with the title and basic explanatory structure of Fernand Braudel's first work are striking; Braudel (1949). See sections 3 and 4 for more information about Fernand Braudel.

that were not unrelated to Simiand. Bloch and Febvre's basic idea was to provide an alternative to overly deterministic frameworks by introducing contingency via history ("see necessity nowhere and possibilities everywhere").[14] Thus they privileged the study of "collective and anonymous" trends. The name they chose for their journal was indicative – *Annales, Histoire Economique et Sociale*. Economic and social impersonal phenomena were their favored subjects for investigation.

This bold shift away from political and diplomatic history was supported by a greater collaboration with like-minded social scientists. Their editorial board reflected this multi-disciplinary commitment: an economist (Charles Rist), a political scientist (Andre Siegfried), a sociologist (Maurice Halbwachs) and a geographer (Albert Demangeon) sat on the eight-person committee.[15] This was also apparent in their selection of articles, which were written for historians, economists and sociologists. For example, in 1929, the sociologist Maurice Halbwachs wrote an article reviewing Aftalion's *Cours de Statistique,* where he gave a mathematical explanation of ordinary least squares regression. This constituted relatively advanced statistical knowledge for the 1920s, and suggested that contributors spoke a similar language and could understand each other's tools – or at least that they were eager to learn from each other and keep abreast of developments in other social sciences.[16]

While the editors called for greater communication among the disciplines, which in principle contained no implicit hierarchy among them, historical approaches were defended as being necessary to nearly all investigations in social science. Consider, for example, the conclusion of a 1929 article by Sayous on 16th century Spanish currency exchange:

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[14] Delacroix, Dosse and Garcia (1999), 112.

[15] As we saw in chapter 3, the founders of the EHA in the U.S. also shared this multi-disciplinary desire.

[16] For information about the state of early 20th century statistical knowledge in the social sciences (and the emergence of probabilistic reasoning) see Morgan (1990).

> "Historical method uses the past to tell us more about the present; more generally it allows, or will allow – once preliminary studies will be numerous enough to permit synthesis – a better grasp of fundamental economic flows. The true historical method for political economy does not consist in asserting the relativity of economic theories (as liberal economists have claimed with disdain); but seeks to uncover the nature of economic institutions via their history. Let's take an example. In the last few years, exchange rates have become increasingly important, without us knowing their laws in times of great instability. By studying the history of exchange rates in Spain and its American colonies in the 16th century, we discovered a period that was quite different from ours but underwent equally serious disturbances, and we uncovered economic principles that are identical to those that took much observation to spell out. Had we had a better grasp of economic history, the contemporary situation would have been understood more rapidly."[17]

While Sayous' methodological stance was not identical to Bloch and Febvre's more sophisticated ideas (they did not really believe in "lessons" from history) it certainly was not contrary to their views. In the introduction to the first year anniversary issue (1930), Bloch and Febvre made reference to Sayous' article, and subtly amended his manifesto to make a more general claim:

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[17] "La méthode historique […] éclaire le présent par le passé; d'une facon générale, elle permet, ou plus exactement permettra, à mesure que des études préparatoires donneront des bases plus solides aux synthèses, de saisir les grands courants économiques. La vraie "méthode" historique en économie politique n'est pas du tout celle qui affirme la "relativité" des doctrines économiques (ainsi que les économistes de l'école libérale se sont plu longtemps à le dire avec un certain mépris): c'est celle qui cherche à indiquer la nature des institutions économiques par leur histoire. Apportons un exemple. Au cours des dernières années, les changes ont pris une énorme importance, sans que l'on ait entrevu d'abord quelles étaient leurs lois en période de perturbations très graves. En étudiant l'histoire des changes en Espagne et dans les relations de l'Espagne avec l'Amérique au XVIe siècle, nous avons rencontré une période d'un caractère très différent, mais agitée par des troubles aussi sérieux, ou l'on a dégagé des principes économiques identiques à ceux que l'on n'a pu établir chez nous qu'après de longues observations. Si on avait mieux connu l'histoire économique, la situation contemporaine eut été élucidée plus rapidement." Sayous (1929), 175-6.

> "Why should we speak of past and present? Reality is one. Today, as yesterday, the *Annales'* goal is to make this unity tangible."[18]

Thus Bloch and Febvre added an appreciation for contingency and the need to study change: "history", they claimed "studied men in time, not in the past".[19]

Sayous, Bloch and Febvre's defense of the historical method was framed in terms of rival conceptions of scientific social science and though Bloch and Febvre's claims aimed at broader goals than the reform of economic knowledge, their critiques may call to mind arguments made by Schmoller and his American followers (Edwin Gay for example). This parallel serves as a reminder that 19th and early 20th century social science seemed to be traversed by recurrent debates about "scientificity" - whose origins were not obvious, and whose closure was never guaranteed. To use the terminology developed in chapter 2, methodological debates on « good method » were at work in early 20th century France, reconfiguring the lines between « insiders » and "outsiders" across disciplines.

Annalistes acknowledged their debts to Simiand on many occasions.[20] This suggests that labels such as "historian", "economist" or even "sociologist" may be misleading starting points in an investigation of scientific change. Simiand for example was sometimes labeled "sociologist" and at other times "economist" (today he is mostly remembered as a "sociologist").[21] Yet he may have had more in common with Bloch and Febvre than he did with many economists teaching in early 20th century France, just as the *Annalistes* may have had much more in common with him than with many French historians.

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18 "Mais pourquoi parler du passé et du present? La réalité est une. En faire toucher du doigt, à tous, l'unité - ce sera demain comme hier le but de nos Annales", Bloch and Febvre (1930), 3.
19 Delacroix, Dosse and Garcia (1999), 127.
20 For example, in 1960, they re-edited a set of Simiand's early methodological statements: Simiand (1960).
21 Chartier and Revel (1979).

While there were strong similarities between the vision and work of Simiand, Bloch and Febvre, the latter accomplished much more than the former. Bloch and Febvre's dissenting views may not have been genuinely new, yet *Annales* was undoubtedly one of the most successful attempts to change scholarship in French history, and arguably French social science. This contrast confirms Novick's views that recurrent debates were not uniformly influential in the history of the discipline: only some of them had effects on the general course of research and activity. Simiand's methodological pronouncements had little, if no direct effect, while the *Annales* created a movement that enjoyed phenomenal success throughout the better part of the 20th century. Indeed, as we see on the time line (Figure 5.1) within a few years of the creation of their journal, Bloch and Febvre were called back to Paris and appointed to extremely prestigious jobs. In 1933, Febvre was given a chair at the *Collège de France*, where he taught economic and social history, with a growing emphasis on religious and cultural trends. Bloch returned to Paris in 1936, to take over the Sorbonne chair in Economic History (Figure 5.1). Both these posts were held by *Annales* scholars for decades to come (Febvre's successor was Fernand Braudel; while Bloch's post-WWII successor was Ernest Labrousse), and the movement grew stronger from year to year – as more and more scholars came to identify with the movement.

2.3 Designing a hybrid space

What made this success possible in light of Simiand's earlier failure? WWI and the devastating effects it had on Comtian / Durkheimian followers certainly was an important factor. Keylor has argued that French sociologists could have been the initiators of the empirical, contingency movement associated with the *Annales*. However, by the end of WWI, Emile Durkheim and the vast majority of his disciples had died – many on the battle-field.[22] Yet, this does not give fair weight to the pioneering dimension of the *Annales* project. Bloch and Febvre innovated on one crucial

[22] Keylor (1975).

dimension: they created a new space, which was first sustained by the journal, but subsequently came to be embodied in real physical institutions.[23] Instead of laboring to change each social science from within, they created a new area that did not really belong to anyone before.

What was this new space? Broadly speaking, it was economic and social history in the symbiotic sense of the term. But more specifically, a more narrowly defined economic history, drawing contributions from statisticians, economists and historians, seemed to emerge as the most viable meeting point for these various scholars. Economic history, as the study of wages, prices, money, and commerce in the past was not new in the late 1920s. There were economic historians in France prior to the *Annales* – for example Emile Levasseur, Henri Hauser or the Belgian historian Henri Pirenne - but among historians, economic history was neither a main nor a separate activity.[24] What seemed to be new, or gaining momentum, was an economic history seen as a pillar of social science. In addition, the space they created had obvious appeal for non-historians, in particular economists. Thus from the outset, there was a tension between the founders' view of a symbiotic social and economic history, and a fraction of their contributors' inclination towards a more specifically *economic* history (studies of prices, exchange rates, use of statistical formulae for example). In other words economic history gradually emerged as the most sustainable place for interdisciplinary dialogue and exchange.

The federative, fundamentally interdisciplinary nature of Bloch and Febvre's creation is illustrated in the evolution from figure 5.2A to 5.2B. Figure 5.2A shows Simand's effect on social sciences in France: though he called for a federative model where economists, sociologists, statisticians etc. all adopted a positive, empirical and unified method, the only scholars who actually applied this view were Durkheimian sociologists. In other words Simiand and like minded social scientists had only

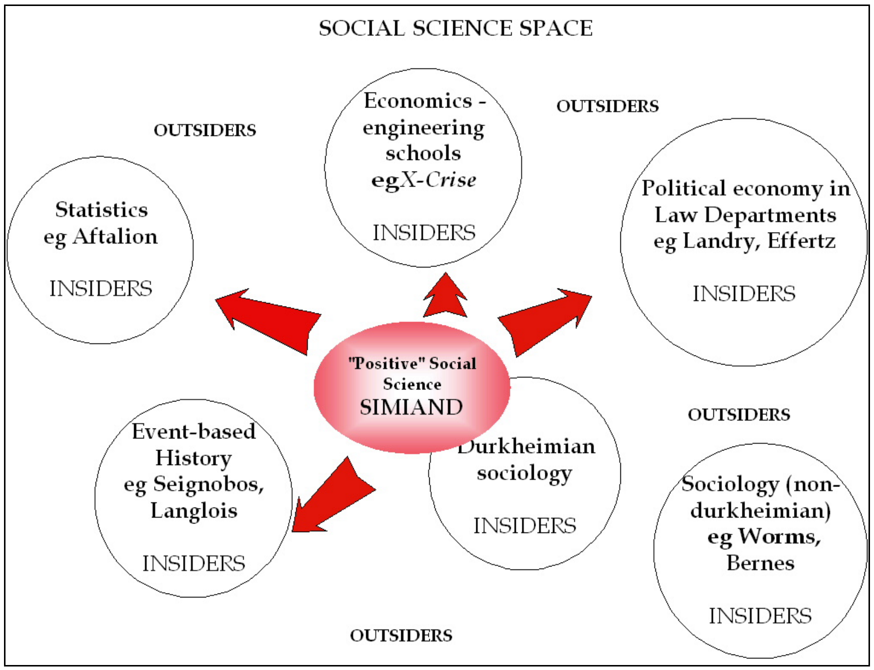
[23] See subsequent sections in this chapter.
[24] Delacroix, Dosse and Garcia (1999). Recall from chapter 3 that Henri Hauser was Gay and Beveridges' collaborator on the International Committee for Price History – and that they had been very disappointed with his work - Dumoulin (1990).

succeeded in erecting another demarcation line between scientific and un-scientific research (adding a circle to the constellation of existing “in” and “out” borders). Contrast this to figure 5.2B. *Annales* did not have a forceful agenda to reform any discipline from within; instead the founders invited like-minded scholars to publish in their journal. Editors and contributors found that they communicated most easily on topics of economic history (as opposed to cultural, religious, military or even social history as these other areas did not draw much participation from economists and statisticians). Economic history thus emerged as the most vibrant area, drawing most of the contributors’ attention – rather than having been decreed as such. This mix of purposeful and spontaneous hybridity proved to be a very successful strategy after WWII, when the radical changes that befell the French university gave a few scholars the opportunity to push forward with their methodological vision.

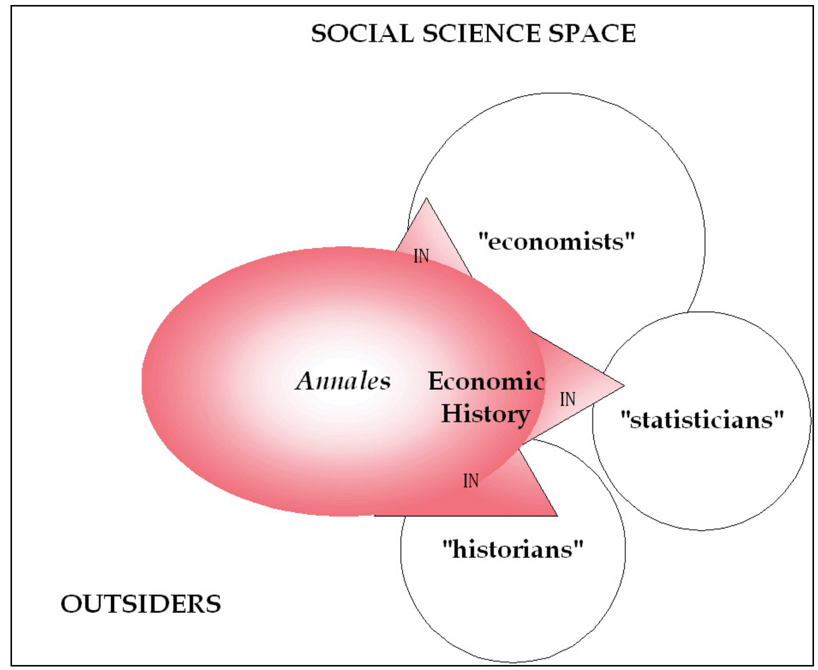
The reader may recall from chapter 3 that Gay and his students had also created a relatively hybrid space in the U.S. using similar tactics – a new journal and a new association. The founders of the EHA may have principally been economists but they increasingly opened their doors to members from other disciplines; for example the CREH funded numerous students in History Departments (Louis Hacker and the Handlins) and they invited the sociologist Leland Jenks to join their ranks. Subsequently, the RCEntrepH was a remarkably interdisciplinary affair. These organizational similarities are hints of the deeper ties that united American “old” economic historians and *Annalistes*.

Figure 5.2 : The Social Science space in France before and after *Annales*

A : Social Science Space in France in the 1920s, before *Annales*[25]



B : Social Science Space in France in the 1930s, after *Annales*



25 This is not an exhaustive picture of all players in French social science in the 1920s—each "circle" is here to illustrate the general point that there were many claims to what constituted proper social science (more or less explicit claims) and that Simiand's goal was to reach out and convert them. This picture of relatively watertight circles is confirmed by Alain Desrosière's overview of French social science in the inter-war period; see Desrosières (2000), 98.

3 A new space for social science in France

3.1 The ideological spectrum of post-war France and its implications for economic history

The experience of war and defeat terribly shook France, though the extreme desire for change was counterbalanced by the pragmatic realization that not everything could be radically altered. Retrospectively it does seem that the French university system underwent some of its greatest mutations at this time – yet, these were not uniformly spread across disciplines. So it may be more useful to think of the decades following WWII as having had high potential for change, with variable results depending on scholars' motivation, their access to limited resources and their ability to create or meet demand for their services. The story of the creation of the *VI e Section de l'Ecole Pratique des Hautes Etudes* and of the *Maison des Sciences de l'Homme*, both *Annales* achievements, constitutes a case where these factors were successfully brought together, thus creating a new space for social scientists in France.

Let us begin with the last factor: a "demand" for scientific and explanatory economic history. We saw in chapter 3 that in the U.S., the demand had been generated by the scholars who had attended the 1940 Rockefeller round-table though it had been reinforced by growing Americanism during WWII. In France, it seemed to have been driven rather by the context. Generally, there was a strong demand for economic and social knowledge spurred by the need to understand the military defeat. The hunger to explain France's demise led numerous young men to study economics. For example, the mathematical economist and econometrician Edmond Malinvaud remembered that:

> "My generation had bitter recollections from the interwar years and the unhappiness of war (…) we were convinced that we could do better than what

> we had lived through (…) we belonged to a group that believed in studying economics to make things work better“.[26]

This was echoed in the economic historian, Maurice Lévy-Leboyer’s concern:

> “I chose economic history because I had lived through the 1930s, and I saw what an economic crisis did ; I lost my father in 1937, I saw the country collapse. It was natural to study economics to understand how a country could be so poorly managed (…) My generation was very frustrated to see this respectable country collapse. It was scandalous. The French showed themselves to be incompetent.“[27]

In particular, demand for a systematic economic history seemed to arise from the ideological spectrum of post-war Europe. The decades following WWII had different political flavors in France and the U.S. It may be quite impossible to generalize about the ideological landscape of a nation yet, to caricature, while the French population (and to a great extent government) flirted with socialist ideals and ideas, the Americans vigorously rejected them. For example, both countries had a very different relationship to “planning”: the French built and administered five year indicative plans, whereas the idea of such direct government intervention was eradicated from American politics at

[26] “Ma génération avait mal vécu, avait un souvenir vivant de l’entre-deux guerres et des malheurs dûs à la guerre (…) Nous avions cette vision qu’il était possible de faire mieux que ce que nous avions vécu (…) Nous appartenions à un groupe qui était motivé par travailler l’économie pour faire en sorte que ça marche mieux» ; Edmond Malinvaud (2004), Interviewed by Cristel de Rouvray, Paris, January 2004.

[27] “J’ai choisi l’histoire économique parce que j’avais vécu dans les années 30, et j’ai vu ce qu’était une crise économique, j’ai perdu mon père en 1937, j’ai vu ce pays s’effondrer. C’est très naturel de s’intéresser à l’économie pour comprendre comment on peut si mal gérer un pays. (…) Ma génération a été très frustrée de voir un pays respectable s’effondrer. C’est scandaleux. Les français se sont comportés comme des imbéciles”; Maurice Lévy-Leboyer (2003), Interviewed by Cristel de Rouvray, Paris, November 2003.

the end of the war (at least at the rhetorical level).[28] Among academics, the picture was similar (and equally un-amenable to generalizations). In France, as the historian François Furet argued, the immediate post-war saw the apex of communist ideologies in intellectual circles - a strong commitment that persevered in spite of bad news from the Soviet Union and satellite states.[29] In the U.S., McCarthyism and its aftermath made it difficult, if not impossible for American academics to openly express their non-conformist political views, or engage in scientific discussions of Marxism.[30]

There were obvious connections between Marxism and economic history. Marxism was a historical theory where social evolution proceeded in stages, each mode of production entailing radical changes in social arrangements. This had two implications for economic history: Marxists would want to write economic history, and historians – whether they were Marxists or not – would have to react to this. Both trends brought people into the field (at least temporarily) for reasons that had very little to do with perennial debates on scientificity. For example, the French mathematician and economist, Jacques Mayer, whose career was mostly spent in national statistics and planning, wrote several early papers on Marxist themes, which involved historical analysis. In the early 1950s, he produced studies of cotton and coal industries in France over several centuries (1709-1914) to test the "law of the falling rate of profit". [31]

Yet, such studies were rather exceptional among French economists, who, for the most part, did not write history. This was certainly true of economists who belonged to law departments, and whose work combined relatively abstract propositions illustrated by occasional empirical references. Since the late 19th century political economy had been a sub-field of law departments and these shared premises tended to reinforce

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[28] In the decades after WWII, indicative or interventionist plans were proposed for many non-communist European countries, such as the Netherlands and Norway; see Bogaard (1998). For the fall from grace of planning traditions in post-WWII U.S. see Balisciano (2000).
[29] Furet (1995).
[30] Morgan and Rutherford (1998), 14-15.
[31] Jacques Mayer (undated, though probably 1952-55) "L'Accumulation du Capital dans Deux Industries Françaises: la Filature de Cotton et la Fabrication de la Fonte (1709-1914)", *ISEA Working Paper.* Lent to C. de Rouvray by Jacques Mayer.

abstract, moral and rather conservative reasoning.[32] This was an obstacle to the adoption of a "positive" and historical view of economics – reinforced by the relatively negative perception of the German Historical School, which had not been well received (largely because it was "German", and relations between the countries were very strained in the first half of the 20th century). [33]

A notable exception among French economists willing to write history was Ernest Labrousse, whose youthful Marxism inspired his initial commitment to economic history. He was appointed to the Economic and Social History chair at the Sorbonne in 1946 (replacing Marc Bloch who was shot during WWII). Labrousse held this influential post for over 20 years and oversaw hundreds of students.[34] Considering the relatively strict guidelines he enforced, this gives a measure of his influence in the field. In 1974, one of his students, Pierre Chaunu declared, perhaps with only a slight exaggeration: "today, the entire French historical school is Labroussian".[35]

Labrousse's research was marked by a continued interest in the origin of social crises and revolutions. He had developed a theory that linked political crises to economic ones, and economic crises to cyclical variations in agricultural prices. He had applied this general model to account for the French revolutions in 1789, 1830 and 1848, showing how variations in the price of key agricultural commodities had had different impacts on the standard of living of different social classes. Yet Labrousse did not develop monocausal accounts, recognizing that agricultural crises were necessary though not sufficient conditions for upheaval. This is where he made the transition to social, cultural and political history arguing that crises were also rooted in class conflict

[32] For information on the circumstances in which political economy was annexed to law departments, see Le Van - Lemesle (1978).

[33] For an overview of most 1950s economists' (in law departments) opinions on economic history, see Morrisson (1988). For an example of the evaluation of the German Historical School by a French law department economist, see Gonnard (1947), 421-429.

[34] Daumard (1988).

[35] "Aujourd'hui toute l'école historique française est labroussienne " Cited in Coutau-Bégarie (1983), 130.

and political contestation.[36] In other words, Labrousse's work was a tribute to Marx, but also to Simiand and Bloch.[37] From Marx he borrowed economic determinism and an analysis based on social class. From Simiand he inherited the general idea of linking agricultural prices to wages, and used the same statistical methodologies. From Bloch he adopted the view that all history should be "total" – i.e. should weave the economic, political, cultural facets of a phenomenon into one unified account.

Labrousse had trained as an economist – as a student in the *Facultés de Droit*, separate from the *Facultés de Lettres*, where Bloch, Febvre and Braudel had trained. He wrote a thesis for the economics department, *Esquisse du mouvement des prix et des revenus en France au XVIII^e siècle* (1932), then attempted to pass the "Aggrégation" in economics, but failed - most probably because he was openly communist and the *Facultés de Droit* were famously conservative.[38] Labrousse successfully completed his "Aggrégation" in history in the late 1930s, and remained in humanities departments thereafter, having completed a second thesis -*La Crise de l'économie française à la fin de l'Ancien Régime et au début de la Révolution* (1943) - which earned him the chair at the Sorbonne. There, he continued to apply quantitative and statistical reasoning to historical problems. His students followed in his steps, producing regional monographs of 18th and 19th century France with special attention to variations in prices and their social and political consequences.[39]

Labrousse and his students very seldom attempted to bridge their findings with more general statements about economic growth and development – rather, they favored conclusions about the social and political consequences of economic cycles (crises, revolutions, distribution of income for example). This was a telling omission, and an indication that different themes fascinated French and American social scientists

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36 Labrousse (1948).
37 Delacroix, Dosse and Garcia (2003).
38 Labrousse was an editor at *l'Humanité* (a communist newspaper) from 1919 to 1924, Labrousse (1980). A common saying at the time was "Facultés de Droit, facultés de droite", which roughly translates to « Law departments are right-wing departments ».
39 Labrousse's most famous student was Emmanuel Le Roy-Ladurie; his thesis work on Southern France was published as *Les Paysans du Languedoc;* Le Roy-Ladurie (1966).

in the mid 20th century. As we saw in chapter 4, in the U.S. the 1950s increasingly gave way to talk of economic development and an interest in the economic past of developed nations for the sake of drawing lessons for developing ones. This view that economic history had something important to say about development was intimately tied to a claim that general scientific propositions could only be derived from long-term empirical study. Neither belief was predominant in France at that time. Concerns with growth and development were less pronounced, and many *Annalistes* were loath to draw explicit "lessons" from the past (recall Bloch and Febvre's hesitations). In spite of these differences, American philanthropists saw a connection between French scholarship and knowledge pertaining to growth and development.

3.2 American Foundations show interest for French social science

Fernand Braudel proved to be much more gifted than Labrousse when it came to exploiting this connection. His own work embodied what many considered to be the *Annales*' potential for producing broad statements about change in the economic and social realm. His study of economic and social trends of the Mediterranean region in the late medieval period had earned him the reputation of being a great synthesizer: someone who could start from an abundance of historical detail and turn it into a persuasive causal account of the way in which "structures" (slow trends in climate, geography, fertility) and "conjunctures" (political events, technological inventions, territorial discoveries) both exerted weight on social and human destinies. [40] Braudel was arguably a geographic determinist. He always began with considerations of topology and climate, as prime movers in human events. However, he would

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[40] Fernand Braudel (1902-1985) is remembered for two massive contributions to European economic history – *La Méditerranée* and *Civilisation Matérielle, Economie et Capitalisme,* Braudel (1949); Braudel (1979).. Legend has it that he met Febvre on a boat crossing the Atlantic in 1937. During WWII, Braudel was imprisoned in Germany, and wrote his thesis on the Mediterranean, sending drafts to Febvre in Paris. This Herculean feat laid the grounds for the legendary stature Braudel acquired soon after his return to France. For more biographical information, see Burke (1990), 32-56.

subsequently amend this strict determinism with differences from one human community to the next, thus acknowledging culture and other man-made constraints.[41] Though Braudel's research interests lay principally in the medieval and late medieval periods, his non-Marxist analysis of the origins and nature of capitalism, and his enduring interest in the wealth of nations convinced many of his readers and colleagues that he had something interesting and important to say about their contemporary world.[42] This certainly seems to have been the opinion of several officers at the Rockefeller and Ford foundations who made the connection between Braudel's work and issues of economic growth and development.

As we see in Figure 5.3, the long collaboration between American foundations and *Annales* had begun in 1947 when Febvre had obtained the creation of the *VI e Section de l'Ecole Pratique des Hautes Etudes* (*VI e Section*) thanks to American funding.[43] This new branch was officially the social science division of an *Ecole* dedicated to specialist research. Febvre died in 1956 and Braudel inherited Febvre's academic positions: both the professorship at the *Collège de France* and the directorship of the brand new *VIe Section*. The RF renewed funding for the *VI e Section* in 1952 - by then the French government was covering all operating expenses and RF money was attributed to cross-disciplinary conferences and economic history only.[44]

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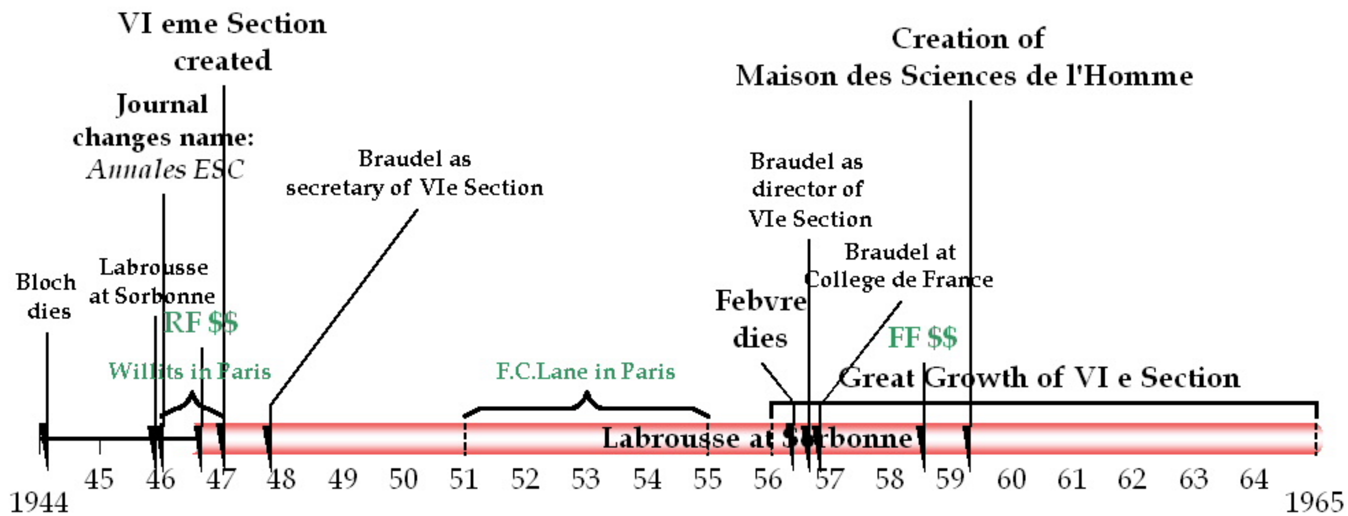
[41] See for example the first 50 pages of Braudel (1979).

[42] Though Braudel acknowledged Marx, his main reference for explaining and theorizing the origin and likely future of capitalism was the German historicist Werner Sombart. See for example, Ibid, 206.

[43] The grant was not huge - $30,000 for 3 years. Compare this to a 1947 grant of $130,000 for 2 years to Charles Rist's economic institute in Paris. Mazon specifies that this grant was only sufficient to cover one fourth of the *VI e Section's* annual expenses. She also mentions that the *VI e Section* got a lot of bad press for using "millions from America"- Mazon (1988), 91-96.

[44] This second grant was smaller than the first: $13,500 for 2 years - Ibid, 81-115. For a measure of the relative size of this grant, see « How much was that ? ».

Figure 5.3: Time Line of events after WWII



When the Ford Foundation took over in Europe in the mid 1950s, it continued conversation with *Annalistes* (the main correspondent having changed from Febvre to Braudel). In December 1959 it awarded $1 million for the establishment of the *Maison des Sciences de l'Homme (*MSH) - a grant that was explicitely aimed at fostering social science in France.[45] The French government matched this grant with approximately $2 million.

The process by which Febvre, then Braudel managed to convince American foundations that historians should be important players in social science has mesmerized more than one historian of ideas.[46] Gemelli has argued that Braudel was able to seduce foundation officers, as his values and agenda were very similar to the ones held by interwar American economists: namely, they all shared a vaguely "progressive" commitment to social science (Braudel was certainly more conservative than Labrousse!).[47] Yet, this is a difficult argument to make, considering the great variety of economists in the U.S. at that time (including for example statistical economists, narrative institutionalists and formal mathematical scholars, all of whom had different political stances) but it can be recast in a slightly different light to account for the Rockefeller Foundation (RF) and Ford Foundations (FF)'s interest in the *Annales.* Instead of comparing Braudel to American interwar economists at large, it may be more useful to compare him to Willits, Bezanson and the economic historians described in chapter 3.

It seems quite plausible that Braudel's rhetoric of multi-disciplinary, integrative and empirical social science (which he borrowed from the founders of *Annales*) would not have gained such favor with American foundations had they not recognized a

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[45] See Ibid, 157. Mazon cited the grant justification: "Les progrès rapides de l'industrie française de l'après-guerre et la détermination de nombreux dirigeants des secteurs publics et privés dans l'accélération du processus de modernisation entraînent une demande d'économistes, de sociologues et autres spécialistes des sciences sociales (FF, PA 60-437, Compte rendu du Conseil d'Administration de la Fondation du 11 Déc. 1959)." For a measure of the relative size of this grant, see « How much was that ? ».
[46] Coutau-Bégarie (1983); Mazon (1988); Gemelli (1995).
[47] Gemelli (1995), 256.

certain congruence between economist-historians in the U.S. and Braudel's work. This begins to explain both the foundations' interest in "history" (in spite of the fact that foundation officers were often reminded that history was part of humanities, not social science) and their willingness to overlook the fact that much French economic history was Marxist![48] Indeed, RF officers were aware of this, yet had been swayed by a rhetoric they were sensitive to, namely an argument that favored "empirical economics", in particular as it involved economic history. They were not opposed to financing mathematical economics (which was on the rise in France), but they saw no objection (quite the contrary) to investigations that started with "observation", in particular historical observation.[49] Thus they had no difficulty seeing economic history, and consequently *Annales*, as a form of empirical social science.

It is indicative to note that many foundation agents sent to France on behalf of social science divisions were "old" economic historians or their friends (Joseph Willits, Frederic Lane, Frances Sutton and David Landes for example, see Figure 5.3).[50] For the modern reader, considering the prominence of mathematical and technical economics today, it might be difficult to understand how someone like Frederic Lane (who earned a doctorate in history at Harvard in 1930) could have been given such a job. This is yet another reminder that these disciplines were not so far from each other, though by the 1950s they were in the process of growing further apart. Lane's Ph.D. may have been in

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[48] American foundations did actually sponsor communist researchers in France - largely because it was very difficult to selectively pick out and not fund radical scholars. However, insofar as they could refuse sponsorship to left-minded social scientists, the foundations did. This was certainly true of Willits, as mentioned in chapter 3. Concretely, this resulted in suspension of certain grants in the early 1950s, for example to the CNRS - see Mazon (1988), 126.
[49] Diary entries for 1946. RAC-RF, RG 12.1, Box 70, "Joseph Willits".
[50] Willits spent several months touring Europe in 1946. As the RF main European office was in Paris, he spent a sizeable part of his time there – Diary entries for 1946. RAC-RF, RG 12.1, Box 70, "Joseph Willits". Lane was sent by the RF as Associate Director of Social Sciences to run the European office from 1951 to 1954 - Gemelli (2003). Sutton and Landes were friends when they were students at Harvard. Sutton was a full time officer at the FF, gradually rising to Executive Vice President of International Affairs. In his early years of employment he worked on the MSH grant and often asked the advice of his friend David Landes, who lived in France as a Ph.D. student and young scholar – Frances Sutton (2004), Interviewed by Cristel de Rouvray, New York City, June 2004.

"history", and he may have taught in the History Department at Johns Hopkins, yet he had been Gay's student, one of the first members of the CREH in the early 1940s, and president of the EHA in 1956-58. Recall from chapter 3 that both the CREH and EHA had been spun as efforts to change economics, not history.

There was a long-standing association between *Annales* and American "old" economic historians. Usher, for example, had contributed two articles in the first year of *AHES* (1929). Subsequently, Lane played an important role in maintaining the connection. He had met Braudel in Venice before WWII and they had remained good friends.[51] Braudel was also quite intimate with Earl J. Hamilton, who, as the reader recalls from chapter 3 was also part of the Gay lineage.[52] These connections were passed on to the younger generations: Braudel and Labrousse's students were made aware of the American economic historians' work. Pierre Chaunu for example wrote Earl Hamilton in 1956 to acknowledge his debt to the method and purpose Hamilton embodied.[53] When Landes came to France he befriended Chaunu and his wife and colleague, Hugette Chaunu.[54] These friendships were eased by deep epistemological connections that united *Annalistes* and "old" economic historians. When Braudel presented his vision, Lane recognized arguments that had been put forward by his friends (Bezanson, Cole, Innis, Hamilton etc.) a few years earlier to justify the creation of the CREH.

Yet, there was a fundamental tension between Lane's commitment to Braudel and the MSH and the activities of many scholars affiliated with *Annales*. This tension came out in Lane's opinion of Labrousse:

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[51] Ibid.

[52] Earl J. Hamilton's papers hold many examples of personal and professional letters between Braudel and Hamilton. See for example, Letter from Braudel to Hamilton, 6 août 1946, Duke University, Earl J. Hamilton papers, Box 2, Folder "Correspondance / papers (1930s-1970s)".

[53] "Je m'empresse de vous dire combien nous sommes redevables à une oeuvre qui est, entre autres, responsable de notre commune passion, pour une histoire économique, dynamique et quantitative, dont nous nous plaisons de reconnaitre ici, en vous, le maitre incontesté"; Letter from Pierre Chaunu, 13 novembre 1954, Duke University, Earl J. Hamilton papers Box 3, Folder "Correspondance - Personal; 1930s-80s".

[54] Pierre and Hughette Chaunu (2004), Interviewed by Cristel de Rouvray, Caen, November 2004.

> "Because I know his work in economic history, I wanted to meet him, although his own kind of research seemed likely to be more within the field of the humanities than of the social sciences division. My talk confirmed this impression (…) He is glad to have economic history attached to the historical section of the *Facultés de Lettres*, even though that cuts it off completely from instruction in economics, because his main interest is infiltration into general history of his kind of economic interpretation."[55] (My italics)

Notice how Lane presented history taught in humanities departments as being outside his responsibilities in France (he insisted that his meeting with Labrousse had been motivated by personal interest rather than professional duty). By Labrousse's "kind of economic interpretation" Lane was referring to the latter's Marxism. In Labrousse's writings, capitalism and markets were unstable and auto-destructive institutions. This contrasted with Lane's work and that of his colleagues at the RCEntrepH (which was going strong in the 1950s, as the reader should recall from chapter 3), for whom capitalistic institutions could prove quite virtuous if they were associated with the right sort of individual liberties and incentives.

Lane's opinion of Labrousse went beyond ideological dispositions: he also seemed to regret the fact that Labrousse was not actively engaging in dialogue with economists, which was a defining feature of economic history in the U.S. (the kind he personally contributed to). In spite of these differences, Lane did not consider Labrousse to be a threat to the overall potential of the MSH, and trusted that Braudel held a vision similar to his on the intimate relationship between economic history and "good" social science. As a result of this trust *Annales* benefited from a large fraction of the resources allocated to French social science (coming both from the foundations and from the French government).

[55] Diary entry for November 7th 1951. RAC-RF, RG 12.1, Box 70, "Frederic C. Lane".

3.3 Economic history emerges as a strategic place

The spread of Marxist scholarship and American foundation money both contributed to turn *Annales* economic and social history into a growing activity in France – adding to the momentum that had already been built by the hybridity of Bloch and Febvre's journal. Fernand Braudel's ability to formulate and embody a scientific, explanatory history was also crucial to this success. In his mind *Annales* research was meant to contribute to a better knowledge of man and society. This was confirmed by the names he chose for his institutions. The journal was now *Annales: Economies, Sociétes, Civilisations.* By dropping the word *histoire*, Annalistes were making bold claims, further embodied in the name of the MSH: "sciences of man". This term was not the same as "humanities" (which in French is *lettres*) and Braudel had insisted on adopting a label that included all social sciences. In a 1955 letter to his friend Hamilton, he said that he was attempting to build an institution "like the London School of Economics". [56] When he became director of the *VI e Section*, he began recruiting the ablest scholars in all fields: a majority of historians, but also anthropologists, sociologists, economists and even mathematicians and engineers.[57] The *VI e Section* experienced phenomenal growth; history departments fared particularly well in French university in general. [58]

While Braudel strove to maintain the appearance of hybridity in the *Annales* project, he also let it drift (and pushed it) towards an even stronger historical stance, and increasingly presented history as the ultimate social science. In a 1958 article he made a strong case for history as the unifying science: its focus on time, on the

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[56] Letter from Braudel to Hamilton, 17 Mai 1955, Duke University, Earl J. Hamilton papers, Box 2, Folder "Correspondance - misc; 1919; 1920s-1970s and n.d".

[57] Malinvaud (1996).

[58] Scanning through the *Annuaires* (yearly summaries of activities and classes taught) of the EHESS (current name of the *VI e Section*), I obtained a rough estimate of the number of faculty (all posts combined). It went from just over 50 in 1956-57 to over 130 ten years later (i.e. 160% growth). A full collection of these *Annuaires* is available from the archivist at l'EHESS, 54 Blvd. Raspail, 75006, Paris. From 1928 to 1966, the total number of academic chairs (all schools/universities in France) increased by 130%. Historians' chairs increased by 150%, while all other social sciences combined increased only by 120%. Absolute numbers are also indicative. In 1966, there were 130 chairs in History and only 40 in all other social sciences combined- see Keylor (1975), 163-8.

succession of events, on repetitions and on discontinuities made it alone capable of highlighting long-term trends at work, the identification of which was crucial for our understanding of the social world:

> "Whether we are in 1558 or in 1958, whomever wants to grasp the world must define a hierarchy of flows, individual movements and then combine them in an overall constellation (…) The world of 1558 was not born in that year, neither was our year born in 1958. Every present day combines movements of different origin and rhythm: today's time is also yesterday's time, the day before yesterday's and the olden days' time." [59]

For Braudel there was no explanation of the present without an understanding of the past, of the events that led to our current day, and of the slow moving forces, like climate, that shaped human evolution. This was not a "stageist" or relativist view of history, but rather a statement about the influence of the past on the present. This heightened commitment to history in general, and economic history in particular, effectively downplayed the contribution of other disciplines. The journal no longer contained as many articles written by or for non-historians. The editorial board changed, now consisting of only three people and two of them were historians. [60] While pre-war issues had presented recent developments in economic statistics and methods, the post-war journal published very few articles of this nature, featuring instead an article that qualified recent econometric developments as useless, without really explaining them. The author – Jean Domarchi - an economist from a law department

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59 "Qu'on se place en 1558 ou en l'an de grâce 1958, et il s'agit, pour qui veut saisir le monde de définir une hiérarchie de forces, de courants, de mouvements particuliers, puis de ressaisir une constellation d'ensemble (…) Le monde de 1558 (…) n'est pas né au seuil de cette année (…). Et pas d'avantage (…) toujours notre difficile année 1958. Chaque "actualité" rassemble des mouvements d'origine, de rythme différent: le temps d'aujourd'hui date à la fois d'hier, d'avant-hier, de jadis", Braudel (1958), 735.

