



Economic Identification: A Contribution to a Comparative Socio-History of Credit Markets

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Note from the editors

Introduction: economic sociology and history

As noted by Jean-Claude Passeron (2006) in his analysis of the assertoric spaces of social sciences, history and sociology share many epistemological principles: as general social sciences, they are distinguished from the more specialized ones (such as geography or demography), and since both rely on empirical work, more than on their ability to produce formal models, they work as sciences of inquiry (*"sciences de l'enquête"*) as opposed to sciences of models (*"sciences du modèle"*), such as economics. But in spite of this epistemological proximity, which one might expect to facilitate inter-field dialogue, the increasing divide in the dynamics of the social sciences has made the opportunity for discussion more and more unusual. This issue is specifically dedicated to the presentation of studies that organize heuristic discussions between history and economic sociology, provided either by sociologists or by historians.

Several insights might be produced by fruitful connections between the two disciplines. Historical approaches to the study of the economy are, first of all, a way to escape the functionalist approach that threatens so many perspectives in economic sociology. Historians offer, for example, precise genealogies of certain market institutions that economic sociology analyses too often through the lenses of a functionalist perspective, reducing the institution to its function in the present. On the other hand, archive work allows us to see how very long-standing institutions are rooted in specific historical contexts and have been frequently reshaped throughout their history by actors who either promoted or fought against them. The form of these institutions often has more to do with the type of coalition that was necessary to establish them, rather than the sole goal they were pursuing. Historical accounts of economic institutions meet the general interest in institutional dynamics of proponents of institutional entrepreneurship or institutional work (Powell and Colyvas, 2008; Lawrence and Suddaby, 2006). The (frequently implicit) functionalist perspective that is the backbone of much work in institutional sociology is often motivated by the assumption that uncertainty is one of the main characteristics of modern

economies (Stinchcombe, 1990; Beckert, 1996). Historians opportunely recall that uncertainty is far from limited to modern economies, since the uncertainty facing a ship-owner who charted an expedition in the 18th Century, or an early middle-ages merchant trying to assess the value of goods, was certainly not lower than what we face today.

Secondly, relying on historical studies of markets and the economy provide some insights to break with an evolutionist vision of markets. There exist some important studies, in particular by Fernand Braudel and Karl Polanyi, that already have deeply interrogated an evolutionist vision, which opposes face-to-face, local, trust-based exchanges from the past with globalized, institutionalized and bureaucratized markets. Historiographical work, including that focused on very ancient periods, may demonstrate how some technical devices were already at play within exchanges, while sociological studies on contemporary markets describe the role of face-to-face negotiations or vernacular devices even within very technological and globalized market relationships. Obviously, even though markets have greatly evolved over time as the role of technologies and institutions became more and more significant, they have also always been multi-faceted realities, connecting institutionalized practices with locally rooted routines. As a result, it can be of great interest for contemporary market studies to draw inspiration from historical studies on markets, to better assess certain mechanisms that seem to have been at play as long as economic exchanges have occurred.

The different studies presented in this issue have been proposed by several academics who work at the boundary of the two disciplines. They all provide fruitful perspectives on connections between history and economic sociology. Gilles Laferté, an economic sociologist, describes the long history of economic identification in France. He develops this concept drawing from the notion of political identification that has been used by historians and political scientists to consider the creation of records on individuals. Economic identification, which corresponds the recording of information on debtors, appears as an important economic institution that, for its promoters, aimed at replacing trust-

based and face-to-face economies with exchanges based on bureaucratic information and automatic technologies, considered to be essential for globalized exchanges. Nonetheless, Gilles Laferté shows how these two credit-granting mechanisms rubbed shoulders instead of succeeding each other after the 19th century. He also points to the role of the state in the development of economic identification.

The issue of state intervention in the economy is also at the core of the paper by *Alain Chatriot*. The historian pays tribute to the historiographical shift of the last twenty years that allows historians, and more specifically modern historians, to reintegrate the history of the state and of economic policy within political history. Such work allows for the consideration of economic institutions as the result of political processes that involve numerous political actors with deeply intertwined interests. More specifically, Alain Chatriot studies the regulation of the grain market in France, which led to the creation of a new economic institution during the summer of 1936: the National Inter-professional Grain Office. He demonstrates how such an institution cannot be understood without considering the functioning of the political institutions of that specific period in France, during which socialists were running the Parliament. He also points out how its continuation after this period was also deeply anchored in the support by the reactionary regime of Marechal Pétain and then by political actors under Liberation. Such an institution cannot be analyzed today without considering how it went through the diverse political processes that connected it to large coalitions and networks of actors.

Placing actors at the core of the analysis leads the modern historian *Pierre Gervais* to show how modern history could provide a more historicized understanding of market mechanisms and economic institutions. He focuses in his paper specifically on the logics of profit calculations practices performed by early modern merchants, raising questions that are very close to some perspectives adopted today in economic sociology. He then demonstrates that although economists would interpret some features of economic exchanges during this period as incomplete information on products and the opportunism of actors, merchants developed techniques and practices to calculate profits and losses. Practitioners from this period were in fact very zealous, echoing their counterparts today, in drawing statements from their activities that show annual

gains and losses. Pierre Gervais also analyses how merchants dealt with the lack of information in economic exchanges at the time, by relying on a combination price/quality evaluation, a technique not so different from what occurs today with some multi-dimensional goods for which no generalized commensurability systems exists.

These similarities between market exchanges from the past and today are even more striking when they include even earlier examples of economic exchange. The paper by *Laurent Feller* deals with the measuring of value in the Middle Age. During this period, actors had to manage exchanges of things which they had very few means to evaluate or compare, since no comparison scales existed. In consequence, it appears that exchange partners went far beyond the nominal price to evaluate products, involving themselves in deep negotiations over the commitments that each party agreed to, in order to stabilize future transactions and facilitate the circulation of items. For example, they might negotiate conditions of monetary and non-monetary payments (objects or servitude) that blurred the boundaries between what economic anthropologists usually define as economic exchange and noneconomic exchange. Although this study describes a period during which the use of money was not yet generalized, it also resonates with some aspects of contemporary economic exchanges, in which suppliers of manufactured products may be willing to cut their prices in return for other types of non-monetary commodities or services provided by their clients (shortened delays in payment, reduced delivery services, greater involvement of the buyer in terms of quantities bought). Historiography obviously provides some insights that might be fruitful for recent development in economic sociology around value and valuation processes.

These different studies also emphasize the shifts that have occurred in economic history. Of course, a great deal of economic history studies – especially those produced in departments of economics – continues to rely on quantitative data and econometric techniques. Yet some historians dealing with the economy have shifted away from quantitative assessments of economic historical events and towards to the studying of precise practices, logics, mechanisms and processes, areas that require specific competencies, like in-depth archive work and historiographical study. The evolution of the discipline of economic history and the specificity of these types of research that are firmly rooted

within history while having strong connections with economic sociology are clearly described by *Philip Scranton* in the interview he gave for this issue. However, the historian also continues to insist on differences in methodology and theory that separate the two fields of study, even though he regrets that sociologists do not collaborate more often with historians. The work of *John Padgett*, who also gave an interview to this issue, provides a contrast. He insists on the joint contribution that sociologists and historians can make to the understanding of social processes.

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Economic Identification: A Contribution to a Comparative Socio-History of Credit Markets

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Over the course of the last decade, historical and sociological studies of credit have flourished. It goes without saying that this subject is at the heart of public concern stemming from recent banking crises and dizzying levels of debt. In addition, credit embodies canonical forms of exchange: gifts and markets. It has a temporal existence and calls on orthodox forms of social relations. It is thus an *ideal research object* for theoretical and historical reflection in the realm of economic sociology. In my previous work, I endeavored to distinguish two ideal-types within the framework of a sociology of credit exchange.

In a *face-to-face economy* (Laferté 2010a), one studies credit circumscribed at the level of small networks based on shared social affiliations, such as a village, a family, a community, or a social group. Webs of relations are herein seen as a system of economic information gathering on people, and act as a guarantee that debts will be repaid. The face-to-face economy is one wherein social norms and obligations allow us to move beyond the ill-adapted concepts of trust (Guinnane 2010) and the *moral economy* (Fontaine 2009; Fassin 2010; Siméan 2011).

Conversely, what one could call an *economic identification economy* (Laferté 2010b) involves mediated, remote forms of exchange. This conception draws its inspiration from *political identification* – governments' efforts to identify their citizens –, which is a concept found in scholarship on the history and political sociology of the construction of the modern bureaucratic government, which was conceived in order to remotely control individuals (Noiriel 2007). Economic identification, in the context of economic institutions, is the creation of records on debtors with the goal of constructing large markets. Contrary to the face-to-face economy, the economic identification economy is based on the categorization of market actors via bureaucratic and automated information gathering techniques (papers, registers, maps, scoring, data mining, marketing, etc.). Codified and standardized, economic information is

extracted from face-to-face relationships and transformed into merchandise, whether a public good or a commodity. Economic identification also has a long history: in France, for example, an attempt was made to create a national debt register as early as 1673 (Postel-Vinay 1998).

Contrary to an evolutionist vision of markets, person-to-person credit without intermediaries and remote credit based on bureaucratic economic identification are two ideal-types of credit-granting mechanisms which rub shoulders within individual credit operations, rather than one simply preceding the other after the 19th century. We will focus here on the different forms of the economic identification economy that have appeared historically as markets have grown. In the current literature, marked by the development of the history and sociology of credit in the United States, the history of economic identification is primarily based on private institutions. By bringing to the table several pertinent elements of the French history of credit markets, a field that remains only partially studied (Effosse/Gaillard 2010; Chatriot 2006), we will be able to suggest a new model: one of public economic identification orchestrated by the government. This chapter calls for further socio-historical research on European credit markets.

A performative benchmark: Credit bureaus

As markets, and thus networks, grow, the density of social relations and therefore the quality of information erodes. How to resolve this informational asymmetry between debtor and lender at long distance?

The solution found in the United States is a model of *private economic identification* (Olegario 2006; Caruthers/Cohen 2010). Credit Bureaus were founded for commercial credit in the 19th century, with the creation of records on domestic entrepreneurs and businesses. Initially, this information was accessible only in the agency's files, but then later in registers widely available for sale. Progressively, these data were codified using accounting norms and by training specialized technicians.

Subsequently, the US consumer credit market became unique in its reconstruction around credit cards (Mandell 1990). National brands – the first in 1966, BankAmericard, which then became Visa in 1976 – replaced the diversity of local credit card programs. They thus nationalized and then internationalized the consumer credit market (Wolters 2000). To resolve the uncertainty problem in the consumer credit market, banks developed credit reporting (records on individuals' debt and financial situations), thereby using techniques developed for commercial credit in the 19th century. Credit reports were used en masse for all consumers, which was possible thanks to interbank cooperation. The first credit bureau to use these techniques opened its doors in 1965. Today, all credit bureaus work with the same underlying idea: the best tool for predicting future behavior is the study of past behavior. The core of the shared information is thus the banking history of clientele provided by banks to credit bureaus. Beginning in the 1970s (Rona-Tas 2009), the technique of credit scoring was added to this (of which the most well-known is the FICO score). These scores make it possible to judge an individual's ability to pay back a loan. By gathering data from digital credit card transactions and the consumer's various bank accounts, in addition to data from public files (personal bankruptcy proceedings, court convictions, directories, etc.), then by collecting further information in questionnaires regarding revenue, employment (seniority and stability), age, residency (type and duration), marital status, number of children, etc., credit bureaus amass an impressive store of data and thereby refine their forecasting models. In 2008, the three main American agencies held records on 210 million people, with more than two billion data entries per month, thus covering 90% of the American adult population (Rona-Tas/Hiss 2008).

Digitizing and scoring consumer banking behavior is a “‘quantitative revolution’ within the retail banking industry” (Leyshon/Thrift 1999: 436). There is no longer the need to know one's clientele personally. The categorization of clients, accessible on a computer screen, has replaced memories of people born of personal relationships. From the early 1970s on, these private identification systems became so powerful that the police and the American government began to use them, as they contained more information than their own files (Mandell 1990; Miller 1993: 11). Today, the social uses of this economic identification are numerous. Employers, lenders, some sports clubs, and private schools all select candidates based on their credit history, which acts as a gage of social trustworthiness. After a number of lawsuits, several laws (the Fair

Credit Reporting Act, the Federal Privacy Act of 1974) were drawn up to forbid the collection of information on race, religion, political leanings, traffic violations, and medical history, among other things.