60 Fernand Braudel and Charles Morazé. The third was the sociologist Georges Friedmann.

had concluded that “econometrics is not the beginning and end of our science. In the current state of affairs, literary economists have nothing to worry about”. [61]

Thus the 1950s witnessed the preservation of Febvre and Bloch’s hybridity within the newly created institutions (*VI e Section* and MSH) but also saw the progressive solidification of *Annales* scholarship into a watertight space, where certain empirical methods and theoretical frameworks were becoming entrenched, to the exclusion of other approaches (in particular new methods of empiricism developed by statisticians, national accountants and econometricians). In other words, the appearance of inter-disciplinarity was maintained, but only superficially. Historians held a privileged seat and controlled the institutions (via Braudel’s unchallenged authority).

This process of sealing the borders, and clearly defining who was in and out did not happen overnight. The 1950s and early 1960s were still unstable times, largely due to the necessity of obtaining American funding and thus keeping open the dialogue with other social sciences – the regular appearance of green dollar signs on Figure 5.3 should be a reminder of many groups’ dependence on American funds. This porosity and the stakes tied to economic history made it an attractive space to scholars outside *Annales*. Several of these challengers (or band-wagoners) were French economists. In the same letter where Braudel had compared the *VI e Section* to the LSE, he added that “he was battling with economists”.[62] Though he gave no names, the pressure could have come from two different categories of economists at the *VI e Section*. Either they were mathematical economists like Guilbaud and Malinvaud, whom Braudel had hired to run small seminars, but more likely they were professors in law departments like Domarchi, who still had control over the economics curricula and diplomas. Unlike the

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[61] “Je désire seulement indiquer que l’économétrie ne constitue en rien l’alpha et l’oméga de notre science. Dans l’état actuel des choses, les économistes littéraires ne doivent contracter aucun complexe d’infériorité”, Domarchi (1958). Domarchi seemed to have found an audience for his complaints in post-WWII *Annales ESC*. This suggested that literary economists might have been seeking refuge with the relatively strong *Annalistes*, a diagnosis further developed in this chapter.
[62] Letter from Braudel to Hamilton, 17 Mai 1955, Duke University, Earl J. Hamilton papers Box 2, Folder “Correspondance- misc; 1919; 1920s-1970s and n.d”.

first, this second group had real incentive to negotiate with Braudel, as many could sense their growing marginalization in a changing national and international economics landscape.[63]

4 The battles for economic history

4.1 Economics after WWII: François Perroux's unusual position

Section 3 has highlighted the numerous factors that turned economic history into a valuable space in France in the 1950s. Notice that the argument is about economic history, and not economic and social history, or *Annales* at large, though there was considerable overlap. The value of this space was confirmed by non-*Annales* attempts to take hold of it, in the late 1950s, early 1960s. These attempts were principally conducted by economists, who, for one reason or another felt marginalized by the changes that occurred after WWII.

The post-WWII situation in French economics has been schematically described as being divided between three general areas. The first area was made up of professors in Law Departments. Their influence in France (and certainly outside France) was on the wane, and the youngest among them were painfully aware of their lag with the Anglo-Saxon world. The two other areas were much more dynamic, and were made up of engineers who ran the nationalized companies (gas, electricity, coal) on the one hand, and on the other, engineers and mathematicians who worked for ministries and central government (INSEE, Ministry of Finances, Planning agencies).[64] Though these three types of economists were not new arrivals on the French scene, their comparative prospects had radically altered. While those in the universities had enjoyed growing prestige since the creation of economics chairs in law departments in the late 19th century, their authority and legitimacy was rapidly challenged after the war. The great majority of innovation and initiative in French economics then came from the last two

[63] Economics was permanently removed from Law Departments in the late 1960s reform of the French University system.
[64] Etner (2000), 329-30.

groups outside university – some of whom contributed to the mathematization of the discipline and earned international acclaim (for example Malinvaud, Allais and Debreu).

Not all French economists neatly fit this classification and the career of the notably non-categorizable François Perroux tells us a lot about the reconfiguration of the discipline after WWII. Much can be gleaned from his interactions with *Annalistes*. Though he did not initially do work in economic history, Perroux was an important player in an increasingly contiguous space: development economics. Shortly after WWII he had created the *Institut de Sciences Economiques Appliquées* (ISEA) – with the explicit aim of doing more empirical research than was habitual in Law Departments. Perroux was also a theorist. He had developed theories on the origin and processes of economic growth that he encapsulated in the notion of "*pôles de croissance*".[65] To convey his theories, he made drawings of these motors of economic growth on maps, representing them as points of attraction and economic dynamism (some of these maps looked like seismic drawings, with epicenters and various intensities of radiation). The basic idea was that the wealth and long-term success of a region depended on the existence of such driving "*pôles*" and that economic performance was not evenly distributed across national space.[66]

Perroux had been quick to recognize the importance of development economics, and he tried to build an international reputation in this field, both for himself and for the ISEA. This probably explained why Kuznets looked to him first as a collaborator on French retrospective accounts before turning to Marczewski and then to Malinvaud (see chapter 4). Perroux had recruited young economists with the profile sought by Kuznets: numerically literate and historically sensitive (as many of them were Marxist). Jacques Mayer was a typical example of these new recruits: a student of mathematics with an interest in Marxism, he had decided to become an economist – and the work he did for

[65] For biographical information on Perroux see de Margerie (1980).
[66] For a collection of his writings see Perroux (1982).

the ISEA had been spun under the general theme of "historical investigations in French economic growth" commissioned by Simon Kuznets (recall section 3.1).

After his initial contact with Kuznets, Perroux made sure to keep a historical team at the ISEA (see figure 5. 4 featuring a picture of Francois Perroux talking to Jacques Mayer and Colin Clark, presumably on the topic of retrospective national accounts). L' ISEA's work in development economics and economic history became bargaining chips in his negotiations with the RF and FF, which had begun in 1946, during Joseph Willits' visit to France. Willits had been intrigued by Perroux and impressed by his collaborators (in particular by the mathematician Guilbaud, whom Braudel would later recruit at the *VI e Section*). [67] The ISEA obtained funds from RF starting in 1949, and was awarded a terminal grant in 1958. These grants averaged $10-15,000 per year.[68]

Perroux's claim on growth studies gradually became his last connection to the world of economics. He could sense changes in the French academic landscape, and he was conscious of the fact that he did not really fit into any of the emerging categories. Though he had attempted to contribute to the wave of mathematical economics, principally by hiring young mathematicians, by the end of the 1950s, they had almost all left. Their departures were motivated by various reasons (some of them personal, due to Perroux's difficult character; but many because they had been recruited elsewhere, and the alternatives were more attractive than the ISEA). Perroux could sense that his lack of mathematical training made him look outdated to these young, up-and-coming economists. And while many still considered him to be a great thinker, he was finding it difficult to identify a niche in which he could incontestably be recognized as an expert. [69]

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[67] Diary entries for October 11th, October 14th and October 17th 1946. RAC-RF, RG 12.1, Box 70, "Joseph Willits".
[68] Gemelli (1995).
[69] Many of the people I interviewed mentioned Perroux's frustration at not being able to contribute to the mathematization of economics and his consequent decrease in stature and influence. See for example Jacques Mayer (2003), Interviewed by Cristel de Rouvray, October 2003.

Figure 5.4: Picture of Perroux and his young collaborators at ISEA, early 1950s[70]

The instability of his situation had serious consequences for his reputation in France. For example, it led to his progressive departure from the national accounting scene, a field he considered to have pioneered: he was not invited to help design and implement the French national accounts infrastructure.[71] Perroux might have found a place among Law Faculty economists (who were also threatened by the increased mathematical nature of the discipline – recall Domarchi's rant against econometrics) in spite of their methodological differences, had it not been for his ideological dispositions: he was not conservative enough to please them.[72] His overall position was certainly not

[70] Pictures dates from 1951. RAC-RF, Photograph Archive, 500S, #227185. I added the names onto the photograph (they were inscribed on the back of the photograph).
[71] Fourquet (1980); Vanoli (2002).
[72] In 1951 Marczewski talked to Lane, and told him that "Rist, Rueff and Baudin [are] 'liberals' and anti-Keynesians who consider Perroux a heretic". The men he cited were traditional, literary economists who taught in Law Departments. Diary entry for October 18th 1951. RAC-RF, Series 12.1, Box 70, "Frederic C. Lane".

aided by rumors that he had collaborated with Maréchal Pétain's regime, though these claims were apparently not founded.[73]

As time went by, l'ISEA found it increasingly difficult to obtain American funds – this worried Perroux as his Institute was not officially part of the French University, hence was not covered by the Ministry of Education budget. The precariousness of his situation became worse when Willits left the RF in 1954. His successors agreed to a terminal grant in 1958 both because he overwhelmed them with letters and because he played the "growth" card, to which they were very sensitive.[74] When the FF took over in France, in the mid 1950s, they established contact with Perroux. This led to a $50,000 grant in 1955, which was never renewed – and seemed to be a "mercy" grant![75] Thus, at about the same time that Braudel was building a vibrant MSH, l'ISEA's future was looking somewhat bleak.

4.2 Economists covet the VI e Section: Perroux versus Braudel

Perroux's precarious position seemed to have led him to challenge *Annales* on the latter's own territory: economic history. This was a difficult position to navigate, as both Braudel and Labrousse's influence were on the ascent. However, by the late 1950s, the *VI e Section* was Perroux's last chance to secure status and lasting influence: a space where he could hope to find support and funds for his type of economics. Braudel seemed willing to collaborate with Perroux up to a certain point, though he may not

[73] The bases for such claims were that Perroux had remained head of the *Institut Alexis Carrel* (Carrel was known for his work in eugenics) until 1944 and that he had authored several papers on communal work plans that had apparently pleased Pétain, and possibly inspired him for his "Charte du Travail"; see de Margerie (1980).
[74] $50,000 in January 1958 for 6 years – Grant Report, 1958. RAC-RF, Series 1.2, RG 500S, Box 17, Folder 153. The ISEA actually got small subsequent grants, though these corresponded to RF's new interest of sponsoring work in developing countries (ISEA had opened offices in North Africa that qualified for this funding).
[75] Grant Report. FF, PA 56-18. The grant was for growth studies, for approximately three years. For a measure of the relative buying power of this grant, see "How Much was That?".

have held the economist in high esteem.[76] He had invited Perroux to teach at the *VI e Section*, and had set up a privileged relationship with the ISEA. Yet, he did not come to help ISEA economists when Labrousse's students criticized and greatly slowed down the work they were doing for Kuznets.

As seen in Figure 5.5, the debate erupted publicly in 1964, with Pierre Chaunu and then Pierre Vilar's virulent critiques against the first installations of Jan Marczewski's efforts to reconstitute French national accounts for the 18th and 19th centuries.[77] These critiques had a devastating effect on Marczewski's team– making it very difficult, if not impossible, for them to draw funds and other researchers into their venture. As seen in Figure 5.5 it took J.C. Toutain, the first and last collaborator on the project, over 30 years to publish his first estimate of French historical GDP series (combining the work of his colleagues who progressively dropped out of the project), as compared to the five to seven years that it took other Kuznets collaborators, for example Deane and Cole in Britain.[78]

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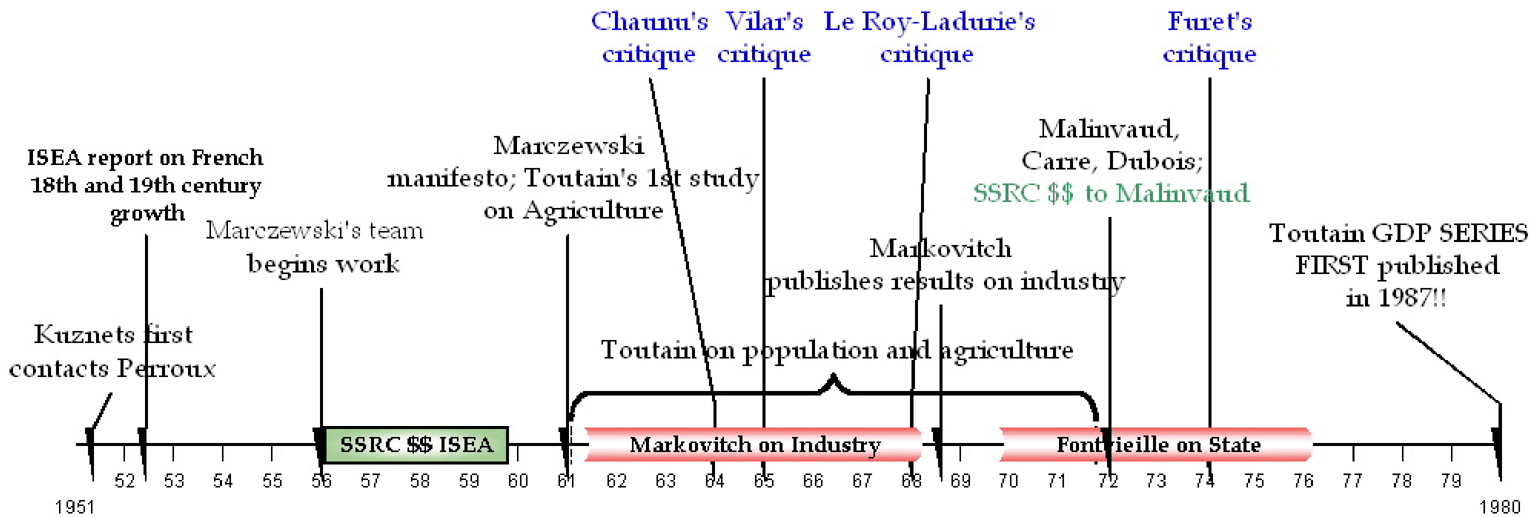
[76] In 1951, in a meeting with Lane, Braudel had some very harsh words for Perroux's work and his ability to revive French economics: "His words about Perroux were too sharp for me to risk recording them from memory. He thought [him] way behind the time and lacking sense of economic reality". Diary entry for October 19th 1951. RAC-RF, Series 12.1, Box 70, "Frederic C. Lane".

[77] Emmanuel Le Roy-Ladurie and François Furet issued subsequent critiques, though these were published in the late 1960s and early 1970s, when the dialogue between historians and economists had effectively been closed.

[78] Deane and Cole (1962); Toutain (1987).

Figure 5.5 : Retrospective accounts' long ordeal in France



Before these debates erupted in print, Braudel had apparently attempted to instantiate collaboration between “historians” and “economists” who worked on the 18th and 19th centuries. Toutain recalled that in 1960 Braudel had organized a joint meeting between Perroux, Marczewski and Labrousse. He remembered a total lack of communication between two men who wanted to dominate the scene: Labrousse and Perroux. He described the latter as a “new-born in economic history”, but someone with a wide culture and a broad view of things (like Braudel, he added). Labrousse, he claimed, was a narrower-minded specialist. Toutain acknowledged that there were “theoretical discordances” during the meeting (Perroux was not a Marxist) but he felt that the real obstacle was competition for leadership. Both Perroux and Labrousse were in favor of the creation of an institute for quantitative economic historians, provided they personally, and independently headed it: “It was a brawl, as both men had strong characters. Consequently, no association was created. Only bad feelings.”[79]

Yet, Perroux did not give up, and made many parallel efforts to insure that his type of economics would be represented at the *VI e Section*. His survival strategy seems to have been to make sure that all studies on growth and development were attributed to him personally or to the ISEA (a strategy that was not impossible considering that these themes were not of major interest to French social scientists at that time). This led him to challenge Braudel on several occasions. For example, in early 1960, Perroux wrote a fiery letter to the director of the *VI e Section* (Velay), asking him why the *VI e Section* had just published a book that touched on issues of economic development. He reminded Velay and Braudel that the ISEA had a monopoly and international reputation in this field.[80] In general Perroux was very suspicious of Braudel’s plans:

[79] J.C. Toutain (2003), Interviewed by Cristel de Rouvray, Paris, March 2003.
[80] “Je te rappelle que l’ISEA, qui fonctionne depuis 15 ans, s’est depuis 10 ans consacré, en France, à l’étude du développement, de la croissance, du progrès et des progrès (…) Il n’est pas question que la VIe section choisisse unilatéralement les travaux qui l’intéressent chez nous, et soutienne d’une façon plus que proportionnelle, des travaux et des initiatives pour lesquels nous nous sommes acquis, dans l’ordre international, une position qui n’est contestée par

> "Not one day goes by without me hearing of something new in the *VI e Section*, testimony of both a total lack of coordination and a lack of honesty concerning the collaboration between the *VI e Section* and the ISEA."[81]

This led to frictions with other economists, who were also trying to gain resources from Braudel. For example, the law department economist André Piatier eagerly solicited Braudel and volunteered to turn the latter's vision into a concrete organizational plan for social science in France (thus completely surrendering to a vision of history as the ultimate synthesizer, and accepting a relatively accessory role for economics, provided he be a part of it). Figure 5.6 is the copy of a drawing he made in the early 1960s – an indication that he had understood Braudel's grand plan for social science, and that he wanted to be the spokesperson for a collaborative type of economist. At the time Piatier had just started a center for research in economic development at the *VI e Section*, which had greatly annoyed Perroux, and suggested that he was not the only French economist who was trying to use development themes to gain resources from the increasingly popular *Annales*.

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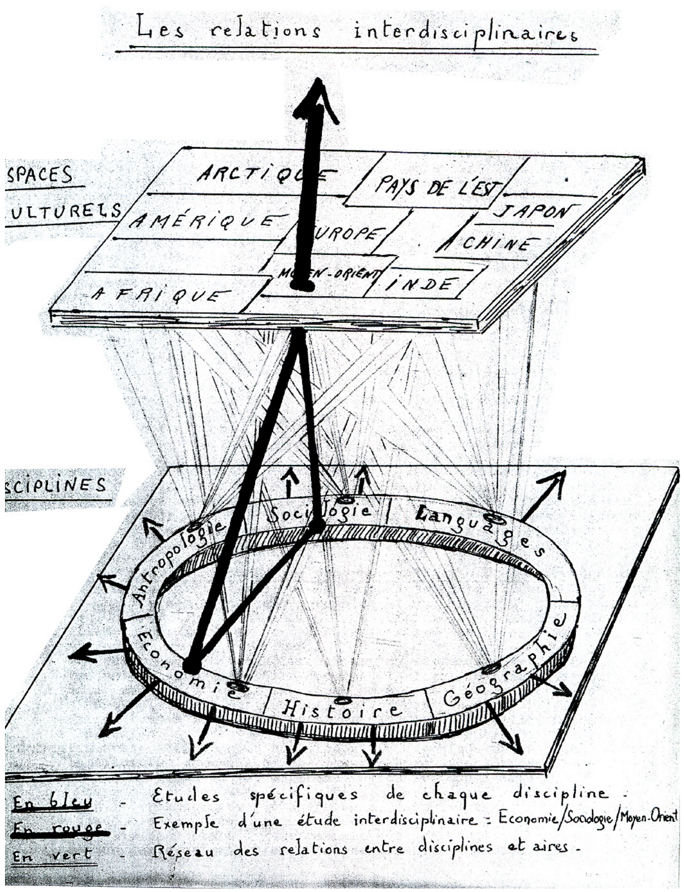
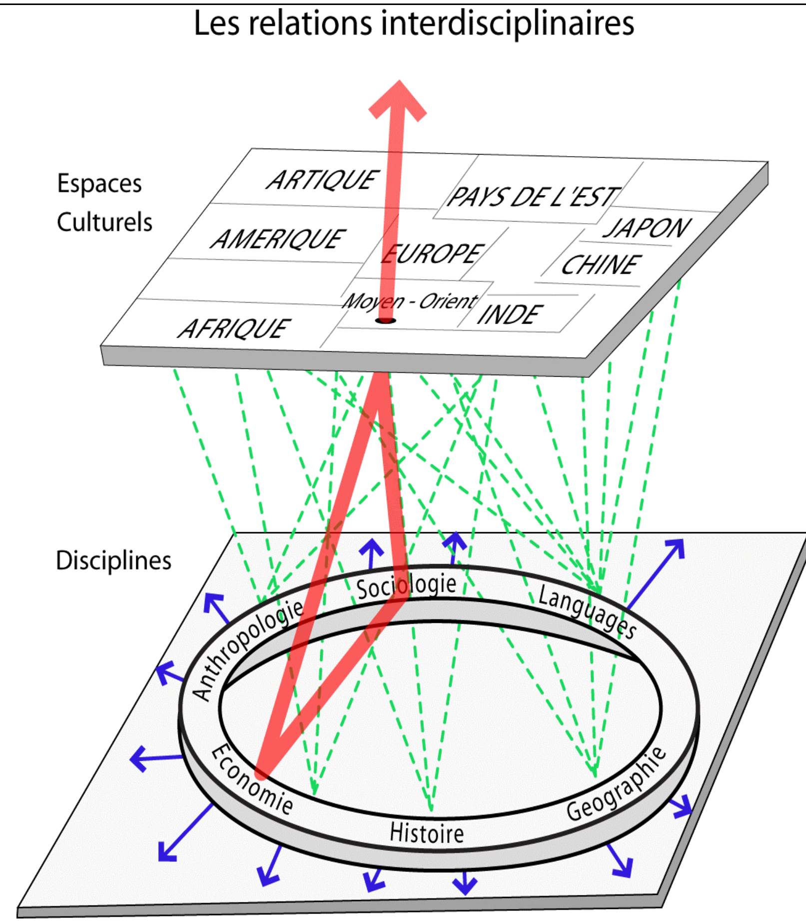
personne". Letter from Perroux to Braudel, 25th July 1960, EHESS Archives, Folder "ISEA (1958-1960)".

[81] Letter from Perroux to Braudel, 25th July 1960, EHESS Archives, Folder "ISEA (1958-1960)". He was referring to efforts by André Piatier and Charles Bettelheim - both traditional economists from law faculties who had found a home at the *VIe Section* – and were both constituting centers for research on « development » (and planification, in Bettelheim's case).

Figure 5.6: Andre Piatier's representation of the *VI e section* agenda for social science (1960)[82]



Copy of original hand-drawn chart



Reproduction

[82] Letter from Piatier to Velay, 29 August 1960, EHESS Archives, Folder "ISEA (1958-1960)".

In Figure 5.6 Piatier had labeled blue lines (small arrows on the lower plane) to stand for “specific studies within each discipline”, the red lines (thick vertical lines) as an example of interdisciplinary study (whatever project he had in mind, it required a sociologist and an economist to collaborate on a problem specific to the Middle East) and the green lines (thin dotted vertical lines) symbolized networks that linked individual disciplines to various regional specialties. The area studies (*espaces culturels*) were a relatively new development in the methodological thinking of *Annales* (principally driven by Braudel, but also by his friend and colleague Clemens Heller) and were being installed at the MSH as the embodiment of a commitment to social science and interdisciplinarity. In reality however, historians principally drove these regional studies.[83]

Piatier’s chart was an interesting mix between a conceptual diagram and an organizational map for the *VI e Section*. This is a reminder that views about good science have methodological, but also social implications, in so far as they imply a hierarchy and division of labor within the scientific community (a general point we developed in chapter 2). The divergence between this ideal map, and the actual division of labor and power within the *VI e Section* are also a hint that the organization of scientific activity is a matter of negotiation and controversy, and that scholars recognize the importance of gaining peer recognition for their particular skills.[84] In a 1960 letter to Velay, Piatier explained the motivation for this chart: he worried that historians from area studies were not attempting systematic collaboration with economists and sociologists. In other words, he wanted to secure an official place for his type of economist and justify it with appeal to Braudel’s vision.[85]

Piatier’s chart was never publicized but it did represent the eagerness certain economists displayed towards the alternative Braudel and his patronage now represented. Such alliances were just one facet of the larger strategic behavior at work

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[83] The relationship between Braudel and Heller, and the genesis of area studies is fully explored in Gemelli (1995).
[84] Bourdieu (1984).
[85] Letter from Piatier to Velay, 29 August 1960, EHESS Archives, Folder “ISEA (1958-1960)”.

and the competition it triggered. As represented in Figure 5.7, the frontier between economics and history was still porous enough to let certain individuals lay claim to an inter-disciplinary space that overlapped with various methodological calls for “empirical” social science.

The fact that people on this chart could compete was indicative of their many similarities (in terms of epistemology, professional networks and vision of their role in society). The sociologist Pierre Bourdieu neatly represented this proximity in a 1967 survey of Parisian academia. In his graphical depiction of faculty members in humanities and social sciences, no one is closer to Perroux than Braudel and vice versa.

In Bourdieu’s graph, the vertical axis represents a positive-negative range: the scholars higher up on the chart have the most honors (Legion of Honor; *Who’s Who* citation for example). This tends to overlap with age, though not necessarily. For example, Braudel and Perroux are much higher up than Labrousse – who was seven years older than them. Malinvaud and Le Roy-Ladurie’s southern position is linked to their youth – a similar study 10 years later would have put them in a much higher position.

Figure 5.7: Economic History in France: clashes and collaboration (1950s and 1960s)

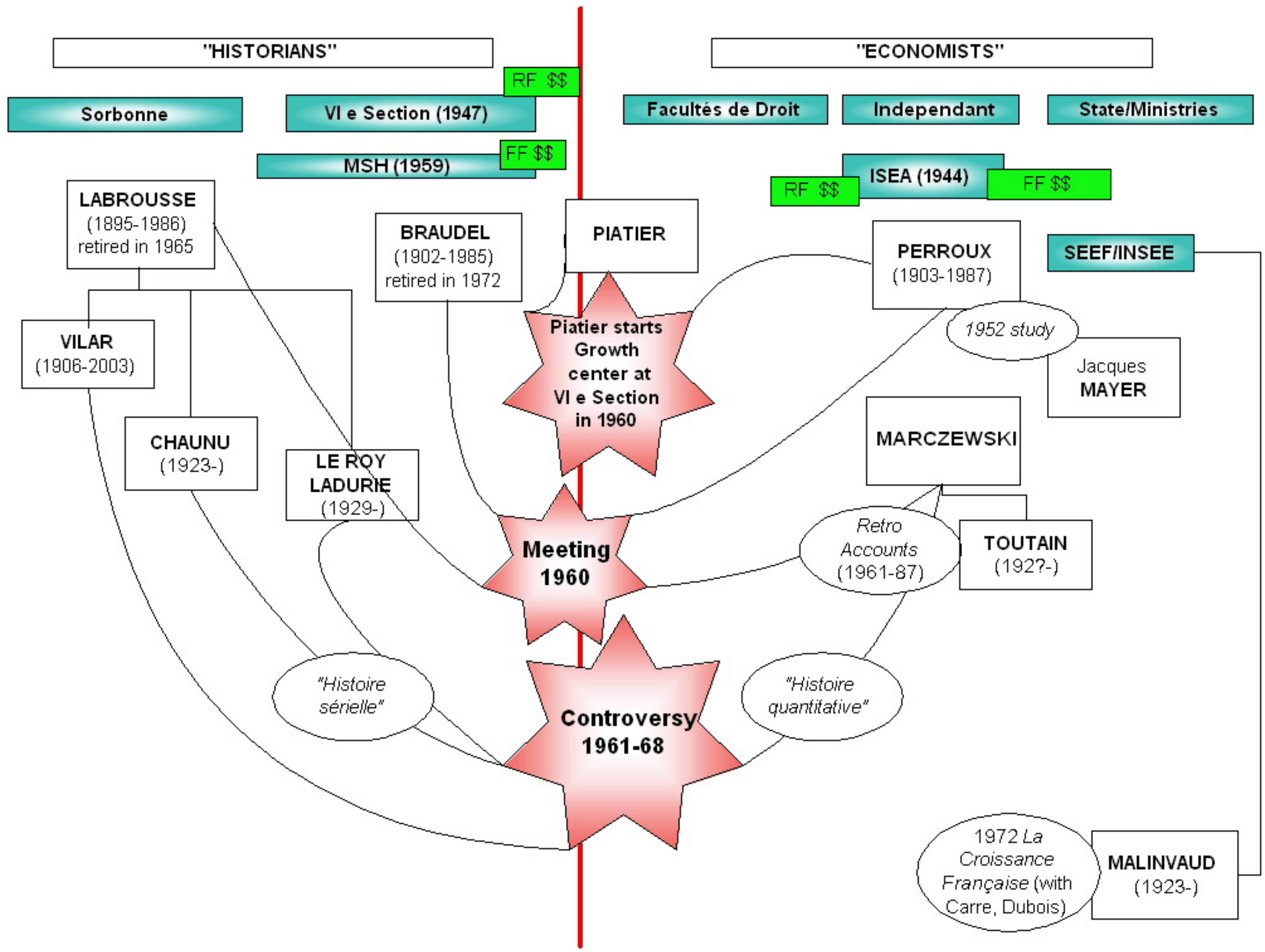
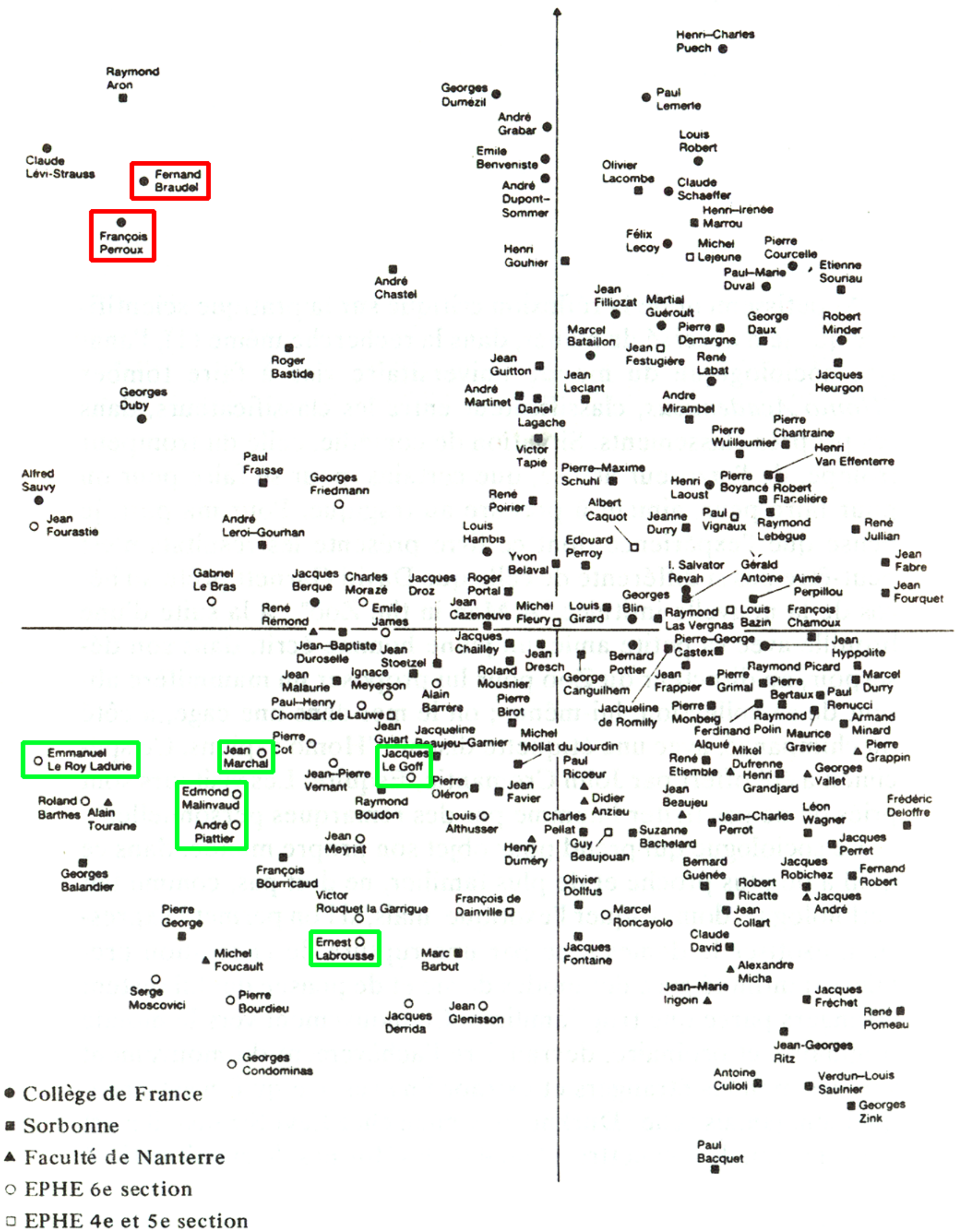


Figure 5.8: Humanities and Social Science Space in France in 1967[86]



86 Bourdieu (1984), 290. The colored boxes are not on Bourdieu's original charts, they were added to make it easier for the reader to identify protagonists mentioned in this chapter. For reference I have also highlighted names of a famous economist in Law Departments (Marchal) and a rising star among young *Annales* scholars (Le Goff).

The horizontal axis represents the different types of power ("social capital") available to academics, on a range from intellectual and popular prestige to pure academic power. The former is defined as the ability to reach a wide and broad audience (in particular an international audience) and includes scholars who were heads of research laboratories, of an editorial board, or had high media visibility; while the latter defines professors who held key positions in the University (appointment juries), thus exerting much influence on the evolution (or reproduction) of higher learning institutions in France. The axis represents a relative measure, so scholars with relatively more intellectual prestige are on the left of the chart (as Perroux and Braudel). Conversely, further right on the chart, you find those who have relatively more academic power. As this study was conducted in 1967, by which time Labrousse had retired from the Sorbonne, his name appears on the left – an earlier study would have depicted him very far on the right.

Notice that all our protagonists are on the left side of the chart. We recognize names of *Annales* historians (Braudel, Le Roy-Ladurie, Le Goff), economists from law departments (Marchal, Piatier) but also rising stars in mathematical economics (Malinvaud). This south-west quadrant would probably also have featured the names of Chaunu and Vilar, though their youth and the fact that Chaunu's first appointment was in Caen and not in Paris precluded them from being featured in this study. Bourdieu's choice to limit his sample to Parisian professors was justified by the fact that academic powers of all kinds were centralized in the capital. Note that Marcweski's professorship was also in Caen.

The closeness between Perroux and Braudel, measured on several dimensions (not just their scholarly work) seems to confirm the analysis made throughout this chapter. Indeed Braudel was much closer to Perroux than to Labrousse, reflecting his conviction that *Annales* history was a path towards better social scientific knowledge, as Lane had pointed out. Following Bourdieu's line of thought, we can argue that Perroux and Braudel were both well positioned to stake a claim on empirical-historical social science – having both earned the reputation of being broad scholars with collaborators they could rely on for the more meticulous monographic accumulation. Both had created research institutes, thus constituting a veritable exception in an otherwise state-run system. The fact that they had been able to skirt the traditional rules of the French academic game and play a riskier one (this is one interpretation of a north-west position) was indicative of the tremendous potential for change that existed in French academia in the 1950s. But it was also indicative of the stakes at play – both on the upside, and on the downside. The stakes were financial, embodied in their relationships to American foundations; political, embodied in the different ideological views that motivated each scholar (Marxism , Keynesianism etc.); and they were professional, embodied in the changing landscape for economics in France. They were also epistemological, as is best seen in the debates that opposed Marczewski's team to Chaunu and Vilar.

5 The many meanings of "empirical"

5.1 Marczewski versus Chaunu and Vilar

Both Marczewski's team and Labrousse's students claimed to be doing "empirical" work, yet they had very different interpretations of what this meant. In his 1961 methodological manifesto (see Figure 5.5), Marczewski - a polish émigré and war veteran who had become a national accounts specialist in France – had argued along Kuznetsian lines in favor of a framework that could provide a comprehensive and precise view of the past.[87] Much like Kuznets, he argued that the superiority of this approach lay in the fact that it provided a systemic view (the best one currently available) for apprehending the many direct and indirect links that connected economic variables: « an instrument capable of measuring, however roughly, the movements of interaction taking place in the [modern] economic world. »[88] According to him, with

[87] Marczewski (1961), 5-7. For Marczewski's work on National Income theory, see Marczewski (1946); Marczewski (1949).

[88] Marczewski (1968), 184. I use this English summary of his 1961 manifesto, to avoid translating the French text. There is no noticeable difference in the core methodological views expressed in these texts.

this framework, the historian could be sure not to let his personal biases enter in the selection of facts, or be swayed by witnesses' views of what had been important in their day – thus contributing, in Marczewski's opinion, to a greater objectivity.[89] While Kuznets' methodological statements had always been formulated for economists – i.e. by stating why his type of historical study was crucial for economics - Marczewski seemed to be addressing both economists and historians. He acknowledged the necessity for economists to study dynamic phenomena, in particular growth, in historical perspective, but he also highlighted ways in which the national accounts framework could aid historians in their investigations.[90] He used the term "quantitative history" for this second, historical usage, which he defined as a more comprehensive, precise and "objective" history.

Many *Annalistes* were infuriated by Marczewski's complete disregard for earlier work in quantitative economic history. The first round of critique came from Chaunu and Vilar (Figure 5.5). Their reactions have tended to be understood as part of the larger issue of French economic historians' disregard for formal economic theory (of which one recent manifestation has been the issue of understanding why there was no cliometrics in France).[91] Forster (1978), for example, has argued that the *Annalistes*' reaction to Marczewski was epitomic of "a French aversion, not only to mathematical model building, but also to [the study of] growth itself, especially industrial growth".[92] This lack of interest in growth was true to some extent. Recall that Labrousse's ideological framework invited him to see changes in the scale of production and consumption as short-term cyclical variations (which he believed were becoming increasingly unstable), rather than manifestations of long-term growth. On the other

[89] Ibid, 186.
[90] Though the scholars he made reference to were principally economists – many were Anglo Saxon - indication that he was very new to French scholarship in economic history. He referenced Kuznets, Rostow, North, Gerschenkron, Leontief, Burns, Mitchell, Perroux, Jean Lhomme, Jean Marchal and A. Barrère. Among historians, he only mentioned Simiand and Labrousse.
[91] See for example Grantham (1997); Crouzet and Lescent-Giles (1998).
[92] Forster (1978), 68.

hand, Marczewski and Toutain were committed to explaining economic growth, an interest spurred by their Keynesian ideas. They were motivated by a desire to confirm Keynes' insights on French historical record. As Toutain recalled: "for us theory was Keynes, and that was it. We had to refine and illustrate him".[93]

Yet, analyses of the debate should not end here if we are interested in finding the emerging lines of demarcation between French "economists" and "historians". These did not appear to be principally or unambiguously ideological. Indeed, there was no ideological homogeneity on either side. Among the economists who had partaken in Kuznets' commission for French retrospective accounts there were Marxists (Mayer, later Fontvielle), Keynesians (Marczewski, Toutain) and those with their own macro-economic theories (Perroux). Among historians, Labrousse and Vilar may have been Marxists, but Chaunu certainly was not (a protestant whose ideals were not far from social Catholicism).

In reality, rather than being ideological, the debate seemed to focus on the trustworthiness of Toutain's data. Both sides had very different notions of what constituted a reliable number. Chaunu made the clearest case for their different views on quantification. He reminded Marczewski that economic historians had not waited for him to do "quantitative history" – and agreed to call this earlier work "serial history".[94] According to Chaunu, the issue was not whether or not to quantify. It wasn't either about fitting the data into a broader framework – Chaunu liked the national accounts framework, and Vilar even admitted that while the search for other frameworks should continue, this one wasn't a bad start.[95] Proof that national accounts, as a framework was not the crux of the disagreement could later be found in Braudel's second edition of *La Mediterrannée* , in which he mentioned that it would be interesting to draw the accounts of the entire Mediterranean region:

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93 "(…) et pour nous la théorie c'était du Keynes point final. Il fallait affiner Keynes et l'illustrer», Toutain, Interviewed by Cristel de Rouvray, Paris, March 2003.
94 Chaunu (1964).
95 Vilar (1965).

> "We would like to have accounts for the 16th century Mediterranean area, not to establish its relative mediocrity or modernity but to determine the essential relationship among its clusters of activity." [96]

Instead the issue seemed to be the quality of the data. For *Annalistes*, the challenge was to provide real, detailed data and rely on no estimates, or as few as possible. In contrast Marczewski had argued that the capacity to fill-in data where it could not be found was one of the virtues of the accounting view, and that these created data were equally or more objective than other numbers, as they were the inevitable product of a pre-defined framework, and not the result of an individual scholars' choice. From his manifesto, and from Toutain's work on agricultural production, both published in 1961, one could glean at least three different types of estimation. The first was conceptual, based on deducing unknown variables from accounting equations (for example, obtaining Consumption, from the difference between Income and Investment; C=Y-I – recall Kuznets' work discussed in chapter 4). The second was derived from experience with contemporary accounts: aggregates tended to vary slowly (unless there was an obvious historical crisis), so linear interpolation between two points was a reasonable approximation of intermediary values.[97] The third was more properly described as order of magnitude work: for example, estimating the grain production of an entire region from a multiplication of known production on a given farm times the number of such farms that could have fit in the entire area.[98]

These different forms of estimations did not convince *Annalistes*, and they certainly did not buy the claim that these numbers were intrinsically "more objective". To the first they replied that if the estimates for income and investment were not reliable (which they often were not, given the sources Toutain used), then neither was

[96] "Nous voudrions faire les comptes de la Méditerranée du 16[e], non pour juger de sa médiocrité ou de sa modernité relative, mais pour déterminer les rapports essentiels de ses masses d'activités les unes par rapport aux autres", cited in Braudel, Ed. (1966).
[97] Marczewski (1968), 177.
[98] Toutain (1961).

the estimate for consumption – i.e. it was always a problem of sources, with or without the framework.[99] To the second type of estimate (linear interpolation) they replied that short cycles actually did exist, and had tremendously important social consequences, as Labrousse's entire career was dedicated to proving.[100] To the third they reacted violently, claiming that this was simply non-sense, and worse than having no data at all:

> "Either the work follows the cautious rhythm of those who build indices of activity, or it desperately strives to fill in its columns, relying principally on the village idiot. In doing so, it contributes to hide reality, and rather than moving us forwards, moves us back."[101]

In general, *Annalistes* made a clear distinction between found and estimated data suggesting that these held different epistemological status. For example, in his work on 16th and 17th century Atlantic trade, Pierre Chaunu always carefully distinguished between numbers that he had found in the Seville archives, and numbers that he had had to estimate. As we see in Figure 5.9 representing the monetary value of traffic between Spain and the New World (third chart from the top), data for which he had an archival source was colored in black, while data he had estimated was colored in white. In these charts, Chaunu was using the *ad valorem* tax paid at departure to compute the total value of yearly shipments (the X axis is chronological, from 1540 to 1640, the Y axis is value in millions). To compute the value of goods on ships whose tax payments he knew, he multiplied their payment by 50 or 100 (the rate went from 1 to 2%, as seen in the second chart from the top). To estimate missing data (he did not have proof of payment from all ships) he approximated their cargo with similar ships for which he

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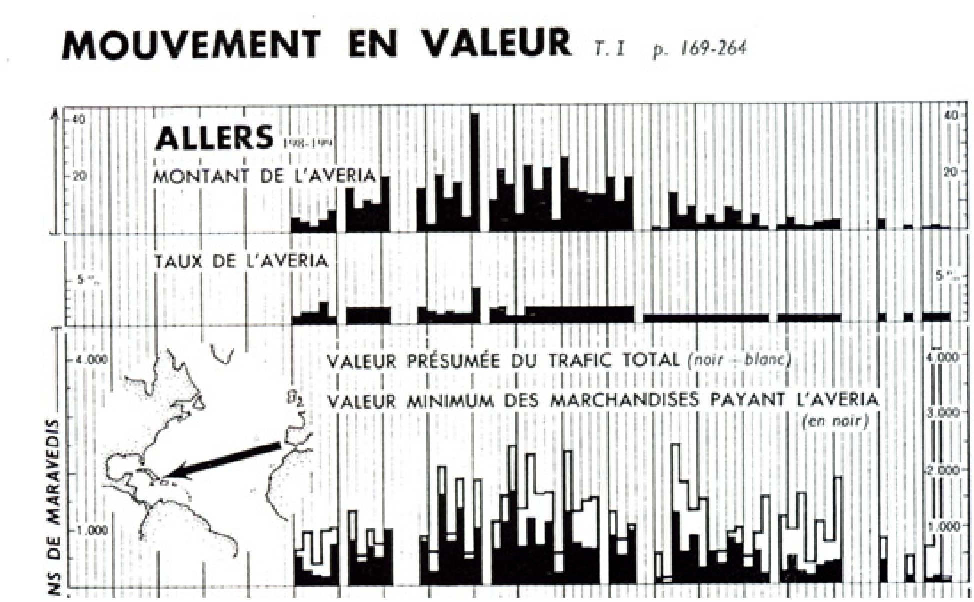
99 Chaunu (1964), 175.

100 Vilar (1965), 309.

101 "Ou bien [le travail] adopte le rhythme prudent des constructeurs d'indices d'activité, ou bien [il] remplit, coûte que coûte ses colonnes en recourant largement à la folle du logis. En agissant ainsi il contribue à masquer la réalité. Elle n'avance pas, elle recule", Chaunu (1964), 175.

had data. Yet, he wanted his reader to be aware of the ratio of estimated to found data for every year. According to Chaunu, this ratio was the measure of certainty.[102] Compare this to Marczewski and other retrospective accountants for whom the principal measure of certainty was the internal consistency of the data (i.e. cross checks), and not the proportion of "found" versus "made" data. Thus Chaunu and Marczweski had diametrically opposed ways of checking data validity: the more Chaunu "guessed", the least confident he felt; while the more Marcweski estimated, the more consistent his data set, hence the more confident!

Figure 5.9: Showing the difference between "found" and "estimated" data[103]



One may be tempted to interpret these diverging levels of trust in estimates to the possession or non-possession of models. In other words, one could ascribe Chaunu's caution and Marczweski's zeal to the fact that the latter had a systemic model of the economy, while the former did not. This was to some extent true, yet recall that neither Vilar nor Chaunu had criticized the general idea of a systemic model. For these

[102] "L'ecart entre les deux courbes fournit une presomption, quant a l'exactitude du chiffre global", Arbellot, Bertin and Chaunu (1957), 56.
[103] Ibid, 58.

scholars, models were fine in principle, as long as they were used to organize data that was found independently– as soon as they intervened to generate data, *Annalistes* seemed to raise their eyebrows.

5.2 Pre-war and post-war economists: questions of time

The reader may also be tempted to ascribe these different levels of tolderance and reliance on estimates (or guesses) as defining traits of members of different professions : historians are inherenly more scrupulous and source oriented than economists, who have always been more casual about exact values, as they are principally concerned with relations between variables or evolution through time ! Yet, such characterizations do not fit scholars like Simiand, who seemed to share many of *Annalistes*' views of quantification and may have treated Marczewski's work with much the same attitude. As Vilar reminded Marczewski in 1964 :

> "When Simiand established the strict rules for observation of prices and wages, his main contribution was to highlight the circumstances in which a number written on a document was an objective fact. He requested that we check, via rigorous inspection, not only the guarantee of authenticity, veracity etc., using the historian's habitual checks, but also that we make sure that it was not an opinion, an appreciation or any other subjective assessment. Numbers had to be the involuntary product of a sucession of decisions or actions (…) It is necesasry that the chosen quantitative expression objectively reflect a reality that does not depend on he who wrote it down, or he who read it . "[104]

[104] "Quand Simiand posa les strictes règles de l'observation des prix et des salaires, son principal mérite fut de montrer sous quelles conditions le chiffre, écrit noir sur blanc dans un document, etait une donnee objective. Il exigeait qu'on s'assurât, par une rigoureuse critique, non seulement des garanties d'authenticité, véracité etc… , par les contrôles habituels de l'historien, mais aussi qu'il ne s'agissait pas d'opinion, d'appréciation, toutes données

By citing Simiand, Vilar was defending a notion of "objectivity" – by which he meant standards for data collection - that differed from Marczewski's in so far as it relied on the critical examination of sources and the collection of numbers (as opposed to the creation of numbers, though averages and indexes built from micro-data were commonly used by *Annalistes* so the issue really was the quality of the inputs). It is certainly true that Chaunu and Vilar investigated topics that were inherently more "historical" than Simiand or the retrospective accountants (if only because they took place earlier than the 18th century), yet one should be cautious not to attribute their dispute to intrinsic differences between "historians" and "economists"- rather it would seem that their arguments overlapped with tensions within economics on the nature of "objective"data, on the issue of proper and reliable observation, or fact. In this conflict, the main issue was not quantification, but estimation.

French retrospective accountants and *Annales* scholars did not see eye to eye on the legitimacy, credibility and use of estimates. If one considers the implications of both positions, one can admit, as the British economic historian Patrick O'Brien recently did, that each side had a point.[105] Even from our contemporary vantage point (that tends to think that all facts are inherently "constructs", and hence never truly "found") there is a case to be made for the higher legitimacy of exhaustive measures (like total population counts), as opposed to estimates based on samples or models (like theories of population growth). However, there is also a case for developing new ways of establishing and checking estimates -Toutain's willingness to use sources that historians were very suspicious of, and his decision to produce rough estimates were just a first step in a sequence of critical evaluations that he, Marczewski and their colleagues

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subjectives. Le chiffre devait être la résultante involontaire d'un complexe de décisions ou d'actions (…) ce qu'il faut c'est que l'expression quantitative retenue traduise objectivement une réalité qui ne dépende pas ni de celui qui l'a inscrite, ni de celui qui la lit", Vilar (1965), 307.

105 Patrick O'Brien, Interviewed by Cristel de Rouvray, LSE, Fall 2003.

would subsequently refine. Indeed, in later publications they performed robustness analysis and reflected on the overall quality and consistency of the data. [106]

If both sides had a point, but proved incapable or unwilling to acknowledge their opponent's position, one could be tempted to ascribe this choice to pure strategic behavior: Labrousse and his students did not want economists treading on their territory. But such views, doubtless true to some extent, keep us from digging further. What could have made post-war economists like Marczewski and Toutain, more eager and willing to use rough estimates? Could it be a question of resources?

Let's recall from chapter 2 that scientific debates and their outcomes are often better understood when they are evaluated both on epistemological grounds and according to the social organization they entail. This may provide a clue about the origins of Chaunu and Marczewski's differing opinions. When we consider the organizational models implicit behind each man's empiricism we notice that" time" was an important feature of the debate. Both sides appeared to disagree about the amount of time it could and should take to obtain these numbers. Perroux's colleagues seemed to feel more pressure than *Annalistes* to deliver results within a relatively short period of time. As Chaunu stated:

> "Toutain succeeded in assembling a massive amount of evaluations that we had access to before him, but that no one had put together or organized before him. Where twenty historians aware of the difficulty of the task would have taken twenty years, Toutain, with very little means took three years." [107]

And Marczewski replied that he had to choose between:

[106] See for example Marczewski (1965).

[107] "Toutain a réussi le tour de force de rassembler sur la France du 18e une masse d' évaluations que nous connaissions, certes, avant lui, mais que personne n'avait réussi à rapprocher d'abord, à ordonner ensuite. Là ou vingt historiens soucieux de la difficulté auraient mis vingt ans, Toutain, avec des faibles moyens, a mis trois ans", Chaunu (1964), 172.

> "Limiting his work to sources validated by historians, even if this meant pushing publication of the first volume of France's quantitative history to the year 2000 [or accepting that] a rapid extension of historical knowledge had become absolutely indispensible for the progress of economics." [108]

In these comments one gets the distinct impression that Chaunu and Vilar did not feel pressed for time, while Marczewski et al. did. These diverging views on the opportunity cost of time may have originated from several factors. As mentioned earlier, by the early 1960s, *Annales* historians did have access to more publishing, research, and assistance support via the MSH. This may have served to reinforce a model of long-term research –10 year *Thèses d 'Etat* were still going strong in French history departments. Compared to this relatively lavish research environment, the ISEA was running on tighter funds: Kuznets had only given funding for three years.

Yet there may have been an element that went beyond financial resources, and was rather linked to the different places each group held, or aspired to hold in post WWII France. For the most part, *Annalistes* did not believe in straightfoward «lessons from history », whereas Marczweski clearly presented his work in light of a pressing need to understand growth for the sake of developing nations. Such statements may not have been a realistic description of the importance of his own work, but they may well have reflected the general disposition held by many rising economists – be they Anglo-Saxon accountants around Richard Stone and Simon Kuznets, or French accountants around Claude Gruson, Pierre Uri, Malinvaud. As we saw in chapter 4, it seemed that Kuznets had a much tighter research schedule than earlier NBER projects, certainly than Labrousse type projects. Thus the fact Kuznets had given them funding for only three years constrained Marczewski and his team's behavior, confronting them to

108 "Limiter aux sources confirmées par la recherche historique, quitte à remettre la publication du 1er volume de l'histoire quantitative de la France à l'an 2000 [mais] une extension rapide des connaissances historiques est devenue absolument indispensable aux progrès de la science économique", Marczewski Ibid, 178.

changes in time horizons that were occuring in a much broader playing field – international economics to aid public decision making.

Yet, in spite of Marczweski and the ISEA's nominal connection to Kuznets and National accounting, French national accountants did not come to their defense in the Vilar / Chaunu debates. Recall from chapter 4 that Malinvaud had been sollicited by Abramovitz in 1962 (just as the Kuznets / Abramovitz post-war growth project was picking up) to take on the French part of the study.[109] The fact that he did not feel the need to come to Marczewski's side was an indication of the tremendous self confidence of established mathematical economists in the 1960s.[110] It was also a reflection of the divide that seperated academic from governmental researchers in France (Malinvaud taught at the *VI e Section* but his main affiliation was L'INSEE).

6 Conclusion

Encounters between post-war French economists and historians proved to be blind men dialogues, so this chapter was not "influences" from one discipline to the other.[111] Rather we used these debates to emphasize the different views these groups had of empirical work, and invited the reader to look beyond issues of quantitative versus qualitative data, or theory versus no theory to see that battles on the legitimacy and reliability of different types of evidence principally revolved around the issue of estimation. Different scholars committed to empirical, historical work did not necessarily agree on the value of estimates, and their willingness to do so seemed to depend on their sense of time and urgency.

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[109] Figures 5.5 and 5.7 make note of his final product, published as Carré, Dubois and Malinvaud (1972). In a January 2004 Interview, Malinvaud confirmed that Abramovitz contacted him in the early 1960s – because they had met at Berkeley, when Malinvaud was visiting there in 1961.

[110] When asked about the Chaunu / Vilar versus Marcewski debate, Malinvaud did not recall having heard about it – though he did say that he had taken a look at Toutain and Markovitch's work and that he was rather suspicious of their data. Edmond Malinvaud, Interviewed by Cristel de Rouvray, Paris, January 2004.

[111] Actually, economists seem to have had very little influence on *Annales*, and the more fertile connections tended to be with demographers, anthropologists and geographers.

As an outcome of these debates, economist-historians appropriated most estimation devices and techniques, and those who held on to "found" data preferred to be called historians. Yet these different relationships to estimation were not intrinsic to either field – earlier economists, like Simiand, had developed standards for evaluating historical data that were not much different from those used and enforced by *Annales* historians. In other words, it may be useful to think of the frontier between *Annalistes* and Perroux's economists as one within social sciences, within economics, rather than one across different disciplines. The fact that *Annalistes* became entrenched, and were remembered as "historians" – when Braudel had done his best to institutionalize them as social scientists - was a result of such encounters, rather than its cause.

This French story also sheds light on events in the U.S. In chapter 4, we saw Kuznets spread his model for economist-history. In stark contrast, Marczewksi was not able to convert many scholars, and most economic historians did not accept his macro, systemic view of the past as the most scientific, objective way to study dynamic social phenomena. This reminds us of the contingency at play in epistemological debates. Kuznets had leveraged a feature of American academia that was simply not widespread in France: the conviction that the past of developed nations (in particular the U.S. and the U.K) held the keys to growth theory. In its stead one found a much more vibrant Marxist tradition that used to past to study social and class relations – where macro views were simply not as relevant.

CHAPTER 6.

THE GERSCHENKRON, CONRAD AND MEYER PARADOX

1. Introduction

The previous chapters have looked into multiple attempts to add a historical component to economic research. Yet, none has been defined as cliometrics. Even Simon Kuznets' work, in spite of his affiliation with numerous cliometricians (as depicted in Figure 4.3) and their willingness to cite him as one of their forefathers, did not fit this label.[1] Chapter 1 offered a broad definition of cliometrics as the art of "applying economic theory, models, measurement and inference techniques to historical questions". Implicit in this definition is a contrast with scholars who founded the EHA and the RCEntrepH, Kuznetsians and *Annalistes* for whom economic theory did not have much (or anything!) to bring to historical investigation (though they may have been measurers). Quite the contrary, their main mission seemed to have been to use historical evidence and analysis to change economics. As this narrative enters the 1960s and the official appearance of cliometrics on the American scene we are thus faced with the task of explaining how economist-historians came to think that economics was "good for" history, instead of the earlier prevailing belief that history was "good for" economics.[2]

[1] For examples of cliometricians citing Kuznets as a forefather see McCloskey (1978); Easterlin (1993).

[2] Though of course, positions were rarely this extreme - rather it was a question of balance. For example, Kuznets certainly used concepts from economic and accounting theory to frame his historical research, but overall, he expected historical perspective to

The anecdotal histories we deconstructed in chapter 2 may provide clues. Indeed among key people and events they cited Alexander Gerschrenkron and a 1957 meeting where two young Harvard economists presented a paper on the profitability of slavery in early 19th century U.S. Alfred Conrad and John Meyer argued that slavery was a profitable activity in all southern regions – either because of cotton farming or slave breeding.[3] The paper was remembered for having caused quite a stir in the economic history community and for having set an exemplar in the budding cliometric community. As Barbara Solow recalled, "if you had to name a single thing you would chose Conrad and Meyer (Robert Solow nods in agreement). That's the *fons et origo* of cliometrics".[4]

Conrad and Meyer were Gerschenkron's students and close acolytes. Gerschenkron had played a key role in organizing the 1957 meeting where they presented their slavery paper (and another methodological paper). In the late 1950s he was actively soliciting philanthropic foundations to sponsor economic history at Harvard, and more generally in the U.S. This is evidence enough to warrant further investigation into his "influence" on American economist-history – a phenomenon that may seem paradoxical to any reader familiar with both his writings and subsequent contributions from cliometricians. One would be hard pressed to describe Gerschenkron's work as "cliometric". He did not use econometric techniques to test hypotheses, nor did he develop economic models and apply them to specific historical situations. In so far as he used any of the economist's tools, they tended to be relatively old adages (for example, that men follow their own economic interest) or non-probabilistic statistical techniques (for example, index numbers, not unlike the 1930s price historians described in chapter 3). Generally, his writings were more on the "history is good for

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provide better theories. Compare this to Conrad and Meyer for whom capital theory helped reframe and solve a specific historical question.

[3] Conrad and Meyer (1958).

[4] Robert and Barbara Solow (2004), Interviewed by Cristel de Rouvray, Boston, June 2004.

economics" side of the spectrum. Thus we may want to know more about the relationship between Conrad, Meyer and Gerschenkron. What happened at the 1957 meeting? Did the slavery paper trigger the cliometric movement? What role did Gerschenkron actually play in the advent of the cliometric revolution?

This chapter begins by exploring Gerschenkron's path to economic history, his first years at Harvard and his adversarial relationship with other Harvard economic historians (section 2). Section 3 describes the legendary 1957 Williamstown meeting and Conrad and Meyer's performance. Section 4 examines the multifaceted economist-history landscape of the late 1950s, the rivalries that existed among different "reformist" minded groups and the crucial role the Ford Foundation played in choosing among these claims. The last section (section 5) explores Gerschenkron's Harvard Workshop and suggests reasons why his agenda proved to be more powerful than his competitors'. Throughout, the chapter argues that, like Kuznets before him, Gerschenkron was able to leverage the relatively new and extremely profitable connection between economic history and development economics to push his agenda and marginalize competing ones. To do so, he brought into economic history a new generation of economists, effectively changing the relationship between economics and economic history and moving the field in a direction he had not entirely foreseen. Yet, the triggering point was not Conrad and Meyer's 1957 performance but rather the critical mass of young economist-historians that Gerschenkron brought into the field thanks to his post 1959 workshop.

2. Gerschenkron and Economic History

2.1. Alexander Gerschenkron's path to economic history

It is nearly impossible to read any biographical essay on Alexander Gerschenkron or the recent book authored by his grandson without being thoroughly impressed by both his exceptional breadth of knowledge and the mix of turmoil, luck and adversity that was his life.[5] Born in Ukraine, in 1904, Gerschenkron lived a quiet, comfortable childhood. His adolescence was considerably less idyllic, bathed in the increasing political unrest of the Bolshevik revolution, culminating with a two-year civil war on the streets of Odessa (from 1918 to 1920). In 1920, when earning a profit was made illegal, Gerschenkron's father, a successful businessman, chose exile. He fled with his family to start anew in Vienna. Gerschenkron thus spent most of his education in the strict Austrian schools and University. He had to teach himself German, but soon excelled – in particular in humanities and Slavic languages, which he had first intended to pursue as a career.

Upon entering University Gerschenkron changed his mind and chose to study economics and political science. He earned his doctorate in "rerum politicarum" in 1928 and held several jobs – including manager of a motorcycle factory and associate at the Austrian institute of business cycle study - before his forced exile in 1938. Gerschenkron's father had converted from Judaism in the late 19th century, and though his son was not a practicing Jew and had married a Christian woman this was sufficient to make the family situation perilous in post-Anschluss Austria. They moved to England, and then to the U.S., where Gerschenkron took a job as research assistant for a professor of Economics at Berkeley (Charles Gulick, who was writing a book on Austria). He spent six years in California before being called to Washington D.C. to work for the Federal Reserve Board, as a Soviet expert.

In 1948 he was offered an appointment by the Harvard Economics Department to teach both economic history and Soviet studies. According to Dawidoff, this double expertise was crucial for getting Gerschenkron the job at Harvard. He was not their first choice as an economic historian (apparently they had first asked Rostow to replace A. P. Usher but he had declined), but his two

[5] Erlich (1979); Rosovsky (1979); Fishlow (1987); McCloskey (1992). Gerschenkron's grandson's recollections were published as Dawidoff (2002).

"hats" made it easier for the slightly reluctant faculty to take him in. Indeed Gerschenkron did not have the most extensive record as an economic historian, having published only one book, *Bread and Democracy in Germany* (1943), where he attacked the landed class (Junkers) for exploiting the rest of the German population and the consequent failure of democracy.[6] The book was to a large extent historical (examining the origins and evolution of the relationship between junkers and state) but clearly aimed at understanding a present concern (fascism in Germany). Like Willits and Bezanson, Gerschenkron's path to economic history was born in the presentist concern of a failing world, though his worries had been much more concretely experienced during his last years in Vienna.

While *Bread and Democracy* had earned him the respect of many peers, it was not habitual for Harvard faculty to invite scholars with such a thin publications record to join their ranks. Yet, they seemed committed to re-staffing the economic history slot – which had now become a pillar of undergraduate and graduate education - so they hired Gerschenkron.[7] With the perspective we have gained from chapter 3, their choice was even more puzzling. Recall that Harvard had been the breading ground for a relatively large group of economist-historians who had earned their Ph.Ds with Edwin Gay in the late 1920s. Among them at least three of them - Earl Hamilton (Duke University), Harold Williamson (Northwestern) and John Nef (Chicago) - were now tenured professors, not yet 50 years old. Could Harvard not have enticed one of them to move? Perhaps they were looking for a younger scholar, so why did they not turn to the CREH (now operating for 7 years) and hire one of the young

[6] Gerschenkron (1943).

[7] Recall from chapter 3 that this was not the first time Harvard economists had trouble finding their economic historian – even though they were now embarking on their 5th decade of economic history and it had definitely become part of the Harvard economics culture. John Meyer, who was a graduate student in the early 50s recalls that history was very much a part of an economist's education. He described the department as having a three legged stool philosophy: theory, statistics and history - Meyer (1995).

economic historians they were financing? By 1948 , the RCEntrepH was just getting off the ground – could Cole not have found Usher's replacement? Considering that all these initiatives were somehow tied to Harvard, it is surprising that Usher was not replaced with someone closer to this clique. Gerschenkron was certainly no stranger to the founders of the EHA. Early records of the EHA suggest that he started attending the annual meetings relatively soon after his move to the East coast, and that he quickly became a prominent member of the association.[8] Yet, as we shall see in this chapter, this was not evidence that he fit in well with other notable American economic historians.

As time went by, Gerschenkron steadily strengthened his reputation as a phenomenal scholar. His early years at Harvard were spent revamping the Soviet Studies interdisciplinary center. This effort fit into his status of foremost Soviet expert, his major contribution being studies of Soviet industrial output commissioned by the RAND corporation in the early 1950s and summarized in his 1955 article for the *Review of Economics and Statistics*.[9] Yet, after 1956 Gerschenkron stopped teaching Soviet economics and ended his formal involvement with this area of study. Part of the reason for this withdrawal was a weak heart that led him to cut back on his workload (he suffered a relatively severe heart attack in the mid 1950s). Yet, an apparently greater reason was his determination to change economic history at Harvard (and by extension in the U.S.): a determination that seemed to grow stronger with every obstacle that was put in his way.

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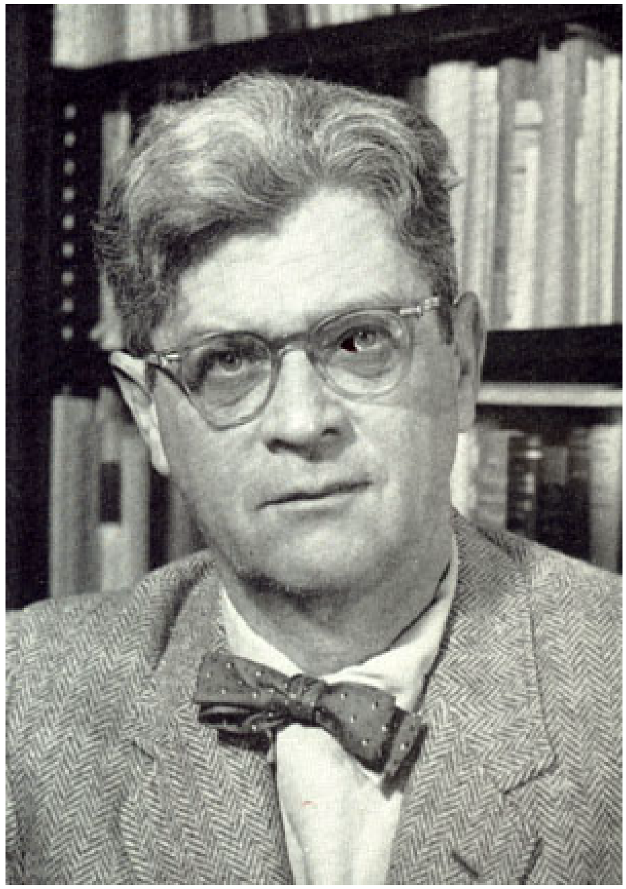
[8] For example, there is a 1947 picture of him with Louis Hunter, Usher, Bezanson and a senator from Vermont at the annual EHA meetings, published in the *New Haven Sunday Register* (Volume CV, number 253) Sunday Sept 14th 1947, 2 - Hagley, Accession 1479, Folder 74.
[9] Gerschenkron (1955).

Figure 6.1: Alexander Gerschenkron (1904-1978)[10]



2.2. Gerschenkron disagrees with the Harvard economic history establishment (1948-1957)

Gerschenkron's first years at Harvard were tumultuous. It does not seem that he was well integrated in the Economics department, and he maintained a rather aggressive relationship with Harvard economic historians affiliated to Arthur Cole's RCEntrepH. It seemed that much of this friction stemmed from the Harvard Economics Faculty's efforts to keep Gerschenkron at bay, while they paved the way for their favored resident economic historian - the junior scholar John Sawyer. Their hope was to give him enough time and freedom to produce

10 From Rosovsky, Ed. (1966).

the publications that would enable his promotion to Associate Professor. They also wanted Rockefeller Foundation (RF) funding, which meant keeping alive the RCEntrepH, which they hoped Sawyer would take over from Cole, when the latter retired. This relatively complex strategy was revealed to RF director Joseph Willits in 1952, during a series of conversations with Arthur Cole, Arthur Smithies (chair of the Harvard Economics Department) and Paul Buck (Provost). The latter justified their choices in the following way: "Sawyer is the man we have been building up to succeed Cole [at RCEntrepH] and carry on. He has personal, intellectual and administrative qualities that Gerschenkron lacks".[11] Smithies elaborated by saying that he had:

> "problems with Gerschenkron, who is a continental European with a narrow idea of economic history" and "talked highly of Sawyer and said he has his associate professorship to make (…) and he was the one who should succeed Cole. He said that they would want the funds for economic history granted, they did want economic history to develop, and that they did not want Gerschenkron in charge but hoped that Sawyer might take over in three years when Cole retires."[12]

This quote suggested that the Harvard economists' dislike for Gerschenkron stemmed from many sources, not least his fiery personality, very well documented in Dawidoff's biography. However there seem to have been two additional factors that truly constrained their relationship with him. The first was what they called "Gerschenkron's narrow view of economic history"; the second was the RF's background presence and the faculty's efforts to hold on to RF funding.

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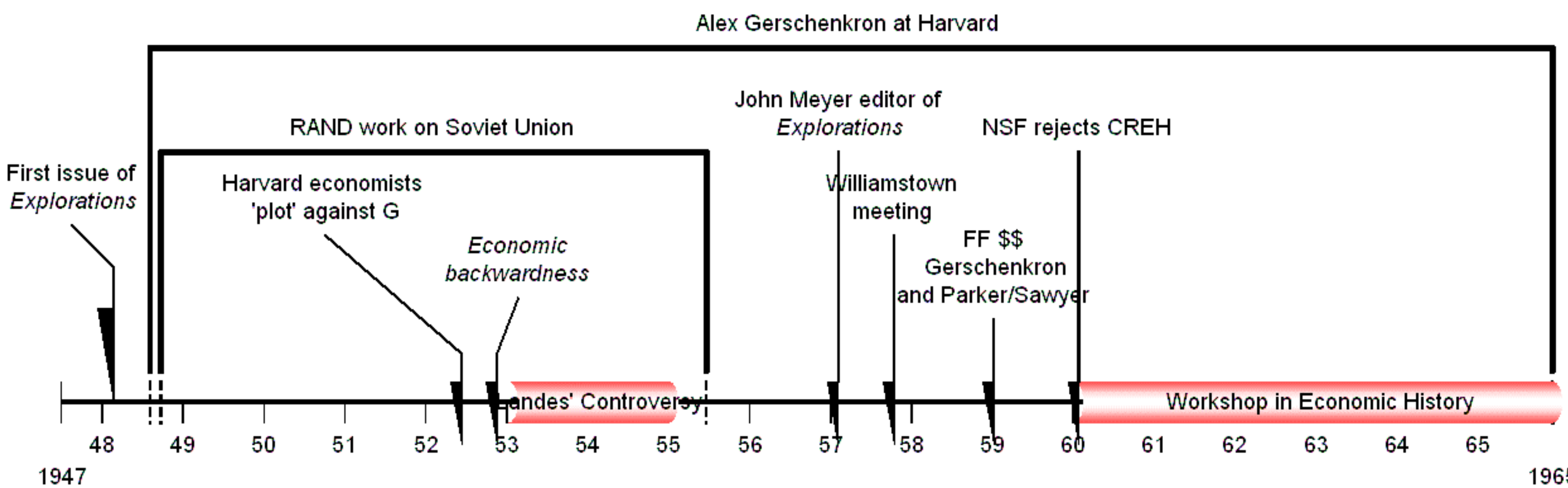
[11] Friday April 11th 1952, meeting with Paul Buck. RAC-RF, RG 12.1, Diaries, Box 70, Joseph Willits, 5 volumes.

[12] Friday April 11th 1952, meeting with Arthur Smithies. RAC-RF, RG 12.1, Diaries, Box 70, Joseph Willits, 5 volumes.

Figure 6.2: Time line of events in sections 2, 3 and 4



As seen in Figure 6.2 Smithies' remark was made only 4 years after Gerschenkron had joined the Harvard faculty. 1952 was an important year for Gerschenkron, not only because Harvard faculty were plotting behind his back! That same year, he had published the first version of *Economic Backwardness in Historical Perspective* – a contribution that would subsequently shape his legacy to economic history.[13] In this essay he argued against the Marxist view that all societies followed an economic development template, that "the industrially more developed country present[ed] to the less developed country a picture of the latter's future".[14] Instead he urged economic historians to consider the fact that "backward" countries had access to the more advanced nations' experience and know-how – an advantage that had changed (and could continue to alter) the pace and path of their economic development. According to Gerschenkron the backward countries could use (and had used) this social and technological knowledge to proceed faster and avoid the pioneering countries' mistakes. After formulating this general hypothesis, he proceeded to validate it with numerous historical illustrations from European economic history - from 16th century German mining engineers to 19th century British textiles or Russian laborers.

Already in 1952, Gerschenkron sensed the importance of this contribution. As Dawidoff reminded us:

> "Gerschenkron saw his historical synthesis of industrial development as the great proof of his scholarly supremacy. Nobody else had such an approach to economic history. Intellectually it made [him] his own man. He was not a

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[13] Published in Gerschenkron (1952).

[14] Karl Marx, *Das Kapital* (1st ed.), Preface; cited in Gerschenkron (1962), 6. Notice that the Marxist view of a stage based economic development would be retained by Rostow, whose 1960 work substituted virtuous capitalist stages to Marxist more pessimistic ones (at least insofar as Marx envisaged necessary squalor and revolution before happier communist times). In other words, Gerschenkron's views would have also been hard to reconcile with Rostow's stage theory.

> Marxist or a Keynesian, nor was he obviously indebted to John Stuart Mill or Max Weber. He was the advantages of backwardness [sic]."[15]

Strengthened by the elegance of this hypothesis, Gerschenkron boldly questioned the merits of the approach chosen by scholars at the RCEntrepH, in a 1953 article published in the Center's main organ: *Explorations in Entrepreneurial History*.[16] What subsequently became known as the Gerschenkron-Landes controversy started with Gerschenkron's deconstruction of the sociological role-theory most economic historians related to the RCEntrepH had been using in their recent work. They had argued that much of a nation's entrepreneurial activity was a function of the rewards (monetary and other) granted to such endeavors in different cultures, and that these different value judgments towards commercial risk taking and money making were at the heart of differential wealth, development and growth patterns.

Gerschenkron's critiques were not only addressed to David Landes (who had famously argued that French entrepreneurship had failed in the 19th century for cultural reasons, as the high echelons of society looked down upon those who made a profit), but also to Arthur Cole, Leland Jenks, Thomas Cochran and John Sawyer.[17] While Gerschenkron did not discard the hypothesis that entrepreneurial behavior varied from country to country, and that this variation could explain some degree of economic backwardness, he wondered whether it accounted for most (or even a lot) of this difference. More critically, he questioned whether sociological theories were the best way to account for the variation in entrepreneurial activity. Thus, instead of relying on the expectations of entrepreneurs, as they were enforced or undermined by the surrounding values of their peers, he suggested that the analyst use plain old economic

[15] Dawidoff (2002), 186.
[16] Gerschenkron (1953).
[17] Landes (1949).

insight. What if entrepreneurial activity was best understood by looking at means and opportunities such as individuals' response to the relative price of land, labor and capital:

> "The very simple assumptions of economic interest and profit maximization which underlie the usual reasoning in economic theory would seem perfectly sufficient to understand the attitudes in question, and references to role expectations appear to be hardly more than a gratuitous adornment." [18]

Overall, one got the sense that Gerschenkron thought that the RCEntrepH was not pursuing a particularly important agenda. Cochran, Sawyer and Landes responded to Gerschenkron's doubts.[19] Sawyer's reply was the most scathing, as it reprimanded Gerschenkron for having presented this paper at a meeting of the International Economics Association (in the summer of 1953), thus allegedly harming the Center's international reputation.[20] Gerschenkron was bitter in his reply to Sawyer, testimony of the great animosity that now existed between the men, evidence that Cole and Smithies' antipathy went both ways.[21]

Landes' objections were more substantial.[22] He held on to the idea that different types of entrepreneurial behavior, or spirit, accounted for much

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[18] Gerschenkron (1954), 111.
[19] Cochran (1953); Landes (1954); Sawyer (1954).
[20] "I cannot concur in language that at times reflects adversely upon the character of the work that has gone on at the Center", Sawyer (1954), 273. Gerschenkron had indeed presented this paper at the international conference in Europe – according to him he had been invited to do so by Professor Léon Dupriez of Louvain. He claimed that the published paper contained only slight modifications from his oral presentation.
[21] Note that by winter 1953 Sawyer had taken a job at Yale. Dawidoff mentioned that Gerschenkron "not so privately regarded Sawyer as an intellectual lightweight", Dawidoff (2002), 184.
[22] When Landes wrote his reply, he was at Columbia, where he had moved in 1953. He had first met Gerschenkron in 1949, while finishing up his thesis at the RCEntrepH as a student affiliated to the Harvard History Department. Landes was one of the few who

variation in economic development. Arguing much as Gerschenkron did (via the synthesis of numerous historical cases) he cited, for example, “many instances when, given comparable endowments, one country invented, stole patents much more aggressively than another”. Such examples enabled him to assert the importance of “business attitudes” and “socially conditioned entrepreneurial behavior”. According to Landes, this was justification enough to encourage further work by scholars at the RCEntrepH. [23]

One might be tempted to ascribe much of this disagreement to different ideological backgrounds. Chapter 3 emphasized the relationship between entrepreneurial themes and the RF, suggesting that the popularity of such an approach to economic history was in part the result of Bezanson and Willits’ need to please the RF Board of Directors. Given this connection, it was not entirely surprising that Gerschenkron would have regarded the RCEntrepH as “a building that was only slightly less reprehensible than a Republican social club”.[24] This was an unfair assessment, as very few scholars at the Center engaged in hagiographical work of particular entrepreneurs. Following Jenks (who was greatly inspired by the sociologist Talcott Parsons), most of them were interested in social attitudes to business activity and the way in which these values could condition roles in society.[25] Gerschenkron, on the other hand, seemed to prefer explanations of human behavior that relied principally on rational behavior and action triggered by economic opportunity.

In addition, Gerschenkron combined a profit-maximizing picture of the individual with a macro-economic view of development. According to him,

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could keep up with Gerschenkron’s tri-lingual mastery of economic and literary sources – he cited German and French with much ease.

[23] Landes (1954)

[24] Dawidoff (2002), 184.