From notaries to the beginnings of public economic identification in France

The absence of credit bureaus in France could initially be explained by the role played by notaries of the *Ancien Régime* at the end of the 19th century (Postel-Vinay 1998). Notaries, due to their legal responsibilities, were aware of their clients' capital and debts, and thus had a monopoly on individuals' financial information and levels of solvency. They could connect people who needed a loan with people who had savings. These loans were both personal and impersonal. Personal, since they depended on the notary's personal network, a network made up of his clientele and broadened by his relations with other notaries and their clientele. Impersonal, as the lenders and borrowers did not know each other beforehand. Credit was thus not restricted to closed social networks, such as the family, the village, or a social group such as the nobility, but instead conformed to the network of notaries to reach a national level. Capital thus circulated from the wealthy to the less wealthy, from the old to the young, and from the city to the countryside.

A rather comparable system of enlarging credit markets also emerged in the Parisian department stores of the 19th century (Albert 2012). The Dufayel department store created a credit register with all the names and addresses of their clients, along with the amounts they owed. The client had to sign this register, and thus it acted as a legally binding contract in court, in the event that the debt was not repaid. Since this system used the client's identity and was recognized by law, it made it possible to create a credit market that went beyond social networks. Furthermore, the debt collectors from these department stores, who were responsible for collecting repayment at clients' homes, went about doing their work by talking to the client's neighbors, and especially building managers, in order to find out if the client was up to date on his rent and if he had a job. In this credit market for the working classes, building managers played a role close to that of notaries: they were an intermediary whose presence made it possible to enlarge the face-to-face economy.

The first breach in the notary's informational monopoly appeared during the French Revolution, with the improve-

ment of mortgage registers. These registers collected information on individuals, and were available on a wide-scale and coordinated by the central government. New actors on the national level (especially Crédit Foncier in the context of Haussmann's reconstruction of Paris) used the records provided by the State to develop remote financial expertise. The similarity of these tools with those used by the government to identify citizens remains to be studied, as civil status documents were also a creation of the revolutionary epoch. The latter were created by the central government to replace preexisting tools used by local networks (Denis/Milliot 2004). Regarding economic identification in markets, bills of exchange played this role for a time, depersonalizing the credit relations as early as the 14th century (Hautcoeur 2008: 8).

If we continue to look at the early 19th century, we observe the creation of the savings account with the goal of encouraging the working classes to save (de Coninck 2012). In 1913, 38% of the population had one. This account identified clients with their first and last names. Other individual characteristics (date of birth, profession, place of residence, signature, etc.) were collected in a ledger accessible at the Caisse d'Épargne savings banks. This savings account brought the working population into the banking era and was a mark of honor for the laborer, as it was proof of an orderly life. This type of economic identification of the working classes also became an instrument for police surveillance, as it proved a person's identity. In addition, economic records were created for members of the working class with the law of January 12, 1895. This law made it possible for lenders to go directly to employers and seize a portion of the employee's salary to pay back a debt that was overdue. These cases were decided by local courts, and the rulings were inscribed in a register that was available to all to consult (Albert 2012). The courts, and thus identification papers, increasingly served to secure market exchanges. Here again, the central government and an enlarged market used the same system of identification, each aiding in constructing the other by borrowing technology from each other, in order to maintain new forms of political and economic relationships at a distance.

Beyond consumer identification, in the 19th and early 20th century, we must also look at the role of Chambers of Commerce, which circulated information on solvent businesses and merchants (Lemerrier 2003). There were also the registers of trade tribunals, which published commercial trade rulings, bankruptcy registers (Hautcoeur 2008),

and the trade register (Zalc 1998), all of which went into the creation of public financial and trade databases. Lending, bankruptcy, and trade registers, as well as salary seizures, were all a matter of public record. On the French market, this collection of public records functioned as an economic identification tool similar to credit bureaus.

State-administered credit

It is important to look at this early history of economic identification, since it is still active today, with the pivotal role being played by public authorities in organizing economic information. In 1946, in the wake of the creation of the different registers discussed previously, the central risk department of the French central bank developed a register of businesses and commercial credit (Miller 2003: 57). More generally, the French central bank, under the auspices of the economics ministry, played a central role from the end of WWII until the 1980s with the system of credit administration established by the laws of 1954 and 1966. The central government wished to fully manage the distribution of consumer credit by controlling interest rates and the volume of capital distributed. The goal was threefold: to control money creation, promote industrial development (automobile, household appliances, and furniture manufacturing, primarily), and protect consumers (against usury) and merchants (by allowing them to take back goods sold on credit in the event of non-payment). In the interwar period, the development of consumer credit was limited to the automotive industry's credit establishments. Because of these restrictions on credit, consumer credit remained for quite some time limited to direct credit provided by merchants in transactions that could not be controlled by public authorities. The public authorities were unaware of these transactions, and they were often subject to high interest rates (Laferté/Avanza/Fontaine/Pénissat 2010). All types of goods were exchanged in this face-to-face merchant-consumer relationship, while it progressively came to be associated with a grey economy on the legal margins.

Regarding official consumer credit, that of credit establishments and banks, it was seen as "pre-savings" (*épargne d'avant*), as opposed to "post-savings" (*épargne d'après*), to use the terms of the French central bank. It was extended solely to *durable* goods (as opposed to perishable goods such as food or clothing) and *useful* goods (as opposed to luxury goods such as watches, bicycles, jewelry, or art). The only other category that qualified for consumer credit was that of professional equipment, for example, automobiles, motorcycles, mopeds, bicycles, household materials, furni-

ture, heating equipment, and pianos – this definition was then broadened on August 2nd, 1955 to include upholstery fabric, household linens, blankets, and rugs. Contrary to the American market, credit in France was officially restricted to certain consumer goods and was not based on credit cards. The remaining consumer credit, the direct credit provided by merchants, was tolerated because it was uncontrollable.

Generalized access to banking services

With the lack of credit cards in pre-1980s France, the checkbook became the favored payment instrument. In order to better control economic exchanges after salaries started to be paid on a monthly basis in 1968, the central government imposed the bankarization of the entire population, with the 1972 requirement that all sums over 1,500 francs be paid by check. This meant that salaries were paid by check, leading employees to open bank accounts, and this precipitated the creation of a new clientele, that of the middle and working classes. 18% of households had a checking account in 1966, whereas this jumped to 87% in 1976. Free checking and the automatic guarantee of checks of less than 100 francs (a veritable automatic credit that banks were obliged to honor) stimulated the rapid development of checking and reduced the circulation of banknotes. Interbank cooperation was essential for the centralization of this system, in order to avoid that a bad borrower open multiple accounts. This cooperation was organized by the French central bank, which, in 1965, created a central checking database, known as the “checking blacklist” (*interdits de chéquiers*) (Salomon 1995). Bankarization made it possible to guarantee transactions by imposing banking control on households, without creating a hierarchy of the clientele according to their solvency. In the mid-1970s, having an account and being able to present a bank account identification document became necessary for many transactions, such as receiving one’s salary, as well as social benefit payments and paying bills. Access, or lack thereof, to banking services became a social issue. Against the will of the banks, who did not want to manage a marginally solvent clientele, the legislature created the right to a bank account in 1984 such that every individual had the right to this form of economic identification now necessary for normal, everyday life. Management and banking rationale regarding budgets spread to all households, sometimes coming into conflict with the diversity of ordinary modes of economic calculation (Weber 2009).

Economic identification in France thus first went through a period of generalizing access to banking services, allowing for the creation of bank account identification documents. In France, this is called a *Relevé d’Identité Bancaire (RIB)* – meaning literally, “statement of banking identity.” The term *banking identity* epitomizes the fact that this is indeed a process of economic identification. The *RIB* plays the role of an economic ID card granted to all members of the French population.

Market liberalization, or normative tensions for French banks

The shifting winds of the 1980s, with laws regarding banking liberalization (1984) and the privatization of the main French banks, precipitated the commercial turn in banking. This shift became particularly noticeable in the 1990s, when it became the norm to evaluate a client based on the potential profit he or she could bring the bank. This created irreducible normative tensions for banks. From the 1960s to the 1980s, for the French, banking was seen and constructed as a public service, with the state acting as manager of last resort, and then this shifted toward a commercial and trade institution in the 1980s, a perception that remains today (Lazarus 2013).

As early as 1967, banks worked to find alternative payment mechanisms, since checking was required to be free. In 1967, six banks (BNP, CCF, Crédit du Nord, CIC, Crédit Lyonnais, and Société Générale) created the first payment card in France: a debit card (*la carte bleue*) for which they charged a fee and which did not come with a credit line. In the mid-1970s, this group of banks joined forces with Bank Americard, which subsequently became Visa. In 1984, Crédit Mutuel and Crédit Agricole joined this network. These cards were initially just a means for payment, since credit was still granted principally by banks and credit establishments in the form of personal loans for specific projects. At that time, there was no longer a list of purchases that would qualify the borrower for a loan, but a banker had to sign off on the loan application, and he thus delivered a moral judgment on what constituted good or bad credit. The rise in revolving credit dates back only to the late-1980s (from 8 billion euros in 1991 to 32.7 billion in 2007), and only 8 to 9 percent of the population had access to it in 2007, constituting only 20% of consumer credit (Ducourant 2010). Payment cards in France are still largely debit only, with no credit line.

Banks and credit establishments were not, however, ignorant of scoring techniques and the practice of rating banking behavior. Although, contrary to the American market, these assessment mechanisms are unknown to their clientele, since French banks build their image around personal service to ensure customer loyalty. French banks have chosen to maintain a network of local branches to propose tailored services to the masses, thereby remaining “personal banks” despite the rise in automated services and risk management and the practice of type-casting the clientele of each bank. The practice of clients frequenting multiple banks for different services is indeed on the rise, but French consumers nonetheless remain loyal to their bank, which has sociological characteristics: the working classes tend to bank with La Poste, rural dwellers with Crédit Agricole, civil servants with Banque Populaire, and urban dwellers and the upper-middle class with BNP or Société Générale. Beyond the digital information already collected, it is up to the financial advisor of a given branch to gather reliable information (profession, marital status, number of children, address, etc.) on their clientele, such that the risk department can then build statistics for each category of clientele. A personal interview still takes precedence over the use of questionnaires, and each bank developed its own system for rating and categorizing clients in the 1990s.

The rapid development of the consumer credit market and the politicization of over-indebtedness in the late-1980s led the public authorities to improve this system. In addition to the checking blacklist already in existence, the national database of incidents regarding repayment of individual loans (FICP) was created in 1989. This database is highly automatized, with perfunctory, normalized information, and requires few personnel (60 employees), as compared to credit bureaus (Jappelli/Pagano in Miller 2003). This database encompasses credit more generally, but here again, only negative information is included, as only incidents are listed. Contrary to many European countries, such as Belgium and Germany, no public database of positive information exists in France, in the name of respecting privacy. In March 2014, the French administration tried to create a database of positive information, which would have included 12 million households, but the Constitutional Council, the highest constitutional authority in France, annulled the bill, arguing once again that such a database was a threat to the protection of privacy. It goes without saying that large banks have files full of information on their clientele and do not wish to share these

with their competitors, and they therefore use their political influence (so far successfully) to make these bills fail.

Understanding the co-construction of the market and the State through a history of different economic identification models

If we were to develop a diverse comparative socio-history of both political and economic identification, bringing together political and economic sociologies of identification, then we would certainly better understand contemporary markets and their diverse forms. Today, European credit markets grant the central bank the most important role for interbank cooperation, thus creating a hybrid of public and private identification. The United Kingdom, Sweden, and Switzerland have markets organized around credit bureaus, while Austria, Belgium, Germany, Italy, Spain, and Portugal have mixed systems. France is at one extreme, as it forbids the creation of credit bureaus in the name of protecting individual privacy, thus granting the French central bank the monopoly on interbank cooperation, but only for sharing “negative” information (Jappelli/Pagano in Miller 2003).

Moreover, the recent introduction of credit cards in China shows how other versions of this story are possible. Supported by a strong state, a low regard for privacy, and a very large market, the Chinese credit market led multinationals to change strategies. In the end, a card was created that exists both on the external market, with a new global brand, Unionpay, and on the domestic market, where it is both a payment card and an identification card. This clearly demonstrates the proximity of economic and political identification technologies (Guseva/Rona-Tas 2014: chap. 7).