[25] Talcott Parsons was a professor of Sociology at Harvard since 1927. Born in the U.S. he had studied at the LSE with Malinowski, and in Heidelberg with followers of Max Weber. In the early 1950s, he was systemizing his thought about individual action, and its relationship to representation (i.e how people see their world and act accordingly). See for example Parsons and Shils (1951).

economic change was the consequence of systemic changes – sometimes spurred by state intervention and legislation - not the initiative of a few individuals. Note the parallel between Kuznets' and Gerschenkron's "ideology". Both men emphasized the State and were quite suspicious of Schumpeter's work. As Dawidoff writes: "he thought that Schumpeter had vastly overstated the impact individuals could have on development".[26] In addition there probably was a cultural rift separating Gerschenkron from the scholars at the Center (all of whom, except for Fritz Redlich, were born American) – remember Provost Buck's comment on Gerschenkron's "central Europeanness". Gerschenkron's part Jewish heritage may also have added a layer of difference between him and other Harvard economic historians.

Yet, differences in culture, religion and ideology were perhaps only fodder to a much more tangible set of differences: namely very different agendas for economic history and ones that were not compatible for both theoretical and organizational reasons. The "advantages of backwardness" might have constituted what the Harvard economists had called a "narrow" view of economic history, but it was actually a relatively powerful framework for organizing and directing research in the field; and one that competed with theirs for resources. Both Gerschenkron and the RCEntrepH had plans for relatively resource heavy research programs: the investigation and validation of their respective hypotheses required laborious empirical work – the accumulation of numerous monographs, case studies of business attitudes in 19th century France, or catch-up policies in 19th century Russia and Japan, and of course studies of American experience – either in terms of entrepreneurial abilities, or in terms of technological catch-up with Britain. Each group needed economic historians to collectively focus on their particular hypothesis, otherwise it would be difficult to make any progress in assessing its merits and validity. In other words, there

[26] Dawidoff (2002), 184.

were simply not enough economic historians, and not enough resources to pursue both agendas at Harvard, and possibly not in North America!

These resource requirements, and consequent rivalry between the two research programs could be gleaned in both Gerschenkron and Landes' comments. As Gerschenkron claimed, their disagreement lay in different attitudes about where the economic historian should start: "How shall we unravel the skein in our attempts to explain the processes of change? Where shall we start and what factors shall we select?"[27] His position could be summarized as objecting to the fact that so many economic historians started with the private entrepreneur and with social attitudes to business. He preferred a discussion that started with investigations of relative degree of backwardness, or at least considered macro-economic features. This was no small matter, as economic historians who wanted to explore either hypothesis would have to devote tremendous resources to test these claims empirically. The conflict boiled down to who could convince the greater number of economic historians to test and further their hypothesis. In 1952-54, when the controversy raged, the RCEntrepH was in the advantageous position. By the end of the 1950s, the tables had turned in Gerschenkron's favor. The next section explores the reasons for this change in fortune.

3. The 1957 Williamstown meeting

3.1. Conrad and Meyer's performance at the Williamstown meeting

Gerschenkron must have been quite aware of his relatively weak position at Harvard, and he strove to find support elsewhere. One of his natural allies was Simon Kuznets, whose vision of the relationship between economics and economic history was certainly closer to Gerschenkron's than to the

[27] Gerschenkron (1954).

RCEntrepH.[28] In 1957, they co-organized a meeting to push forward their vision for the nexus between economics and economic history. Gerschenkron had been appointed by the EHA as the main coordinator for the 1957 annual meetings, which he began organizing in the fall of 1956.[29] Instead of the habitual two day EHA annual affair, Gerschenkron was able to put together a three-day joint meeting with Kuznets' Conference on Research in Income and Wealth. This Conference had been established by the NBER in the 1930s and was an annual project aimed at coordinating measures of American income and wealth – it was the ancestor, and to some extent the model for the IARIW described in chapter 4. The 1957 meeting was principally aimed at regrouping scholars working on measurements of the 19th century American economy in a national income accounts framework. For example, session titles included "U.S. and Canadian Income and Investment" or "Meaning of economic growth as measured by secular estimates of national income and product". [30] The meetings were held at Williamstown, Massachusetts, in September 1957. They were attended by members of both the EHA and the Conference: nearly 150 participants (approximately 70 were affiliated to the EHA).

One of the striking features of this three day conference was the roster of participants, and the high concentration of individuals who were later credited for helping or effecting the cliometric revolution: Kuznets, Gerschenkron, Conrad, Meyer, North, William Parker, Anna J. Schwarz, Harold Williamson, Abramovitz, Easterlin, Stanley Lebergott and Goran Ohlin. There seems to have been some contact between Gerschenkron' s students and colleagues (Ohlin, Meyer and Conrad) and Kuznets' crowd (North, Schwarz, Abramovitz, Easterlin,

[28] Remember his unfavorable assessment of the work and vision of the Entrepreneur scholars described in chapters 3 and 4.

[29] For evidence that Gerschenkron was in control of much of the agenda setting for these meetings see documents in Harvard University Archives, HUG FP45.12, Folder: "Economic History Convention, 1957" and Hagley, Accession 1479, Folder 64.

[30] Preliminary Program of conference. Hagley, Accession 1479, Folder 64.

Gallman).[31] Both Easterlin and Gallman, for example, recalled attending the NBER session where Conrad and Meyer presented their slavery paper.[32]

Officially, there was only one overlapping session, called "the integration of economic theory and economic history", a methodological daylong session that Gerschenkron had insisted on holding.[33] The session was chaired by Simon Kuznets and attended by an impressive panel of discussants, including Hamilton, Ohlin, North, Smithies, Evsy Domar (an MIT growth economist) and Martin Bronfenbrenner (a notable "generalist" who taught at Wisconsin but was in constant conflict with most of his "institutionalist" colleagues there).[34] Only two papers were scheduled for discussion; the first was Rostow's paper on "The Interrelation of Theory and Economic History".[35] The second was Conrad and Meyer's paper on "Economic Theory, Statistical Inference and Economic History".[36] The bulk of the discussion revolved around methodological themes, in line with Gerschenkron's intention and his injunction that much of economic history could be reconsidered in light of basic economic principles.

In their paper, Conrad and Meyer went significantly beyond basic economic principles. They began by arguing for a probabilistic approach to causality in history. Shrinking away from the extreme positions of total randomness (they cited Benedetto Croce), or total determination (they cited Marx, or rather his interpreters) they proposed that economic historians rank statements about the past according to their probability of being true.[37] In arguing for a midway position, they made extensive reference to historians and

[31] For depictions of the links between Kuznets and his students see Figure 4.3; for Gerschenkron and his students see Figure 6.3.
[32] Gallman (1992); Easterlin (1993).
[33] Letter from Gerschenkron to Jerry Blum, History Department, Princeton, November 21st 1956. Harvard University Archives, HUG FP45.12, Folder: "Economic History Convention, 1957".
[34] For biographical information on Bronfenbrenner, see Goodwin (1998).
[35] Rostow (1957).
[36] Notice that this Conrad and Meyer paper was not the one frequently cited as the founding stone of cliometrics.
[37] Conrad and Meyer (1957), 542.

philosophers of science (from Leopold von Ranke to Karl Popper), testimony that they were not ignorant of the difficult debates about causality in history; and perhaps indication of Gerschenkron's ability to stimulate philosophical reflection among his students. Conrad and Meyer had both been graduate students at Harvard in the early 1950s. Conrad was one of Schumpeter's last students – and had finished his thesis with Leontief. Meyer had written on business investment decisions and developed an early focus on econometrics, industrial organization and corporate finance.[38] By the mid 1950s they had both begun teaching economic history. Meyer had stayed at Harvard, having done some graduate work with Gerschenkron, and been asked to take over John Sawyer's lectures on American economic history, as the latter had just left for Yale. [39] Conrad had gone to Northwestern where he taught American economic history for Harold Williamson. According to Meyer, their similar duties and Gerschenkron's stature had prompted lunchtime conversations around economic history and stimulated them to write the two Williamstown papers.

Just as the roster of philosophical allusions denoted their affiliation to Gerschenkron's erudite mind, the second part of their methodological paper was indication of their own interests. The authors broke away from their survey of an ongoing debate in the philosophy of history, and introduced very recent material from econometrics. They cited Herbert Simon and his now famous "causal ordering" condition: if one could show that changes in one variable triggered changes in another, and that the relationship was not reversed, then one had good reasons to believe that the first variable caused the second. This was subject to the effect of additional "random" interferences (the error term in a regression model), and the requirement that one exhaustively spell out the relationship between all relevant variables in the system.[40] They cited Simon's rain, wheat

[38] Meyer (1995).
[39] Meyer's 1955 paper applying Input-Output analysis to 19th century Britain had been written as a term paper in Gerschenkron's graduate course; Meyer (1955).
[40] Conrad and Meyer (1957), 528.

and price example: changes in precipitation levels could affect both crop yields and wheat prices; just as crop yields could affect wheat prices; but changes in prices could not affect rain, and could only affect yields if new relations were introduced into the system (like farmer incentives, use of fertilizers for eg.) Using Simon, they argued that he had found a convincing way to establish causality and circumvent the obstacle faced by all social sciences (and history), namely the lack of experimentation. Provided one could find enough variation in the presumed causal factor, and provided one had a hypothesis (driven by a theory, or generalization); econometric techniques could be used to make probabilistic statements about this hypothesis and increase our belief in a given historical interpretation.

They then cited their own work on the profitability of slavery, and the related hypothesis that slave breeding had occurred in southern states where land was not fertile. Having said that traditional historical analysis could not put an end to the debate about whether or not certain owners bred slaves (because diary entries could be interpreted as the truth or bad faith, not as reliable evidence – recall the disagreement between Kuznets and Landes), they claimed to be able to test this allegation on demographic data of slave population in different southern states: if the demographics differed greatly from one region to the next (i.e. if the plantation states had many more young males while the non-plantation states had many more very young and very old people) then there was good reason to believe that slave breeding was occurring.

Implicit in this test was the general proposition that men acted according to their economic interest, and that so-called genteel norms against slave breeding had little real impact on actual economic activity. This proposition tied back to Gerschenkron's point that behavior should be explained in terms of economic interest, not cultural pressures. Conrad and Meyer found that there were different demographic patterns, but they only showed this by reporting average numbers, rather than exploiting the probabilistic reasoning they expounded in the methodological paper. Generally, there was actually very little connection between their methodological paper and the supposedly illustrative study on slavery. The slavery paper was not built around an econometric test, or even a probabilistic argument. Instead, it proceeded by estimating (in a very rough and not always convincing way – resembling “order of magnitude” exercises we will encounter in chapter 7) the average price of a male field hand, the average cost of land per slave and the average cost of working capital and maintenance per slave – then plugged these numbers into a yield equation to establish the average rate of return on owning a slave. They then compared this rate to interest rates on capital markets and concluded that they were similar enough to justify the belief that slavery had indeed been profitable. Their argument was not probabilistic (in the econometric sense) as the “similar enough” verdict was not established in statistical, or significance terms, but by simply comparing averages (with no discussion of variance).

Actually, the only consistent line through both papers was a certain disregard for the current state of scholarship in economic history. In the methodological paper, this came out as a rather open dismissal of “studies conducted under the heading of entrepreneurial research“, and the authors accused these un-named culprits of focusing on supposedly exceptional events and individuals.[41] Considering Conrad and Meyer’s connection to Harvard, this was clearly a jibe at the RCEntrepH. In 1957 Meyer served as editor of the Center’s *Explorations in Economic History* (Figure 6.2). He had used this opportunity to publish several articles that not only completely avoided the topic of entrepreneurship but also used relatively complex economic theory to test a historical hypothesis. For example, Michael Lovell used interest rate theory to

[41] Conrad and Meyer (1957), 523. This verdict was unfair. As seen in the previous section, historians at the RCEntrepH were quite willing to acknowledge regularities in human behavior, though these were contingent on the particular social atmosphere of the time (i.e. norms and customs mattered). Notice the parallel between this critique and the 1900s and 1920s accusations Simiand, then Bloch and Febvre made against traditional historians.

establish that the Bank of England has acted as a lender of last resort in the 18th century, much earlier than most historians of Central Banking had acknowledged. William Parker analyzed steel output movements in various regions of Western Europe. Lance Davis used corporate finance theory to analyze the sources of industrial finance of 19th century American textile industry.[42] The great majority of articles published in the last two issues of Volume IX and the first issue of Volume X of *Explorations* – coinciding with Meyer's short editorship - were highly quantitative and made explicit reference to contemporary theoretical literature in economics. However, once the editor had changed (Barry Supple for a year, then Jane Jack in 1958), *Explorations* reverted to articles about entrepreneurship.

In other words, both Conrad and Meyer were well aware of the different strands of economic history at Harvard and in the book length version of their slavery studies, they thanked Gerschenkron as their main inspiration.[43] Overall, they seemed to be arguing for an economist-history along his lines, or what they perceived his line to be. Yet Gerschenkron did not feel that the meeting had achieved what he set out to do, and most other participants recalled that while the slavery paper was intriguing, Conrad and Meyer had perhaps been slightly too "cocky" to be taken very seriously.[44] In any case, there was no "revolution" in American economic history in 1957.

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[42] Davis (1957); Lovell (1957); Parker (1957).

[43] They mention that "their greatest debt is to Professor Alexander Gerschenkron, our teacher and critic" - Conrad and Meyer (1964), viii.

[44] Gallman recalled how cocky they had seemed and did not remember a tremendous amount of controversy around the slavery paper - Gallman (1992). While he was not opposed to their work, he did not find it very inspiring. Easterlin recalled being unimpressed, and having shared these feelings with William Parker - Easterlin (1993).

3.2. Gerschenkron evaluates the Williamstown Meeting

When Gerschenkron reviewed his students' performance – in a September 1957 exchange of letters with Kuznets who was writing a summary report of the session – he made clear that he was "on their side", but that he wished they had done a better sales job. In particular, he regretted that they had spent much time discussing econometric techniques (in particular the error term) thus generating confusion about the truly useful way theory could be applied to history:

> "putting that one single equation on the board including the controversial "e" term was less than perfectly felicitous, to say the least. That, of course, is not how an historian who is willing to apply theoretical models will proceed. What he actually will do is to construct a given model, try to apply it to his observed phenomena and see how well it fits. He will then proceed to construct and to apply other models and to observe the goodness of the resulting fit and somehow, with every additional model he will get some better idea as to what is left outside his models. And that balance in some cases may be capable of some systematization and in most cases will not be."[45]

For Gerschenkron, economic theory was an aid to historical investigation, and conversely, historical investigation could refine models (history was good for economics). Compared to earlier groups of economic historians discussed in this thesis, he seemed much closer to the *Annales* – someone like Braudel who was happy to use a model (like national accounting) to organize and make sense of evidence, rather than to generate evidence, or remain within the necessarily restricted bounds of a given representation of reality. This was not the way Conrad and Meyer had used the yield equation in their slavery paper. They had

[45] Letter from Gerschenkron to Kuznets, September 23rd 1957, Harvard University Archives, HUG FP45.12, Folder: "Economic History Convention, 1957".

indeed started with the equation and applied it to 1800-1860 southern agriculture, but discussions of the slave market and competing uses of capital had certainly not opened the door to any discussion on the pertinence or universal application of the yield equation. Their paper was a much more straightforward application of a model to a historical question; and only towards the conclusion did they start spelling out "lessons" from history, such as the economic viability of slave, or near slave labor in contemporary under-developed nations, and the consequent necessity for political action. Yet such lessons were for policy, not for economics.

Generally, Gerschenkron regretted that his message had not come across at the Williamstown meeting, and he told Kuznets that he had found most EHA's members' performance "ludicrous".[46] Yet, he seemed to identify one good thing from the session:

> "Economic history is in a poor way. It is unable to attract good students, mainly because the discipline does not present any intellectual challenge (…) This is a deplorable situation and I feel it is at this point that all this talk about growth and economic development comes in. I think Rostow was perfectly right in stressing it. Economic development may be a fad and a fashion, but (…) it gives economic history a great chance to snap out of rut and stagnation and to receive new and fruitful intellectual impulses. The question therefore is not whether all work in economic history should be limited to problems of growth and development, but it is really the problem of whether economic history will, or will not, miss a tremendous opportunity to rejuvenate the discipline by addressing themselves in the past to the grave economic problems that are now the general concern to those interested in economic development (…)

[46] Letter from Gerschenkron to Kuznets, September 23rd 1957, Harvard University Archives, HUG FP45.12, Folder: "Economic History Convention, 1957".

> On the whole the meeting greatly reinforced my feeling that what is needed is to re-inject into the profession a number of first rate young economists who have developed an abiding interest in economic history. With eight to ten men placed in good universities, the complexion of the discipline would quickly change. This is not a difficult thing to do. We did that to the field of Soviet economics with a surprising degree of success. "[47]

Considering Kuznets' own view that economic history (i.e. the observation of phenomena over the long run) was the only source of information for reliable statements about economic development, Gerchenkron's call to seize the "development" opportunity must have sounded very familiar. However, there was a distinctly strategic element to Gerschenkron's vision that separated him from Kuznets: Kuznets was doing economic history almost incidentally, as a result of wanting to examine the growth of nations. Thus he was never interested in reforming or rejuvenating economic history *per se*. On the other hand, Gerschenkron wanted to exploit the post WWII "fad" for development to revive the field and re-inject it with intellectual stamina. They also differed on the confidence they placed in economic theory: Gerschenkron seemed comfortable with economic theory, though the fact that he was surprised and slightly disappointed by Conrad and Meyer's performance was an indication that he was not exactly at the frontier of econometrics and mathematical economics. As Kuznets emphasized:

> "You see less of a gap than I see between economic theory (as it currently exists or even as it existed in the past) and what economic history can borrow and use from it with some reasonable chance of fruitful results. Or to put it differently, I have more sour view of the accomplishments of

[47] Letter from Gerschenkron to Kuznets, September 23rd 1957, Harvard University Archives, HUG FP45.12, Folder: "Economic History Convention, 1957", p. 3.

> economic theory and its serviceability in interpreting the processes of historical change (even as they affect the economy alone) than you apparently have." [48]

They found their common ground in a rhetoric that presented economic history as a *sine qua non* of growth and development studies: a rhetoric that was gaining large momentum in the growing cold war environment and had come to shape much of the Ford Foundation's interest in the social sciences.

4. The Ford Foundation and the contenders for American Economic History

4.1. Gerschenkron solicits the Ford Foundation

In his assessment of the Williamstown meeting, Gerschenkron had told Kuznets that the only remedy he could envisage to the stagnation in economic history was to recruit a new generation of economist-historians; this claim was consistent with the general inertia that he must have felt constrained by. As Conrad and Meyer's experience had shown, there was no momentum to pick up whatever seeds of change they (and Gerschenkron) wanted to sew. The 1957 Williamstown conference had served as confirmation of Meyer's short-lived attempt to change the nature of *EEH*. Hence, it is not surprising to find Gerschenkron, as early as the summer of 1957, testing the Ford Foundation's (FF) interest in helping train a new generation of economic historians. He sensed that it would take more than a controversial article or a new editor to change the field.

The FF's Economic Development and Administration division (EDA) was somewhat predisposed to listen to an argument about the importance of economic history, having inherited many of RF's Social Science Division funding

[48] Letter from Kuznets to Gerschenkron, September 28th 1957, Harvard University Archives, HUG FP45.12, Folder: "Economic History Convention, 1957".

categories. In chapter 5 we already saw the continuity between the RF's involvement in France in the 1940s, and the FF's subsequent presence. This was also true of research in the U.S. When the FF emerged in the early 1950s, the size of its endowment so considerably dwarfed RF's that no competition was really possible.[49] RF social science officers decided to retreat into niche markets (in particular in the developing world) and the FF took over much of the areas Willits had been interested in. As economic history had been one of Willits' favored fields, FF took interest in it.[50]

But there was a second factor pushing in favor of economic history at FF, a factor we have already encountered in chapters 4 and 5 – namely the increasing popularity of development economics, and the seductive belief that the past of advanced nations was experimental grounds for current policy in underdeveloped nations. By the mid 1950s, in spite of its present-minded agenda, the EDA was particularly vulnerable to rhetoric blending economic history and development studies.[51] In 1956 (nearly two years before the Williamstown meeting), EDA officers organized a round-table on economic history, and invited Abramovitz, Gerschenkron, Kuznets, Rostow, Sawyer, George Stigler (whom we will encounter again in chapter 7) and the British economist A.K. Cairncross to participate. In the preliminary letter sent to participants, they were asked to reflect on several questions:

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[49] See "How much was that" interlude.

[50] Evidence of this very straightforward transfer of interests and competences can be gleaned from Willits' diaries. As his directorship came to an end, he received several visits from Thomas Carroll, who came to seek Willits' opinions and advice in sponsoring social sciences. Carroll became the first Director of the EDA, and held his post from 1954 to 1961. See for example October 21st 1953, visit from Thomas Carroll. RAC-RF, RG 12.1, Diaries, Box 70, Joseph Willits, 5 volumes.

[51] Economic history was not considered for the sake of history – but rather in a very instrumental way, for the sake of solving present day problems. This ambiguity can be gleaned in an EDA officer's diary. After a visit from two economic historians (Parker and Sawyer) he commented: "we could give no indication now of the reaction their formal proposal might meet, partly because of its historical orientation". Excerpt from Allan Cartter's diary, June 16th 1958. FF, PA 59-27.

> "What is the relation between work in economic history and work in economic growth or economic development? It is often said that historical study should be guided by theoretical hypotheses and that lack of such hypotheses is a serious weakness in the field. Is that true? Why is the personnel of the field so depleted in the United States as compared, say, with Britain? What are the most promising lines of development in economic history at present?"[52]

From the minutes of the meeting, one gets the distinct sense that Gerschenkron was more vocal than any other participant and that he was pushing two lines at once. His first argument was that progress in economic theory would make historical study more scientific – this was the "theory as a tool" argument. But he was also pushing for a "history is good for economics" view, stating that "economic historians would bring to economic problems a better knowledge of social and political factors".[53] This second line was closer to Rostow who claimed that "there was a need for economic history to develop raw data to develop propositions in economics" - to which Kuznets acquiesced, but mentioned that tying the data with theory was no easy matter (something he had learned at the NBER!). And Gerschenkron concluded with a relatively general point on the continuum from "history to theory" – it was not useful to think in terms of empiricism and theory, but rather a "whole spectrum of levels of abstraction". This conversation had echoes of the perennial debate about the proper degree and place of empiricism in theorizing, and the willingness of all participants to recognize the "spectrum of abstractions" suggests that the circle

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[52] Letter from Loyd G. Reynolds, Director EDA, to Moses Abramovitz, January 9th 1956 FF-Area III, General Correspondance 1956. This exact letter was also sent to all other invitees.

[53] Minutes of the meeting on Economic History, January 24th 1956, FF-Area III, General Correspondance 1956.

depicted in Figure 2. 3 may indeed be a useful way to think about these debates among economists.

But the most distinct element that emerged from the (brief) minutes of the meeting was the feeling that many participants understood the strategic potential of the meeting. Having sensed the interest from the big donor, they were quite willing to sink their differences (Kuznets and Rostow were in the same room!) and market their discipline using the development theme, or any other EDA emphasis. Rostow, for example, added “problems of decision making and administration” to the list of the economic historian’s competencies – which is almost too obvious to be true, considering that the EDA stood for economic development and administration![54]

At the end of the meeting, the participants were asked to rule on two economic history proposals that had been submitted a few months earlier: the first was Hamilton’s proposal to study interest rates in Europe from the late 17th century to 1880. The second was Williamson’s proposal to study “income and wealth in the 19th century”. Neither project was supported. Recall that both Hamilton and Williamson were Edwin Gay’s students - and that no member of the Gay network was invited to the FF roundtable! Hamilton’s proposal was deemed to narrow - his previous work on prices had not “contributed enough to economic analysis” and the interest rates would be of no interest in isolation. Williamson’s project was deemed too loosely organized: Kuznets warned that “a strong director would be needed and a large amount of time devoted to making the data comparable” – as he had learned from running projects in retrospective accounting.[55] Instead the participants called for more fundamental measures to draw “new people” into the field.

[54] Minutes of the meeting on Economic History, January 24th 1956, FF-Area III, General Correspondance 1956.
[55] Letter from Reynolds to Carroll, January 27th 1956, FF-Area III, General Correspondance 1956.

Gerschenkron had his own idea on how to do this, and he approached the FF in late summer of 1957. He used the outcome of the Williamstown conference as confirmation that economic history was in an impasse, and that the FF could help:

> "the meeting at Williamstown was helpful and perhaps not quite unsuccessful (…) Nevertheless it is my feeling (…) that what is needed is the creation of a new generation of economic historians who could drastically reform the teaching of economic history in the country, and at the same time provide the general economist, and particularly the man interested in economic development, with relevant problem oriented and problem molded materials. If anything, the meetings greatly reinforced my feeling that this is a task of magnificent promise and first rate urgency."[56]

Such a letter may be interpreted as suggesting that he was not sure Conrad and Meyer's approach would have the desired effects, and that he wanted to adopt a new strategy to reform the field. In his subsequent exchanges with FF officers he outlined his plan, stating his intention to train students. But he also mentioned that the field's rejuvenation crucially depended on a whole-hearted embrace of his hypothesis on the importance of relative backwardness – which was his entry point into development theory, a point he emphasized to FF officers:

> "the purpose of the grant is twofold: a) to stimulate the study of past economic backwardness and of its gradual diminution over long periods and b) to revitalize the discipline of economic history in the country. The

[56] Letter from Gerschenkron to Kermit Gordon, September 16th 1957, FF-PA 59-26.

two aims are closely related in the sense that the attainment of one almost necessarily involves attainment of the other."[57]

His justification for using a single unified framework rested on evidence of past work in the field. According to him, miscellaneous accumulations of monographic studies had not yielded, and could not yield, synthetic or analytic results. They had to be devised with a unifying goal, namely the advantages of backwardness. [58] He suggested that the FF provide him with a fellowship program, to train the scholars capable of producing these more focused monographs. Such men would have a typical economists' toolkit and would have to commit to write a thesis on some problem related to past economic development.[59] In other words, his plan was not only methodological (use economists' tools), it was also theoretical: he wanted to encourage the use of the advantages of backwardness hypothesis. Seen in light of the competing research programs outlined earlier, Gerschenkron had devised a new way to confront the entrepreneurial historians: rather than convert them, he would outnumber them. Gerschenkron would have to wait an additional year before the FF made its decision. In the meantime, potential contenders had found their way into the EDA's office.

4.2. The William Parker and John Sawyer initiative

In January 1958, shortly after the 1957 September Williamstown meeting William Parker and John Sawyer jointly approached the FF, voicing their concern about the state of the discipline, and asking for funds to train young scholars. In these early exchanges with FF, they mentioned that they had been thinking and talking about this for two years – that they were very concerned that the causes

[57] Letter from Gerschenkron to Neil Chamberlain, January 3rd 1958, FF- PA 59-26.
[58] Letter from Gerschenkron to Neil Chamberlain, January 17th 1958, FF- PA 59-26.
[59] Letter from Gerschenkron to Neil Chamberlain, January 17th 1958, FF- PA 59-26.

of American economic development had still not been fully understood, or worse, misrepresented.[60] Recall that Sawyer had attended the 1956 FF roundtable, so he was well aware of FF's potential interest in this area. He and Parker had met at Harvard, when Parker was writing his dissertation – which he began with Usher, and finished with Gerschenkron, though Parker never felt particularly comfortable with the Russian émigré.[61]

Their proposal, and subsequent grant, came to be known as the interuniversity grant, as it federated economic historians in four geographically dispersed universities: Abramovitz at Stanford, Ross Robertson at Indiana, Parker at the University of North Carolina and Sawyer at Yale.[62] The four university partnership was justified along a pluralistic / division of labor vision of the field. According to them, if economic history was to contribute to growth studies, it needed to focus on American economic history, and do so from several complementary angles. As Parker wrote in 1958:

> "Jack Sawyer and I have natural division of labor (…) He has been interested in business enterprise, in entrepreneurship and in the social context and organization of innovation and production. I have been

[60] Letter from William Parker to Neil Chamberlain, March 6th 1958. FF, PA 59-27.
[61] Parker (1991).
[62] Parker included a brief biography of the fourth scholar, Robertson, in a letter to Neil Chamberlain, suggesting that he was not well known at that time: "He was an economist at the Federal Reserve Bank of St Louis for a number of years, and went to the Indiana School of Business to head up their program in business and economic history last year. His textbook in American Economic History was published a few years ago and is becoming, should I think, the most widely used in the field (at least Jim Potter and I use it, and Jack Sawyer intends to). He has a greater interest in monetary institutions and in questions of corporate organization than the rest of us and I feel sure we will all work very well together. He has the incidental advantage of adding to the geographical balance of our group and to the balance between 'pure' economics departments and business schools (which now stands at two to two, since I am in a business school, for all intents and purposes)". Letter from William Parker to Neil Chamberlain, September 24th 1958. FF, PA 59-27.

> interested in technological change in relation to the resource endowment in the natural setting for economic growth." [63]

As summarized by a FF officer, the Parker / Sawyer proposal boiled down to four points:

> "-Rejuvenate the field of economic history by demonstrating the vital part which it can play in attacking current economic problems.
> -Train a group of graduate students in economic history who could spread this refurbished approach.
> -Substantively, to rewrite American economic history.
> -Illuminate and improve theory of economic growth, of which the U.S. stands as an outstanding case."[64]

The obvious difference between this proposal and Gerschenkron's was the exclusive focus on *American* economic history. This may remind the reader of the project proposed by the "old" economic historians 16 years earlier (see chapter 3), due to its focus on the American record and a desire for interdisciplinarity.[65] There was even a feeling that economic historians could contribute to America's worldwide reputation. Sawyer and Parker made reference to the tenuous international political situation, suggesting that it could benefit from a scientific study of their nation's success. They even made rather explicit mention of growing Cold War tensions:

[63] Letter from William Parker to Allan Cartter, May 12th 1958. FF, PA 59-27.
[64] Excerpt from Diary, Neil Chamberlain, June 4th 1958. FF, PA 59-27.
[65] See for example the following sentences in their proposal: "The study of American economic history since 1800 is, by its very nature, a study of economic growth" - Proposal sent by William Parker, undated. FF, PA 59-27, p. 3.

> "Perhaps even outside the U.S. resentment and envy of American wealth might be reduced if its origins were shown to lie in social processes not wholly foreign to the history or future hopes of most mankind." [66]

Yet the InterUniversity agenda seemed more coherent than Willits et al.'s position, or at least more clearly focused around growth, testimony that the identification of economic history with growth questions was complete by 1960 (recall that CREH economic historians were looking for a much vaguer "philosophy", that pertained to civil society and harmony rather than economic growth). This commitment to study growth went along with an unproblematic reliance on economic theories and tools. They urged economic historians to "incorporate techniques and concepts of modern economics" and "develop with the help of modern economic theory plausible hypotheses about the inter-relationship among major elements and to test them".[67] Note that this is not very far from the generic definition of cliometrics offered in chapter 1.

The Gerschenkron and the Sawyer / Parker proposals were both approved by the FF in December 1958, under the same appropriation. The grant file made a point of stating that these were the first economic history grants awarded by EDA. Parker / Sawyer were awarded $125,000 for five years; Gerschenkron was awarded $75,000, also for five years.[68] While both grants may have appeared to FF as two ways of furthering the same goal, these projects were actually quite different in nature, and had diverging outcomes. While both had decried the uselessness of "scattered and uncoordinated research", only Gerschenkron's plan proved capable of counteracting this tendency.[69] Before we explore the roots of his ability to draw a dozen young economists to economic history, we shall

[66] Proposal sent by William Parker, undated. FF, PA 59-27, 4.
[67] Proposal sent by William Parker, undated. FF, PA 59-27.
[68] EDA: Harvard University and University of North Carolina, recommended action brief. FF, PA 59-27.
[69] Letter from Sawyer to Allan Cartter, May 1958. FF, PA 59-27.

examine the third proposal submitted by economic historians to the FF in the late 1950s, namely that of members of the Committee for Research in Economic History (CREH), who made one last attempt to hold on to a multidisciplinary, "live and let live" tradition in American economic history. [70]

4.3. The Committee for Research in Economic History (CREH) initiative[71]

The CREH proposal was drafted by David Landes, Douglass North, George Taylor, Hugh Aitken and John Dales – notice that none of them had been invited to the 1956 FF roundtable. This last project was informally submitted in late 1959, at which time both earlier projects had obtained funding, and it was declined. The proposal had been long in the making, preparations having begun around the same time as the Sawyer-Parker negotiations, i.e. right after the Williamstown meeting.[72] In December 1958, just as the FF was deciding the fates of the Gerschenkon and Parker / Sawyer grant, a sub-committee of the CREH met to "discuss the present weaknesses in economic history, how the field might be strengthened and what role the CREH might most usefully seek to play in strengthening and promoting economic history."[73] The opportunity for economic historians to seize the growth fad had not passed them by, as minutes of a February 1959 meeting of the CREH Trustees revealed:

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[70] I borrow this expression from Sawyer, who asked Gerschenkron to let the RCEntrepH pursue its agenda, just as it did not stop him from pursuing his. For reasons explained earlier (resources and the size of any serious empirical historical project), this was not a viable strategy - Sawyer (1954).

[71] The reader will remember the CREH from chapter 3. Though it had lost RF funding in the early 1950s, it had remained in existence, with little means to directly influence research in economic history. By the late 1950s, all its founding members had either died or retired, and new scholars now staffed the CREH.

[72] See for example a September 1958 Memo from Cameron to Aitken, Dales, Landes and North discussing their upcoming December 1958 meeting and strategies for wooing the foundations. Hagley, Accession 1479, Folder 97.

[73] Minutes from a meeting of the sub-committee, December 5th and 6th 1958. Hagley, Accession 1479, Folder 97.

> "Professor Taylor agreed that the Foundations did seem disposed to give grants for research projects in areas related to economic development. He raised the question to what extent research programs in economic development do involve work in economic history. A general discussion followed of the extent to which contemporaries are being forced into economic history by the very nature of the problem, of the quality of the economic history work that is done in this way, and of the degree to which professional economic historians are being by-passed in the awarding of grants for economic development and in the execution of the research done under these grants. There appeared to be agreement that the contribution economic history has to make to the analysis of economic development is a solid and substantial one (…) that went far beyond merely channeling empirical material to the theorist."[74]

However, they were concerned that too little work in economic history was geared towards this development audience, largely because very little work had been "interpretative", and did not really address "the causes of economic growth (and decline)". As they saw it, the discipline's failure to issue interpretative statements was due to a lack of coordination:

> "It would help the monographic literature in the field if authors held roughly the same view as to the major goals of economic history, their books would reflect some basic unity of purpose and this in turn would greatly facilitate the development of both comparative and interpretative history."[75]

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[74] Minutes from a meeting of the trustees, February 22nd 1959. Hagley, Accession 1479, Folder 99.
[75] Minutes from a meeting of the sub-committee, December 5th and 6th 1958. Hagley, Accession 1479, Folder 97.

They proposed two routes to create this unity: the first was to train a new generation of economic historians (they emphasized both the historical and the economic side of this revamped training – unlike Gerschenkron, their goal was not to incentivise top notch economists to do economic history, but appeal to an aspiring interdisciplinary scholar). The second was to organize a conference on specific topics regarding economic growth, and let groups of experts decide which topics were worthy of subsequent profession-wide investigation.

Members of this group seemed to share the desire to assert a strong "economic historian" identity, engulfed neither by economists nor historians. This stood out clearly as a differentiating factor from the Parker / Sawyer or Gerschenkron initiatives (which were really framed in terms of economics and economists), and even from the 1956 round-table, where all had agreed that economic history should not be separate from economics.[76] This difference was partly explained by the less homogeneous nature of the group: David Landes and Hugh Aitken were young members of the RCEntrepH. Douglass North was a U.C. Berkeley Economics graduate who had just spent several years at the NBER. George Taylor was a contemporary of Nef, Innis, Bezanson and Cole – he had written his Ph.D (1929) on "Agrarian discontent in the Mississippi Valley preceeding the war of 1812"at the University of Chicago Economics Department, under Chester Wright and Frank Knight.[77] John Dales was a Canadian economic historian. The reason they co-authored the proposal was that they were all officers of EHA, or members of the CREH in the late 1950s.

As part of this autonomous "economic history" identity they insisted on developing a whole new curriculum for economic history graduate studies: an

[76] Minutes of the meeting on Economic History, January 24th 1956. FF, Area III, General Correspondance 1956.

[77] George Rogers Taylor interviewed by Hugh Aitken, October 16th, 1973, 2 tapes available at Hagley Pictorial Archives; the transcript of this interview is at Hagley, Accession 1479, Box 99, Folder 1973-74.

"interdepartmental" track with a firm grounding in both disciplines, with an identity of its own. As Hugh Aitken wrote in October 1958:

> "I look forward to a day when we will no longer be asked whether we are primarily economists, or primarily historians, when we will no longer have to identify ourselves with either an economics or a history department, but when we can describe ourselves as economic historians without further qualification."[78]

Douglass North also voiced this desire to reinforce the economic historian's identity, when reviewing a preliminary program for the growth conference:

> "The papers should be given by the people who are in the field of Economic History and are pioneering in the direction outlined in the proposal rather than people who are outside economic history and who either because of testing hypotheses or working with long run time series (such as National Bureau people) incidentally touch upon the field. I think we are trying to put over a revolution from within rather than without, and I would therefore exclude someone like John Meyer, who demonstrates virtuosity in the field from without. I think the role of people who are in effect outside the field should be in criticism, discussion etc.. like Kuznets, Hoselitz, Fabricant"[79]

Applying his criteria, he listed the following people as "true" economic historians: Henry Rosovsky, David Landes, Rondo Cameron, Douglass North, Goran Ohlin, Eric Lampard and Morris D. Morris – he also suggested inviting a

[78] Memo from H. Aitken on the Future Organization and Policies of the CREH, October 6th 1958. Hagley, Accession 1479, Folder 97.
[79] Letter from North to Aitken, October 2nd 1959. Hagley, Accession 1479, Folder 97.

scholar from Britain (and proposed Habbakuk). Notice that his list was "research program" neutral, as he included scholars from the Gerschenkronian vein (Rosovsky, Ohlin) and the entrepreneurial vein (Landes). In doing so, he hinted at a vision for economic history that was not so different from the interuniversity proposal: a belief that economic history could be tackled via many angles, provided it focused on growth and provided it use tools of economic analysis. However the CREH's program differed from the Parker-Sawyer proposal in one important way: it did not privilege American economic history, and emphasized the necessity for all economic history to be comparative – much like Gerschenkron had.

Having considered their funding options, members of the sub-committee on the future of the CREH decided on the following strategy: they would ask the NSF to sponsor a week-long conference and invite Foundation officers to attend. The outcome of the conference would be research projects that individual scholars could submit to these Foundations, with special emphasis on graduate training as well. They never got past the first step. The NSF declined their proposal for the conference in early 1960, and conversations with officers of the FF discouraged them from soliciting them further.

5. Gerschenkron's Harvard Workshop in Economic History

5.1. A successful workshop

By the time CREH members got wind of their rejection, Gerschenkron's workshop was on its way- he had just recruited his first students, Paul David and Albert Fishlow, whose exceptional performance earmarked the workshop as a place for talented students. This only served to exacerbate the CREH's frustration at having been rejected. As David Landes wrote to Taylor, in January 1960:

> "I just heard about the NSF rejection and the fact they did so 'on the advice of other specialists in the field of economic history from such august institutions as the SSRC and the National Bureau'. I don't like to sound discouraged or persecuted but I am afraid that our field, which the NSF described as "crowded", is indeed filled to overflowing with a small but voracious group of insiders who have managed to take over such funds as were available and are systematically unenthusiastic about others' efforts and proposals (…) We will probably get a far more favorable hearing by putting Kuznets , Gerschenkron and a few of the leading lights of the NBER on our committee than by all the cogitation and ingenuity in the world".[80]

Yet, in expressing his frustration, Landes may have misjudged both Gerschenkron and Kuznets' influence on the foundations (in this particular instance), and not taken into account the simple fact that the CREH proposal had come last, third in a succession of apparently similar proposals (as we can see on the time line depicted earlier in Figure 6.2). As Parker pointed out to Taylor a year later, in June 1961:

> "When several of us undertook to get the grant of $125,000 from the FF for the Interuniversity Project in American Economic History, it was only by the merest good luck that a request from the [CREH] for funds did not arrive until a few months after our award had been made. Had it arrived earlier, it would have thrown both proposals completely off the rails."[81]

Indeed there is evidence that the FF did not really discriminate between the Parker/Sawyer proposal and the Gerschenkron one, so there is little reason to believe that they would have looked at the CREH proposal in a different light.

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[80] Letter from Landes to Taylor, 29th January 1960. Hagley, Accession 1479, Folder 99.
[81] Letter from Parker to Taylor, June 16th 1961. Hagley, Accession 1479, Folder 99.

However, as Parker said, they may have been disheartened by the lack of coordination in a relatively small field. Thus, the FF officers did not consciously cast their vote in favor of Gerschenkron's brand of economic history: their understanding of his project stopped at the conjunction between development economics and historical studies. Unlike the situation in the early 1940s, where Rockefeller Foundation officers had exerted much influence on the field of economic history, the FF did not discriminate between different approaches within the field. They took for granted the fact that economic history could contribute to growth studies and sought to sponsor whomever worked in this direction (which, as we saw, encouraged practically every economic historian to adjust his or her spiel).[82] While the FF's intervention in this highly contested field was somewhat uninformed, it certainly was not inconsequential. Indeed, the Harvard workshop's success soon began transforming the field of economic history.

Gerschenkron's annual reports to the FF revealed the care with which he handpicked each of his funded students: he accepted only those who had performed well on their second year exams, and in both his graduate courses in economic history. Given these criteria, he had to reject a "considerable number of applications".[83] As we see in Figure 6. 3, though the grant was set to expire at the end of 1963, Gerschenkron obtained a 5-year extension (no additional money, but the right to spread the balance on subsequent years). In 1968, he still had some money left, and asked the FF for the right to spend it all in the years to come. He thus ran the workshop for 12 years on the initial $75,000 grant – a feat that thoroughly impressed FF officers. The grant was officially closed in 1973.[84]

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[82] Curiously enough, no one ever questioned the epistemological validity of this claim: could historical studies really unveil useful empirical or theoretical information, in development, or for that matter any other kind of economics?
[83] Letter from Gerschenkron to Joseph McDaniel, May 10th 1953. FF, PA 59-26.
[84] Memo from Peter de Janosi, June 13th 1973: "Final comments on grant 59-26 for workshop in Economic History". FF, PA 59-26. He made several interesting points in this memo: "The grant to Professor Gerschenkron's seminar in economic history was

A look at Figure 6.4 should convince the reader that Gerschenkron's workshop bred a phenomenal number of remarkable scholars. All the names listed under the "Workshop" label spent a period of at least one year at Harvard, working with Gerschenkron (those with an MIT label were not officially supervised by Gerschenkron but they participated in the workshop). The expression "working with" may be a bit misleading as Gerschenkron was a famously hands-off advisor. Once he has selected them (and "seduced" them into writing a thesis in economic history), they were relegated to the offices of the Economic History Workshop. They would meet once a week, as a group, to discuss one of their papers, or the work of an invited scholar - Gerschenkron would remain perfectly silent for the duration of the discussion, only speaking up in the last 15 minutes, to issue his judgment on the better or worse arguments he had heard. [85]

The table depicted in Figure 6.5 lists these students' thesis topics, and their first jobs. They overwhelmingly chose topics in American economic history, which many justified as being a safe strategy considering Gerschenkron's awe-inspiring knowledge of European economic history. There isn't much evidence that they chose it for political concerns like those voiced by Parker and Sawyer in their proposal to FF – i.e. they were concerned with growth, but did not have a strong conviction that the study of American economic growth would reveal American exceptionalism.[86]

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part of the Foundation's intensive effort to develop and strengthen the economics profession. Economic history received only modest attention within this context, but Gerschenkron, nonetheless managed to produce an entire new generation of economic historians who turned out not only as excellent historians, but also modern economists trained in contemporary quantitative techniques. The small grant lived on as Gerschenkron used the money frugally and supplemented it from as many sources as possible. This grant was a gem!"

[85] David, Interviewed by Cristel de Rouvray, Palo Alto, January 2004; McCloskey, Interviewed by Cristel de Rouvray, San Diego, January 2004; Sylla, Interviewed by Cristel de Rouvray, San Diego, January 2004; Temin, Interviewed by Cristel de Rouvray, London, March 2004.

[86] Peter Temin , interviewed by Cristel de Rouvray in London, March 2004.

Figure 6.3: Timeline of events in section 5

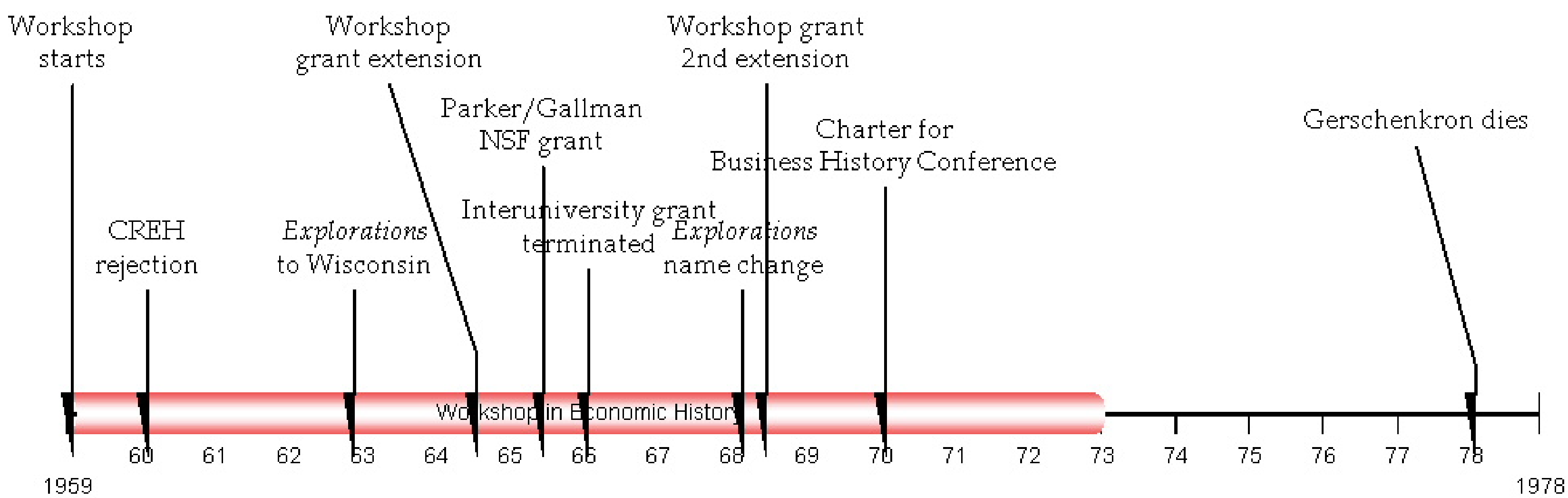


Figure 6.4: Alexander Gerschenkron's students

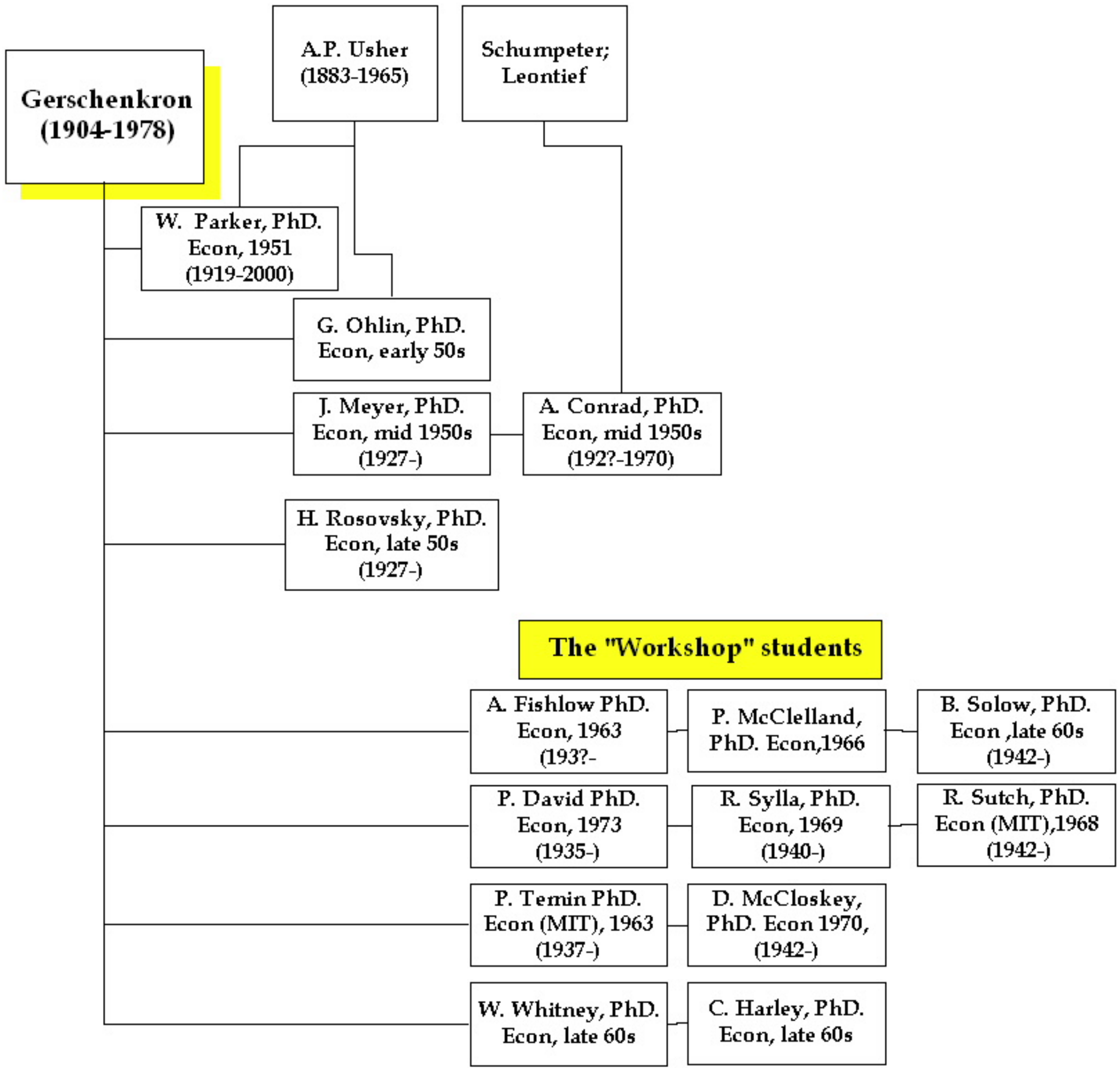


Figure 6.5: Gerschenkrons' students' Dissertation Topic and First Job

Name	**FF $$**	**Year in**	**Year out**	**Dissertation Topic**	**First job**
Albert Fishlow	yes	1959	1963	Railroads in 19th century U.S.	Asst. Prof. Econ. U.C. Berkeley
Paul David	yes	1959	1963	Economic growth of Chicago. Left workshop before finishing his dissertation.	Asst. Prof. Econ. Stanford (full Prof in 1968)
Peter Temin (MIT)	yes	1961	1963	Iron industry in 19th century U.S.	Fellowship at Harvard; then Prof. at MIT Business School (Sloan)
William Whitney	yes	1962	1964	The structure of the American economy in the late 19th century. Left the workshop before finishing	Asst. Prof. Econ University of Pennsylvania
Peter McClelland	yes	1963	1965	New Brunswick Economy in the 19th century	Instructor Econ. Harvard
Richard Sylla	yes	1966	1968	The American capital market, 1846-1914: a study of the effects of public policy on economic development	Asst. Prof. Econ. State University North Carolina
Stephano Fenoltea	yes	1966	1967	Resigned from the workshop	
Donald McCloskey	no	1966	1967	The economics of the British pig iron industry	Asst. Prof. Econ. University of Chicago
Barbara Solow	no	Mid 60s	Early 70s	19th century Ireland	
Charles Harley	no		1969	Economics of British Shipbuilding in the 19th century	Asst. Prof. Econ. Univeristy of British Columbia
Richard Sutch (MIT)	no		1967	Expectations, Risk, and the Term Structure of Interest Rates	Asst. Prof. Econ. U.C. Berkeley

Did Gerschenkron's students apply, push or even use the "backwardness" hypothesis in their work? One of his first students, Henry Rosovsky did so in his study of the Japanese late 19th century catch-up with the West. But this was probably the most obvious instance of applied "backwardness" research. For Gerschenkron's workshop students, the connection lay rather in the fact that backwardness was one of many historical theories aimed at explaining economic development. Paul David recalls feeling the limitations of Keynesian demand side theories and supply side theories that took technology, culture, demography and politics for granted. He was much more enthused by the work of Paul Baran, and other development economists who argued that history mattered – that patterns of development were not uniform or deterministic.[87] As we shall see in the next chapter, Albert Fishlow chose his thesis topic for similar reasons: as a refutation of Rostow's take-off and universal "stages" theory of economic development.

5.2. Who is in? Who is out?

Geschenkron's students greatly impressed economic historians in the U.S. This can be gleaned from the Parker's reports to the FF. Because both his interuniversity grant and Gerschenkron's grant had been awarded at the same time, under the same appropriation, Parker felt the constant need to evaluate their performance against the Gerschenkron benchmark –which he felt they had fallen short of. In 1966, as Parker liquidated the few dollars remaining from the original $125,000 grant, he reflected:

> "I do not feel that the quality of this work was as high as that which Professor Gerschenkron obtained by use of his grant at Harvard. There clearly the money produced at least four outstanding young men who are carrying out work in the field."[88]

In this same letter, Parker mentioned that his lesser performance could be linked to the heterogeneity of the interuniversity group (which, if you recall, was initially presented as a strength):

[87] David (1999).
[88] Report from Parker to Irma Bischoff, March 22nd 1966. FF, PA 59-27.

> "My own impression is that the project was more useful for the individual work which it stimulated than for its collective results (…) [Myself, Gallman and Abramovitz] have shared a common interest in developing a statistical basis for the study of American economic history. This interest we have shared with a number of scholars who were not in the project (…) e.g. Easterlin at Penn, North at Seattle, Fogel at Chicago and Davis at Purdue. This is the group interested in the development of a statistically and theoretically valid understanding of American economic development. Those whose interests are more directly in business history, in which I would include Sawyer and Robertson, form a separate constellation."[89]

Thus, in the space of 8 years (1958-1966) Parker seemed to have moved in a direction that made him feel quite distant from Sawyer and other economic historians interested in business and entrepreneurship. He now saw himself as a part of a well-defined group working on statistically and theoretically valid American history (in particular agrarian development). Despite his feeling of failure (in the InterUniversity grant), this new venture was on the way to becoming incredibly successful: in 1965 Parker and Gallman had obtained an NSF grant to build a very large statistical picture of agricultural production in 19th century U.S.[90] In addition, Parker now saw entrepreneurial research as a separate group. This separation was in part the product of "new" economist-historians' efforts to push entrepreneurial themes out of their discipline. For example, as seen in Figure 6.3, they resurrected *Explorations* in 1963, by moving the editorship to the University of Wisconsin, and gradually changing the flavor of the articles. A brief glance at the tables of contents of issues published in *Explorations* in the 1960s reveals a marked decrease in articles relating to

[89] Report from Parker to Irma Bischoff, March 22nd 1966, FF- PA 59-27
[90] Gallman (1992).

entrepreneurship, particularly evident after 1965. In 1969, the name was changed from *Explorations in Entrepreneurial History* to *Explorations in Economic History*, sealing the transformation. But the separation of economist-history from business history was also aided by the fact that entrepreneurial historians set out to recreate a community of their own. Recall that the Harvard RCEntrepH had been shut down in 1958; by the mid 1960s, a still numerous group of business historians were at work solidifying their own institutions. For example, they set up the Business History Conference, the first meeting had been held in 1954 but the permanent, associative status was only established in 1970 (Figure 6.3).[91]

Gerschenkron certainly had a hand in this separation– not least because he encouraged his students to be very suspicious of scholars of entrepreneurship. As Temin recalled:

> "They were the enemy! (…) We used the business school library which Cole had put together, and we thought that was terrific but it never occurred to us to associate the library with Arthur Cole! (…) But we wouldn't talk to them because we were firmly in Gerschenkron's camp, and all this qualitative stuff like entrepreneurship was just terrible. (…) I think I met Redlich once. I'm not sure I even ever met Cole (…) Gerschenkron invested an enormous amount of emotion in this, and so it wasn't just that these were people who disagreed, but these were, in a sense, the enemies, the forces of darkness."[92]

6. Conclusion: the Gerschenkron paradox

Alexander Gerschenkron's actions in the late 1950s were crucial for the emergence of the generation of economist-historians who chose to call themselves "cliometricians". Without his stamina and dedication to framing

[91] A brief history of the Business History Conference is available at http://www.h-net.org/~business/bhcweb/about/index.html; Chandler (2004).
[92] Temin (2004).

monographic research in a coordinated, coherent way, many fewer economists would have been drawn to economic history. Not only did he identify the opportunity to ride the development "fad", but he had a vision for overcoming the field's tendency to wither away in miscellaneous monographs and seduced his students with talk of historical theories.

Seen from this perspective, the Gerschenkron paradox is less daunting. He enticed young economists to take on historical questions with their tools, and did so with the conviction that their work would gradually contribute to a historical view, perhaps a theory of economic development. The young scholars responded, not only because they could ascribe to this "big picture", but because Gerschenkron also brought a degree of excitement and euphoria to the field. His students recalled his mentorship as an incredibly exciting time, a crucial factor in explaining their relatively large numbers in a rather small time period. This is where the "enemy" played a crucial role; and the barrier between economist and entrepreneurial history was partly erected on Gerschenkron's ability to inspire his students with suspicion of entrepreneurial study. Compare this to Kuznets' students and colleagues, who may have been awed by his capacity for detailed work, and tried to replicate this dedication in their construction of retrospective data series, but could find it tedious.

Thus one cannot discount Gerschenkron's personal role in creating a critical mass of young economist-historians. Yet there was an enduring tension between his vast, big picture work (history matters for economics) and his claims that economic theory would aid history, claims that were first taken up by Conrad and Meyer, and became an integral part of most workshop students' research. This would become the cornerstone of cliometrics - though the workshop would not be the stage where economist-historians completely shed their aspirations to historical theorizing. Chapter 7 looks into the circumstances that brought cliometrics to life and definitively pushed American economist-historians towards the "economics is good for history" side of the fence.

CHAPTER 7.

THE 1960s RAILROAD DEBATES AND THE CRYSTALLIZATION OF CLIOMETRICS

1. Introduction

According to most accounts and informal recollections, the term "cliometrics" was coined in late 1960 or early 1961. The term's inventor was a mathematician, Stanley Reiter, whose association with economists at Purdue University (Indiana) had begun in an applied mathematics seminar and continued in an economic history forum first held in December 1960.[1] In all anecdotal histories of cliometrics (see chapter 2), this first Purdue meeting was cited as one of two founding stones of the movement: the other being the 1957 Williamstown meeting examined in chapter 6. Yet the connection between these two events is hard to assess – indeed, neither Conrad nor Meyer attended the 1960 Purdue meeting, and the topic of excitement was not slavery but railroads. In addition, the "new economic historian" group identity only emerged in the mid 1960s, in the wake of what is now known as the "railroad debates": a series of published and live discussions on the railway's impact on 19th century American economic growth.[2] Thus there seems to be a case for examining these

[1] Hughes (1991).
[2] The first appearance of the term "new economic history" in print was North (1963) as confirmed in Fogel (1964a). Journal articles announcing the new methods and new ideas

debates as a crucial moment in the creation, or crystallization of the cliometric movement and identity.

Robert William Fogel started the debate at the first Purdue meeting when he argued that the railways had not provided significantly cheaper transportation than the best available alternative - a combination of water and road. He published the first segment of this argument in a 1962 article in the *JEH* and the full version of his findings appeared in the 1964 publication of his Ph.D. thesis: *Railroads and American Economic Growth: Essays in Econometric History*.[3] While Fogel – Simon Kuznets' student - was busy refuting the commonly held belief that the railroad had been indispensable for growth, one of Alexander Gerschenkron's students, Albert Fishlow, was tackling a similar issue. In his Ph.D. research he examined the validity of Walt Rostow's hypothesis that the railroad had been the "leading sector" triggering American economic take-off in the mid 19th century. This overlapping endeavor brought Fogel and Fishlow together and spurred sustained dialogue among students from both the Kuznets and Gerschenkron lineage.[4] It also pitted them against representatives of the "indispensability" thesis – Rostowians (though there were few of these) and proponents of the view that the railroad had fundamentally altered the ways and course of American business (Alfred Chandler, Fritz Redlich, Louis Hacker for example). In the course of these exchanges there was much opportunity to define "cliometrics", both by what it was and what it was not. This chapter uses the railroad debates as a platform for answering the following questions: on what grounds was the cliometric identity forged? In particular what did cliometricians

surfaced in the mid 1960s, and self-consciously "new" textbooks were published starting 1970. For examples of journal articles see North (1963); Fogel (1964a); Hughes (1965); North (1965). The first textbooks, or volumes of collected essays were Andreano, Ed. (1970); Fogel Robert and Stanley Engerman, Ed. (1971); Lee Susan and Peter Passell (1979).

[3] Fogel (1962); Fogel (1964b).

[4] Fishlow's thesis was published as Fishlow (1965).

mean when they claimed to be applying “theory” to history? Was this a genuinely new development?

The chapter begins with a description of the first cliometric meeting and the course of the railroad controversies (section 2). Section 3 then analyzes Fogel and Fishlow’s work to get a better understanding of what they did when claiming to be using “theory”. Section 4 looks at these young cliometricians‘ graduate training and the “transformation” of economics after WWII. Section 5 analyses Fogel’s famed counterfactual and compares it to other uses of “comparison” in economic history: could the counterfactual method be seen as an original alternative to the comparative work advocated by both Kuznets and Gerschenkron? Throughout, the chapter argues that the railroad debates were the crucible that brought cliometrics to life and forged the new economic historian identity. This was done in an adversarial way, thus completing Kuznets and Gerschenkron’s efforts to marginalize the entrepreneurial approach to economist-history. Yet cliometricians departed from their mentors in two important ways: they retained the American-centrism that had characterized the founders of the EHA and they argued that history needed economics, not the reverse.

2. The railroad controversies

2.1. The first cliometric meeting

In December 1960, a dozen scholars met in Indiana, at Purdue University, for a multiple day conference that would subsequently be known as the first cliometric meeting. Three young scholars in the economics department had organized the meeting: Lance Davis, Jonathan Hughes and D. McDougall. In the early 1960s, the Purdue economics department was not among America’s best, but an entrepreneurial dean of the Graduate School of Industrial Administration (who oversaw the economics department) was committed to changing this.

Starting in the mid 1950s, E.T. Weiler had hired tens of young, promising scholars – with retrospect Purdue was the breeding ground for more than one ground breaking initiative in empirical economics (Davis and Hughes' cliometrics and Vernon Smith's experimental economics were two prominent examples of this success).[5] In the late 1950s, Davis, Hughes and McDougall had written a "manifesto" for Weiler, arguing that Purdue had a comparative advantage in computational economic history: few schools had more than three economic historians (Harvard, Columbia and Johns Hopkins may have been the exception) and Hughes had already used the school's computation facilities to process amounts of historical data that no other economic historian had handled – data on steamship travel and cargo.[6]

When Davis, Hughes and McDougall set out to organize the December 1960 meeting, they were able to tap the relatively small (but well defined) network of economist-historians that their graduate training had introduced them to. Davis and McDougall were Johns Hopkins graduates, and had worked with Simon Kuznets. Jonathan Hughes had been a graduate student at the University of Washington, where he had worked with Douglass North. He had interrupted his academic career for a finance job, but was recruited to Purdue, where his old friend Lance Davis (who had been an undergraduate at the University of Washington and had taken Douglass North's class) had also just arrived. This served to mutually strengthen their interest in economic history – a "big idea" economic history as they characterized Doug North's teaching (dealing with questions such as the origins of wealth and growth).[7] Their connection to Kuznets and to North accounted for the presence of most

[5] Hughes (1991).
[6] Hughes and Reiter (1958); Hughes (1991). Lance Davis , Interviewed by Cristel de Rouvray, Pasadena, January 2004.
[7] Hughes (1991).

participants.[8] Robert Fogel recalled hearing Davis and McDougall's names among Hopkins students, and this connection most probably accounted for his invitation to Purdue; Easterlin and Gallman were also Kuznets' students.[9] William Parker had just been named co-editor of the *JEH* with Douglass North; Betram had spent a year at the NBER with North.[10] The only notable absentees were Harvard economist-historians, specifically Conrad and Meyer, whom Fogel, at least, remembered expecting to see at the meeting, as their 1958 paper on slavery had caused quite a stir at Johns Hopkins.[11]

It is quite likely that most participants shared a desire to "change" economic history, to turn it into a more vibrant, more "scientific", more economic field. Recall that both North and Parker had been involved in drafting "reform" proposals in the late 1950s. Yet the failure of the North / Landes appeal for funds, and the mitigated success of the Parker / Sawyer initiative should be taken as an indication that the will to change did not necessarily entail the actuality of transformation. This is also consistent with the effects of the Conrad and Meyer slavery paper. As we saw in chapter 6, though it was delivered in 1957, published in 1958, it did not really trigger an immediate following.

In anecdotal accounts from the late 1960s and more recent interviews, witnesses of the cliometric revolution did highlight the importance of the slavery paper for the movement's emergence, though it appears that this diagnosis was made after the fact, and strengthened by later events, namely the mid 1970s

[8] Davis remembered a dozen attendees - Davis (1990). North recalled meeting Fogel for the first time at this meeting - North (1993). I don't have an exact count of attendees to the first meeting; only 6 papers were given, one was co-authored, so 7 scholars officially spoke: J.H. McRandle and J.P. Quirk on "An Econometric Study of Strategic Decisions with Respect to the Anglo-German Naval Armaments Race, 1900-1914"; Fogel; Parker on "A Statistical Framework for Agricultural History"; Bertam on " The Process of Canadian Industrialization, 1870-1900"; G.S. Murphy on "The Simple Structure of Some Historical Methods" and J. Snyder on "Ancient Sumerian Economic Documents"- Purdue University Department of Economics, Ed. (1967), vii.
[9] Fogel (1990)., Fogel, Interviewed by Cristel de Rouvray via telephone, February 2004.
[10] Gallman (1992).
[11] Fogel (1990).

controversy around Robert Fogel and Stanley Engerman's own work on slavery.[12] As seen in Figure 7.1, a closer look at the chronology of events and at the concentration of "identity creation" moments in the mid 1960s (in Blue), overlapping with railroad papers (in Red), points instead to the railroad debates as the birthplace of a well-articulated cliometric group. In Robert Gallman's opinion, "the creation of a special cliometric group with a sense of identity came from the Purdue meetings [and ensuing sharp] exchanges between cliometricians and historical traditionalists [on railroads]".[13]

2.2. The railroad studies

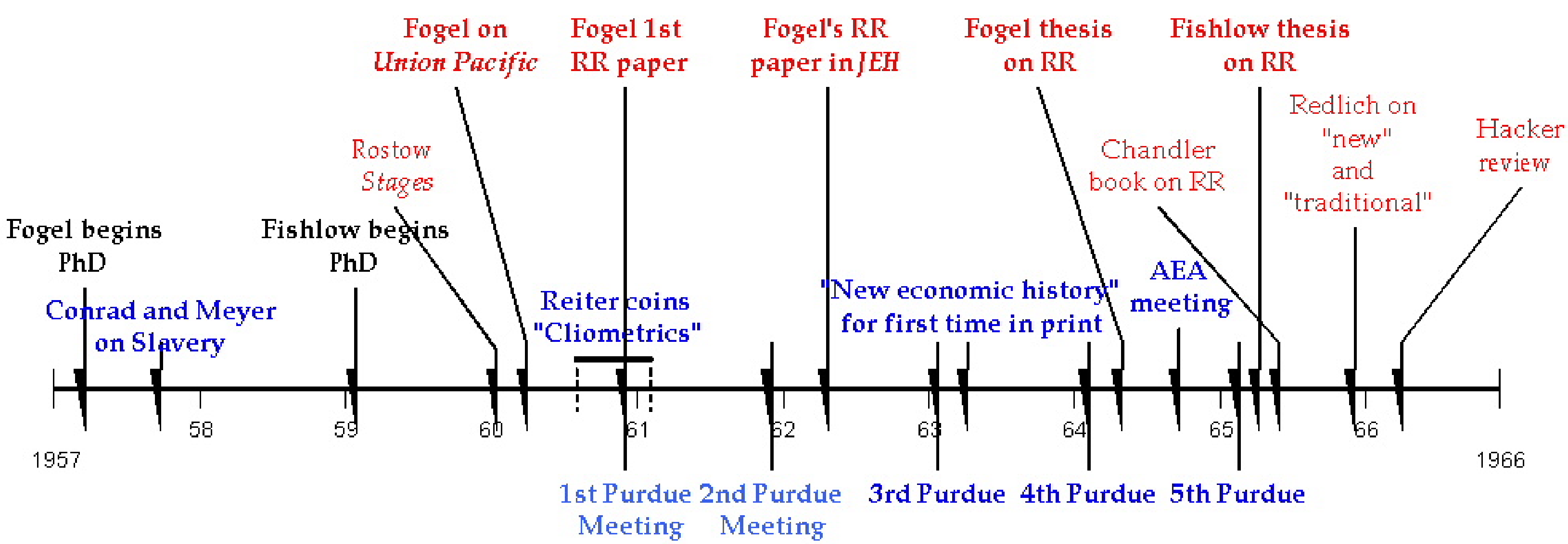
In December 1960, Robert Fogel presented one of the chapters of his Ph.D. thesis, which aimed at measuring the extent to which the railroad industry had contributed to 19th century American economic growth. According to Fogel, Kuznets had given him the idea:

> "I got the idea from one of [Kuznets'] lectures. He pointed out that although there had been much discussion of the economic impact of the railroad, no one had yet measured the extent of their impact or analyzed the sources of the productivity gains associated with them."[14]

Fogel had come to Johns Hopkins in the late 1950s and must have attended Kuznets classes in 1958 or 1959, just about the time when Kuznets was organizing a meeting to refute Rostow's theory of economic growth. Recall from chapter 4 that Kuznets found the concept of leading sectors to be remarquably unscientific (because he found it too vague).

[12] Engerman and Fogel (1974).
[13] Gallman (1992).
[14] Fogel (1990).

Figure 7.1: Time line of railroad controversy



In response to Kuznets' criticisms, many of the attendants had cited Railroads as the "obvious" example of an industry that had had "leading sector" effects on Western economies. Kuznets' general point, as Fogel righly recalled in the above quotation, was that one could not say this until one had measured the actual impact of the railroad at an aggregate level.

Fogel set up his work in conscious opposition to the existing historiography on American railroads. He mentioned two authors who had helped propagate the joint beliefs that railroads had been indispensable and that their exact contribution could not be measured: Leland Jenks and Walt Rostow. Recall from chapter 3 that Leland Jenks was one of the senior scholars at the RCEntrepH, a prominent sociologist, and a friend of Arthur Cole's. In 1946, a few months after he had been invited to join the CREH (to replace Kuznets who had resigned during WWII) he had published an article in the *JEH* entitiled "Railroads as an Economic Force in American Development". In this very general article where he listed all the potential ways in which the railroad could have affected the American economy, Jenks had stated:

> "By rendering this transportation service, the railroad in operation has doubtless added directly to the real income of the U.S. and indirectly to economic expansion. There appears to be no satisfactory technique for giving a precise measure to the extent of this contribution."[15]

Recall that this willingness to draw conclusions without pushing for systematic measurement had also been one of the divisive factors between Kuznets and Rostow.

While Kuznets' commitment to measurement may have reinforced Fogel's conviction that this was the right way to set up the problem, the idea of restricting historical conclusions to a precise quantitative statement seemed to

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[15] Jenks (1946), 12.

have predated his interaction with Kuznets. Indeed, the master's thesis he wrote at Columbia in the mid 1950s under the guidance of Carter Goodrich - published as *The Union Pacific Railroad: a Case in Premature Enterprise* (1960) – also set up a measurement problem, displaying many of the devices that Fogel used in his Ph.D. analysis (notably the order of magnitude and counterfactual methods, see sections 3 and 5).[16] This hints at the existence of several traditions of measurement in mid 20th century economist-history – thus cautioning us against a dichotomic quantifier vs. qualifier view of various scholars involved in these debates. As already shown in chapters 4 and 5, there were different types of quantifiers among mid 20th century economist-historians.

In the Purdue paper, Fogel presented one of the building blocks of his argument. His driving concept was the notion of "social savings": the profits in railroad company books represented some of the benefits accrued by the U.S. from building the railroad – yet, to really understand the way in which this faster mode of transportation had affected overall productivity, one needed to factor in public benefits, reflected in productivity gains due to cheaper transportation. Fogel thus set up the problem in terms familiar to students of cost-benefit analysis, though he was not so much concerned with the cost – the issue being rather a measure of the comparative cheapness and beneficial side effects of two modes of transportation – hence the rather unusual term "social savings". The Purdue paper was devoted to computing one of the many components of the lower cost of transportation – the inter-regional distribution of goods. In this paper, Fogel showed that the water rates were actually lower than rail rates, and that even when one added the extra wagon mileage, the loss in time and the increase in shipping risk, the overall savings attributable to interstate transport of agricultural goods was less than 1% of the national income of 1890. When his completed thesis came out in 1964 he had estimated the total social savings to be a disappointingly small figure: less than 5% of 1890 national income. Fogel

[16] Fogel (1960).

claimed that this small number had surprised him as he had expected a much higher number – which only served to strengthen his commitment to the result.[17]

Fogel had begun his graduate work in 1958; by 1960 he was one-third along the way.[18] He was not aware that one of Alexander Gerschenkron's students – who had begun his graduate work in 1959 - was working on a very similar problem.[19] Gerschenkron's student had also started from the desire to challenge existing historiography, and very specifically Rostow:

> "I came up with railways, in part because of W.W. Rostow's work (…) and its emphasis on the railway as the cause of the take-off in the United States. It didn't really seem to make much sense to me. From the little that I knew of economic history at the time, the idea of the take-off in the United States seemed quite a misleading emphasis. Rostow had elaborated somewhat on the railways, and I saw that there was a chance to do something."[20]

Fishlow wanted to examine Rostow's claim that the railroad had triggered benefits up and down the industrial chain: for example a higher demand for iron and steel products. As could be gleaned from his table of contents, such an assessment of the railway's impact had to be multifaceted, and could not be reduced to a computation (however specific) of the savings due to cheaper transportation.[21] Thus Fishlow differed from Fogel in three important respects; the first was his willingness to use other people's estimates of the numbers he

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[17] Fogel (1990).
[18] Ibid.
[19] Neither was Fishlow was aware of Fogel: "Needless to say, when I selected the railways I didn't know much about Mr. Fogel. I read his earlier stuff on the Union Pacific, which was a master's thesis (…) We first met at Harvard in 1961" - Fishlow (1998).
[20] Ibid.
[21] Fishlow (1965).

needed for his computations (often estimates that had been made by rail executives or legislators to justify or warn against building a new branch). This revealed his conviction that the social savings estimate was necessarily very rough, and perhaps not the best use of a scholar's time and energy. In contrast, Fogel estimated each of the steps himself. The second was the fact that Fishlow had left the door open to discussions about seemingly un-quantifiable factors, thus not entirely refuting the indispensability "myth". The third was that his analysis was principally built on an examination of forward and backward linkages (in order to assess whether or not there was such a thing as a leading sector sending virtuous impulses up and down the industrial chain), whereas Fogel's analysis was principally built around productivity within the rail sector. Their respective final theses blended both components, but it may have been the result of their post 1961 interaction, rather than identical ways of originally setting up the study.[22]

Fogel and Fishlow were both present at the second Purdue meeting in December 1961 – where an even greater number of young economist-historians were drawn into the railroad conversation. In particular, many of Gerschenkron's students joined the group (Fishlow, David and Temin), adding to the existing Kuznetsian majority. Lance Davis remembered this second meeting as the real beginning of all the "fun and excitement".[23] Many of the participants played Fogel's game, critically examining the particulars of his computation. As Fogel recalled of the first meeting his audience "wanted [him]

[22] According to Paul David, who shared a graduate student office with Fishlow at Harvard, Fishlow was more interested in the Hirschmanian and Rostowian analysis of industrial growth, and Fogel only added Rostow (and industrial linkages) to his thesis after meeting Fishlow. In his remembrance, Fishlow's work on social savings was definitely inspired by Fogel - Paul David, Conversation with Cristel de Rouvray, Oxford, August 2004.

[23] Davis (1990). I only have a list of the seven scholars who gave papers, which included Davis, Easterlin, David, Parker, Gallman and Brady - Purdue University Department of Economics, Ed. (1967), vii.

to explain in considerable detail how [he] had estimated this or that factor." [24] By the second and third Purdue meetings, other scholars had taken up pieces of Fogel's demonstration, or calculation to replicate, discuss or adjust it. As Gallman somewhat ironically recalled (an opinion doubtless shaped by Fogel's subsequent influence in the profession): "[Fogel] has always had the knack of obliging everyone to talk about what he wants to discuss. He did it with the railroads".[25] Thus, the railroads, and more specifically, Fogel's way of tackling the railroad question became a focal point and an exemplar of the kind of study new economic historians undertook.

The focalizing power of railroad debates was further exemplified in a session of the 1964 AEA meetings – where cliometrics was officially introduced to hundreds of economists. Fogel chaired a session on "Reappraisals in American Economic History".[26] The four papers presented at this session all directly, or indirectly addressed the railroad social savings issue. Peter Temin's paper was on 19th century iron manufacturing and considered the importance of the demand for rails in accounting for technological changes in the iron industry. Albert Fishlow's paper examined the actual pattern of American inter-regional trade in the first half of the 19th century – and while he did not spell out the implications of his findings for the social savings calculations, there was an obvious connection between this exercise and his earlier railroad work. Roger Ransom's paper was on canals.[27] Fogel's paper was a general "discussion" in which he aimed to use the three previous papers to define "the typical product" of new economic history. He pointed to quantification, theory guided measurement and counterfactuals as the trademarks of this new school. All panel participants, and more generally all cliometricians may not have had identical

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[24] Fogel (1990).
[25] Gallman (1992).
[26] Three papers were presented, and Fogel's "Discussion" paper was a summary statement; Fishlow (1964); Ransom (1964); Temin (1964).
[27] Roger Ransom had just earned his Ph.D. (1963) from the University of Washington, where he studied with Douglass North.

definitions of their own work, or of their movement, but they recognized that the railroad studies were their first contribution to historiography, and a stamp of legitimacy for their new movement:

> “For those of us who lived through the exciting days of the "cliometric revolution," the publication of Robert Fogel's *Railroads and American Economic Growth* represented a very major milestone - it was as if we now had proof that we had left the bumpy and unpaved dirt road of the first few years and could see ahead a straight and well-paved highway into the future.”[28]

Conrad and Meyer’s work on slavery, Davis and Hughes’ work at Purdue, the budding Harvard economic history workshop were all crucial elements in the appearance of a new economist-history, but the critical event so important to a movement’s genesis was built around Fogel’s controversial study.