Thus, when we look at the different paths credit markets have taken to get to where they are today, the sociology of market globalization is called into question. Indeed, despite international institutional controls (the Basel Committee) and the creation of global groups (large banks and brands such as Visa, MasterCard, and Union Bank), and despite the rapid diffusion of banking techniques at the global scale and the existence of reference markets, credit markets remain largely national constructions. Legal history, and especially differing concerns regarding privacy due to different political histories, as well as the role and power of the central government and the size of the domestic market, are all discriminating variables which differentiate economic identification models around the world.

In all countries studied, we can see how the identification of individuals by governments is a step in the construction of enlarged markets, with the contractualization of the economy as the foundation for this process. Conversely, private economic identification also serves as political identification for individuals. The state and the market both need to remotely identify citizens and consumers in order to construct large legal and/or trade spaces. Contrary to liberal economic thinking which sees the market and the State at two extremes, these two institutions exchange and share their technologies, and, historically, the construction of one aids in the construction of the other.

Thanks to economic identification, a client can pay around the world with one card, of which the brand "Visa," "MasterCard," or "Unionpay" is recognized and accepted everywhere, as it identifies its owner as the holder of an account managed by a bank; this person is thus a client whose solvency can be investigated. Thus it is the card, or perhaps tomorrow the smartphone, which authorizes the consumer to enter the market, just as the holder of an American passport can cross the French border without a problem. Conversely, the stateless, or the holder of a passport from a poor country who cannot enter a rich country, is the unlucky companion of the card-less consumer or the worker without a bank account, excluded from the market and its social protections. Bank cards and account numbers have become economic passports which now allow individuals to engage in exchange relationships with complete strangers.

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The Laws of the Markets: Historical Perspectives on Political-Economic Regulation. The Example of Twentieth-Century French Agricultural Policy

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For decades, the focus of modern political history was restricted to the realm of politics, emphasizing issues such as parties, elections or the role of the press, for example. Economic history, on the other hand, was primarily interested in working with quantitative data over the *longue durée* or monographs of precise sectors or businesses. Moreover, economists have had a specific relationship to history since the market has generally been interpreted as an institution that can only be understood in the very *longue durée*. The historiographical shifts of the last twenty years have slowly changed this situation as the history of the state and economic policy has been reintegrated into the field of political history (Baruch and Duclert, 2000). Thanks to the influence of early modernists, modern historians have begun to consider economic policy and state intervention. Issues such as consumption, that had been left aside, have been placed back on the historiographical agenda revealing the links between consumption practices and public debate (Chatriot et al., 2006). While this work has been influenced by cultural history it does not fall fully into that sub-field. The juridical regulation of fraud, especially in the realm of food and drugs (Stanziani, 2005), the management of shortages in times of war, or the measures that determine access to credit are some of the various forms of state intervention that take place in this area. Some markets have generated studies by different social sciences on areas like the French wine market, whose construction and regulation is now well-known (Laferté, 2006; Chauvin, 2010) or more recently the fruits market (Bernard de Raymond, 2013).

To illustrate these new trends and in an effort to avoid an arid historiographical paper, I would like to offer a reflection on an area I have been exploring (Chatriot et al., 2012; Chatriot, 2013), specifically the history of the grain market, and the difficult process of its regulation in France during the first half of the twentieth century. In terms of

agricultural policy, this choice allows me to follow the diversity of actors who intervened in this political process. During the summer of 1936, the Popular Front government created a National Inter-professional Grain Office (*Office national interprofessionnel du blé*, ONIB). This new entity was the culmination of a long debate and numerous previous measures that reveal the functioning of Third-Republic institutions. Moreover, this new institution, created by the socialists, was paradoxically maintained by the reactionary regime of Marechal Petain, and even more surprisingly, was kept under the Liberation and up to the present day. The choice to study the creation of this institution also corresponds to a more methodological reflection.

Grain Crises

"Grain is a product of the soil, and from this perspective, it belongs to economic and trade policy. It must also be seen as a basic need for public order and, from this point of view, it belongs to the realm of politics and the Reason of State". This speech from the Abbot Galiani in his polemical, *Dialogue sur le commerce des blés*, (1770) is an appropriate point of departure for examining the question of grain in the twentieth century. Following the eighteenth and nineteenth centuries, which were marked by regular harvest revolts and the question of adequate provisions (Bourguinat, 2001), the deregulation of prices brought forward once again the question of public policy and state regulation.

Confronted by the old problem of regulating a market that was a necessity for the population as a whole, public authorities (legislative, executive and administrative) managed crises of over-production and the destabilization of the international market after World War I. During the 1920s and 30s, grain was consistently perceived as *the* question. It was presented as a market to protect or as prices that needed to be taxed. To the old problem of unpredictability – bad harvests necessitated imports while good years required exports – was added the shock of a world war. The

equilibrium that protected French farmers through protectionist policies and tariffs introduced at the end of the nineteenth century was no longer viable (Lebovics, 1988; Aldenhoff-Hubinger, 2002; Chatriot, 2010).

With the war, Russia no longer exported and European production was in an upheaval. At first, there was a sharp rise in prices due to increased dependence on production from abroad. While peasants initially celebrated this situation, governments were concerned with a general decrease in buying power [“la vie chère”], and quickly, the situation changed. Farmers were suddenly confronted with the disorderly increase of supply, technical innovations and a static demand (Stovall, 2012). This led to new crises in 1928 and again at the beginning of the 30s up to 1933 when good harvests led to the collapse of grain prices.

Previously, the instruments of state intervention in agricultural policy were limited to protective tariffs based on subsidies, incentives for export, and quotas. However, these measures were quickly shown to be insufficient for radical market variations. Confronted with these fluctuations, new groups dealt with the question of grain in France and internationally. Until now, few historians have explored the series of conferences on grain that took place in Rome in 1927, Geneva in January 1930, in Rome in March and April 1930 and especially in London in May 1931 then in August 1933 with the creation of the International Wheat Advisory Committee (Graevenitz, 2009).

Agricultural unions reemerged across France as the historical division between the unions of large property holders and the unions of republican orientation tied to mutualist institutions was replaced by new confederations (Barral, 1968). At the same time, a specialized union developed, the General Association of Grain Producers. This association, founded in 1924, emerged out of the grain crisis (Pesche, 2000).

Above all, grain producers demanded a high price for grain as opposed to politicians who wanted to drive prices down to satisfy their constituents. The association was tied to the Chambers of Agriculture and stated in 1929: “If our grain policy must remain subordinated to bread policy, which is itself subject to the demagogical influences of politics and the press, we might as well give up any hope of maintaining grain production or technical innovation in this area in France” (Rémond, Hallé, 1929, 35). According to this logic, grain producers were opposed to the increasingly modernized and well-structured millers of the inter-war period.

Furthermore, beyond new specializations in agricultural production, various political movements were actively seeking support in rural France during the 1930s (Paxton, 1997; Bensoussan, 2006).

The legal measures that followed as a result are worthy of special attention. A first law “on the grain trade” was adopted on December 1, 1929 and was quickly completed on April 1, 1930. These laws gave *de facto* power to the Minister of Agriculture to intervene by decree on the origin and nature of grain used in mills. This was an essential measure because behind its technical appearance, it created a specific type of protectionism. On April 30, 1930, a law was proposed against the speculation on the grain trade through the creation of a permanent stock of grain and flour. This law was completed with a supplementary measure in the law of April 7, 1932 when the cost of storing grain became an essential question.

Renewed price variations quickened the rhythm of laws throughout 1933. On January 26, a law was voted to “protect the grain market”, with the aim of financial intervention to promote stock surpluses and control prices. On April 14, a law authorized the Minister of Agriculture to grant subsidies to encourage the use of indigenous grain for use other than human food and alcohol—a strong symbolic statement that was made necessary by surpluses and a collapse of prices. July 10, another important symbolic step was taken with the establishment of a minimum price for grain. While it was only in force for a brief period, the law set out all the controls that were made possible for controlling the market. The difficulties of executing the law forced the vote of December 28, 1933 reforming a number of articles.

Additional laws were passed in March and July 1934. The law of December 24, 1934 was presented as “an attempt to clean up the grain market”. It followed a decree of October 1934 that had attempted the delicate operation of codifying all the legislation on the issue. Next there was an attempt to return to a free-trade approach against the restrictive measures established by the law of July 1933. The consequence however was that the bottom fell out of prices. Multiple technical laws were passed in the spring of 1935 and July 13, 1935 and the use of decree-laws allowed for a first series of radical measures that were completed during the summer and through October 1935. The decree-law of October 30 returned to a certain number of previous measures by suppressing special taxes on production.

A first set of conclusions may be drawn from this quick overview. First, one must consider what a specialist of the agricultural question justly referred to as “the soliciting of Parliament among grain producers” in 1936 (Salleron, 1936, 421). Since members of parliament were sensitive to the question of grain prices, the agrarian lobby maintained a certain influence. While some laws took years to pass, and were blocked by the Senate, they were all ultimately voted in. In the political system of the French Third Republic, the Parliament exercised a certain superiority in decision-making for the economic and social policies.

The second conclusion becomes clear in the inadequacy of the successive choices. Independent of the more or less liberal, statist, or professional approaches, the principle trait of all these attempts at regulation was the incapacity to find a form of intervention adequate for the grain market. The measures were often considered illusory because there was insufficient credit to implement them and because the state did not control the wholesalers. Many were critical of this “enormous mass of laws, decrees and acts that are successively regulating our grain trade,” and that can only be explained by “the errors of legislators in the area, the lack of an overall plan, demagogical solutions, and lack of familiarity with the most fundamental economic laws.” (Touzet, 1936, 6).

Institutions between the State, the Market and Individuals

The creation of a Grain Bureau occupied a central place in the great reforms of the Popular Front. The parliamentary debates on this entity were particularly vigorous during the summer of 1936. The institutionalization was immediately commented upon by numerous legal economists that were interested in the forms of regulation that are now referred to as a planned economy. By examining the creation of this institution, I would like to present broader conclusions on how to study institutions and their role in the political regulation of the economy.

In his doctoral thesis of 1934, Jean Sirol saw the Grain Office as “the proof of Parliament’s will to bring forth a powerful and competent organization that is separate from itself: this is an implicit, and particularly interesting, recognition of parliament’s inability, either due to the slow nature of parliamentary procedure or its incompetence, to handle countless contemporary economic problems.” (Sirol, 1934, 370-371) For this young jurist, the institution “mark[ed] the progress of state socialism and reveals that

it is not at all revolutionary, but, to the contrary, regulates and plans the national economy. Moreover, there is no doubt that among the masses, there is a new fascination for these complex organizations, nourished by considerable sums of money and whose impact on the economy is without question as long as it represents the state. This is the result of a trend that is directly opposed to liberal ideals. A trend one sees not only in France, but also in the entire world and perhaps even more so in other countries (USA, USSR, Italy, Germany, Austria and Switzerland)” (Sirol, 1934, 371). The question of the comparison of the situations on an international scale is very important of course (Solberg, 1987; Way, 2013). Obviously, the question spread far beyond the realm of grain market management.

The project for a national grain office had a long history among socialists (Lynch, 2002). They had proposed it to Parliament as early as January 1925 and again in October 1929 in the form of an “institution for complete control over grain imports and a national fertilizer office.” The idea was taken up again through individual efforts in 1929 and 1934 but it appeared for many years to be too ideologically charged and the legislative and regulatory solutions were too limited.

The situation changed with the response by the President of the Council, Léon Blum, to the massive strikes that led to the Matignon Accords, and with the important social laws, published in the *Journal officiel* on June 26, 1936, on paid vacation, collective bargaining, and the 40-hour work week. The leftist government sought the creation of the Grain Office for the agricultural sector. While the most famous laws were voted through quickly without opposition in the Senate, the agricultural questions remained problematic. The bill, allowing for the creation of a professional office controlled by the state as a response to the failures of previous legislation, was presented as early as June 18, 1936.