2.3. Traditional historians fight back

Fogel’s study was not only effective for focalizing the energies of budding cliometricians but it also succeeded in drawing into the conversation those who came to be known as “traditional historians”. Much of their work insisted on the “forward and backward linkages” - economic, technological and psychological features that had come with the railroad. For example, Alfred Chandler, who had spent his graduate years at the Harvard RCEntrepH, published a book in 1965 where he argued that a thorough understanding of the widespread impact of the railroads on American business required investigation of the ways in which it affected transportation costs, corporate finance, corporate management, labor relations, modes of competition and government regulation.[29] According to Chandler, many of these elements were not amenable to measurement, and he

[28] Davis (2000).

[29] See the table of contents of Chandler, Ed. (1965). For a breakdown of the seven points that make up all “indispensability” arguments see O'Brien (1977), chapter 1.

acknowledged Fogel's work as an enterprise that could never overcome this fundamental limitation:

> "To measure statistically the full impact of the transportation revolution in which the railroad played the leading role in an extremely difficult task. The stimulus the railroad gave to the westward movement, to the expansion of wheat and cattle production, to the coming of new commercial routes, and to the adoption of mass production methods in manufacture or iron and consumer durables is almost impossible to pin down with precise, meaningful figures. But while economists may argue as to the extent to which the railroad affected the nation's income and product, few deny that the railroad was a significant force in the growth of the American economy during the second half of the 19th century."[30]

Among the factors that escaped measurement, Chandler particularly stressed the spread of modern information and decision systems – this fit into an Arthur Cole like vision that economic development was the result of the growing sophistication of the entrepreneurial system:

> "No existing business required so many, so varied, and so intricate short term operating decisions, and none called for such difficult long term decisions as to pricing and allocation of resources (…) Every day, railroad managers had to make decisions controlling the activities of many men to whom they rarely talked or even ever saw. Moreover, these operational decisions had to be made (…) quickly (…) The long-range decisions on the setting and adjustments of rates and the determination of costs, profits and losses were also endlessly complicated. Therefore both the short term operating decisions of coordination and appraisal and the long term

[30] Chandler, Ed. (1965), 23.

> policy decisions – involving expansion of tracks, equipment, terminal and other facilities and the methods used to finance such expansion were unprecedented in their intricacies."[31]

Such factors did not even figure in Fogel's assessment of the importance of the railroads, nor did it represent an important part of the critiques he received from Fishlow and other young cliometricians. Thus, "traditional" historians began seeing *economist*-historians as scholars who focused on measurement problems, ignored or evaded non-quantifiable features, in particular those having to do with the psychology and organization of individuals.

Yet, even while scholars like Chandler, Fritz Redlich or Louis Hartz tried to emphasize the narrowness of Fogel's exercise, and draw the profession's attention to the many other factors Jenks had listed, they still found themselves caught in his game – as they would embark on discussions of the validity of his assumptions, the representative nature of the water or rail rates he chose etc.[32] For example, in heated exchange with Fogel, in the mid 1960s, after the publication of their respective books, Chandler recalled having "replied" using maps to show canal routes, and reminding the audience that lakes and waterways could freeze during the winter.[33] These were the factors that Fogel had tried to put monetary values to, and did not really matter for Chandler's argument about entrepreneurial and organizational skills within the railroad

[31] Ibid, 97-100.

[32] In his study of the gradual expansion of cost-benefit analysis in American government bureaucracies, Porter has shown that disputes around the particulars of a given calculation were very frequent, if not the norm, and that different branches of government ultimately recognized the necessity of having matching rules and standards for computation of costs and benefits, otherwise their discussions could get stuck in unsolvable points of detail; Porter (1995), 175-82. So there may be something inherent to cost benefit analysis that gets all participants focused on the same narrow estimation issues!

[33] Chandler (2004).

corporation. Nevertheless he invested time and energy in refuting these. Similarly, in a scathing article reviewing Fogel's 1964 book, Louis Hacker spent much of the review scrutinizing the details of Fogel's computation, when Hacker's main point was about the way the problem was set up, not really the execution![34]

When they were not picking through the details of Fogel's computations, "traditional historians" were making more general points about his method. Their arguments revolved around three topics: evidence, use of formal economic theory, and legitimacy of the counterfactual method. The "evidence" argument was a derivative of the measurement issue. According to Chandler et al., the quantitative demonstration was overly constraining as it necessarily excluded all the intangibles (psychological and organizational aspects of the railroad's influence). To document these crucial aspects he preferred to invoke evidence from contemporaries – his 1965 book was a collection of essays about the railroads, many of which had been written in the 19th century:

> "The following readings focus on the railways as pioneers in the ways of modern big business. They concentrate on institutional innovation more than on quantitative economic growth and do so by letting the innovators speak for themselves."[35]

This aspect of the debate may call to mind the Kuznets versus Landes disagreement evoked in chapter 4, and can be understood, in part, as disagreement over what constituted reliable empirical information – yet another round in the perennial debate about the proper way to "observe" in economics.

The second focal point for discussion was the value of formal economic theory. As Fogel wrote in the mid 1960s:

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[34] Hacker (1966).
[35] Chandler, Ed. (1965), 11.

> "It seems clear that what is most novel and most important in the new economic history is not the increased emphasis on measurement but the reliance on theory to measure that which was previously deemed un-measurable."[36]

Yet the exact meaning of the word "theory", in this case as in others, was not often clear. Though it was sometimes qualified with the adjectives "economic" or "statistical", it was mostly used in a very broad way, sometimes even to refer to ontological dispositions, "Weltanschaungs", as Redlich called them: "a certain articulate outlook on the world (…) the old approach [dealt] essentially with institutions (…) [while] the new approach often [went] directly at macroeconomic processes, thus disregarding institutions."[37] This was certainly how the "traditionalist" historian Louis Hacker interpreted the debates:

> "[Traditionalists] look to the creation of a new climate – political, psychological, social, legal – and the appearance of real structural changes in the American economy (…) [If one wants an explanation of the impact of the railroads] one can find it in Schumpeter's seminal idea of entrepreneurship, when new men can emerge in a fluid society to reactivate economic processes and in William Graham Sumner's equally seminal idea of the folkways and the mores."[38]

As seen in chapter 2, this was the point Lamoreaux made in her brief analysis of the origins of the cliometric revolution. According to her, the two sides, old and new, were not divided along the use of the theory, but the choice of theory –

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[36] Fogel (1964a), 381.
[37] Redlich (1965), 480.
[38] Hacker (1966), 171-2.

cliometricians were more enthused by theories of human behavior predicated on individual rational choice or macro-economic theories, while the “old” guard was more seduced by sociological role theories and systemic institutional explanations.[39] Cliometricians did not acknowledge these different *Weltanschaungs*, preferring to characterize traditionalists as people with no theory, or no explicit theory (which as Hacker and Redlich showed, was not true). We thus realize that these claims for “theory” played an important rhetorical role, often meant different things to different participants and were perhaps not the clearest way of defining cliometricians or their opponents.

The last element of dispute between “traditionalists” and “cliometricians” was the issue of counterfactual analysis. To prove his point that railroads could have been dispensed with, Fogel had considered an alternative world with no iron horse and where a combination of road and water transportation was used instead. He had imagined a much vaster network of canals, to supplement for some of the missing rail routes. Traditional economic historians were not comfortable with these “figments”, which according to them, had no grounding in reality.[40] In response, cliometricians had claimed that all causal statements in history implied a counterfactual, thus the question was not deciding whether they were legitimate, but rather whether some counterfactual worlds were more legitimate than others.[41] Cliometricians referred to economic theory as the foundation of their (better) counterfactual worlds.[42] For example, when reviewing the work of American cliometricians, the British economic historian Patrick O’Brien reported that Fogel “had made full use of economic theory to assist him to describe a counterfactual economy without railways.”[43] This brought the conversation back to the “economic theory” prong described above,

[39] Lamoreaux (1998).
[40] See for example Redlich (1965).
[41] For a thorough discussion of the various positions in the counterfactual debates see McClelland (1975), 147.
[42] See McCloskey (1990), 92.
[43] O'Brien (1977), 23.

and the discussion ended in stalemate, as the word "theory" was being used strategically by both sides. But in the process both camps were forced to bring to light the elements that underlay their notions of explanation and causality, elements that prove particularly useful for the historian who seeks to understand why these debates could have been so important in the crystallization of the cliometric identity.

3. What cliometricians meant when they claimed to be applying "theory" to history

3.1. Textbook economic theory

In his 1964 book, Fogel used the word "theory" to refer to what a modern reader would recognize as neoclassical economic theory, sometimes, but not always expressed in mathematical terms. For example, he would invoke the "theory of increasing returns" to say that his use of 1890 actual water rates biased the social savings in favor of the railroads, as a world with more water traffic would surely have seen lower water rates (due to decreasing marginal costs of transporting more goods by water, because the cost of transportation factored in the large sunk cost of building and operating the canals, irrespective of how much merchandise was actually transported).

The most frequent theory both he and Fishlow used was price theory. For example, they estimated the increase in national income due to Western settlement (permitted by railroads) by looking at land values – as, in theory, the price of the land was the present value of all future profits earned from farming it. Fogel used this equation for computing the benefits of cheaper transportation costs, but did not discuss the behavioral assumptions underlying neoclassical price theory. He was not a committed free marketer who saw the world in terms of individuals pursuing their self-interest (actually he was an open radical student leader well into the 1950s), but rather an instrumental user of precise tools that could help him think though an empirical puzzle.[44]

But these examples of textbook economic theory did not constitute the bulk of activities that Fogel undertook under the label "theorizing". When one takes a closer look at Fogel's study and the responses it triggered one realizes that much of the analysis involved simplifying assumptions (for example, assuming that the weight of a butchered cow had not changed over a 20 year period), proxies (for example, using a sample survey of 20th century British household consumption as a proxy for 19th century American household consumption) and mathematical tricks (for example, assuming that the relationship between two variables was linear). Cliometricians and traditionalists may have called them "theory" but these procedures were certainly not on the same level as the formal theory of rent. Thus, an understanding of the origins of the cliometric revolution has to take into account these different types of "theory" and account for their origins.

3.2. National accounting devices

In each step of his analysis, Fogel would start with a list of data that he would ideally be able to use if he were to obtain an exact solution to the computation he had defined. Unfortunately these data did not always exist. For example, in the framework he presented at Purdue in 1960, he listed four types of data he would need to estimate the import demand for western agricultural goods from each of the eastern and southern states (for example, how much grain New Hampshire had imported from the Midwest in 1890). He began by stating that this demand was the difference between the state's production and its consumption. He would thus need data on farm production and variations in

[44] For information about Fogel's Marxism, see Schrecker (1986), 61, 87. Mark Perlman recalled reading a letter of recommendation from Carter Goodrich that made note of Fogel's student politics – Perlman (2003), conversation with Cristel de Rouvray, Duke University, June 2003.

stock – for the production side - and on total consumption and net exports - for the consumption side. Fogel had found data on farm production, net exports and variations in stock, but he had no source that documented total consumption of state residents. Instead he had to estimate it. To do this he multiplied total state population figures – which he obtained from the 1890 census, using half weights for children - by the average consumption of wheat, corn, pork and beef products as had been established by a 1909 British survey of urban workers.[45]

What kind of "theory" was Fogel using in performing the total consumption estimate and deriving the agricultural imports for each state? In the first place, he had relied on a national accounting identity to define state imports as the difference between the components of local production and consumption. This "theory" was conceptual rather than causal. Indeed, as mentioned in chapter 4, national accounting equations were accounting identities, built from an exhaustive classification of economic activity. These equations never implied action of one variable upon another – for example, there was no justification for saying that an increase in local consumption would trigger an increase in imports, as there were no underlying dynamic or behaviorist assumptions. Hence, while one could use the word "theory" to describe this accounting identity, one should recall that it was the kind of "theory" that Kuznets was comfortable with, and the reader knows from chapter 4 what Kuznets thought of most speculative economic theory! It may thus be more appropriate to call it a systemic framework, as Kuznets would have called it, or a "measuring instrument" as was suggested in chapter 4 (to highlight the data creation power of this view).

In the second place, Fogel had used a British survey as a proxy for consumption among American households. He had also assumed that children ate half as much food as adults. There was no formal theoretical justification for doing this. If there was any legitimacy behind such assumptions it would have to

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45 He only went through the detailed calculation for wheat - Fogel (1962), 180-4.

be empirical. Fogel recognized this, and validated his estimates with small studies of regional American consumption – that seemed to fit with the British estimates. These procedures were very common in Fogel's work: for example he had to determine how many bushels of wheat made up one sack of flour using "representative" empirical information. Note that such estimation procedures were much more prevalent in his 1964 thesis than in his 1960 *Union Pacific* – where he had actually given up the idea of pursuing a social savings calculation, having said that the relevant data simply could not be found.[46] This change in willingness to use empirically grounded proxies for the sake of data estimation may have been linked to his move to Johns Hopkins and Simon Kuznets' influence. Thus it may be useful for the 21st century reader to think of many of Fogel's "assumptions" as devices that he had learned from Kuznets, his students, and the wider community of national accountants.[47]

The reader may recall from chapters 4 and 5 that numbers generated within a national accounts framework were not un-problematic among economic historians. The accountant's simplifying assumptions might have been more or less justified on empirical grounds, yet they were never "found" in any archive or read off an almanac. This brings us back to the arguments that divided Jan Marczewski's retrospective accountant team and the *Annales* historians in France. National accounting procedures may not have been "high" theory, but they nonetheless belonged to a macro-economic view of the past that the likes of Redlich, Chandler and Hacker did not instinctively accept or find particularly useful. Much of the disagreement lay in the estimated, or guessed nature of these numbers – and Fogel brought it to another level, by adding order of magnitude exercises to his list of acceptable evidence.

[46] "Unfortunately the data needed to determine the unpaid benefits falling into categories (b), (c) and (d) are not available" - Fogel (1960), 99.

[47] For example in his chapter on intra-regional savings he mentioned that he had followed the "method of the Department of Agriculture" in estimating the "national disappearance of butter per capita" (i.e. consumption) - Fogel (1964b), 62.

3.3. Order of Magnitude exercises

In his 1962 and 1964 texts, Fogel often made reference to "orders of magnitude" or "back of the envelope" calculations, and referred to these as "theorizing". With these terms, he meant both the nature of the result he sought (i.e. not exact numbers, but the range) and his use of a set of pragmatic assumptions that could simplify the computation. The entire study was structured to get a social savings estimate that would not be exact, but be in the right order of magnitude: i.e. he wanted to know if development of the railroad had saved Americans tens, hundreds or millions of dollars. Thus, while his final 5% of GNP figure was much discussed, his own study did not make much of it, except that it was smaller than what he had expected.

He contrasted this pragmatism to the ideal way of solving the problem, the exact computation method, which he claimed could not be fulfilled, due to data and resource problems. According to Fogel, the problem of understanding *exactly* how much the railroad had contributed to inter-regional transport savings was a linear programming issue. In the 1962 paper (which became chapter 2 of the 1964 book), he thus spent quite a bit of time explaining this procedure to his reader, though he actually never ran the computations.[48] Behind this unused feature lay the distinct feeling that a large enough computer would have been

[48] As he explained it, the idea was to run two separate computations. The first would establish the total cost of transporting all the actual 1890 wheat, corn, pork and beef between states using the cheapest possible routes. To find these routes, Fogel would stipulate the amount of wheat, corn, pork and beef that was shipped from each primary market, the amount of each received by each secondary market. He would then plug in the alternatives that could have been used: the existing wagon, water and rail routes, and the rates charged on each segment. Solving the linear model would then entail distributing the entire produce to its destined locations minimizing the total cost, and under constraint of volume capacities of each mode of transport. The second computation would be identical, except that railroads would not longer be an option. The difference between these results would represent the money saved by having railroads. The computations were large because there were 11 primary markets and over 40 secondary ones - Ibid, 26-28.

able to solve the problem in a direct, very exact way – though the 1964 reader would have to make due with Fogel's order of magnitude calculation as a sufficient substitute.

Fogel's pragmatism surfaced at each stage of the computation. It intervened in the general set up, justifying for example his focus on the transportation of only 4 (instead of all) agricultural goods, as they constituted more than two thirds of total freight. It also intervened at a micro level, when it came to construct estimates for each of the steps that led to the final number. For example, he had used cooking books to establish the wheat equivalent of a certain amount of bread:

> "The budget studies gave estimates not of wheat, but of pounds of bread consumed. So there was an issue of how one got from pounds of bread to the wheat requirement (…) I went through the different sources that I had used, including a number of formulas that reported the amount of wheat that commercial bakeries use in a pound of bread. I had also examined a sizeable list of cookbooks of the time, including those that were common in the rural areas."[49]

This was an ingenious and efficient way to define the likely range of household wheat consumption.

This "order of magnitude" approach to measurement – which Fogel often called "theory" - was distinctly different from the emphasis on quantification provided by Kuznets and his accounting peers, who were much less prone to using "back of the envelope" assumptions. When they did, these assumptions had to be legitimized on an empirical, rather than rough order of magnitude or theoretical basis. It was much closer to Conrad and Meyer's strategy for estimating the total cost of owning and working a slave. They had obtained a

[49] Fogel (1990).

very rough set of estimates derived from previously published data and reduced wide ranges of observed prices to much narrower ones, without much justification.[50] This had led to some harsh critique, in particular from Gallman and Easterlin, who were Kuznets' students.

Fogel seemed to move from one extreme to the next – he could replace the linear programming with an approximation (which entailed dozens of rough assumptions), just as he could delve very deeply into the empirical justification for a single assumption. For example Fogel criticized Fishlow for having assumed that the average weight of slaughtered cattle had stayed the same in the twenty years from 1840 to 1860. Fogel had evidence to suggest that a different assumption would have been more appropriate, and could have avoided some strange and contradictory results. Indeed, his examination of corn production showed:

> "A major change in animal husbandry over the twenty year period. There was obviously a major movement away from grazing and mast feeding to corn feeding. Such a turn suggests a rise in the slaughter weight of hogs and cattle."[51]

If one considers Fogel's "theoretical" approach to economic history as a combination of order of magnitude work and national accounting devices, one can see the relatively marginal role played by "high" economic theory in the actual solving of the problem. Price theory, the notion of marginal cost or price elasticity for example were used opportunistically as short cuts to justify assumptions that were an exception in a set of otherwise empirically justified ones. Also, Fogel did not have a model of the 19th century American economy

[50] For example, the price of a acreage per slave was found to vary between $90 and $1400, a range they reduced to $450, with justifications as loose as: "the typical case was probably half way between these two" - Conrad and Meyer (1958), 101.
[51] Fogel (1964a), 386.

that explicitly spelled out the relationship between the transportation sector and the rest of the economy. He did not proceed as Conrad and Meyer had, setting up one equation (model) whose terms needed to be calculated to solve for the variable of interest. Fogel admitted to not having had a "model" for his 1964 work when he presented a new version of the railroad argument in his 1979 presidential address to the EHA. In this 1979 version, he developed a two-sector model. [52] Thus his initial use of the word theory was quite heterogeneous and masked a tension between different types of quantification - evidence of the multifaceted education he, and other cliometricians had received in the immediate post WWII U.S.

4. The transformation of American economics and the training of a cliometrician

4.1. The transformation of American economics

The challenge of understanding where cliometrics might have come from lies, in part, in the characterization of 1940s and 1950s economics, and the ways it differed from earlier years. As the economic historian Barbara Solow exclaimed when trying to account for the cliometric revolution: "economics had changed"![53] There is a growing literature about the "transformation" of 20th century economics – the picture that emerges from these studies is a mutation of the general look and feel of the economics profession in the decades surrounding WWII. While the interwar economics profession supported variety in beliefs, ideology, methods and policy recommendations, post-war economists were much more homogeneous. Historians of economics refer to this change as "the transformation from pluralistic to monolithic neoclassical economics".[54] This

[52] Fogel (1979).
[53] Robert and Barbara Solow (2004), Interviewed by Cristel de Rouvray, Boston, June 2004.
[54] See the essays in Morgan Mary and Malcolm Rutherford, Ed. (1998b).

phenomenon was most acute in the U.S., though it had delayed repercussions in practically every Western developed nation. In France, for example, the transformation arguably only occurred in the late 1970s, early 1980s.[55]

Though historians of economics agree about the existence of this transformation, there is some discussion around its exact timing, the factors that precipitated it and the outcome of the process. How important was American economists' experience in WWII for bringing about these changes? Was there a purposeful weeding out of radical economists, or any type of non-conventional social scientist? Did mathematical and statistical expression unambiguously take-over to the detriment of other methods? Though certain accounts have argued that WWII, and more importantly the Cold War created a politically conservative environment that suppressed any form of profound or humanistic discussion in economics, encouraging the profession to seek refuge behind a mathematical, technical veil, the vast majority of explanations shuns from such a Manichean view in favor of a more subtle appreciation of the phenomenon. True, there were some changes, but one should be weary of attributing them to cold-blooded mathematization or the inevitable march of progress.

A more comprehensive way of accounting for the transformation is to look at it in terms of changing standards of "scientificity". According to Morgan and Rutherford a shift in these standards occurred in mid 20th century economics. While interwar economists associated "the scientific status of work more with the personal qualities and attitudes of the economist qua scientist" - a vision that was compatible with advocacy, provided the pundit refer to alternative positions in an evenhanded way - post-war economists placed the onus of objectivity onto tools, thus restricting all argument to the choice of method. When it came to policy advice, post-war economists agreed that a scientific approach would yield a single, uncontested recommendation. The role of the mathematization of the discipline should thus be understood in this

[55] Lebaron (2000), 130-1.

context: it certainly was not the case that economists did not use mathematics before WWII, or that the adoption of mathematics would automatically trigger a pro-market view of the world. Rather, mathematical expression proliferated, as it proved to be a very powerful ally in these newly charged debates over method.[56]

So what were the factors that triggered these changes in notions of science and objectivity? Was the economics profession undergoing a boom, a great increase in numbers that may have changed the dynamics of the profession? The Great Depression, and even more importantly WWII and the Cold War seem to have triggered a demand for expert economic advice – a demand that could not possibly be satisfied by a profession that supported widely different takes on and remedies to a given problem.[57] These events may have provided the pressure and impetus for economics to change, but they also provided means. In the course of war service in particular, many economists were employed in very concrete tasks: for example resource allocation, price control or the conversion from one productive activity to another.[58] By working on such problems they found that some of their existing tools were very useful (simple mathematical optimizing models and statistical measurement devices for example), but they also shared assignments with physicists and mathematicians whose skills and ways of thinking rubbed off on them. For example, the general field of operations research was developed as a result of such synergies. In other words, war experience served both to reinforce the status of certain existing methods, and to create new ones.[59]

During WWII economists gained a reputation for successful problem solving (in stark contrast to the reputation they had earned from government work during the Great Depression) which entrenched demand for their services

[56] For a discussion of the complex process that led to the so-called "mathematization" of economics, and of its ties to a much larger debate about the appropriate role of mathematics in science, see Weintraub (1991); Weintraub (1996); Weintraub (1998).
[57] Coats, Ed. (1992).
[58] Goodwin (1998), 63
[59] Mirowski (2002), 203.

in the post war era. For example, the Council of Economic Advisors (a permanent advisory body to the President) was created in 1946. Such demand triggered a new hierarchy within the academic field of economics: those who could contribute more tools for decision advice were better rewarded. But this demand (and the perceived success of the service) also buttressed the profession's confidence in these tools. It could well be that post-WWII economists did not have unambiguous faith in individual rationality or the superiority of free markets. Their tools, however, contained such assumptions (often for the sake of mathematical simplicity). The fact that these tools "worked" when applied to concrete problems (such as the most cost effective way of allocating the country's steel capacity to the war effort) may have ironically served to validate the underlying assumptions. As Morgan and Rutherford wrote: "it is because of the success of their tools that economists came to believe in the ideas behind them. This is certainly an interesting reversal of the normal internalist history of economics that portrays ideas ("thought") as the leading light in any account".[60]

4.2. The education of a cliometrician

While these changes in economics have been well documented, their implications for economic history have not been thoroughly spelled out. One may wonder whether cliometricians' post WWII education had something to do with the fact that they distinguished themselves from older economist-historians (including their mentors) by the belief that economics was good for history, while the vast majority of economic historians the reader has encountered so far believed that history had something to bring to economics.

Graduate education in economics changed dramatically in the immediate post WWII era. Paul Samuelson's best selling neoclassical textbook first appeared

[60] Morgan Mary and Malcolm Rutherford (1998a), 14.

in 1948.[61] Shortly thereafter mathematical analysis became the backbone of most graduate programs. As Paul David recalled, even undergraduate curricula had been affected by the time he entered Harvard:

> "The very idea of a unified theoretical framework for studying economic activity was a powerful one. Remember, at this time [early 1950s] Samuelson was already having a big impact on the way undergraduate economics was taught at places like Harvard – even though the *Foundations of Economic Analysis* were not assigned until you got to the most advanced theory course."[62]

Though he qualified this statement by adding that his "initiation into advanced economic analysis occurred before 'the neoclassical system' was *the* form in which theoretical analysis was presented to students"; perhaps an indication that education in the 1950s was somewhat eclectic and bred an instrumental (rather than religious) rapport to theory. Paul David claimed that he always considered a range of "off the shelf" theories and textbook analyses when he aimed at explaining a particular situation. In the same interview, David mentioned that he was one of the first students to take the graduate econometrics course at Harvard, with Hendrick Houthakker as his teacher, and Fishlow as the Teaching Assistant. As the technical requirements multiplied, other courses were marginalized and eventually dropped. As Barbara Solow recalled: "when I was an undergraduate student, you needed two foreign languages – as an economics concentrator. When I was a graduate student it changed: you could substitute mathematics for your second foreign language".[63]

[61] Samuelson (1948).
[62] David (1999).
[63] Robert and Barbara Solow (2004), Interviewed by Cristel de Rouvray, Boston, June 2004.

These changes were not only occurring at Harvard – across the nation, students were being increasingly exposed to a more homogeneous theoretical body, especially in micro-economics (there still was quite a bit of controversy around Keynesian ideas, and some universities, like Purdue, did not even teach macroeconomics!).[64] In the micro-economic curriculum, price theory held a special status. Recall that price theory had been the most common piece of textbook theory used by Fogel and Fishlow. Fogel's reference was George Stigler's 1954 work, *The Theory of Price*.[65] In the mid 1950s, when Fogel was at Columbia studying for a masters degree, Stigler was still teaching there and Fogel remembered him as one of two people who most influenced him at Columbia (the other was Carter Goodrich):

> "George J. Stigler taught the graduate micro-economics sequence (…) Stigler made microeconomic theory come alive. He emphasized not its elegance but its applicability to a wide range of issues in economic policy. He continually moved between theory and evidence, carefully considering the empirical validity for the assumptions that theorists made about the slope or other aspects of the shape of key functions."[66]

As argued by the historians of economics Hands and Mirowski, Stigler's version of price theory was free of any psychological assumptions concerning the behavior of economic agents, and carried strong connections with a general way of thinking he had developed as a war consultant: a pragmatic, "can do" attitude.[67]

Fogel's exposure to Stigler introduced him to the Operations Research (OR) tradition that emerged at Columbia in the early 1940s, in the wake of

[64] Hughes (1991).
[65] Stigler (1954). Note that this was the only theoretical reference in Fogel (1960).
[66] Fogel (1993).
[67] Mirowski and Hands (1998).

Harold Hotelling, Henry Schultz and their heirs – some of the mathematically most advanced economists of the interwar and immediate post-war period.[68] Stigler had been Schultz's student and had joined Columbia just as Hotelling was starting the Statistical Research Group (SRG) – which did consulting work for the Mathematics Panel of the National Defense Research Council. According to Mirowski, this led to the emergence of OR, a way of thinking that Mirowski described as:

> "Look for "quantifiable" variables, even if the problem resists quantification (…) Use statistics to paper over the uncertainties and unknowns of the problem as portrayed, both for the economist and the agent, and to churn out implementable predictions for the client. Keep psychology out of it. Remember OR is just 'social science done in collaboration with and on behalf of executives'."[69]

The OR mindset was a powerful mix of pragmatism and mathematical sophistication, and encouraged economists to think in terms of order of magnitude. As Stigler recalled:

> "One subject I worked on was the vulnerability of aircraft to various kinds of firepower (20mm. cannon, .50-caliber machine guns, etc.). Within six months after our group began work on this subject, we were consulted by other war-research agencies on the details of aircraft vulnerability. One day I would be measuring a secretary to estimate how many square feet of target a seated pilot made, and a short time later I would be gravely discussing that number with another research group."[70]

[68] Mirowski (2002), 193.
[69] Ibid, 204.
[70] Quoted from Stigler (1988), 62. in Mirowski (2002), 206.

Stigler apparently had not felt the need the measure the exact size of a pilot in a plane – a secretary (a woman presumably) would provide an adequate estimate.

Mirowski interpreted these experiences, and the "can–do hubris" that economists derived from the success of their war jobs, as the roots of their post-war scientific ontology. Economists developed a reflex of systematically cutting up problems by way of staggered approximations. Implicit in this way of working was the idea that the analyst could subsequently check the empirical validity of each of his assumptions, and adjust the calculations in light of any changes. But, for the sake of setting up the exercise, the crucial factor was the "order of magnitude" – i.e., the result did not need to be exact, provided it was approximate to the real value.[71] Hence the emphasis was much less on the empirical validity of each assumption than on the process of identifying each step of the analysis. Actually, it is not clear who was responsible for checking the validity of each estimate, as economists like Stigler seemed to think that their job primarily consisted in finding original ways of setting up the problem so that it could be measured.

We have already stressed the importance of order of magnitude analysis and linear programming in Fogel's work, and the differences between this type of quantification and the older national accounts tradition. The importance of the OR mindset for understanding the origins of cliometrics was further confirmed in the genesis of the name. Recall that it had been coined by Stanley Reiter, a mathematical economist who was at Purdue with Davis and Hughes, running a seminar in advanced mathematical and computational techniques. A decade earlier, Reiter had worked with Tjalling Koopmans on transportation optimization models (for example, how to configure shipping routes to minimize the number of miles sailed with empty cargo) – an example of their joint work

[71] Note that in physics, "order of magnitude" means a factor of 10 – this is not the way Fogel used the expression.

was presented at a 1949 conference organized by the Cowles Commission (where Reiter had spent several years).[72] Koopmans' 1944 work on transportation had been a pioneering element of the development and spread of linear programming techniques at the nexus of industrial, military and theoretical lines brought together by WWII.[73] Though he had not met Fogel when he coined the term, there was an interesting parallel between his background and the linear programming methodology Fogel longed to apply.

The similarity between Fogel's "ideal solution" and Reiter's work with Koopmans deserves notice. Of course, OR transportation models were principally about the way things should be, rather than the way they were, and Fogel was claiming to describe the 19th century American economy as it was. However, to compute social savings, he had used two benchmark, ideal worlds: the first was described in his linear programming scheme (how much should it have cost to distribute all agricultural goods from the 10 primary markets to the 40 or so secondary markets using existing transportation lines); the second was his famed counterfactual (how much should it have cost to move all merchandise on actual and imagined canals?). In both cases the "should" was approximated with the smallest possible cost – i.e. the optimal cost, and Fogel simply assumed that this optimal cost was the best proxy for the actual one. This was a very strong assumption, mostly when one recalls that dozens of economists were hired by the War Production Board to specifically ensure that goods were transported in the best possible way, evidence that it was not spontaneously so. If this was the case in the 1940s, it is hard to believe that the 1890s saw a perfect minimization of transportation costs. Thus Fogel's reference points were very similar to OR analysts' more normative and prescriptive work.

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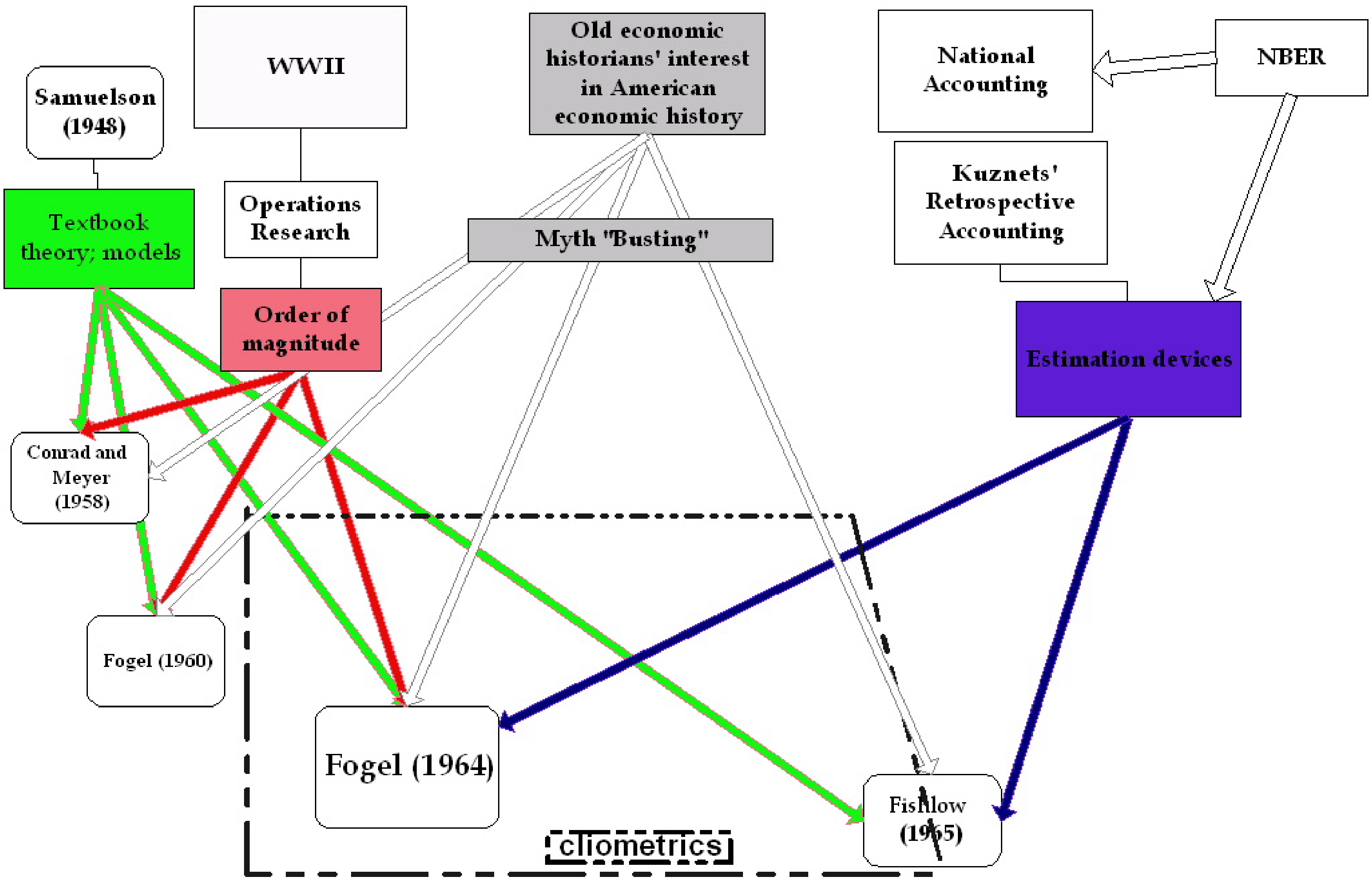
72 See "A Model of Transportation" in Koopmans, Ed. (1951).
73 See Dantzig's 1963 chart representing the origins and influence of linear programming; shown in Klein (2001), 130.

Cliometrics seems to have been intimately tied to the education of a post WWII empirical economist, and specifically to Fogel's education: as he blended Kuznetsian macro empiricism with OR pragmatism and chards of textbook theory. Compared to Conrad and Meyer's work, Fogel's originality was his reliance on national accounting practices. This is depicted on Figure 7.2 where these two separate traditions of empiricism (and quantification) are symbolized on either side of the picture: the operations research branch is in red (dark), while the national accounting branch is in blue (light). Textbook theory is in green. Notice that Fogel (1964) was the only one to combine three elements. Conrad and Meyer (1958) and Fogel (1960) were missing extensive use of accounting devices; while Fishlow (1965) did not display much order of magnitude or linear programming work.

As seen on Figure 7.2 there was a fourth component to cliometrics, namely an interest in traditional themes of American historiography, what Fogel called "myths". In spite of all the disagreement around method, there was a striking similarity between Cole's heirs (Chandler, Redlich, Handlin) and Fogel, Conrad and Meyer (and to a lesser extent Fishlow): the isolated interest in America's history, which contrasted deeply with Kuznets and Gerschenkron's commitment to comparative history. This difference is best illustrated in Fogel's use of counterfactuals.

Figure 7.2 : the making of "cliometrics"



5. Kuznets, Gerschenkon and the cliometricians: comparison v. counterfactual

5.1. The counterfactual method: what Fogel actually did

In the course of the railroad debates, much ink was spilled on whether Fogel's counterfactual world was a legitimate procedure for establishing historical causal relationships. In his 1964 thesis, Fogel used the counterfactual world in two ways. The first was a relatively unproblematic reference to alternate modes of transportation: if American producers and consumers had not had access to the railroad in 1890 how much extra would they have had to pay to transport the same bundle of goods on roads, canals, lakes and seashores? Fishlow had also made reference to these transportation substitutes – and most readers found the procedure relatively straightforward (after all, one could imagine a widespread, breakdown of the railroad system due to natural catastrophe or rogue management!). This alternative situation was simply a rhetorical device to make clear that there were other means of transporting goods in late 19th century U.S.

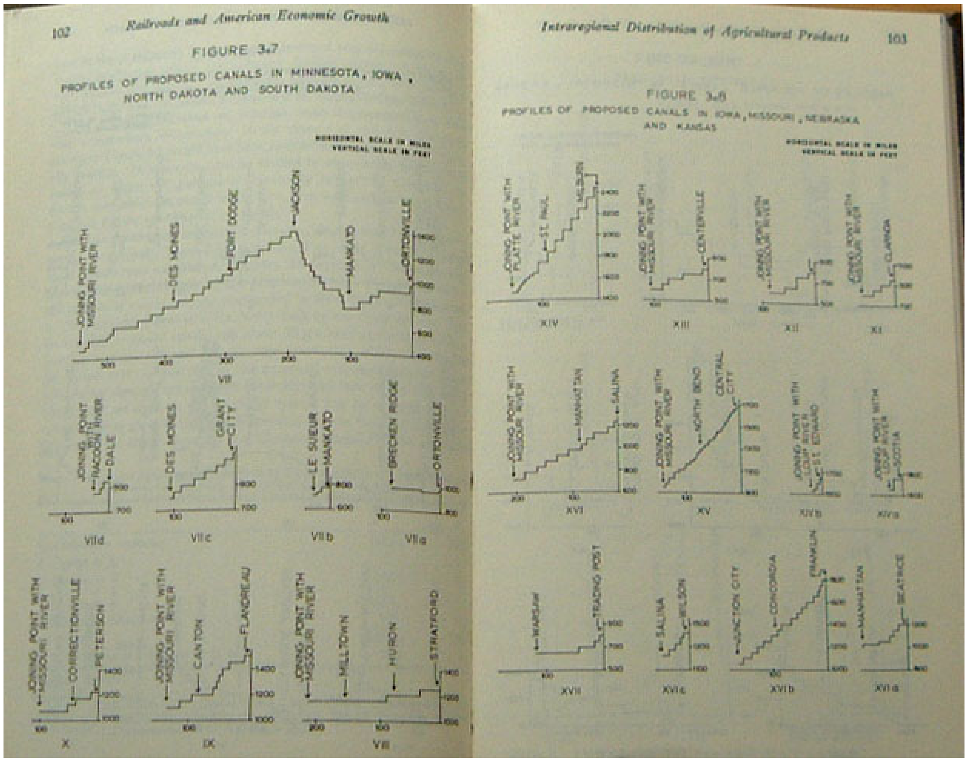
Fogel pushed this counterfactual world much further in his second application. When it came to computing the savings that had been earned from rail usage in intra-regional transport, he stated that a fair comparison would have to take into account both disaffected and potential new canals. According to him, had there been no railroad, the economy would have developed an alternative mode of cheap transportation. He added that given the momentum and scope of canal construction in the first four decades of the 19th century, it surely would have continued had it not been for the railroad boom of the 1850s. Hence, he imagined additional canals: "37 canals and feeders", representing "5,022 miles in length", whose construction "would have brought almost all of the agricultural land in the Midwest within 40 straight line miles of a navigable waterway". This extended water network had implications for the range of settled land (hence national income from land value), price of transportation (hence national income from cheap water transport), and he even computed the costs of construction of this additional canal mileage, to show that the investment would have been worthwhile.[74] He concluded:

> "This proposed extension of the internal water transportation is more than a historian's hallucination. In the absence of railroads, the canals shown (…) would have been both technologically and economically feasible. This assertion can be verified by considering the two main issues on which the question of feasibility turns: the nature of the terrain over which the canals could have been built and the water supply available for the operation of the canals."[75]

Fogel's imagined canal network was not the product of an economist's over-optimism or ontological belief that the market always worked. He did not simply state that the economy would have adjusted to a lower level of technology and developed an equally efficient alternative mode of cheap transport. Rather it seemed to be the product of an OR mind: the canals he imagined were part of the economy as it should, and could be. He argued that both the knowledge (know-how) and the physical conditions would have permitted such an extension: "[none] of these developments required new knowledge. They merely involved an extension of existing technology."[76] He almost went as far as drafting engineering plans for each of his proposed canals – he had five pages of tables showing both the horizontal scale in miles, and the vertical scale in feet for each new canal (a sample can be seen in Figure 7.3).