The text was quickly attacked however. Long debates in the two houses led to modifications to the text in July and August. Leon Blum stepped in to support the Minister of Agriculture, Georges Monnet, and the law was finally voted through in the last session of August 15, 1936. The debate focused on various elements: the status of the regulating body (the office was a public entity, a category of institution that was flexible but greatly criticized during the thirties); the problem of its leadership, given the inter-professional nature of the office; the question of how to

fix grain prices annually; the creation of organizations for grain storage that were separate from the power of wholesalers; and, lastly, the organization of a monopoly for managing imports and exports. Georges Monnet's other projects, including the expansion of the regulation of markets in the context of collective bargaining with the possibility of necessary extension by the minister, were also blocked by the Senate. The inter-professional nature of the Grain Bureau was a particularly tricky point of debate. The professional relations between producers of wheat, millers, bakers and consumers (represented by trade unions) were sometimes difficult. Another source of conflict was the suspicion that the Government wanted to establish total state control over the wheat market. Opposition to the creation of the Grain Bureau often took extreme turns in the press in the form of caricature or denunciation. Joseph-Barthélemy, a conservative jurist and editorialist for *Le Temps*, wrote on July 28, 1936: "The well-stocked Grain Office has nothing to say... the plan is to cage off agriculture by leaving each one his niche and his reward. Such efforts are worthy of dogs."

However, reactions to the actual creation of the institution varied. In her study of Breton peasants, the political scientist Suzanne Berger has shown that the "Landernau [the Central Office of Mutualist Agricultural Works of the Finistère] protested against the authoritarian creation of prices fixed by the state, but benefited from the fact that the law asked producers to stock grain for the creation of a grain cooperative with access to the necessary silos. [...] The Grain Office was the creation of a Leftist government that Landernau feared and detested, but it actually meant a quasi-monopoly on the grain trade in Finistère by the central office." (Berger, 1975, 154). Some figures who were opposed to the Grain Bureau had already pointed this out: "There is no sense in burying one's head in the sand. The Grain Office has been welcomed with satisfaction throughout the peasant world. The discussions that have unfolded on the level of ideology have left them indifferent. Undoubtedly, the office appears to be an improvement to the previous situation." (Leroy, 1939, 75).

The ONIB was an inter-professional public organization, with producers, wholesalers, handlers, consumers and administrators. Its most important responsibilities were the fixing of prices, storage, and a monopoly on imports and exports. The fact that the harvests of 1936 and 1937 did not produce a surplus guaranteed the ONIB's creation. Problems did not emerge until the harvest of 1938 and

functional difficulties that led to the reforms of the decree-laws of July 29, 1939.

A thorough examination of this new institutional form in France may also generate foreign comparisons, especially when other experiences could serve as a model or a counter-model (the Canadian Wheat Board (MacGibbon, 1952), cooperative experiences in Italy and Germany, or the State Grain Company in Czechoslovakia). The question of the French colonies can be also raised in the study of agricultural policies (Swearingen, 1985).

Choosing to study an institution in order to analyze economic policy is a means of responding to essential questions. Of course, it is influenced by the neo-institutionalism of economists and political scientists. But it also corresponds to advancements in the History of Science and reflections on the scale of historical analysis. This is not a descriptive administrative history but rather a means of framing large-scale problems within concrete debates. The institutional scale allows for a consideration of the origin of sources: that of each document, but also the relationship between them that constitutes the very basis of the archives: working on institutional sources forces us to reflect on the institutional techniques themselves and on the practices of its members.

The example of creating an institution necessitates an investigation of multiple temporalities: 1) the emergence of often contradictory doctrines; 2) the experiences inspired by precise problems; 3) the actual decision to create the institution; and finally, 4) the continual creation that transforms an institution over time. In this analysis, one must reconsider other possible solutions to the same problem, and locate the diversity of debates around such a project (Chatriot, 2002).

Once the institution is created, an institutional study has the advantage of avoiding a simple discursive analysis. It allows for an understanding of functional practices. One can study the members of the institution, their origins, their connections, their affinities and relationships to other institutions (which sometimes offers an understanding of a network of institutions) (Lemerrier, 2003). The research in this field is not restricted to a systematic prosopography. Rather, it must be adapted to a given institutional form to fully utilize the merits of a method that is built on an institution's specificities. The study of the institution must consider juridical constraints as well as concrete elements (budget, personnel, rhythm of work). It is also a question

of understanding the progressive dynamics of the institution, the conflicts that it encounters, its place in the process of public decision making, its opportunities for consultation, expertise, recourse, arbitration or sanction.

Obviously, French agricultural policy of the first half of the twentieth century cannot be reduced to the regulation of the grain markets. However, grain was one of its most symbolic and important components. Moreover, its influence was essential over the long term because, beyond the Grain Office's existence throughout the century, one must consider that this type of regulation (decisions on regulated prices made collectively, organization of storage, protectionist measures) was at the heart of the first European agricultural policy (Noël, 1988; Fouilleux, 2003; Knudsen, 2009).

However, one must also consider that in spite of the fact that rural studies have had a strong influence on history and sociology (Hervieu, Purseigle, 2013), the functioning and the administration of agriculture are still relatively unexamined. Certainly, the phase of agricultural modernization of the 1960s has fascinated social scientists, as have the links among agricultural union members with rightist political parties. But the question of agricultural policy has been largely forgotten and too often reduced to a simple conflict of interests.

Through the example of the debates on grain regulation, I have tried to insist on the connection between public policy and institutions, on the weight of past experiences and the question of the form of economic organization. I have also focused on the relationship between union actors that defended their interests but were also carriers of a technical expertise (Rabier et al., 2007).

The question of agricultural policy is also informed by spatial considerations, as it takes place on local, regional, and international levels. The international crisis of the grain markets in the 1930s forces us to change frames of reference in order to better understand the choices made. It forces us to examine international organizations, not only from the logic of diplomatic history but also from the logic of political history (Rosental, 2006; Ribi Forclaz, 2011). Certain international actors of this market, such as for example the big companies of the grain trade are very understudied (Morgan, 1979). We also have to study better the phenomena of speculation on this type of market. The crisis during the 1930s could be compared with the

great crisis on American market at the end of the XIXth Century (Norris, 1902; Levy, 2012).

In the context of French politics, the difficulties of finding an adequate form of regulation is tied to a particular moment in the history of the Third Republic, with the different obstacles of the 1930s and the rupture that was brought on by the Popular Front (Nord, 2010).

Other examples from Modern History could be chosen to explore political history approaches to economic and social questions. At the crossroads of legal and economic history, one could follow the question of the progressive codification of labor law or more generally the evolution of social protection (Chatriot et al., 2011). Fiscal questions have also been recently drawn out of the world of specialists in financial history and have found a more appropriate place in the field of political history with the question of consent to taxation (Delalande, 2011; Huret, 2014).

There again, far from a political history reduced to parliamentary debates or elections, recent research has led to an understanding of the plurality of actors and the institutions at play. Only a complex vision of the history of the state (Sawyer, 2012) that is open to social elements can help us understand questions that have been left aside for far too long by historians.

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Early Modern Merchant Strategies and the Historicization of Market Practices

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Among historians, there is no lack of interest in markets of the Early Modern era, which can be roughly defined as the period extending from the Renaissance to the beginning of the 19th century. The study of Early Modern trade has steadily gained in importance since Braudel provided the first general account, which traced the emergence of modern capitalism to this period, and more specifically in the development of its commercial life, although in part contrary to its "normal" course (Braudel 1979). European trading communities have been explored in numerous monographs, with an emphasis on ports and the so-called "Atlantic world" (Hancock 1995; Jeannin 1996, 2002; Lespagnol 1991; Morgan 1993; Matson 1998; Price 1996). The operations of Early Modern markets have been probed empirically, and with an emphasis on the ubiquity of credit relationships (Finn 2003; Fontaine 2008; Hoffman et al. 2001; Muldrew 1998) and on the significance of private, self-regulated networks (Hancock 2009; Trivellato 2009).

At the same time, market development has come to play a central role in the account of the transition to modern, industrial capitalism. Earlier analyses stressed technical, organizational or more widely Schumpeterian innovations and the rise of the factory. Pre-19th century developments were mere preliminaries; at most, primitive capital accumulation or the dismantling of various premodern institutional or ideological obstacles set the stage for growth, rather than providing its engine. Recent accounts almost always turn to earlier periods, to a preceding age of commerce which explains and underpins the later, spectacular growth of the 19th century. Explanations in the literature are quite diverse: from a standard growth theory in which market unification brought about increased overall demand and lower costs (Rothenberg 1992; Meyer 2003; Van Zanden 2009) to neo-Schumpeterian paeans to the scientific spirit or the revaluation of bourgeois virtues characterizing Western Europe (Mokyr 2005; McCloskey 2011) or less sunny variants in which escape from stagnation became possible thanks to the stolen profits of imperialism, or the

sheer luck of local coal availability (Pomeranz 2000; Wrigley 1989). The common thread among these diverse theories is a shift away from 19th-century machines and factories, towards an 18th-century market economy and its attending institutions, ideals and practices which takes center stage not as a foil to full-blown capitalism, but rather as its cradle.

Early Modern social historians of markets and economic-minded historians of Early Modern growth have a hard time connecting, however. The latter, usually trained as economists, tend to assume models of market behavior which are hard to reconcile with the empirical accounts provided by the former. Early Modern markets were opaque, with information largely unavailable, and barriers to entry ubiquitous. Above all the economic agents operating in them rarely engaged in the kind of profit-minded calculating activity generally associated with the classic utility-maximizing rational agent. Few calculations of profit were ever made; accounts were poorly kept, if at all, and almost never balanced; and interest-free credit was the rule rather than the exception (Gervais 2012, 2014; Jeannin 1996: 82; Toms 2010; Yamey 2000). Market historians, however, generally do not go beyond these observations, and shy away from proposing a coherent economic description of what they observe which could compete with the standard economic analysis of market operations and provide an economic model accounting for these behaviors. Instead, they retreat into invocations of embeddedness, or references to social obligations competing with market attitudes, such as community networks or *noblesse oblige*, which are used to explain the difference between observed agent behavior and expected utility maximization.

As with all epistemological generalization, there are exceptions. A few French economic historians, myself included, have tried to explore the possibility of economic narratives of the Early Modern era, whether model-based or not, which would depart from standard economic conceptualization (Daudin 2005; Gervais 2012; Grenier 1996; Verley 2013). That such a cluster of heterodoxy would occur for this particular topic is probably no coincidence: precisely because we know so much about Early Modern markets,

and because they are so widely used in historical economics, the disconnect between historical knowledge and economic modeling is at its most glaring, and invites research. There is also the lingering influence of earlier work on household and moral economies, including the Braudelian non-capitalist markets cited above (Braudel 1979: vol. 2; Bruegel 2000; Merrill 1977; Thompson 1966).

But in one respect at least this is, at best, an incipient program of research. Unlike economic sociology, which is methodologically sociological, current economic history generally tends to be methodologically more economic than historical, a point recently and forcefully made by Francesco Boldizzoni (Boldizzoni 2011), which also holds true for most of the French heterodox school. Economic history is much closer to historical economics than to a history of the economy which would work on the periodization of economic practices, ideas and attitudes, historicizing them in such a way as to build historical moments which would clearly differ from one another in the vocabulary and the economic models used to describe them. The recent resurgence of neo-institutionalism is a case in point; while a number of authors have elaborated complex descriptions of the interplay between institutions and market mechanisms, the latter are basically considered as constant, with institutional influence taken as an exogenous force slowing down, accelerating, redirecting, and indeed making possible what remains largely an unchanging given (Greif 2006; Ogilvie 2011).

The work presented here is an attempt to explore the possibility of a more historicized understanding, taking as its starting point the apparent lack of concern for profit calculations evinced by even the largest Early Modern merchants. What economic – not social, not cultural – model could bring a merchant at the top of his profession, managing very large flows of money and goods, to not draw a balance sheet regularly, or to not charge interest whenever possible? Can we find a truly economic logic which would make such practices rational and utility-maximizing, and not merely the manifestation of a rationality somehow incomplete, or uninformed, or hobbled by handicapping external circumstances?

What was tracked: profit, assets, or credit flows?

Two families of sources allow us to understand how Early Modern agents approached market activity in general and profit in particular. Merchants wrote both account books

and correspondence, with account books being partly normalized through double-entry accounting. The latter technique, developed in the late Middle Ages, enabled its users to record complex sets of transactions into multiple accounts, which could then be balanced to show a profit or a loss, with the result of these balances recorded in a profit-and-loss account. However, most agents did not use double-entry accounting and simply recorded transactions as they occurred, or at most in "accounts current" books, corresponding to what was called "single-entry" accounting. Moreover, in most of those cases in which double-entry was used, individual accounts were seldom balanced, and a general computation of profit and loss over the whole set of existing accounts was even rarer, except in cases such as partnerships or stock companies which required regular reports to investors.

The standard narrative in accounting history maintains that since balances and accompanying profit calculations were made possible by double-entry accounting, the slow progress of such methods reflected the development of a truly calculating, capitalist spirit (Weber 1930: 18-19), thereby demonstrated to be present certainly as early as the 17th century among the most astute agents of the era, and among the managers of new, large stock ventures, such as the various East India companies (Carlos 1996). While practitioners only slowly discovered the possibilities of the form, textbooks had long pointed out that double-entry accounting provided tools for tracking profits per product line, tracking overall profits, and using such information to develop efficient business strategies (Edwards 2009).

The slow move toward systematic profit calculations, especially among merchants, has been ascribed to the overall structure of the markets (Yamey 2000). Early Modern markets were highly segmented. Information and goods circulated extremely slowly and imperfectly, and as a result prices could fluctuate wildly and unexpectedly, while agents were unable to change prices of goods long since sent off at earlier prices, or of orders passed weeks or months before the sudden change in a market. Prices were at best informed bets on the future, and moreover varied with the quality of goods, which was far from standardized and subject to unexpected ups and down; a merchant receiving a load packed in far-away places and transported under complex constraints could never be sure of what he would find upon opening its crates and barrels. In this narrative, profit tracking through double-entry book keeping was a revolution waiting to happen, with imperfect

market development the only hindrance to the realization of its full potential.

A close examination of the way agents conceived of profit and its place in their activity produces a very different picture, however. Accounting textbooks always mentioned profit and loss calculations within a larger framework of credit evaluation. The goal was to clean up what accounting authors of the Early Modern period called "incomplete" transactions, that is, transactions which were limited to a net profit or a net loss, with no change in the credit position of the operator towards his or her various creditors or debtors. Whether an inheritance was received, or funds were spent on storage, or a ship sunk, the end result was a net increase or decrease in the assets owned, with nobody else owing more or less as a result. Sums won in a card game ended up being treated the same way as a profit on a shipping venture, as comparable entries in the profit-and-loss account. Indeed both French and English textbooks explicitly argued that all these operations were essentially the same, and that there was no need to distinguish between the various form of profits and losses involved (Gervais 2012).

The complete absence of theoretical reflection on the sources of profit could be written off as an optical illusion. The empirical itemization of specific profits and losses compiled through double-entry accounting in a profit-and-loss account could have been enough to fulfill the needs of the practitioners. But turning to actual account books, one has to admit that even the most advanced users of double-entry accounting usually wrote down profits and losses in such a way as to make any detailed profit calculation impossible. Levi Hollingsworth, a Quaker merchant of Philadelphia active in the 1780s, left a large set of books which prove that he was an extremely zealous, and in some way modern, accountant, drawing up a general financial statement for his business every year, for instance, which showed the net gain or loss occurred in the past year. But even Hollingsworth recorded profits and losses indiscriminately into a year-round profit-and-loss account, which he used as a cleaning-up tool. He then transferred the balance of this running profit-and-loss account to a second, separate profit-and-loss account opened specifically in order to register his overall profit in the final balance at year's end. The result of this was that in this final yearly balance, the profit-and-loss account contained a few lines at most, and gave no detail as to the sources of profit (N.B.: specific examples in this paper are drawn from the

databases built by ANR MARPROF, available on demand at <http://marprof.univ-paris1.fr>).

Other merchants using double-entry accounting shared the same approach, registering their overall profit, if at all, as the sum of very diverse individual entries sometimes accumulated over years of activity, and in such a way as to make impossible any analysis of the actual sources of these profits, except through a painstaking rereading of dozens of original entries, often scattered over several pages. Even special accounts devoted to a given product ("Flour account", "Wine account") usually could not be used as a basis for profit calculations, because of the multiplicity of suppliers, qualities and prices involved, and because no effort was ever made to trace and individualize one lot of goods from its arrival to its departure. Lots acquired at various times and at various prices were jumbled together and redispached throughout various transactions, so that nobody could possibly find out how much had been made on a specific barrel or package. Moreover, goods bought and sold were often regrouped into general accounts ("General Merchandise" being frequently used), or even transferred from suppliers to buyers without transiting through the specialized accounts supposedly listing them. A merchant could thus have one "Flour" or "Sugar" account, but simultaneously buy barrels of flour from supplier X and record them as part of a cargo for one of his shipping ventures, or buy barrels of sugar from X and sell them to Y, without either set of goods ever being listed in the specialized "Flour" or "Sugar" accounts (Gervais 2012, 2014).

And this was far from the worst possible situation when it came to merchant accounts in the Early Modern period. Using the somewhat arcane and complex tool of double-entry was a characteristic of the largest traders, or at least of the economic agents most committed to building detailed accounts. The majority of the population which did keep records mostly used single-entry records, which left even less space for profit analysis. Transactions were at best recorded in "current accounts" books, usually without any profit-and-loss account at all, and at worst, and more commonly, recorded in chronological order in day books. Accounting for profit did appear in the specific situation in which several investors had to be given accounts of the results of a joint investment. This was the case not only with the better-known official joint-stock companies active in international trade, such as the various East India Companies, but also for any informal partnership and joint venture, such as shipping ventures. This has led some authors to link profit accounting to the "socialization" of

capital (Bryer 2000a, 2000b). But even then what such investors received was usually a rather schematic account of the net result of the venture, with no effort made to detail its sources. In most cases, anyway, crucial information would have been missing to assess the key variable, the price at which the goods had been sold, since the partners had no way to check either the quality of the goods sold or the extent to which the sales price was the "best" price given the conditions at the endpoint market. Indeed the goods traded, whether regionally and nationally (wheat, wine, cloth), or overseas (colonial products, finished European goods, and the highly peculiar "merchandise", West African slaves) were usually not traceable even from the point of view of the merchant who took primary responsibility for trading them, since the loads eventually sold were usually made up of several different lots originally bought at various prices. As a rule, no attempt was made to separate the goods by source at the time of the final transaction, a practice which forbade any detailed profit statement since final selling price and original buying price were actually averages built over several, often very different, lots of goods (Gervais 2011).

Overall, the core goal of double-entry accounting among merchants was to accurately track complex flows of credit between multiple accounts in order to achieve a detailed, "true and fair" view of all assets, but most particularly credit relationships, when a final balance was drawn (Gervais 2012; Wolnizer 1991). With this goal in mind, net profits and net losses had to be cleaned out rather than analyzed, since they could throw off the valuation of certain assets, and did not add anything to the measurement of credit flows – they were "incomplete", in the words of the authors of textbooks. Thus they were not analyzed, not only because market conditions made such an analysis pointless, but also because the focus of merchant accounting activity was elsewhere. This is particularly remarkable when one notes that significant numbers of large producers, such as ironmasters or landed proprietors, had routinely compiled, analyzed and drawn advice from detailed profit and loss sheets at least since the end of the Middle Ages (Fleischman/Parker 1997; Toms 2010). Why, then was the focus so different among merchants, who arguably made up, if not the most powerful, at least the most dynamic and successful economic group throughout the Early Modern era? To answer this question, one has to turn to merchant practice, and to the strategies underpinning both these practices and the accounting practices they gave birth to.

Market segmentation and market control, the tools of merchant domination

Merchant strategies, which can be recaptured in particular through correspondence among the actors, were not merely the result of partly imperfect markets. What was prominent in merchant minds was their ability to manipulate market segments, as well as supply and demand within them. In this overall framework, cartelization was a key feature. Market activity always took place within a peer group, ideally achieving oligopoly or oligopsony control over the buying and selling of a particular type of commodity in a particular place. No market was free, no actor was isolated. Even at the lowest level of the merchant world, that of the village grocer or urban hawker, prices were never set through a straightforward confrontation of offer and demand, but rather as the local manifestation of complex battles between competing oligopolies. The key to this strategy was the ability of merchant subgroups to control access to a given market segment, and constitute themselves into merchant "rings", quasi-cartels with strong oligopoly and/or oligopsony positions over this particular market segment.

The existence of such rings, incidentally, can be directly observed through quantitative study of account books: by plotting the network relationships created by transactions between personal accounts, and between merchandise accounts and personal accounts, one can determine in both cases that these transactions were grouped into sub-networks with little communication among each other. This is particularly striking when analyzing transactions involving merchandise accounts. Typically, such an account (say, "Flour") was linked with a very specific subset of suppliers and customers, with little if any relations to other merchandise accounts, and even to other subsets of actors. A few key players, on the other hand, shared with the principal owning the account books the distinction of being connected with several subnetworks at once. This, again, was never mere chance, but rather the direct result of a particular status such "bridge" actors had achieved with respect to the overall operation of the merchant owning the accounts. They could be the merchant's main agent in some crucial capacity or location, his or her official partner in a partnership, or his or her unofficial banker and provider of cash or credit (Gervais 2012; McWatters/Lemarchand 2013).

Building trusted groups of collaborators, however, was not in and of itself enough to achieve a cartelized position. This could stem from a variety of sources, but the most general starting point for this process in the Early Modern period was probably the ability to achieve a decisive comparative advantage in quality control. Because goods were not standardized, and imitation and fraud ubiquitous, a buyer could never be sure of the exact nature of what he bought (Beaur/Bonin/Lemercier 2006; Gervais 2008). A piece of cloth could be the luxury product of a French royal manufacturer just as well as a cheap imitation from across the border in Flanders or Switzerland. Good-looking flour could turn out to be an inferior product, gone to rot and inedible after a few weeks. Expertise was needed to detect even basic blemishes in most products, and even experts could be fooled. One merchant could not hope to reach a satisfactory level of expertise for more than a very limited set of goods, and even then could not be sure of the results. Specialization was thus only a partial solution, and not a very satisfactory one, since it restricted merchant activity to a very small number of markets. Since all markets were both heavily segmented and collectively managed by well-defended quasi-cartels, no economic agent could hope to dominate one market enough to generate massive gains. Rather, the gains one could hope to reap were commensurate with the number of market segments one took part in; in this regard at least, specialization was a self-defeating strategy.

The way to solve this particular conundrum was to avail oneself of a network of trusted suppliers and agents who would provide expertise for a particular market segment – a step which would also turn the group of allied peers I mentioned earlier into a formidable tool for market control. Lack of access to such specialized networks constituted a well-nigh impenetrable barrier to entry for newcomers. A would-be player on any market, say colonial sugar in Bordeaux in the mid-18th century, for instance, needed to create his (the colonial trade was entirely dominated by male actors, see Haggerty 2011) own network of planters and intermediaries in the sugar islands of the Caribbean, and to find buyers who would be willing to provide him with outlets for the sugar imported. On both sides of the equation, the lack of trusted partners meant a sizeable risk, or indeed the near-certainty, of being loaded with an inferior product, or losing on sales because of underbidding on the part of the buyers or commissioners taking the sugar. This was a universal problem: in the absence of detailed institutional standardization and norms of the kind introduced in the second half of the 19th century, hierarchies

of quality were essential to determine the price of a product, and these hierarchies were as much the product of a joint negotiation between buyers and sellers as the translation of any intrinsic characteristic of the good itself. To a large extent, quality was what the two parties to the transaction decided it was, and absolute mutual trust and co-operation was essential if one wanted to get what was commonly called "the best price."

Incidentally, this best price was not always the highest selling price or the lowest buying price; what was important was the price/quality combination. Higher priced, higher quality goods could bring higher profits on some market segments if customers were willing to pay more for what they liked better. Profit, insofar as it resulted from the price/quality combination built by the supplier, was thus also heavily dependent on an adequate match between that combination and the tastes of the target customers. Proper information on every aspect of the endpoint market, from customers' tastes to the evolution in prices, was essential. Suppliers' and buyers' networks thus doubled as sources of information on the state of a market segment, which was the other major barrier to entry that merchant rings relied on. Without detailed and correct data on both customers' preferences and price levels at a specific place and time, a merchant was sending goods blindly, and hopes of return were little more than wild bets (Gervais 2008). Coordination through information flows was essential, but information was easily controlled in a world in which most economic agents operated privately and in almost total secrecy. Again, a would-be player had to gain sources of information, which means that on a given market segment, only those with what would be called today "insider information" could trade with a reasonable hope of profit. Indeed, when for various reasons (distance, wars, etc.) the flow of information was missing or insufficient to ensure informal coordination, merchants went to great lengths to build special mechanisms enabling some degree of formal coordination, such as the public and regulated auction system Spanish merchants used in Spanish America (Lamikiz 2014). But in general, the possibility of achieving a very high level of control over market information and of preventing others from accessing it was the second main weapon whereby rings achieved complete dominance over given market segments.