[74] Fogel (1964b), 79-81.
[75] Ibid, 94.
[76] Ibid, 92.

Figure 7.3: Horizontal length and vertical depth of each imagined canal[77]



Here again, the foundations of Fogel's assumptions were ultimately empirical: the final test of plausibility was pragmatic: could it be done? If he had to build these canals, would he have been able to do it? Such practical thoughts were not unfamiliar to generations of American Army engineers whose cost-benefit calculations had been used as official sanction and benchmark for decades of water transportation public projects (canals, dams, navigation, locks).[78] His easy adoption of this engineer's mentality is further evidence of the post-WWII convergence of certain economists' thinking with models from engineering. Fogel seemed to belong (or to imitate, which is an important distinction, considering the rhetorical power of the OR aura) to a generation of "hands-on"

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[77] Ibid, 101.

[78] The history of the Army Corps of Engineers and their pioneering role for diffusing cost benefit analysis in the U.S. (well before economists began working on more abstract theoretical bases of cost-benefit analysis) is well examined in Porter (1995). His chapter 7 is entirely devoted to their role in public water projects and legislation in the 1920-1960 period.

economists, whose criterion for legitimizing their method was "does it work?" Theoretical considerations of a philosophical and ontological kind were seen as a waste of time. In other words, Fogel never bothered justifying his methodology on epistemological terms. His concerns were always practical.

5.2. Are counterfactuals illegitimate "figments"?

The practicality of this counterfactual method was lost on most of Fogel's readers who immediately attacked him on epistemological grounds. The most frequent criticism concerned the unreality, hence futility of the whole approach. As a member of the RCEntrepH, Fritz Redlich, wrote:

> "Fogel investigates what would have happened to American economic development if there had not been any railroads. Now, as every schoolchild knows, there were railroads. That is Fogel investigates what would have happened in the event that something else had happened which could not have happened. I emphasize now the phrase, which could not have happened. Technological development follows its own logic. Once the atmospheric engine had been developed into an efficient steam engine (…) it was only a question of when the steam engine would be put on wheels (…) [This] does not make Fogel's product history. "[79]

One could say that Redlich and Fogel had two very different *Weltanschaungs* concerning the role and dynamics of technology in society, but it seems more profitable to think of their differences in terms of attitudes towards problem solving. Redlich understood Fogel's question and the need to erect some sort of reference point to be able to tackle it: to see whether the railroad had mattered you had to be able to refer to an economy without it. But, in Redlich's mind,

[79] Redlich (1965), 486-7.

there could be no justification for inventing a world that had not existed. You needed to find a world that had existed. Hence, his appeal to comparison:

> "Everyone who knows French economic history in the eighteenth, and English economic history in the eighteenth and early nineteenth centuries knows that then there was economic development underway without the railroad. The fruitful question seems to be (…) at what point would economic development, underway by 1800, have become an arrested development for lack of adequate transportation? The stop would have come rather late in England, because the ore and coal were available in close proximity (..) the United States development would have come to a halt much earlier, or several national economies might have developed."[80]

According to Redlich, the scientific way to go about tackling Fogel's problem would have been to proceed via comparison: looking at actual economies where there had been no railroads. This, in his opinion, would be good history – and, incidentally, good economics, since Redlich shared Edwin Gay's students' views that good economics depended on good economic history.

5.3. Beyond Comparison? An American centric history

The reader must recall that many earlier *economist*-historians shared the belief that comparison was the only genuine way to jump from monographs to general statements. In the U.S., Kuznets had expressed this view as early as 1941, during the round table at the Rockefeller Foundation, when Gay's students were trying to build an agenda for a purely American economic history. The idea of using spatial comparisons had later been adopted by the founders of the RCEntrepH – who encouraged studies of entrepreneurial behavior across time and place (though their focus on the entrepreneur had been born from their

[80] Ibid, 487-8.

study of American history and nothing seemed to change their mind about this Schumpeterian interpretation of economic development!). The reader should recall that Gerschenkron's scientific research program was also inherently comparative. His framework needed to be populated with concrete examples of different degrees of backwardness and the ways in which this retardation had spurred or hampered development. He was looking for thresholds: at what point did backwardness become a handicap, rather than an advantage? To find these thresholds he would need studies of several countries at different stages of development to pinpoint which factors had been their Achilles' heel.

So Redlich had no monopoly on using comparison as the handle to make causal statements and Fogel himself had thought of proceeding comparatively before he adopted the explicit counterfactual method. Indeed, in his 1960 study of the Union Pacific Railroad, he had already introduced the idea of benchmarking a railroad economy to a non-railroad one:

> "If there had existed a nation which had been the twin of the United States in every respect except that it had not built the Union Pacific, the social rate of return on the investment could be determined by first finding the differences in annual income between the two countries over the life of the investment, and then finding the rate of discount which made the present value of these annual income differences equal to the amount of the investment. However, since the United States did not have such a national twin, this method of procedure is ruled out."[81]

In other words, while the ideal controlled experiment involved a comparison between two real and almost identical countries, when this was not possible the scholar needed to find a different way of proceeding. Fogel had chosen the counterfactual.

[81] Fogel (1960), 98.

In Fogel's mind there did not seem to be a trade-off between comparisons and counterfactuals. Comparisons had the merit of involving "real" cases, where the complex interaction of all economic variables had run its course, producing side effects that the theoretician could not have predicted (this is what some scholars have called the "seamless web of history"). But counterfactuals more neatly resembled a controlled experiment, as the analyst could literally leave everything unchanged, except for the variable of interest. For Redlich there may have been a higher degree of artificiality to this second method but for Fogel it was just another facet of his pragmatism. His counterfactual was actually not rooted on a specific theory that would tell him how to build the alternate world – it was an ad hoc combination of empirical facts (actual 1890 water rates), plausible developments (for example build a canal in this place) and theoretical precepts (theory of rent). Hence we may want to amend O'Brien's 1977 assertion and recognize that the counterfactual was not built on a model of the 19th century economy, but rather on a combination of assumptions that Fogel may have called "theory", but as we showed in section 3 were actually a combination of eclectic methods and devices from mid 20th century economics.[82]

The fact that the analyst controlled this world made it a strong rhetorical device. Indeed the real difference between the comparative and counterfactual method was not the truthfulness or realism inherent in each method. As mentioned above, each method was an imperfect substitute for controlled experiments. So the choice between them was much more a matter of persuasion. In his analysis of counterfactuals, the historian and philosopher Geoffrey Hawthorn has highlighted the rhetorical power of this device. According to him, the essence of historical explanation lies in the act of situating the actual occurrence that needs to be explained in a spectrum of possible events. This

[82] Recall that O'Brien had said that Fogel's counterfactual world was built on economic "theory" - O'Brien (1977).

spectrum can never be exhaustive (who are we to know what could have happened in all possible worlds!), but it can be more or less persuasive.[83]

Kuznets and Gerschenkron had chosen to build their spectrums from a chain of actual events: Kuznets wanted national income time shots for as many countries at as many points in time as he could obtain; while Gerschenkron wanted pictures of various nations' degrees of relative backwardness (which he spelled out in terms of capital availability, mobility of labor, state policies, availability of scientific knowledge). In both cases, the spectrum implicitly constrained the realm of possible worlds. For example, if Gerschenkron wanted to know whether it would have been possible for Russia to finance its railroads privately, he could refer to Germany – where private individuals and investment banks had done the job – and show that certain features of German society (namely effectiveness of contract and law enforcement) were missing in late 19th century Russia. Thus the German case set a possibilities limit to the Russian past: no banking elite meant no possibility of private railroad financing.

Had Fogel followed a Kuznetsian or Gerschenkronian comparative strategy, he would have needed to establish a spectrum of countries before and after the advent of the railroad, indicating their national wealth and growth at each time. For his Union Pacific study, he could have compared 1860s U.S. to 1890's Russia, for example, to test his explanation about the causes of the

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[83] Hawthorn (1991).; in this wonderful book Hawthorn showed counterfactual analysis in action when analyzing the causes of repeated Plague epidemics in Europe. According to him, a historian will have explained a particular outburst only if he can answer the following question: "was there a possible world where the Florentine authorities would have known what to do?" To prove that lack of scientific knowledge was not the main cause, Hawthorn showed that a medieval Italian city-state would have been unable to implement the quarantine and mobility restriction procedures necessary to contain the epidemic. So unless the counterfactual world was one where people had 20th century knowledge in the 14th century and had large enough bureaucracies to implement and enforce these policies, and had a sense of national interest that went beyond commerce, the plague would not have been contained. Hawthorn argued that these extra clauses were impossible given the historical and economic context of medieval Italy (and medieval Europe), and hence that such a counterfactual was implausible. Implausible counterfactuals are not persuasive.

bankruptcy, as both could be seen as cases of state led premature enterprise. For his 1964 study, he could have done as Redlich had suggested: looked at the U.S. before the railroad boom, or France in the late 18thcentury.

However, there was an obvious difficulty with this type of exercise: when one changed countries, and even when one changed epoch, one inevitably varied too many factors at once. The biggest and least tangible of these uncontrolled factors were specifically all elements that could not be measured ("institutions", "culture", "entrepreneurship" etc.). Considering that the RCEntrepH insisted on the fact that social expectations (and the consequent roles adopted by entrepreneurs) were crucial to understanding national income growth (just as Rostow had insisted on psychological "propensities" to take advantage of a take-off period), any causal argument about the railroads that relied on cross-country, or cross-temporal comparisons would have to wrestle with these non-measurable elements (as Kuznets had realized when confronting Rostow and Landes, as we saw in chapter 4) .

And this was exactly the point that Fogel wanted to deny: he wanted to bust the "myth" that the railroads had affected people's mindsets and created enough entrepreneurial energy to warrant unprecedented economic expansion. By using an imagined counterfactual economy, he could simply erase them from his analysis – as he could assume that nothing changed except the medium of transportation. Whereas Kuznets and Gerschenkron had left the door open to cultural explanations of economic growth, Fogel had shut it tight. Yet in the process of doing so, he also eliminated any motivation to do comparative economic history – to actually delve into the particulars of another place, or another time! [84] As we see in Figure 7.4 (which is identical to Figure 7.3 except

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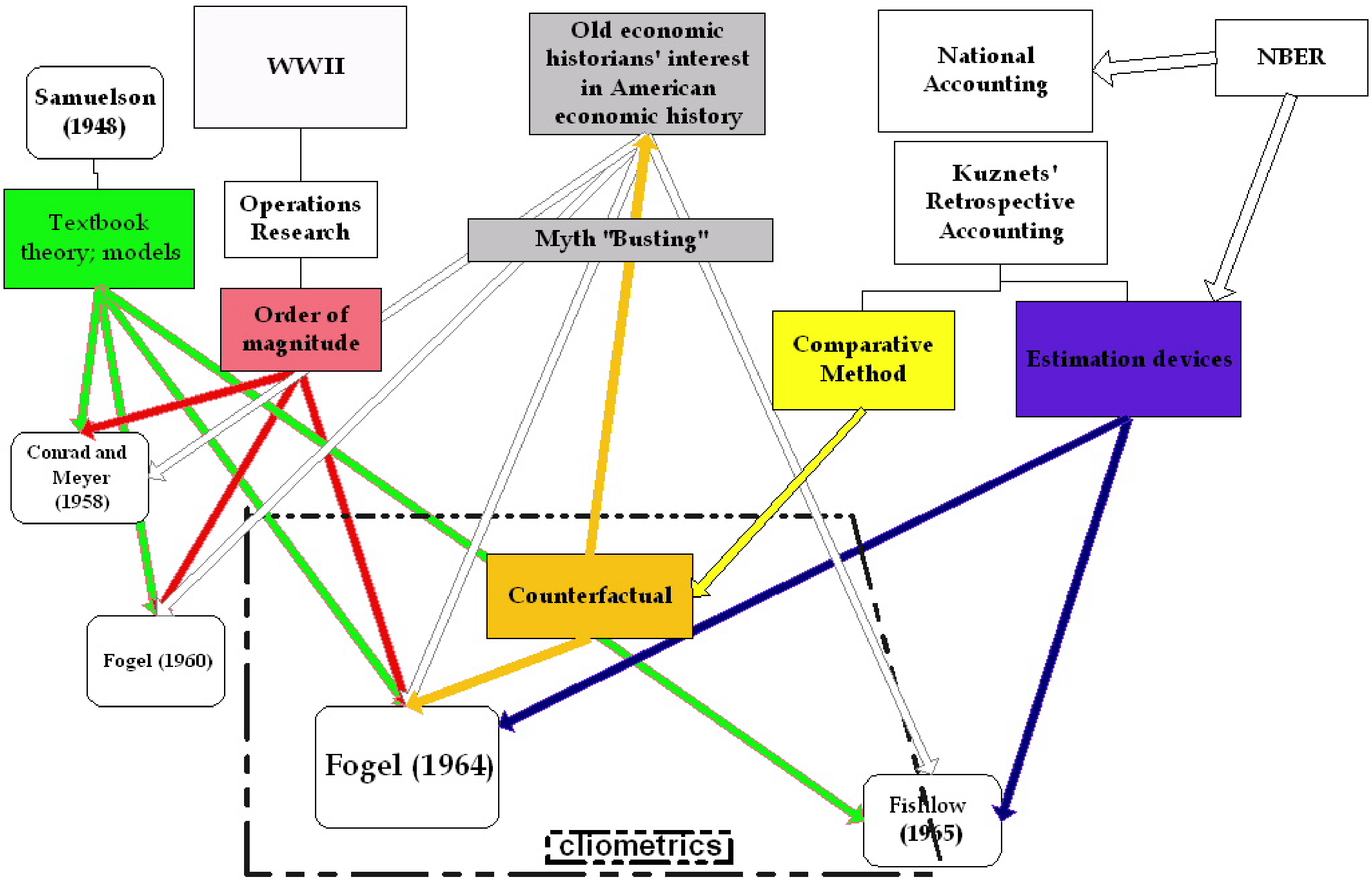
[84] The counterfactual method has since become a standard tool among *economist*-historians, to the point where it can sometimes be depicted as less problematic than plain old comparative work. See for example Robert Bates' comment about not being able to use a counterfactual, and adopting a "less direct" method of evaluating the efficiency of the International Coffee Organization (ICO) by comparing prices within the

that it spells out the relationship between Kuznets, comparison, counterfactual and Fogel), the counterfactual can be usefully understood as a variation on the comparative method that had been so important to Kuznets. Yet this innovation combined to strengthen the American only feature of cliometric research, as it avoided investigation of non-American history, thus constituting a fundamental departure from Kuznets' economist-history.

ICO region to prices that actually prevailed outside it. According to him, this method is "less direct" because it cannot control for differences in quality and markets - Bates (1998).

Figure 7.4: The counterfactual as a "super" comparison



6. Conclusion: the irony of the railroad debates

The railroad debates spurred by Robert Fogel's provocative calculation of the iron horse's contribution to 19th century American economy provided a focal point around which the cliometric identity crystallized. Though there was much reformist energy among many late 1950s economist-historians, the debates both shaped and constrained the nature of this reform, and the establishment of a "new" economist-history. For example, the comparative agenda that had been at the heart of both Kuznets and Gerschenkron's battles for economic history was lost to the following generation, in favor of a much more American centric approach. The railroad debates were also crucial because they involved "traditional historians" – thus clearly defining the line between who was "in" and who was "out" of the new economist-history. By the late 1960s, the likes of Chandler – who had once belonged to a group within the Harvard economics department - were definitely excluded from the face of "scientific" economic history, as defined by economists. They gradually left the EHA, *JEH* and *EEH* to create their own institutions.[85]

Yet, there was nothing necessary to this exclusion of cultural, entrepreneurial or institutional interpretations of past economic events, as the situation in France revealed. In the 1960s, French *Annalistes* were still primarily interested in materialist explanations of economic change, but by the end of the decade they were seriously considering the impact of *mentalités* – ways of thinking and doing, the "customs and mores" cited by Hacker. So for those readers who are wondering why Willits, Bezanson, Cole, Jenks and their students disappeared from American economist–history, they may want to reflect on the fact that their research funding was quite suddenly terminated in the early 1960s and that they never had a Gerschenkron (or a Braudel) to mobilize people and resources. Note that cliometricians' success at eliminating

[85] Chandler (2004).

the entrepreneurial, institutional line thought in American *economist*-history was ironic on two counts. For one thing they continued to focus on the American themes that had been selected by Willits and Bezanson's heirs. For another, Douglass North brought institutions back into economic history in the 1980s, and psychology and mindset in the 21st century.[86]

Yet, the irony becomes even stronger when one considers that the issues that Chandler emphasized were not only ignored by cliometricians, but they failed to see the connection between 19th century railroad management innovations and the tools they used to "scientifically" prove their point. Chandler listed many of the tools developed by these railroad operatives, including cost accounting, cost-benefit analysis and general "control through statistics" – ways of seeing the world that had been developed to cope with extensive rail networks, and that were completely taken for granted by the mid 20th century. Fogel acknowledged his debt to 19th century transportation engineers, though he did not see the irony of the connection:

> "It was really the literature because people were comparing railroads to waterways all along. Let me say there is virtually nothing I did in my work on railroads that was not anticipated by some state legislator or other public figure. [For example,] there was hardly a session of state legislature that dealt with a proposal to build a canal or a railroad in which the advocates did not refer to the predicted increase in land values or use that idea to estimate the social benefit that wouldn't be covered by the income of the road."[87]

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[86] North (1991); North (2005).
[87] Fogel (1990).

He added: "economists did not discover cost benefit analysis. It really comes out of engineering." [88] And Chandler had shown that railroad managers had turned it into a much more widespread way of thinking. So railroads may not have mattered for 19th century economic growth (if you believe Fogel), but they pioneered methods that mattered tremendously for Fogel's analysis and the crystallization of cliometrics.

[88] Ibid. More precisely, it came out of transport engineering, as Porter showed in his study of the Army Corps of Engineers. Porter (1995), chapter 7.; more generally cost-benefit analysis came from the French civil engineering tradition, see Walliser (1990), 4-11.

INTERLUDE

HOW MUCH WAS THAT?
American Philanthropic Foundations and Economic History in the mid 20th century

1. How much was that?

Throughout the preceding chapters the reader has learned about numerous grants awarded to economic historians in the U.S. and France; from the 1929 Price History grant to Kuznets and Abramovitz's 1963 post-war growth grant; from Perroux's ISEA grants to Braudel's $1million for the *Maison des Sciences de l'Homme*. As the foundation records only contain nominal values (the actual dollar amounts received, with no discussion of cost of living, cost of research, inflation etc.) the figures are not easily comparable. The general purpose of this interlude is to help readers understand the relative value of each grant, provide benchmarks to compare these amounts across time and place and get a sense of the buying power they represented. It also provides an overview of the Rockefeller and Ford Foundations' participation on the American and international scene and a measure of their influence in immediate pre and post-WWII social science research. How much did these foundations give? What percentage went to social sciences? How big were the economic history outlays? What could scholars buy with these grants? What did a successful initiative in economic history cost? Was there any clear correlation between this success and the size of the grant?

2. **American foundations' total endowment, annual expenditure and share spent on social science: 1930-1960.**

WWII marked a watershed for social science research in most developed nations (including the Soviet Union), resulting, among other things, in a general increase in resources available for research in economics, anthropology, political science, statistics, psychology and practically every other science of "man".[1] In the U.S., the military, and then the federal government (by way of the National Science Foundation - NSF) recognized the importance of financing academia thus fundamentally altering the rules and mechanisms of research in economics.[2] However, the arrival of such mighty sponsors did not crowd out a much older tradition of philanthropic commitment to scientific research. For mid 20th century economist-historians for example, the Rockefeller Foundation (RF) and Ford Foundation (FF) were the main providers of financing, until Robert Fogel and Stanley Engermann set a new precedent in the late 1960s by obtaining NSF support for their work on slavery.[3]

The RF was established in the early 20th century and began sponsoring social sciences in 1923, though the official Division of Social Sciences was only created in 1929.[4] The FF was created in 1936, but only became a national and international foundation in 1950. Thereafter economics was mostly confined to the Economic Development and Administration division (EDA) created in 1952. Yet, one should note that social science initiatives (including economics) were also considered in other divisions, like Education and International Affairs, so that EDA's budget is best interpreted as the minimum available to social science

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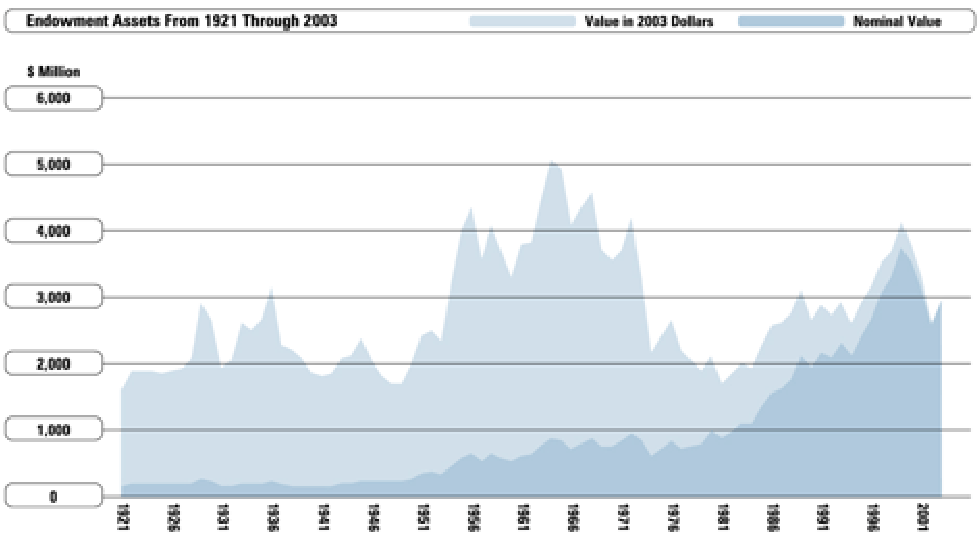
1 Goodwin (1998).
2 Mirowski (2002).
3 Engerman and Fogel (1974).
4 Grossman (1982).

in any given year. Charts 1 and 2 show the evolution of total endowment, annual expenditure, and share for social sciences for both foundations, from the date of their creation to the 1960s. In charts 1.B, 2.A and 2.B, the thicker lines are built from 2003 adjusted dollars – thus permitting a consistent comparison across time and foundation.

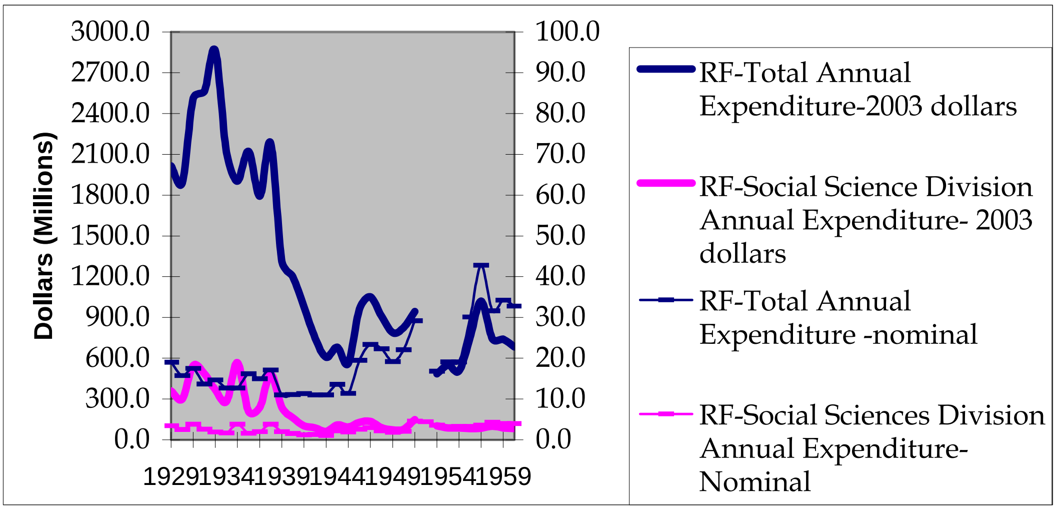
Chart 1.A gives an overview of the evolution of RF total endowment from 1921 to 2001; the darker curve shows the steady increase in nominal amounts, with a more noticeable increase after WWII and a real jump starting in the 1980s. The light curve -real figures - is more messy, showing peaks and troughs throughout the pre war and WWII period and a very large increase in the 1950s and 1960s, the highest point was reached in 1966, when the RF had an endowment of 5 billion dollars (in 2003 terms, which corresponded to less than a billion in 1966 values).

Chart 1.A: RF total endowment 1921-2001[5]



5 Source: http://www.rockfound.org/display.asp?context=2&Collection=9&SectionTypeId=13&Preview=0&ARCurrent=1

Chart 1.B: RF total and social science expenditure, 1929-1960[6]



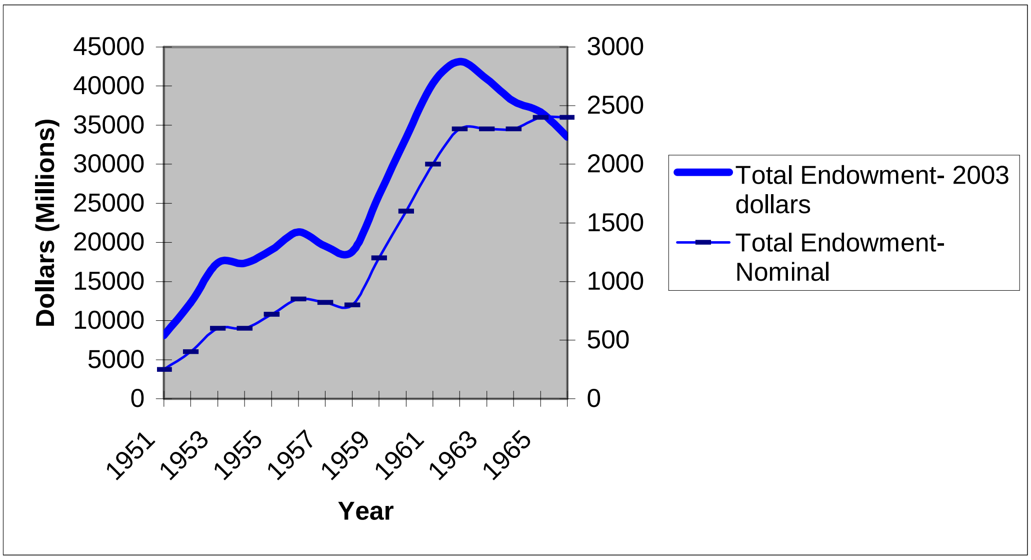
The lines on chart 1.B follow two different scales; the thick lines – 2003 values - are measured on the left side axis, while the thinner dotted lines are measured on the right side axis. The 2003 curves help show a more considerable variation in expenditure than is apparent if one only looks at the current price lines. For example we see that the amount of money available for social science decreased considerably in the late 1930s (as did the total expenditure) and remained steady until 1960 (whereas total expenditure increased after WWII). In general, the cut in social science expenditure was more than proportional. Indeed, in the first decade of operations, the Social Science Division spent on average 18% of total RF expenditure, while in the following decades, the average was 12% (1940s) and 14% (1950s). This is consistent with chapter 3 and the diagnosis that social sciences were in a crisis, or on a lifeline at RF when Willits began his directorship in 1939. Note that, on average, during this 1929-1960

[6] Sources for this chart come from RF Annual Reports, available at the Rockefeller Archive Center, Sleepy Hollow, New York. The 2003 adjusted figures were obtained by using a share of GDP index, available at http://www.eh.net/hmit/compare/. The share of GDP index was preferred to a consumer goods indicator in order to give readers a sense of the size of these amounts as related to the economy at large – and to account for drastic movements in annual GDP variations in the mid 20th century. This choice is particularly relevant for endowment figures, as investment portfolios – made up of equity on stock markets - were likely to vary with nominal GDP.

period, the Social Science Division had a $194 million annual budget (in 2003 adjusted dollars), but that the last decade's average annual spending was down to $93 million. This was the size of the RF "pot" that economist-historians could have aspired to. Let's now compare it to FF "pot" – to see if it was considerably larger for mid 20th century economists, as much literature on the size of the FF endowment would lead us to expect.

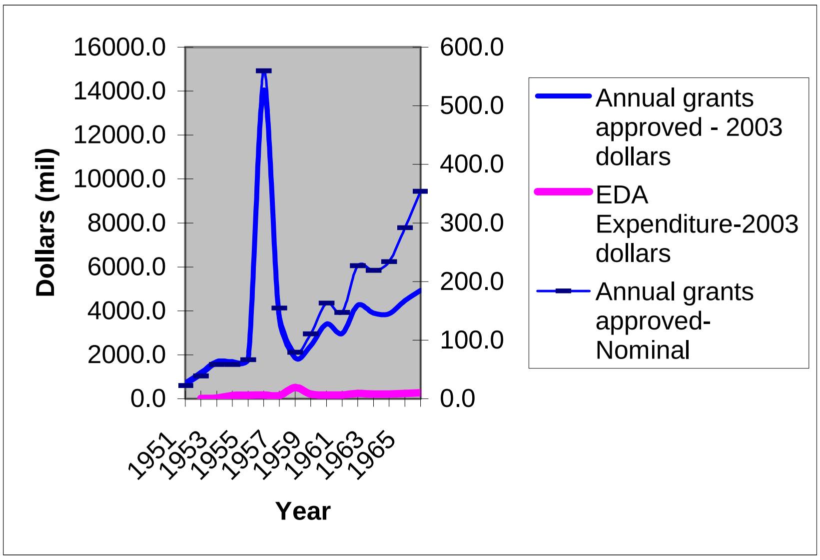
As can be gleaned from chart 2.A, the FF's endowment was indeed very large. In 2003 terms, when the FF appeared on the scene (early 1950s) it had an endowment of $8 billion, by 1960 this figure had climbed to $33 billion. Compare this to RF's evolution from $2.3 to less than $4 billion in this same period, and you get a sense of the potential watershed for academic research induced by the FF's creation!

Chart 2.A: FF total endowment 1951-1966[7]



[7] Sources for this chart come from FF Annual Reports, all available online at http://www.fordfound.org/elibrary/search/browse-year.cfm. The 2003 adjusted figures were computed at http://www.eh.net/hmit/compare/, using the share of GDP index.

Chart 2.B : FF total and social science expenditure, 1950-1966[8]



Here again, on chart 2.B thick lines have their scale on the left and the thin line's values should be read on the right. The EDA budget (minimum available to social scientists) was quite small compared to annual expenditure, and it averaged 6.5% of total expenditure during the 1950-55 period and 5.5% during the 1956-1960 period. This corresponded to $456 million (2003 dollars) in the first 5-year period (averaging $91 million per year) and $1.4 billion in the second 5-year period (averaging $280 million per year). This was indeed much larger than the RF's expenditure. However, while economist-historians may have had access, in principle, to a much larger pot, the reality was quite different – if anything, the RF earlier period was comparatively richer for American economists interested in history.

[8] Sources for this chart come from FF Annual Reports, all available online at http://www.fordfound.org/elibrary/search/browse-year.cfm. The 2003 adjusted figures were computed at http://www.eh.net/hmit/compare/, using the share of GDP index.

3. Grants to economist history

Table 1.A : RF grants to economist history initiatives (nominal)

Start	End Date	Name	Description	Nominal $
1929	1933	International Price History Committee	Cover labor and expenses of individual scholars in 10 countries working on collecting worldwide historical price data	250,000 for 5 years (50,000/year)
1933	One time payment	Ibid.	Emergency grant to cover Committee's debts	75,000
1941	5 years, lasted 10 years (1950)	Committee on Research in Economic History (CREH)	Research grant	300,000 (1st installment was 60,000)
1950	One time payment	Ibid	Interim grant to enable CREH to find other sources of funding	10,000
1952	1957	Harvard Research Center for Entrepreneurial History	Grant to aid in setting up and running center + research	150,000 for 5 years (30,000/year)
1952-58	1952-58	Ibid	Total amount of money received from RF (including above grant)	219,000
Late 40s-early 50s	Late 40s-early 50s	Kuznets' SSRC Committee for Growth studies	Money to pay for international retrospective accounts and Kuznets' time	70,000 (over several appropriations)

Table 1.B : RF grants to economist history initiatives (2003 dollars) [9]

Start	End Date	Name	2003 $[10]
1929	1933	International Price History Committee	3 million
1933	One time payment	Ibid.	1 million
1941	lasted 10 yrs	CREH	2.8 million
1950	One time payt.	Ibid	76,400
1952	1958	Harvard Center for Entrepreneurial History	1.5 million (several grants)
Late 40s	Early 50s	Kuznets' SSRC Committee for Growth studies	535,000 (over several appropriations)

Overall, RF gave approximately $9.5 million (in 2003 terms) to American economist-historians. This was a very large figure compared to the FF's grants. As shown in table 2.B the FF gave $5.6 million (in 2003 terms), a smaller amount both in absolute and relative terms. This divergence may be interpreted in light of the analysis presented in chapter 3 – there seems to have been an element of "agency capture" in the early years of American economist-history, and the noticeable support that RF gave Edwin Gay and his students is certainly not unrelated to their personal ties and connections to Willits and Bezanson. No economist-historian had similar clout inside the Ford Foundation.

[9] To put these amounts in comparable terms, I use a price deflator. This is appropriate since we want to think in terms of salaries (as most research costs seemed to be research wages) – and salaries changed with inflation. Though CPI indices can be misleading when comparing over long periods of time, our earliest and latest grants are separated by less than 4 decades, which is perhaps not too distant for these calculations. The eh.net website describes the CPI benchmark, as "most often used to make comparisons (…) This series tries to compare the cost of things the average household buys such as food, housing, transportation, medical services, etc. (…) It can be interpreted as how much money would you need today to buy an item in the year in question if it had changed in price the same as the average price change."

[10] To obtain this figure I took the total grant amount and converted it in 2003 terms, using the mid year of actual grant length (so if grant lasted from 29 to 33, I took 1931 as the reference year); conversions were done at http://www.eh.net/hmit/compare/ using CPI index.

Table 2.A: FF grants to economist history initiatives

Start	End Date	Name	Description	Nominal $
1953	?	Kuznets' SSRC Committee for Growth studies	To improve and expand research on long term changes in the size and structure of nations	32,200
1953	1965	Ibid	For international retrospective accounts and Kuznets' time; several appropriations (including above)	Nearly 370,000
1954	1959	Kuznets – Johns Hopkins	To support a critical review of the literature and statistics on the comparative economic growth of nations	60,000 (12,000/year)
1963	1966, but extended to late 60s	SSRC Committee for Growth studies	International study of post-war growth, with Moses Abramovitz	300,000 (they planned it to cost 100,000/year)
1958	1963	Parker/Sawyer: Interuniversity grant	For research of scholars in 4 universities (Parker, Sawyer, Abramovitz and Robertson)	125,000 (25,000/year)
1958	lasted until 1970	Gerschenkron workshop	Training graduate students	75,000

Table 2.B: FF grants to economist history initiatives (2003 dollars)

Start	End Date	Name	2003 $
1953	1965	Kuznets' SSRC Committee for Growth studies	Nearly 2.3 million
1954	1959	Kuznets – Johns Hopkins	400,000 (81,000/year)
1963	1966, but extended to late 60s	SSRC Committee for Growth studies (post war growth study)	1.7 million
1958	1963	Parker/Sawyer: Interuniversity grant	780,000
1958	1963, but lasted to 1970	Gerschenkron workshop	440,000

There are no obvious patterns that emerge from this constellation of grants (and certainly no increase in resources available to economist-historians as a result of the FF's appearance in the early 1950s) – both RF and FF awarded a relatively wide range of financing packages, varying in size and duration, and greater amounts of money did not necessarily mean greater amounts of actual support (as the value depended on the number of researchers involved, and the projected length of the project). Yet, one can see that the largest grants were Edwin Gay's International History of Prices (which was budgeted at $600,000/annum but ended up costing $800,000/annum, in 2003 terms); the CREH (budgeted at $560,000/annum, but ended up lasting longer and costing approximately $350,000 per year); and the numerous grants for Simon Kuznets' retrospective accounts initiatives (financed via the SSRC), which seemed to have cost about $175,000/year. Recall from chapter 4 that Kuznets' work was rather cheap, considering the number of people and places it involved, and compared to previous economist-history initiatives.

It is difficult to evaluate the relative size and value of these grants without a more substantive benchmark: indeed, different grants were supposed to cover different numbers of scholars, and it may well be that the larger grants simply paid for more people. In addition, once one went beyond models where researchers worked alone, or with small amounts of coordination, it may well be that communication costs (institutions, means of sharing, travel etc.) greatly increased overall costs – so a rough measure of overhead may be useful to compare these grants.

4. **Benchmark 1: How much did it cost to hire a junior researcher?**

In 1954, Kuznets applied to the FF for a grant to cover his annual expenses associated with his work on comparative quantitative growth. He asked for $5,000 per year (1954 dollars) for his main assistant (Lilian Epstein who had been helping him with his statistics for 20 years) and $4000 for a junior researcher (student or new Ph.D. with a small allowance for travel). In 2003 terms, he was asking for $34,000 for Epstein, and $27,000 for the Ph.D and travel allowance (i.e the Ph.D salary was lower than this figure). Compare this to a request from Arthur Cole, who in 1950 asked for $16,000 to aid 6 Ph.D. students in their final year. If divided equally, this would have amounted to $2600 per year, which corresponds to $20,000 in 2003 terms. Finally, compare it to the fellowships awarded by Gerschenkron at his Harvard Workshop – all students were given $3500 per year (which if you take 1963 as the mid-period benchmark year corresponds to $21,000). In other words, it seems that Ph.D. labor cost no more than $27,000 per year and was probably lower, around $21,000.[11]

[11]Letter from Lowell J. Reed, president of Johns Hopkins University, to T.H. Carroll, September 16th 1954, FF - PA 55-28; Letter from Alexander Gerschenkron to Neil W. Chamberlain, January 3rd 1958, FF - PA 59-26; Letter from Alexander Gerschenkron to Joseph McDaniel , May 10th 1963, FF - PA 59-26; Letter from Arthur Cole to William Pew, undated, but filed in 1950, RAC-RF, RG 1.1, Series 200, Box 397, Folder 4709.