A side result of this particular form of market exchange is that prices were never used as public signals on an open market on which demand and offer faced off, and in some cases were not even the best signals available to gauge the

state of a market. Very often, public prices were lagging behind actual market evolutions, and sometimes did not reflect them at all. It has been shown that price-setting in the Early Modern era was largely cyclical, with a very strong path dependency at any particular point in space and time. In other words, there was a "customary" price for a particular good at a certain point in the yearly cycle (Grenier 1996). The main strategy merchants used to extract more than the "customary" profit was to play on quality, as we have seen. But they also derived their best gains from temporary imbalances in these market segments, in particular price differentials. Much as with contemporary stock exchanges today, these differentials generated profit only for the merchants reacting to them early, since latecomers would enter after the imbalance had been corrected. Merchants constantly traded information on prices in the markets they were familiar with, because such information was not public, and constituted insider knowledge which could be exploited for profit. Such flows of information were of course strictly restricted to the members of a ring, and reinforced the barriers to entry newcomers would face.

The most sought-after situation for merchants arose when exogenous shocks – a war, a bad harvest, any unusually large imbalance between supply and demand – would generate exceptional departures from the "usual" price, and huge monopoly profits for the groups able to step in and buy or sell at these moments of crisis. Collusion, cornering and speculating were thus rampant, in spite of all institutional efforts to limit what was seen as a source of scandalously undeserved profit, as well as of dangerously wild swings in market prices considered especially problematic when it came to vital goods such as wheat (in Great-Britain, injunctions against "forestalling, regrating and engrossing" go all the way back to the Middle-Ages...). In crisis situations, the cartelized nature of a local market segment tended to become even more pronounced, turning it into an arena in which two merchant rings, one of buyers, one of sellers, battled each other. Agreement between the two rings translated into brisk market activity, while lack of agreement brought transactions at a standstill. In this particular context, the true gauge of the state of the market was thus changes in the quantities of goods bought and sold, much more than price changes. A radical departure from the equilibrium price hypothesized by economists would be translated into a disappearance of buyers or sellers, and the outcome of the conflict would be a new price, as in classical economics, but this price would not have been reached through an

open, transparent bidding process. Rather, secret negotiations would take place, and their unfolding would be primarily dependent on the availability of capital and credit on each side of the battle, and on bets on the future evolution of supply and demand.

All this should prompt economic historians to reevaluate what are usually analyzed as Early Modern market "imperfections". In these markets, insider trading, buyer and seller cartels, price-fixing, speculation, market cornering – in short virtually unbreakable barriers to entry and imperfect information reserved for the privileged few – were not bugs, they were fixtures. Merchants living off of these tools had no reason to agitate for transparent information, easier access to capital and credit, or State-run standardized norms as long as they were on the right side of the rings controlling a given market segment. Outsiders could call for all these, and more generally for an end to privileged positions, for better freedom of entry into a market, and for the destruction of tariff and non-tariff barriers, but they would quickly change their tune as soon as they themselves had consolidated a cartelized position. The tug-of-war between militants of the free market open to all and defenders of traditional market management by a privileged few was thus constantly reborn with new actors, and should not be seen as reflecting any deep-seated opposition between two contrasting political economies (Hirsch 1991).

One should also note that this type of economic organization led to a stark differentiation between actors with market power and all other economic agents. Extra profit could be made through the market management of price/quality combinations and the exploitation of exogenous shocks to customary prices, but producers who were not themselves actors on markets were barred from benefiting from such techniques. Indeed, for each given market they were at the mercy of the specialized networks who operated on that market, and basically controlled it. The result was a plethora of informal cartels, what I would propose to call merchant "rings", which could force on their suppliers extended delays for payments, or transfer storage costs to them, while manipulating selling prices to their best advantage and gouging customers and suppliers as much as customary price structures allowed for, and much more whenever crisis circumstances arose (Margairaz 2014; Villain 2014). Control of market access was buttressed by a combination of privileged information and such cartel-like networks, and was often complemented by merchant ownership of the means of communication,

including the roads themselves whenever privileges were granted, as in the cases of turnpikes and canals (Gervais 2004). By the end of the 18th century, this economic organization was widespread enough to generate an increasingly visible shift in economic and social power away from the earlier landed elites, to the benefit of what may have been a merchant ruling class.

Merchant rings, credit relationships and the mechanics of merchant accounting

The crucial result from the point of view of our initial question on merchant strategies, however, is that the maintenance and development of the merchant "rings" to which a merchant belonged held precedence over any other consideration. Each specific ring fulfilled crucial functions in terms of the relationship a given merchant maintained with the market segment over which the ring held sway. The ring provided connections to a network suppliers and buyers both trusted and knowledgeable, who also functioned as the source of the information necessary to conduct transactions efficiently. It unified local players in such a way as to prevent outsiders from creating dangerous competition, and enabled its members to enforce their demands on producers and customers alike. It served as a mutual insurance fund, since profits and losses were usually spread over several partnerships within the ring; and, last but not least, it could be used as a quasi-bank, since the members could draw on each other's credit whenever the need arose.

This banking function is particularly important when one considers that most transactions in the Early Modern period were done on credit. Credit came in two forms: interest-free book credit, and commercial paper, more or less formalized (by the 18th century, simple IOUs were ubiquitous, and the earlier, strictly regulated letters of exchange had largely faded away). Book credit was even less formalized: once an account was opened, the account holder routinely both granted and was granted free credit in the form of unsettled transactions. Debts owed on transactions simply recorded in account books could reach impressive levels. A single account in the Bordeaux trading house of Abraham Gradis in 1755 was found to be in the red by 40,000 *livres tournois* for close to two months; using daily wages of construction workers as a basis for comparison (Baulant 1971), such a sum would be equivalent to EUR 2 million today. An overdraft of this magnitude was possible only with close business associates, but even small village grocers loaned out relatively large sums, albeit in smaller

amounts and over dozens of customers to whom they extended credit.

As I pointed out in the introduction, free credit used in such a generalized way is something of a puzzle from the point of view of today's political economy. What possessed these creditors, and why didn't they convert these informal book loans into commercial paper? IOUs had two major advantages over book loans: they could be negotiated at a discount, at least among the people who knew both creditor and debtor, and in some cases in much larger circles, and above all they were limited in time, and always bore interest once the period for which the loan had been originally extended was completed. Rather than loaning 40,000 *livres tournois* for free, Abraham Gradis could have drawn up a note of hand for the same sum, valid for a week or a month, after which it would have started paying interest. That he chose not to do so must be understood in the wider context of a very different political economy characterized by markets dominated by merchant rings. Then, and then only, does the choice of an interest-free loan become economically logical.

In a way, opening an account, at least for a fellow-trader who would eventually end up owing or being owed large sums of money, was tantamount to creating a partnership. Even at the lowest levels of commerce, a book account in a village store meant that the holder was granted the status of privileged customer, with a right to some free credit at least. In all these cases, the account holder was engaging in a mutual relationship which included duties, including economic duties, on his part as well. Credit could and did circulate both ways, and a merchant in trouble economically could turn to the network of people with whom he or she was "in account" to borrow some cash, take goods while withholding payment, or even ask for a credit loan in the form of a note of hand which he would discount elsewhere. Even more importantly, book accounts were the economic manifestation of the solidarity between members within a merchant ring. Because they traded with each other and were allied, each member of the ring would have accounts with at least several other members, and would thus be able to draw easily on the free credit thus extended by some of their partners.

Free book credit was only one element of a larger structure of credit, which, thanks to quantitative analysis of accounts, can be shown to have been crucially important to merchant activity. To start with, part of the existing interest-free book credit extended to business partners could be

turned into a negotiable instrument. Merchants routinely cleared debts between themselves by using each other as a clearing house: a debt owed by merchant X to merchant Y would be offset in the books of merchant Z, who would debit X's account and credit Y's account with the amount owed by X to Y. This simple compensation method, with no recourse to cash or commercial paper, amounted to 20% of the value of all transactions in 1755 for Gradis in Bordeaux, and 25% for Hollingsworth in Philadelphia in 1787. Other merchants may have been less prone to this kind of clearinghouse-like activity: the Chaurand firm of Nantes, at the end of the 1770s, recorded clearing transactions worth only 5% of the overall amount exchanged. But the slack was more than taken up by the other dimension of merchant credit, commercial paper (generally in the form of simple IOUs). Once this commercial paper was factored in, merchant firms ended up with well over 50% of the value of all their transactions incorporating some form of credit (75% for Hollingsworth, 69% for Gradis, and a still hefty 52% for Chaurand). Overall, it is no exaggeration to claim that most Early Modern transactions took place on credit – which means in turn that the credit function of merchant rings was absolutely essential to its members.

The resulting constraints on Early Modern economic agents go a long way towards explaining both their credit practices and their accounting practices. Free credit was a badge of solidarity within a merchant ring, which we can define as a tightly-knit group of operators working to keep their hold on a market segment. Possible losses from forgiven interests were a marginal concern for a ring member, compared to the burning need to stay within the ring and to foster ring solidarity and cross-participation. Even in purely calculating terms, the windfalls from ring activity were certainly much larger than whatever profit one could derive by charging interest, especially since one would presumably also have to start paying interest to others too. Similarly, both strategies and profits stemmed from one's position in a ring, not from any particular venture or relationship. Calculating the net result of a particular transaction, at a certain price/quality level and at a certain point in the ring's lifecycle, was not particularly enlightening. Indeed, many transactions could end up in apparent net losses if taken in isolation, while being part of a highly profitable larger whole. Thus in 1755 Gradis loaned for free on account more than two hundred thousand *livres tournois* (equivalent to about 10 million euros in today's money!) to one François Bigot for months on end with not the slightest apparent return, but this generosity becomes much more understandable when one discovers that the said Bigot

was in fact the intendant du Canada, and a key actor in providing Gradis with highly profitable royal contracts for the supply of the province (Gervais 2012).

Overall, nurturing one's participation in rings had to be the paramount goal for any rational merchant. This was profit maximization for sure, and could bring about the tracking at regular intervals of overall gains in assets. But the particular political economy in which this search for profit took place meant that "profit" was much better measured as "credit", and not only quantifiable credit, but rather a wider notion of credit which encompassed the qualitatively assessed position of power one achieved within the rings of which one was a member. In this qualitative approach, credit was the ability to trust and be trusted within a bi- or multilateral relationship in which all sides were expected to act predictably and consistently according to implicit collective rules of behavior, rather than simply trying to maximize their own personal gains. Such a trusting relationship enabled flows of monetized credit, of information on markets and products, and of the products themselves, managed in such a way as to ensure a fair distribution of the profits arising from the control the group thus created maintained on a market segment (Gervais 2012, McWatters/Lemarchand 2013).

Even double-entry accounting could provide only partial tracking of the gains of a merchant in such a universe, since a good deal of the "gains" could be of a wholly non-quantified nature (entry into a new ring, e.g.). Moreover, the most interesting indications, strategically, came from the qualitative evolution of the set of accounts which directly reflected ring activities, which explains why all textbook authors insisted on the necessity of "weeding out" costs and profits to gain a "true and fair view" of the situation of these "complete" accounts. Each personal, merchandise or venture account was a strand in a narrative describing the life of a particular ring (McWatters/Lemarchand 2010), and its value had to be carefully tracked. On the other hand, the figures thus recorded were far too context-dependent to be used outside the specific framework of the ring activity which gave rise to them, and generally to offer any larger strategic lesson for future activities. Even overall assets measurement was merely a sign of the dexterity with which a merchant juggled with his many commitments, and no general conclusion could be derived from it either. As for costs, their meaning could change radically from one account to the next, depending on who was paying what to whom. Because ring activities were usually managed through complex subcontracting structures, particularly in the form of temporary partnerships or commission-

ing activities, a "cost" could actually turn out to be a payment from a principal to the merchant recording it, and acting as commissioner. As with profits, the meaning of costs was wholly contextual, and tracking them in a detailed way would have been a largely pointless exercise.

Conclusion: from the age of commerce to industrial capitalism

Presenting the Early Modern era as a credit-based society in which a well-established merchant class dominated the economy through its collective control of highly segmented markets should lead us to discard the usual disparaging analysis of these same Early Modern markets are somehow "inefficient" or "incomplete". Such denunciations make sense only with respect to the ahistorical, theoretical free market of classical economics, which was nonexistent then and remains a figment of economists' imagination nowadays. These informal groups which I refer to as "rings" managed local markets with considerable efficiency during the Early Modern era. They built formidable barriers to entry for newcomers, ensured a smooth flow of goods and information, prompted large segments of rural and urban society to increase their participation in the market exchanges they organized, and launched colonial ventures which spanned the entire world. This last point is particularly remarkable: the expansionist streak Europeans started to develop around the Renaissance was entirely motivated by the ruthless push for capture and domination of new market segments (and of the corresponding products and trade routes) *within* foreign countries, a tactic unknown to earlier merchant groups in either Europe or Asia. Admittedly this was not merely a merchant program: European rulers, as much as European merchants, wanted complete control of local products in far-away places, and they were the ones who launched most colonial ventures. But the simultaneous rise of merchant control of local markets abroad as a tool for economic domination, and of European colonial expansion, is probably more than coincidental.

At the other end of the period, historicizing the Early Modern search for profit, and more generally the rules and attitudes governing market-based processes at the time, should also prompt us to recast in an entirely new light the transition from the commercial society of the 18th century to the industrial capitalist society of the 19th century. Giving up on market control as the key tool of economic domination may have to be explained in terms of a breakdown of a well-established and fairly stable system, rather than as a straightforward, universally appreciated progression.

Indeed the feelings of failure, chaos and dislocation which came with the early Industrial Revolution in the mid-19th century are much easier to account for if one accepts that the new, industrial capitalist focus on productivity and costs was a revolutionary break with the past, upsetting centuries-old habits and intellectual attitudes (Gervais 2004). The truly impressive advances in cost accounting realized from 1850 on can also be read as a symptom of a radical change in the way profit was pursued and measured. Last but not least, the lag between the cluster of inventions marking the closing years of the 18th century and the acceleration of the rate of growth in the second third of the 19th century makes much more sense if the first phenomenon was a side effect of the development of market society in its earlier form, while the second was the sign that this same market society was in the process of collapsing and being replaced by entirely new economic forms.

Merchant rings and segmented control of the market, at least in the somewhat schematic way in which they are presented here, do not provide an all-encompassing explanation of society in the Early Modern period. One would have to articulate them with other aspects characteristic of the period, particularly State-based economic power and the relationship of both groups, merchants and State officials, to the land, its owners, and its production. Such a path of inquiry would take us even further into a specific analysis of how Early Modern agents were conceptualizing and pursuing profit, and of the extent to which their conceptualization was different from the one holding sway today. More generally, economic historians should reconsider narratives positing a continuous, unchanging profit motive throughout history. In many ways, Aristotle's *chrematistic*, already accepted more or less at face value by Marx, has never ceased to underpin the concept of utility maximization, reducing the need for any time- and space-specific analysis. Profit was profit, capital accumulation was capital accumulation, and standard economic analysis applied regardless of the social-institutional forms within which these figures were deployed. While the narrative turn in many social sciences may have led in some cases to the aporia of universal relativism, there are still valid theoretical grounds for believing that profit, economic power and the way to measure and accumulate both must be more contextualized historically, and not simply assumed to be what we mean by these terms nowadays.

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Measuring the Value of Things in the Middle Ages

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A French version of this text has been read in Torino in 2012 at a "Round table" organised by the European Research Council program SAS (Signs and States), about the notion of "value" in the Middle Ages.

The difficulty of understanding the value of things in the Middle Ages is one of the obstacles to our understanding of economic life in that era. The issue is first of all associated with the ways medievalists quantify and use numbers. Value was first investigated when studying prices in the 19th century, as a prerequisite to any knowledge of the economy. This avenue, explored by major researchers like J. Thorold Rogers and d'Avenel in the 1880s-1890s, reached its apogee in the 1930s with the extensive investigation launched by Beveridge (Dumoulin, 1990). It has been ignored over the last decades, while economic history was losing ground in the eyes of French medievalists. For some twenty years their researches and their reflections turned rather to qualitative data wherein the issue of non-mercantile exchange, of donation and its economy, has gained a more prominent position than the study of mercantile exchange, initially considered as research on prices and values, or the relations between things (Testart 2001). This evolution is well known, if not always explained and understood thoroughly (Feller 2011a). It is supported by a number of methodological assumptions which make more complex the study of an economic history which specialists now wish to embed in a social context. I would like to stress that this viewpoint is short-sighted. The main focus since then has been the study of the relations between people through things. The aim is to include the understanding of the value or price ratios established between things into the latter. The study of prices in the long- or short-term, carried out by comparing the factors or the functions of supply and demand, should not be considered in any way incompatible with the study of social determinants interfering with pure market mechanisms. On the contrary, the former combine with the latter as part of their expression. This combination does, however, admittedly undermine the understanding of the processes at

play in their formation. Not only the measurable, economic data, but also describable and accessible but non-quantifiable facts should be integrated into the equation (Feller/Gramain/Weber 2005). Consequently, although measuring value is a challenge and its resolution may throw light on many aspects of economic life, it is not an exhaustive description and it is also necessary to explore the procedures involved when objects are exchanged, such as conversion systems, to better grasp the reality of exchanges. In the same spirit, I would like to present here a few reflections on the measurement of values, on prices and counterparts, mainly but not only, during the early Middle Ages.

Mercantile exchange and non-mercantile exchange

Exchanges, regardless of their aim and of whether or not they are commercial, depend on an accurate knowledge of the corresponding items owed or expected by each of the protagonists. Any transaction, regardless whether it is a market transaction, is based on the knowledge of the value of the goods to be exchanged and the establishment of a comparison scale between them (Testart 2001a, 2012). The context and the result of the latter guide decisions in how to settle the exchange. A transaction can be offset in money or in kind and, in the latter case, using any type of objects, from cattle to grain, pepper to iron, considered as equivalent to what is offered. The measurement of value, the comparison between what is offered and what is obtained in exchange, always takes place, at least implicitly. The actors must know what they have given and what they have received and can determine with relative accuracy whether they owe or are owed anything, whether they have received more or less than what they have transferred, who is creditor and who is debtor. This awareness leads to the building of complex relationships between individuals potentially including relations of dependence or subjugation through credit when that is indeed at the heart of the relations between individuals. The relations between things, constructed through the assertion of values, contribute to the establishment of relations between people, of friendship or of dependence.

The value in question must be equally known, at least intuitively and approximately, so as to form the basis of

structured social relationships. The way it is expressed and measured is however highly variable and often difficult to interpret, particularly as resorting to equivalents does not always involve the expression of a monetary value. This is made more challenging since medieval documentation only rarely provides the background necessary to understand of what is at stake in transactions.

Goods and products are in circulation and counterparts are offered to compensate for them in exchange; the loss of goods is balanced out by the acquisition of other goods. However, if in our modern lives everything is settled through money since it is the equivalent of all things, enabling both the measurement of value and the offset of and exchange, the situation is completely different in ancient economies. Measuring operations as well as payments are often, if not always, affected by a number of factors. These factors may be associated with existing relationships between the transacting parties before the exchange, which are only rarely market relations. They are also linked with the intent of the action of exchange as well as the availability of means of payment. Finally, they relate to the diversity of the measurement units, which is one of the salient features of the ancient economy (Testart 2011). The ubiquity of these factors was such that the use of money to measure value, although frequent, was neither compulsory nor necessary.

The issue of the category of exchange, specifically the opposition between mercantile exchange and non-mercantile exchange, is at the heart of the problem. Whether goods are exchanged against other goods or against money, there is no guarantee that each of the parties considered seeks to gain immediate material profit. The entanglement of economic relations with social relations may be such that market exchange cannot account for what actually takes place between the vendor and the purchaser at the time of the sale (Polanyi 1983; Humphreys 1969). The junction of a supply and of a demand leading to the formation of a price, itself adjusted by a monetary payment, is not sufficient to account for the amount and the means selected to settle it. At the same time, prices are stated and their payment closes a sequence or terminates an exchange. Consequently, we must consider that the management of rarity, the allocation of available resources, the arbitrations performed by the agents in favour of such or such good or of such or such action, are just as important in the process of formation of prices as the social determinants of the transaction (Arnoux 2009; Lévi-Strauss 2001; Weber 2005). No more, no less.

In this sense, the value of things seems impossible to measure. It is part of the field that can not be ascertained by searching for regularities. The unit price of a plot of land may seem unreliable because its sale includes a collection of facts which underpin the processes through which the parties have agreed on a price and on the objects required to pay it off. G. Levi had thus shown that, at the end of the 17th century, in Piedmont, the sale of the plots of land only took place upon the completion of a relation which was first of all a relation of credit (Levi 1989). The transfer of property for value consideration only happened when it was necessary to cover debts: the price demanded and paid integrated the whole process which had taken place thus far and which was closed by the final transaction, establishing an exchange for the settlement of all outstanding claims. Studies relating to the sale of real estate during the early Middle Ages have also been used to single out different cases which lead to price variations for the same object or the same surface area: a sale may mask a new customer, in which case the price may be vastly overstated, since the aim is to obtain services and friendship, not to acquire land. Such a transaction may serve to seal a friendship or prepare the inheritance transfers accompanying a wedding. The price stated and paid may then be low since there again the effective trading differs from what it appears to be initially (Feller/Gramain/Weber 2005; Testart 2001a: 731,735).

In this case, the regularity should be sought somewhere else than in the relation existing between the usage value and the exchange value of the object, or between its intrinsic value and the final price obtained for it. Rather, the regularity can be found in the amounts which have circulated between the purchaser and the vendor prior to the sale, and in the history of loans and refunds which, carried out or deferred, lead to a situation where a sale is the only option. But the sale price does not provide any indication of the value of the good transferred: to understand this value, we must be able to trace the entire chain of obligations contracted by the parties, which is often all but impossible, but may sometimes be attempted.

Knowledge of the value of a good is a knowledge shared among the different actors. These actors perform on several levels at the same time, making it necessary sometimes to use quantitative estimates in order to measure the object transferred, to assess its value and to understand that of the counter-offer, and sometimes it is not.

The instruments of assessment and of payment

The first item to examine is the instrument of exchange: it enables us to know which context we are in.

The systematic use of money is not necessarily the rule, as mentioned above. Exchanges may be paid off in kind and different systems can coexist. The best known system was that explored some time ago by Jean Gautier-Dalché and recently re-examined by Wendy Davies (Gautier Dalché 1969; Davies 2002, 2007, 2010). It relates to a peripheral region, Galicia, which was confronted with specific economic problems due to its boundary position. The investigation relied exclusively on written documents recording market-related transactions, i.e. sales in which a *pretium*, a price, was mentioned. It concerned the period from 900-1000 C.E. In the Galician documents, sale language is different from donation language, and the absence of a mention of price invalidates the transaction. There are therefore two levels, that of mercantile exchange where things are described, measured and assessed and that of non-mercantile exchange where the value is not necessarily mentioned.

Florence Weber, Agnès Gramain and myself have made analogous observations on a dossier of Abruzzese documents dating from the 850s-880s. Following the methodology recommended by Florence Weber, we have distinguished the acts wherein the goods exchanged, land, gave rise to a precise description, with a measurement of the surface area, listing of the limits and mention of a *pretium*, from those in which the surface area was not given but where a *pretium* was nevertheless fixed (Weber 2005; Feller/Gramain/Weber 2005: 73-91). In the latter case, whether the payment was made in money or in kind or as a mix, we thought it appropriate to mention a *counterpart* in the exchange, since, in spite of the appearances, the sale of the land may not always have been a commercial transaction. The files in our possession showed that the transfers of plots of land for value consideration could indeed mask new customers or other transactions, such as preparing weddings and hence settling the question of dowries, or possibly covering debts (Feller 2002). It seems like if the distinction between sale and donation was less accurate in the Abruzzi than in Galicia.

Under these conditions, the nature of the objects used to offset the exchange becomes crucial. In Galicia, the price consists of clothes, of breeding products, and sometimes

gives rise to an assessment in silver *solidi*. This varies by chronology and geography: in Sahagun, in the centre of León, up to around 930, assessments and payments were made using clothes and agricultural products. After that date, the prices were expressed in *solidi*, but not necessarily offset in money. Conversely, a few dozen kilometres from Sahagun, in the archives of the Cardena monastery in the East of León, the transactions express values in *solidi* before 930 and after 960. During the decades 930-950, the transacting parties resorted to manufactured objects (Davies 2002: 155-159). However, the moneys mentioned, essentially “*solidi*”, are not in cash, but in reporting currencies: when gold or silver appear, they play the same part as the other objects, valuable or not, represented in the payment, and do not act as the equivalent of objects. Money here is used as one of the elements present in complex bartering procedures. On the other hand, the objects used in the exchanges by the Galicians are quite numerous. In three quarters of the cases they used agricultural products: drinks, bread, grain or cattle. Horses, oxen, mules, goats and donkeys are represented in transactions which thus include the traction and portage instruments. The working context is never far away. Manufactured objects are a little scarcer, but the price lists also include silver plates, precious swords, wax or silken clothes, and belts. These apparently heteroclit lists provide indications on the aims pursued by the transacting parties: the provision of objects of domestic use, associated with prestige or work, is certainly part of what they were looking for.