COST OF JUNIOR RESEARCH LABOR in 1950-1965 period in U.S.: $21,000/annum (2003 dollars)

5. Benchmark 2: How much did it cost to coordinate more than one researcher?

In the same 1954 proposal, Kuznets estimated the total cost of research (excluding his salary, but including his assistants) to be $12,000 per year – i.e., if you subtract $9000 already mentioned above for Epstein and the Ph.D student, he added $3000/year for typing, printing, publishing, correspondence etc. In 2003 terms, this overhead amounts to $20,000. Compare this to Alexander Gerschenkron's proposal for the Harvard workshop – whose expenses he estimated at $20,000 per year, $5000 of which represented publication subsidies, university overhead and general secretarial assistance. Using 1963 as the mid-period benchmark year, we obtain the workshop expenses at $30,000, in 2003 terms. Gerschenkron obtained this figure by assuming an average of 3.5 students per year – so it would seem that Kuznets and Gerschenkron's estimates were consistent: Kuznets needed $20,000 to coordinate the work of 3 people (himself, Epstein and junior researcher); while Gerschenkron needed $30,000 to run a center with 4.5 people (including himself) – i.e they both budgeted $6600 per person.

COST OF CENTER WITH MORE THAN ONE PERSON – BUT LESS THAN 10 and MINIMAL OVERHEAD EXCLUDING SALARIES in 1950-1965 period in U.S. : $6,000/annum/person (2003 dollars)

COST PER RESEARCHER OF COORDINATED RESEARCH, INCLUDING SALARIES OF JUNIOR RESEARCHERS: $6,000+ [$21,000-$27,000]= [$27,000-$33,000]/annum/person (2003 dollars)

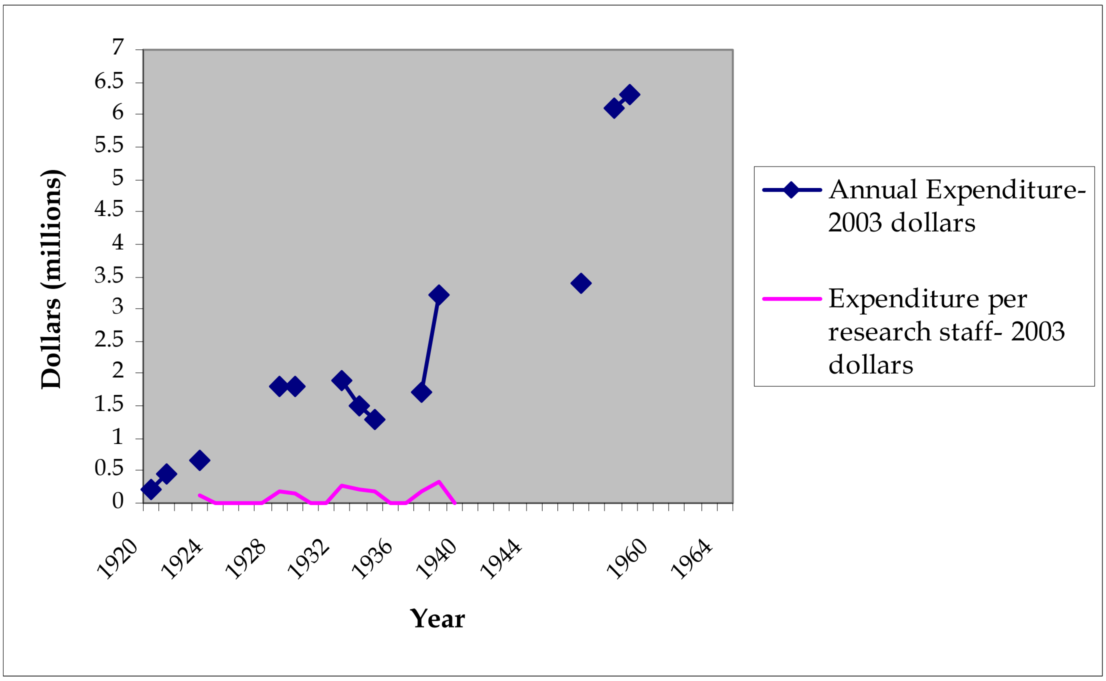
Given these benchmarks, the CREH could have hired 13 researchers per year (over 10 years), or twice as many if they had chosen to spend all the money in the first 5 years, as the grant originally called for (as seen in Table 1.B they spent $ 3.6 million in 10 years, in 2003 terms). This is exactly what the CREH did – in a 1946 report to the RF (normally their end of grant report, but actually a request to extend remaining funds for an additional 5 years) they mentioned having "26 young social scientists" at work and had spent half their funds.[12]

Based on these benchmarks, the Price History project could have hired and coordinated 30 junior researchers for 5 years. They actually employed nearly 20 researchers, though it is unclear whether all were given salaries from the RF money. In case they spent all the money that had originally been earmarked for them, and had to ask for an emergency grant because they had incurred noticeable debt! Could it be that their large scale, empirical project may have cost more per researcher? A useful benchmark for estimating the cost of such projects is the NBER.

[12] Exploring America's economic contribution, May 1945, RAC-RF, RG 1.1, Series 200, Box 397, Folder 4705. These are very rough calculations and we don't know if they employed the 26 scholars throughout the entire 5-year period; however figures are consistent with a research project whose main expense was young scholar research labor.

6. **Benchmark 3: How much did it cost to run the National Bureau of Economic Research (NBER)?**

Chart 3: Expenditure at the NBER (1920-1965) – in 2003 terms[13]



The first observation to draw from this chart is the gradual increase of NBER expenses - aside from a temporary contraction during the depression- reflecting large needs of the extensive empirical inquiry into business cycles (though lack of organization and financial laxity of the first directors – Mitchell, Burns and Fabricant – has also been blamed).[14] The steep ascent after 1960 may

[13] Source: expenditure (and income) data can be found, for a selection of years, in Rutherford (2004); the number of research staff at the NBER was taken from the quarterly bulletins, published by the NBER since 1922. I only had access to 1922-1940 bulletins on the web http://www.nber.org/newsbulletin/. Both series have been adjusted to reflect 2003-equivalent dollar values, using a CPI index– the conversion was made at http://www.eh.net/hmit/compare/.

[14] For an account of the NBER's financial difficulties, see Rutherford (2005).

reflect the increasing cost of hiring qualified labor, as academic and government salaries were increasing.[15]

The second observation pertains to the relatively stable cost per research staff – in the period 1920-1940 it averaged $200,000.[16] Note that this amount does not reflect actual salaries – but rather total NBER expenditures divided by the number of scholars who worked there full time, and were authors of the NBER publications (i.e. presumably including their salaries and overhead costs such as support staff, offices, printing services etc.). This is much higher than the estimated $27,000 -$33,000 per young researcher in the Gerschenkron, Kuznets examples. It is quite likely that NBER researchers were earning a better income than young scholars, however it is difficult to establish how much more they earned. As an indication, in the 1930s, most senior economists who taught at "very good schools" made around $12,000/annum (which translates to $150,000 in 2003 terms, i.e. 7 or 8 times more than the Ph.D stipend).[17] When George Stigler was appointed to the Walgreen Chair at the University of Chicago in 1958 (a Business School chair), his salary was $25,000 (which translates to $160,000 in 2003 terms) – though he mentioned in his autobiography that this salary was the "talk of the economics profession".[18] Hence if we take $160,000 as the highest possible salary for top quality researchers, the NBER still spent $40,000 per annum on additional costs per capita, nearly ten times as much as either Kuznets or Gerschenkron had budgeted (and this is a minimum estimate).

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[15] Fabricant made note of this in the mid 1960s, see Ibid.
[16] This is consistent with anecdotal evidence of NBER costs; for example, in 1937 Wesley Mitchell applied to the RF for funds to cover 6 projects (the work of 5 men) and asked for $75,000 a year – he thus anticipated costs to be at least $15,000 per man – in 2003 terms, this amounts to $200,000 per person.
[17] This figure was drawn from Malcolm Rutherford's research on Institutionalism in the Interwar period.
[18] Stigler (1988), 157.

Interlude: How much was that?

COST OF A LARGE COORDINATED EMPIRICAL INITIATIVE INCLUDING SALARIES – in the 1930-1950 period in U.S. : $200,000/annum/person (2003 dollars)

Using these more generous estimates of salaries and costs per researcher in large measurement projects, one can get a new perspective on the expenses of the Price Project. If their costs were at all comparable to the NBER (which is a difficult claim to make as the Price Project did not have the real–estate expenses incurred by the NBER, so it's better to think of this figure as an upper bound), with $4 million, Gay and Beveridge could have hired at least 4 senior researchers for the entire 5 year empirical project (and support them with secretarial, publication, research assistance resources). This is a meaningful figure because it gives an indication of the real source of cost: data collection. The Price project was discontinued in 1933, as it had proved unable to control costs, and scholars were spending much more time accumulating data in archives than had originally been expected.[19] As the much longer experience of the NBER seemed to have proven, this was relatively standard in quantitative data collection projects. And as Malcolm Rutherford has argued, the patience and reiterated commitment of the RF was not to be discounted in explaining the achievements of the NBER, as these were quite slow in coming. In retrospect, the Price Project seems to have been of the same nature as many NBER projects, and its early discontinuation (compared to all other NBER projects, most notably Mitchell's business cycle work) may have nipped it in the bud. But one may also wish to compare it to the NBER on a more social and contextual level – it was a grant typical of the 1920s, an epoch characterized by many as the crest of enthusiasm for empirical work in the social sciences, and a relatively new and sizeable commitment from foundations to sponsor research of this sort.[20]

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[19] Rutherford (2003).
[20] See for example Rutherford (2005).

In other words, for certain empirical initiatives (in particular large scale, not necessarily international, but involving data collection, compilation and presentation from more than one source) money was important, but time commitment was equally, if not more crucial. Such projects gained substance and credibility after several years, and were often the product of a few scholars rather than armies of data collectors. Their work seemed to require more than the habitual 5-year moratorium offered by practically every foundation grant. A comparison between the short lived Price Project, and the much longer lived NBER and SSRC Quantitative Growth projects highlights this point.

A comparison between the cost of running the NBER and Kuznets' projects highlights a more historical point. Compared to Mitchell's endeavor, Kuznets' project seemed incredibly cheap, in spite of the fact that it involved, at some time or another, dozens of scholars in many different countries (as we saw in chapter 4.) These studies lay the foundations for the work that earned Kuznets the 1971 Nobel Memorial Prize. The striking difference between Kuznets and Mitchell's research empires was the infrastructure that Kuznets was able to leverage: many of his collaborators had appointments in existing statistical institutes (Malinvaud at l'INSEE for example), and in the American case, the data had already been, and was continuing to be collected by the NBER. In other words, Kuznets' research may have cost less to the FF, but it was building off of a radical change in the economics landscape brought about by WWII – the rapid expansion of national income rationales, and official (national or international) statistical institutes.

7. France: what could $10, 000/ year and $ 1million buy?

In chapter 5 we read about several different grants from American foundations to social science research in France – a sample of these is listed in Table 3.

Table 3: Sample of RF and FF grants in France

Starts	End	Name	Description	Amount (current $)
1947	3 years	RF VI e section	To help establish the 6eme section – according to Mazon it was only enough to cover 1/4th of the 6eme section's operating expenses in this period.	30,000 or 10,000/year
1952	2 years	RF VI e section	Renewal of previous grant under guarantees that no money would be used for operating expenses	13,500 or 7,750/year
1949	N/a	RF ISEA	The RF awarded several grants, which amounted to the following average figure	10,000- to 15,000 /year
1959	N/a	FF -MSH	To help build and run an interdisciplinary social science unit + library; the French government matched this grant with $2 million	$1,000,000
1955 or 1958?	6 years	FF - ISEA	Research and operations grant	50,000 or 8, 200/year

Aside from the exceptionally large MSH appropriation, these grants all averaged to $8-15,000 per year. What buying power did this represent on the French academic labor market in the early and late 1950s?

In Table 4, we see the French Franc equivalent of $10,000 a year in the early and late 1950s

Table 4: Franc equivalents to dollar grants, 1950, 1959[21]

	1950	1959
$10,000	3.5 million Francs	4,9 million Francs
$1,000,000		491 million Francs

In Table 5 we see the cost of academic labor in France:

Table 5: Salaries for professors, research unit directors and secretaries at the College de France and the VI e Section in the 1950s[22]

	College de France Annual salary and (number of positions)		VI e Section: annual salary and (number of positions)	
	1950	1959	1950	1959
Senior Professor	1.1 Million (12)	2.8 Million (47)	N/a	N/a
Sous-Directeur de Laboratoire	1 Million (30)	(12)	N/a	N/a
Directeur	N/a	N/a	805,000 (2)	1.9 Million (31)
Secretary	235,000 to 470,000 (3)			436,000 (2)

In other words, when l'ISEA or the *VI e Section* received $10,000 in 1950, that was the equivalent of three times the annual salary of a senior professor (like Perroux or Febvre who were both professors at the *College de France*). By 1959, this was only equivalent to twice their salaries. Compared to Stigler's American salary in 1958, a French professor at the *College de France* was paid 4 times less.

[21] Using an exchange rate calculator at http://www.eh.net/hmit/exchangerates/ where we find that $1U.S. bought 350 old French Francs in 1950, and 491 in 1959. The exchange rate was stable from 1950 to 1956, and the French Franc began losing value after 1956.

[22] All figures from from Education Nationale (1950), *Budget Voté de 1950*, Paris: Imprimerie Nationale, chapitre 31.14; Education Nationale (1959), *Budget Voté de 1959*, Paris: Imprimerie Nationale, chapitre 31.14. Available at Archives du Ministère de l'Education Nationale, 101 rue de Grenelle, 75007, Paris.

As for the FF's gift of $1 million in 1959 (491 million French Francs) it was equivalent to 1/1000th of the French Ministry of Education's annual budget in 1959 (which covered all education in France, from kindergarten to higher education, including vocational training), but it was also five times the amount paid in salaries to *all* staff of the *VI e Section* (full time directors, part time directors, research students, secretaries and even concierges and errand boys). Indeed in 1959, the French ministry of Education had a budget of 481 billon French Francs, and the *VI e Section* spent 93 million French Francs on salaries. In other words, the money these foundations gave either to Perroux or to Braudel gave them considerable buying power on the academic scene.

CHAPTER 8.

CONCLUSION: LEARNING FROM THE PAST?

1. A perennial debate on the nature of reliable evidence

Why did mid 20th century American economists show such an interest for historical investigation and how did the cliometric revolution change the way they wrote history? Chapter 2 suggested that both the commitment to history, and debates around cliometrics were related to ongoing questions about empiricism in economics. Most economic historians both in France and the U.S believed that the only scientific path to economic knowledge was the observation of long-term phenomena – i.e. history (recall Figure 2.3 depicted economic history on the left side of the economic space). They may have contrasted this choice with abstract and speculative modes of reasoning (on the right side), but as Figure 2.3 aimed to show, this was more a matter of speech and rhetoric than a representation of the actual spectrum of positions in the debate over scientificity. As the circular figure suggested, there never was a dichotomy between deductivists and inductivists but rather, as Alexander Gerschenkron told the Ford Foundation in 1956 "a whole spectrum of levels of abstraction" (see chapter 6).

By thinking in terms of "spectrum" rather than dichotomy, this thesis has uncovered one of the main theatres of debate among economic historians. In the 1940s, 50s and 60s, the issue was not whether observation should come before or after theory (as we would expect in a dichotomous world), but rather it revolved around diverging notions of reliable evidence. From the 1940 Rockefeller

Foundation round-table meeting to the mid 1960s railroad debates, scholars disagreed about the nature of trustworthy and useful "facts". What information could or should the economic historian use in his empirical quest about past events? This was not the first or only instance when economists debated such questions (for example 18th century political arthimetists – the ancestors of national accountants – had triggered similar controversies) but the mid 20th century, in particular in America, does stand out as a moment of unusual recurrence in these questions.

This thesis began with a disagreement between the CREH economist-historians (Edwin Gay and his students) and Kuznets. As we progressed through Kuznets' battles with Rostow, Marczewski's run-ins with Chaunu, and Fogel's disputes with Chandler we saw emerge a division between observers who were willing to put estimates in their bag of evidence, and those who were not. For the participants, this was never a trivial issue, and the fact that they agreed on the big picture (that economics should involve a large investment in empirical investigation, in particular of long term phenomena) only seemed to make the debates more critical and virulent. In other words, debates raged across sides of Figure 2.3, but also within the left side. Thus, there is a case for moving from a Figure 2.3 picture of the perennial debate on empiricism, to the following view – which essentially is a "zoom" onto the left side of Figure 2.3.

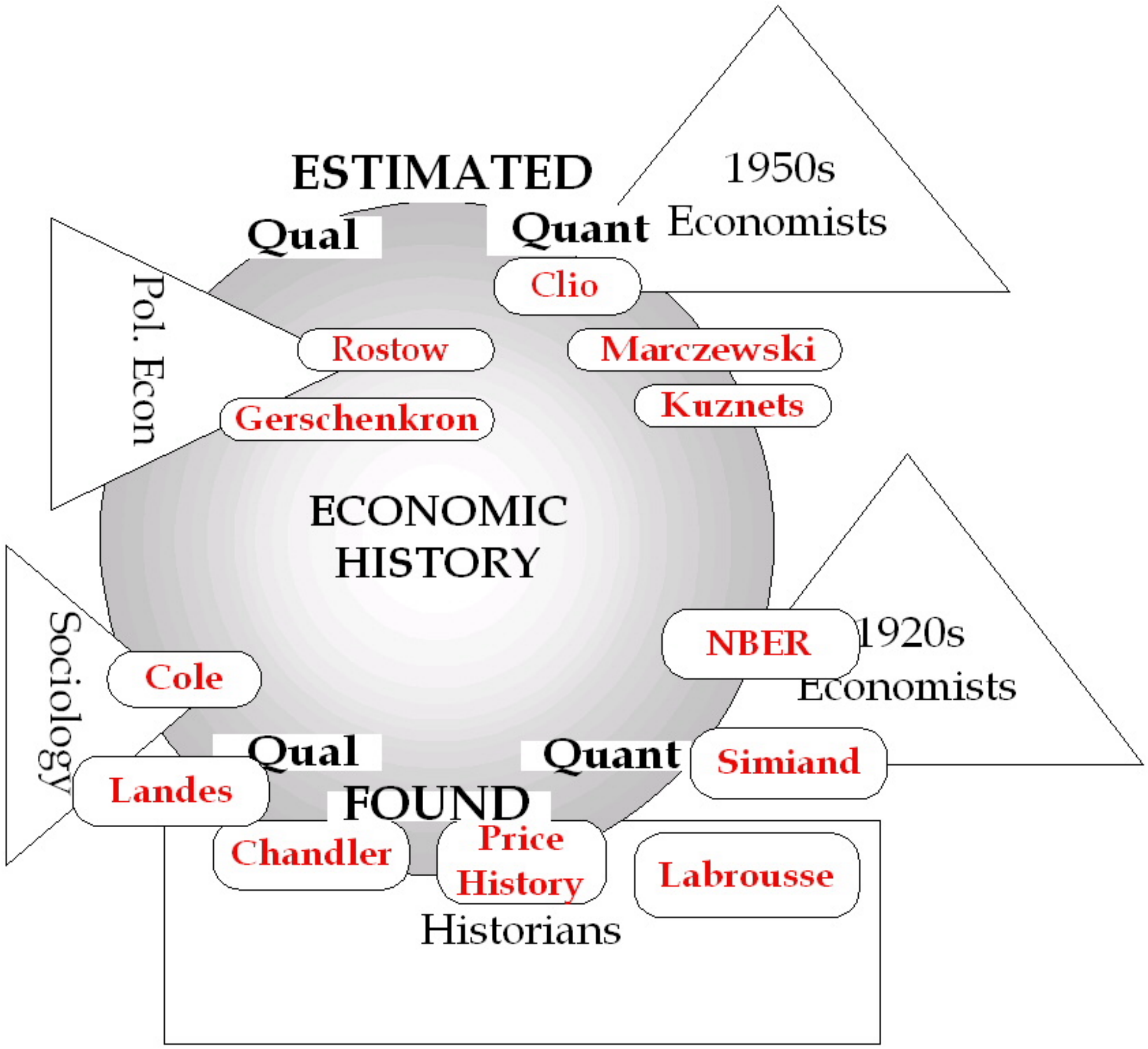
In Figure 8.1 the main line of division is North / South – with "estimators" like Marczewski and the cliometricians on the North side of economic history, and those who relied on found data (in archives or almanacs), like Simiand or the international price project historians on the South side. Notice that this cleavage has nothing to do with qualitative versus quantitative data (represented on an east / west divide) – and this should dispel any view that retrospective accounts or cliometrics were innovative because they introduced quantification into economic history. Gay and Beveridge's price history project, Simiand's history of

wages, and Labrousse's quantitative studies of agricultural prices are a reminder that economic history was already quantitative in the early 20th century.
As we saw in chapters 4, 5 and 7, the issue was not whether or not to quantify, but rather deciding which numbers were reliable, or better than others.

Also notice that the North / South division does not overlap with theoretical versus empirical investigation (i.e. the inductive / deductive dichotomy). On both hemispheres you find researchers who expected to *have* nature speak to them – for example Kuznets or the price historians - just as you find those who wanted to *make* nature speak – for example Gerschenkron who adopted the view that all human behavior could be explained with elementary economic principles, Rostow who fit evidence into his stages theory, Marczewski who fit French economic history into a Keynesian mold or Cole who fit American history into an evolutionary view of the entrepreneurial system.
This is the meaning of the triangles that break into the economic history space: for example, Cole used sociological theory to make American history speak about entrepreneurial systems. This should dispel any views that cliometricians brought "theory" (in the sense of a body of general principles about individual and collective behavior) to economic history – as Lamoreaux pointed out in her brief history of the cliometric revolution, many of the so-called "old" economic historians were scholars with theories of their own, just not the same theories as cliometricians.

In other words, Figure 8.1 is both a zoom and a fractal of Figure 2.3. It is a zoom as we are looking at economic history only (including economist – history) - not economics at large - and using this space to show debates on the left side of the spectrum (the empirical side). It is a fractal on two counts. First in the literal sense of a fractal: reproducing at a smaller scale patterns that exist at a larger scale.

Figure 8.1: Perennial debates on the nature of reliable evidence in economic history



As mentioned above, we find a range of methodological positions extending from full empiricism (NBER-like hopes that the data will speak for themselves) to abstract speculation (recall from chapter 3 the British graduate student who told RF officers that the RCEntrepH did nothing but speculate about the relationship between entrepreneurial optimism and growth). However these extremes are not depicted on a West-East dimension as they were in Figure 2.3. The point is rather that questions of reliable evidence overlapped with more general issues of scientific method such as the proper origin of theory (should it spring from the evidence as Mitchell hoped, or should it come from logical bases as Solow maintained?).

Secondly, Figure 8.1 is a fractal of Figure 2.3 because it shows that notions of reliable evidence also exist on a spectrum, and not in a dichotomous space. In other words there are different types of estimations, just as there are different types of "found" data. For mid 20th century economic historians there was no clear divide between found and estimated data, rather a constellation of each. For example, while many American economic historians seemed willing to accept Kuznetsian estimates, they were very critical of "back of the envelope" estimates (recall Gallman's negative impression of Conrad and Meyer's 1957 slavery paper). This may have been linked to the uneasiness of conflating the world as it was with the world as it should be (the trademark of an OR mind), yet participants tended not to express their doubts in these terms, preferring to say that this was sloppy data collection. The fact that they could see nuances among estimates warns us against fitting all scholars into one of two categories (finders and estimators) though there clearly were some scholars who rejected estimation outright (like Burns, for example, who found Kuznets' data habits mercenary!).

Nuances also existed among finders. In France, when Vilar cited Simiand to remind Marczewski that the best guarantee of data quality was the process by which it had been recorded: the more automatic the process and the least vulnerable to the choices of any man, the better the resulting data. Thus accounts from a large business with standardized procedures, parish records or tax receipts were reliable sources, provided the historian had a clear view of the collection process. "Old" American economic historians seemed to share this view, and this was a reason why numbers were often privileged (recall that Gay had written a quantitative thesis on enclosures, and that Usher had sung the praise of quantification in the 1930s). These considerations often surfaced in the study of slavery, where American economic historians did not await cliometricians to argue that there was difference between numbers (accounts from a plantation, prices, interest rates, balance of trade) and autobiographical accounts from plantation owners, even though both were technically “found”.

The progress from Figure 2.3 to Figure 8.1 constitutes this thesis’ main contribution. It helps us move our understanding of economic methodological debates away from clear dichotomies like induction versus deduction, or the choice of quantitative versus qualitative procedures. Concretely, the issue of whether or not estimated data could serve as reliable, useful evidence was a matter of much debate among economic historians, and continues to divide them.

2. Diverging outcomes in France and the U.S.

Whether this concentration of debates on reliable evidence was the cause or the consequence of economists’ mid 20th century interest in history is a moot point, as the comparison between France and the U.S. revealed. On the one hand there were clear instances when economists’ interest in history was triggered by questions that had nothing to do with such epistemological musings – such as for example, Jacques Mayer’s Marxist impulse to “test” the law of the falling rate of profit. On the other hand, there were times when epistemological questions about the proper place and nature of evidence did push economists into becoming economic historians – this was Simon Kuznets’ path. The more important point is that once economists had opened the economic history box – by creating associations, committees, journals and drawing the attention of foundations – the debates seemed to take a life of their own. In other words, once there were numerous economists competing for economic history (as we saw in chapter 6), debates on legitimate evidence were bound to resurface.

Given the very different outcomes of these debates on reliable evidence in France and the U.S. the reader will wonder about the factors that made retrospective accounting and cliometrics possible on American soil, while French economic historians shunned both. The reader must first recognize the legitimacy of each position – the fact that debates about reliable evidence were recurrent and resulted in different outcomes at different times and places is an indication that "each side had a point". The reader may want to ask herself about the conditions that make estimates, rough or not, different from data lying in old account books. Are they both constructs, and thus equally shaped by their "manufacturing conditions"? Is reliability based on consistency (data cross checks) in any way superior to confidence based on critical source examination and triangulation (recall chapter 5)? These are not simple questions and the rich history of objectivity and quantification now available to the 21st century reader suggests that the social sciences have been through more than one transformation in their notions of objectivity and that the current situation in economist-history (i.e. cliometrics) that accepts estimation as a necessary procedure (whose risks can be hedged with sensitivity analysis) may not last indefinitely (Kuhn would say this is a defining feature of pre-paradigmatic science).[1]

Having acknowledged the epistemological legitimacy of these different positions on the nature of reliable evidence, we can now recognize that current divisions between historians and economists within economic history were not the cause but the result of such debates. *Annalistes* did not shun estimation *because* they were "historians". Quite the contrary, they were following the methodological precepts of Simiand, who was considered an "economist" in the 1930s. The fact that Malinvaud, an "economist" and "econometrician" who worked on historical accounts with Kuznets and Abramovitz in the 1960s could, in a 2004 interview, recall thinking that Simiand's work was "very strange" is evidence of a change in economists' notion of objectivity.[2] This post-war

[1] Daston (1992); Porter (1995); Desrosières (2000).
[2] Edmond Malinvaud, Interviewed by Cristel de Rouvray, Paris, January 2004.

transformation reconfigured the lines of division between disciplines and entailed changes in the meaning of the labels “historian” and “economist”.

Nevertheless we are still confronted with the task of explaining how and why “estimators” took over economic history in the U.S. at the exact time (mid 1960s) that “finders” in France were blossoming. This brings us to the delicate task of tying scientific debates to the social and political world in which they take place. Among the external factors considered in previous chapters was a certain degree of happenstance. For example the fact that Lane was such good friends with Braudel, and that he happened to be the main scout for the Ford Foundation in France in the 1950s was a stroke of luck for Braudel and certainly increased his chances of getting substantial American funding, thus placing him and his acolytes in a much more powerful position than Marczewski and his colleagues. Yet this contingency should not obstruct the noticeable impact of both the “growth and development” craze in the U.S and American economists’ wartime experience. The first contributed to draw more economists into economic history and increase their sense of urgency, while the second accounted for much of the evolution from Kuznestian to cliometric methods. Neither of these had any counterpart in France.

The demand for “development” knowledge created a clear opportunity for American economic historians to appeal to philanthropic sponsors (Kuznets was the first to recognize this, but Rostow, Gerschenkron, the RCEntrepH, the CREH and Parker / Sawyer all attempted to exploit it in the 1950s). The availability of considerable funds made it possible for numerous young scholars to enter the field, but it also tied this new generation to the RF and the FF’s agenda, in particular their time horizons. In Kuznets and Marczweski’s views there was a clear feeling that Ashley’s, Gay’s, Willits’ and the *Annalistes*’ call for research programs that would last decades was unreasonable, as results needed to be generated more quickly. This time compression was an element from “outside” economic history, and may have been tied to the growing influence of development agencies and other international organizations whose task was to aid development in poor countries. But it was not directly imposed onto economic historians (though Marczewski and Kuznets may have spoken as if they were directly addressing this public concern for helping developing nations). Rather it seems to have been mediated through Foundation agents. Thus foundation officers played an interesting intermediary role in our narrative: neither fully inside nor outside science, they imported shreds of political and economic culture into the dynamics of research, and increased economist-historians' sense of urgency and pragmatism. They did not have this effect on the MSH (though they did have it on l'ISEA and on Marczewski via Kuznets), an indication that their influence was directly proportional to their grantees' degree of financial dependence. Note that this is not a point about Foundations' strong agency; the argument is not that the Ford Foundation preferred estimation or Gerschenkronian economic history to Chandler or Landes' approach. Quite the contrary, chapters 5 and 6 showed that FF officers were not really equipped to see the nuances inside economic history, in particular inside economist-history. Instead, the grantees they chose voluntarily adopted the constraints of a seemingly more urgent research schedule and this increased their tolerance for estimates.

Foundation agents did not play such a crucial role for the migration from retrospective accounts towards cliometrics. Instead we identified American economists' experience during WWII and ensuing post-war ontology as an important factor in accounting for the "revolution". In particular, the willingness to think in an operations research mindset - combining a commitment to conclusions framed in quantitative terms, the muddling of lines between "the world as it is" and the "world as it should be" and the willingness to estimate data in a "back of the envelope", "order of magnitude" sort of way as we saw in chapter 7 – was a clear legacy from the war, and had been transmitted to young economists in the revamping of graduate education that occurred in the 1950s.

From this perspective it comes as no surprise that cliometrics did not migrate to France in the 1960s and 1970s (while *Annales* was still strong). If Kuznetsian estimations were a matter of real epistemological debate, if there was much less sense of urgency in the economic history community (immune from development talk) and if French economic historians were completely isolated from an OR culture then the order of magnitude work so crucial to the crystallizing moment of cliometrics was not going to stick! Indeed, recall from chapter 5 that among all types of estimation Toutain used in 1961, Chaunu had identified order of magnitude estimates to be “worse than nothing at all”. As neither Kuznetsian nor OR estimates were acceptable to *Annalistes*, other aspects of the cliometric agenda (theoretical modeling, econometric hypothesis testing) were ignored in France.

3. Using the past to say something about the present; or using the present to say something about the past?

Chapter 1 began with the observation that the mid 20th century stood out as a unique time for American economist-history, compared to earlier and later periods. In chapter 3 we discovered that until the 1930s economic history was blended into economics as proponents of historical views co-existed with other types of economists. This is consistent with a picture of a pluralistic economics in the U.S. before WWII, and the emergence of a relatively autonomous field of economic history right after the war can be seen as one of the many facets of the subsequent transformation. As the discipline became less pluralistic, those who felt threatened tried to section off a piece of it for themselves; yet once the transformation was completed it’s no surprise that young economists should have taken over this space. In other words, the cliometric revolution can be used as a point in the time line of 20th century American economics: by the mid 1960s the transformation marking the end of pluralism had been sealed.

When Cole, Bezanson, and their acolytes (Redlich, Aitken) commented on the state of economics and economic history in the 1970s they despaired that their efforts to create a separate space for economic history had turned against them. In their eyes, cliometrics was not what economists interested in history should be doing; in particular they lamented the fact that economic historians were no longer digging into the past to say something about long-term phenomena and social dynamics.

The issue of whether or not the past can ever yield lessons about the present was conspicuously absent from debates around economic history. The *Methodenstreit* is remembered for having pitted observers against hypothetico-deductivists, not presentist versus past-minded economists, as if observation of the past were the same as observation of the present, only at a larger scale (more data). This is certainly how Kuznets justified his interest in history (a long term view). Neither Schmoller nor Kuznets had any strong historical stance – i.e. they did not ascribe to an evolutionary or stagist view of economic dynamics. Rostow and Arthur Cole may have leaned more in this direction, believing that societies could only be understood in the context of where they came from and where they were going, yet they were seldom challenged on the historical nature of their theories, the focus of the debate lying instead around the validity of their empirics.[3] In the 1950s and 60s, economist-historians did not really have a consensus position on why the past had something to do with the present.

To a certain extent the cliometric revolution put a temporary hold on the necessity to confront this question. Indeed by replacing a "history is good for economics" with the "economics is good for history" view, they buried the claim that the past had something to say about the present, and instead suggested that the present had something to say about the past. This is one of the definitions of

[3] In economist history, the crucial debate on historicity – on whether societies function according to mechanic or time-sensitive principles - would become a standard feature of the literature much later, largely thanks to Paul David's work on path dependence in the mid 1980s- David (1985).

"Whig history": starting from the present and looking back for pieces of the past that are similar or seem to have led to the present situation. This is not an unreasonable method (though it has its limitations). Indeed, if one considers that contemporary economic theory is the product of past ideas and events, we are very likely to find instances where grain markets, central banks and insurers did function according to what we recognize as modern day theoretical principles. But as all Whig histories it has the double handicap of ignoring events that did not make it to the contemporary world, and not inquiring into the factors that made the survival of one way of thinking, or one way of transacting more likely than the others.

While there may have been fundamental differences between old economic historians' and cliometricians' agenda, the cliometric interlude appears to have been more beneficial (to the old agenda) than Cole, Willits and Bezanson could glean from their late 1970s vantage point. For one thing, the cliometric movement temporarily increased the number of professorships in economic history within economics departments. For another, it's leading practitioners evolved to a much more blended position in the 1980s. Paul David and Douglass North are notable examples of scholars whose started out in cliometrics, but later contributed general ideas (path dependence and new institutionalism) that are today part of a standard economists' conceptual toolkit, thus effectively adding some historical perspective to many economists' work. Hence, from a 21st century perspective, the story told in these chapters (the mid 20th century peak in separate economic history) is a parenthesis in an otherwise blended call for historical perspective in economics. It also acts as a reminder that economics is a science much more like astronomy, evolutionary biology and geography (sciences that study events over time) than one like mechanics.

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Appendix 1.

Archives and Collections Cited in this Thesis

U.S.

Anne Bezanson Papers, held by Doris Souza (her niece), Boston, Massachusetts

Ford Foundation Archives, 320 East 43rd Street, New York, NY: http://www.fordfound.org

Hagley Museum and Library Archives, Wilmington, Delaware: http://www.hagley.lib.de.us/manuscripts.html
Economic History Association Papers

Harvard University Archives, Boston, Massachusetts: http://hul.harvard.edu/huarc/
Arthur Cole Papers
Simon Kuznets Papers
Alexander Gerschenkron Papers

Rockefeller Archive Center, Sleepy Hollow, New York: http://archive.rockefeller.edu/
Rockefeller Foundation Archives
SSRC Archives
Officer profiles

Rare Book, Manuscript, and Special Collections Library, Duke University, Chapel Hill, North Carolina: http://scriptorium.lib.duke.edu/
American Economic Association Papers
Douglass North Papers
Earl J. Hamilton Papers

University of Pennsylvania Archives, Philadelphia, Pennsylvania: http://www.archives.upenn.edu/
Industrial Research Department (IRD) papers

France

Archives de l'Ecole des Hautes Etudes en Sciences Sociales, 54 Blvd. Raspail, 75006, Paris
Fichiers Centre de Recherche Historique
Fichiers ISEA
Fichiers Editions de l'EHESS (aux Archives Nationales, Fontainbleau)

Archives du Ministere de l'Education Nationale, 110 rue de Grenelle, 75007, Paris
Budgets 1950, 1955, 1959

APPENDIX 2:
People Interviewed in the U.S. and France

U.S.
Alfred Chandler (June 2004, at his home, Boston, Massachussets)
Paul David (January 2004, at his home in Palo Alto, California)
Lance Davis (January 2004, at Cal Tech, Pasadena, California)
Richard Easterlin (January 2004, at the ASSA meetings in San Diego, California)
Alexander Field (January 2004, at the ASSA meetings in San Diego, California)
Robert Fogel (February 2004, via phone)
Claudia Goldin (January 2004, at the ASSA meetings in San Diego, California)
Deidra McCloskey (January 2004, at the ASSA meetings in San Diego, California)
Douglass North (February 2004, via phone)
Hugh Rockoff (January 2004, at the ASSA meetings in San Diego, California)
Anna J. Schwatz (June 2004, at the NBER, NY, NY)
Robert Solow (June 2004, at his home, Boston, Massachussets)
Barbara Solow (June 2004, at her home, Boston, Massachussets)
Peter Temin (Fall 2004, at the LSE, London)
Gavin Wright (September 2004, at Stanford, California)

France
Maurice Aymard (November 2003, at l'EHESS, Paris)
Pierre Chaunu (January 2004 and November 2004, at his house, Caen)
Francois Crouzet (November 2003, at his home, Paris)
Patrick Fridenson (January 2004, at l'EHESS, Paris)
Jean Yves Grenier (January 2004, at l'EHESS, Paris)
M. Heffer (January 2004, at l'EHESS, Paris)
Emmanuel Leroy-Ladurie (January 2004, at his house, Paris)
Maurice Levy-Leboyer (November 2003, at his house, Paris)
Edmond Malinvaud (January 2004, at his home, Paris)
Christian Morrisson (January 2004, at his home, Paris)
Jean Claude Perrot (Novemner 2003, at his home, Paris)
Jean Claude Toutain (March 2003, at my home, Paris)
Andre Vanoli (September 2003, at l'INSEE, Paris)

Sample Letter sent to interviewees

Dear …

I am writing in the hope that you will consider being interviewed for my research in the history of American economic history. I would be incredibly grateful if you could find the time to speak with me during …

I am a PhD student at the LSE, where I work with Professor Mary Morgan on the evolution of economic history in France and the United States, 1940-1980. I am currently in my third year of research, wrapping up my primary work- which has consisted in visits to archives (so far I have examined the records of the Rockefeller Foundation, the Ford Foundation, the AEA, the EHA, Harvard, the University of Pennsylvania and the Parisian EHESS) and interviews (so far I have conducted interviews of Annales historians and French economists active in the period from 1950 to 1970). I use this information to draw as complete (and lively) a picture of the stakes, choices and changes of economic history in the post-war era. I hope this new material, and the comparative framework it is set in will cast a new light on the history of our discipline. I have included a copy of an article I wrote on “old” economic history in the United States, as an example of the research I have done so far.

I am now in the process of organizing my American interviews. As I live in London, I am not able to travel frequently to the USA and am thus hoping to interview as many people as possible in one place. This place is the 2004 ASSA meetings. If you were able to give me between 30 minutes and an hour of your time, that would be wonderful. I would be happy to provide a set of questions in advance. I am aware that you gave an extensive interview to the Cliometric Society a few years ago, and I would like to use this information as a starting point for a slightly different discussion.

I will be …

Please feel free to contact me by email if you have any questions, or require further information

Sincerely,

Cristel de Rouvray

Structure of all interviews

The interviews were tailored to each person, often along chronological and informational lines (what did you do, where, how?). However, there was a standard part to each interview, where the interviewee was asked to do free association on 10 words. The task was presented in this way:

> "let's imagine we are in the 1950s and early 1960s, could you please tell me what came to mind when you thought of the following words? If your associations have since changed (i.e. if something different comes to mind today) please say so."

Empirical
Complexity, Complex Social Phenomena
Objectivity, the possibility for the historian to be objective
Comparative
Old Economic History
Institutionalist / Institutionalism
Entrepreneurship
Kuznetsian (Simon Kuznets)
American
Laboratory