The Abruzzi offer, for the prior period, similar lists (Feller 1998a: 361-385). We suspect that these prices are not there solely for their exchange value and were not simply substitutes for money. They admittedly express a measurable, and often a measured, value. But their presence in the exchange also has a classification effect: transferring a plot of land in exchange for a goat or a draft animal does not have the same meaning as transferring it for a horse or a valuable sword. The objects changing hands should be considered not only as a function of their value but also as a function of their particular status and what that conveys. The common features of these operations are clear: the plot of land transferred in exchange for a movable property which acts as a store of value; the transfer is a form of realisation of an asset. But the need to sell, to dispose of a property object which is also a production factor, is not the same if we wish to obtain money, a draft animal, a weapon or a war animal in exchange. We can also see that the holder of valuable or sought-after objects, by transferring them against plots of land, exerts a protective function,

giving a good whose transfer may appear as a modality of its patronage, in return for a plot or a farm business. Providing draft animals to a customer without the latter having to run up debts with the seller may express the protection that the patron exerts on the customer and is also a relatively versatile form of expression of social domination.

Such situations appear more clearly in the cases where the transactions are obviously not commercial, as in the file of Bishop Meinwerk of Paderborn, the main elements of which date back to the beginning of the 11th century (Reuter 1995; Feller 2013). Land gifts are not free but require the transfer of objects, whose value may be significant. Sometimes, this transfer is accompanied by foodstuff incomes paid annually to the donors. The exchange is here ambiguous, since it mobilises values, represented by quantities of non-negotiable gold, furs, weapons, victuals, and plots of land: it is not possible, in this case, to tell whether the quantities exchanged have a common measure or not. This information obviously did not present any interest for the transcribers of the extraordinary series of exchanges inscribed in the bishop's documents, a text whose status is close to *gesta episcoporum*. The exchanges are presented in this file as different from transactions; rather, they are described as successive donations inspired by the piety of the secular donors, who transfer their plots of land, and the benevolence of the bishop, who gives portable goods, not so much in return nor specifically because of the donation, but voluntarily. The manipulation of these acts in the document – which has some features of the cartularies – leads to a succession of transfers without avowed economic aim. We can see clearly what is happening: quite often, after transferring a plot of land, the donators benefit from an income or their properties are transferred back on a temporary basis, with the addition of other plots of land. They have in fact contributed in order to consolidate their annuity, transferring their capital in whole or in part either by obtaining an annuity or by consolidating their exploitation (Feller 1999; Morelle 1999; Manzano 2013).

That case is extreme. Usually, in cases involving men and women not belonging to the high aristocracy, objects are assessed based on accounting units. For northern Spain, we know that money was minted until the 11th century and that the circulation of foreign coins was a marginal economic factor, mainly linked with the presence of pilgrims (Davies 2002; Manzano 2013). Consequently, the payments specified in *solidi* were made most often in kind, as in central Italy, where the payment in *res valentes* was

mentioned the most frequently: *de mea mobilia valente tot* or *in appretiatum valente tot* were the most frequent mentions in the payments (Feller 1998b).

Having said that, things are not necessarily that simple: the *solidus*, in Spain, can refer to a silver weight measurement at the same time as a monetary accounting unit. By extension, the *solidus* was also a reference for all sorts of objects which could serve as a means of payment. Thus, the texts mention *vaccae solidares* or *boves solidares*, cows or oxen worth a *solidus*. Volumes of corn could be used as means of payment and as a monetary assessment tool. This enables us to deduce for Galicia the existence of several parallel assessment scales: the high values are expressed in *solidi*, the middle values in volumes of grain, the smaller values in cattle (Davies 2002; Gautier Dalché 1969).

So there exist, even in a society poorly provided with currencies, systems for measuring the value of things accurately, using material objects of daily life, but linking them always, in the background, to a monetary value, which was fictitious inasmuch as currencies do not circulate, or at least not at the social levels that the written documentation enables to reach.

The substitution of objects with currencies is therefore common, either because the transacting parties want to give the exchange a particular meaning or because monetary substitutes are economically necessary. The absence of available cash in the early Middle Ages has been established archaeologically and which created significant problems.

The monetary reform of Charles the Great at the end of the 8th century supposedly made available to the public a means of assessment and of payment: the multi-purpose *denarius* (Toubert 1988; Grierson 1976). In certain regions, this can indeed be noted. Thus, the file of the acts of Totone di Campione, in the region of Milano, which is particularly captivating because it relates to a family documented over an exceptionally long period (from 720 to 820), shows the transition of the Lombard monetary system over a few years. In the first decade of the 9th century, the prices were stipulated in *denarii* and paid with that currency, which was therefore in circulation (Rovelli 2005). Conversely, the archaeological excavations, especially those performed in Rome in the last decades of the 20th century, are characterised by the almost total absence of monetary findings for the period from the 5th to the 11th century (Rovelli 1993, 2000; Delogu 2007). The large Roman exca-

valuations of the *Crypta Balbi* were conducted in an exemplary manner and it is inconceivable that the archaeologists have ignored a material as important as currencies, whether in the form of entire coins or fragments of coins detected with a sieve. The *Crypta Balbi*, on the other hand, can be found in an area of permanent habitation, though admittedly of varying density, where activities of transformation and of exchange unfolded continuously during the relevant period (Sagui 1990; Manacorda 2001). Nevertheless, no money was found representing the Carolingian or Ottonian period. The same goes for the Confession of Saint-Peter, also excavated, which has not delivered any monetary item from the 9th-11th centuries, as would have been expected. The explanation lies, for the numismatist who focused on this problem, in the legal tender capacity of the denarius in Italy. Contrary to what is generally accepted, it was a coin with high liberatory power and hence difficult to mobilise for daily purchases. It was supposedly only used for large payments, notably admission fees to acquire a tenure and land purchases: such is at least the reason put forward to explain that no coin was lost in premises where they could have been mislaid easily. The means of payment for daily life exchanges would not be monetary cash but different forms of payment.

This observation should be put in the monetary context of the early Middle Ages, when payments in ingots estimated by their weight on the one hand and barter on the other hand were frequent, even in economies where commerce was developed. Since the 1980s, significant attention has focused on the prominence of scales and weights found, most often in fragments, on sites excavated in Northern England and Ireland, in areas occupied by the Vikings in the framework of their mercantile exchanges (Kruse 1988). It is possible to imagine that, in the absence of means of monetary payment properly speaking, economies largely based on mercantile exchange nonetheless developed thanks to objects which could be substituted for minted currencies. Precious metals, providing that their value is known and the weights are accepted by the parties, have a monetary usage and may be substituted for minted currencies, which may partially explain the relative rare mentions of currency as payment in early medieval sale documents. The use of these metals, however, creates the same problems as conventional means of payment. The weight had to be measured and that the final quantity had to be known as well; a guarantee was required for the weights and measures as well as for the quality of the precious metal weighed. This was finally part of the sovereign's role, regardless whether the latter was a lord or the king. The

absence of a metrology which was perfectly reliable had to render transactions difficult in cases when ingots were weighed. Consequently, the use of precious or simply desirable objects, even to settle small purchases, is logical and provides an immediate and more practical solution for exchanges.

Moreover, a whole economy resting on the one hand on deferred payments and on the other on barter up to the modern era. The use of cutting sticks, for example, is a form of practicing credit and recording debts to defer payment: it has been shown to exist very well in the early Middle Ages (Kuchenbuch 2006; Clanchy 1979). Credit in different forms obviously exists and is recognised as a necessity of economic life (Bougard 2010): it enables economic actors to remedy the lack of liquidity and to engage into operations to anticipate harvests or to invest in infrastructures (Feller 2008a). Economic life was, even then, somewhat sophisticated, and its agents mastered its fundamental techniques. They knew how to defer payments. They also knew how to perform conversion operations which were neither rudimentary nor primary. These techniques, present as of the Carolingian era, were diffused and refined until they reached the degree of perfection they exhibited at the end of the Middle Ages.

It is thus possible to dispense with cash, at least up to a certain extent. This is in the context of the economy of the early Middle Ages, in which actors pursued different goals according to their social positions or circumstances (Grierson 1959).

Converting goods, knowing their value

Here we should distinguish social levels and consider the actors' intents.

I shall take two examples from the Correspondence of Lupus of Ferrières (Noble 1998). He needed lead to improve the roof of the abbey of his monastery. Consequently, he sent a delegation to the King of Wessex, requesting a gift of the quantities of metal necessary. He promised in return to pray for the salvation of the sovereign's soul and sent his serfs to Quentovic to collect the metal, near the mouth of the Canche where it was unloaded (Levillain 1927: 70-74). This transaction obviously had nothing to do with mercantile exchange. We are here in the presence of an asymmetrical exchange wherein a measurable material good, a certain quantity of lead, is exchanged against another, invaluable, the monk's prayers, within a system

which operates on several planes: what was exchanged in these cases did not correspond in kind nor in value to what was given. At stake are the donation of a certain quantity of metal and the salvation of the king's soul. The monk's prayer is the medium enabling the transformation of the donation of metal into an instrument of the king's salvation. We are dealing with a true conversion of a valuable material object into a good whose counterpart in the exchange is infinite, prayer for salvation. The counterparts obtained by each of the actors has no commercial value. It was not truly an exchange but rather a conversion of an object with a commercial value (lead) into another value in terms of salvation (prayer). The game of actors thus takes place on two planes, both economic and spiritual. Both parties acquired something of a value. But the value is not the same: the exchange functions without requiring a material medium. What is at stake here is the competence of the monks to carry out transactions based not on the exchange of values but on their conversion (Toneatto 2012). We are not dealing with a system of exchange of donations any more than with a commercial system, but with another in which the transformation of one type of thing into another is essential to establish relationships between people and institutions. The concept of value is then upgraded to a capacity to incorporate into material goods elements which are neither quantifiable nor measurable.

Exchanges between monasteries and the secular persons do not always take place this way. They are deeply ingrained in the real economy, where valuable things circulate and are exchanged against a price, regardless of the form of payment. Shortly before the operation we have just described, Eginhard, also wanting to cover with lead the roof of the abbey of Seligenstadt, had to commit himself to pay 50 pounds of silver to acquire the metal, as part of a true commercial transaction involving the preparation and the exchange of letters of intent binding both parties, Eginhard to pay and the owner of the metal to deliver it (Eginhard, *Correspondance*: 36). The exchanges between lay and religious institutions may thus occur in different logics, sometimes privileging the search for salvation in the context of a Christian economy, sometimes the search for material goods (Iogna-Prat 2011).

Lupus of Ferrières is particularly interesting for the observer of economic life: he deserves some attention. He ran his monastery outside any form of exchange economy, as if self-reliance, which constitutes the monastic ideal, meant that he did not want to buy anything nor to sell anything.

Thus, in several of his other letters, he informed his readers of the difficult situation of Ferrières. He complained that the loss of his dependence on Saint-Josse deprived the monastery and its monks of its normal supply of clothes and of food (Levillain 1927: 181, 191). He could not face the collection of charges which, in addition to the maintenance of the monks and the reception of the guests, the annual donations to the king. He consequently had to buy corn on a regular basis and the brethren were frequently compelled to feed on vegetables instead of bread. They were obliged any longer to wear patched-up clothes. Saint-Josse did not supply an income but did provide objects. In 845 during a famine, Loup even had to sell a few sacred vases to feed the community and his *familia*. He did not loan but was compelled to de-stock, and his treasure was not in monetary form. The purchase of food commodities seemed to him to be an abnormality, although even the rule of saint Benedict always offered the possibility of resorting to the market, either to buy what the monastery did not produce or to sell what he had in surplus. Obviously, on this particular point, Lupus did not conform himself to the representation of a good abbot, at least in his economic practice. According to saint Benedict rule, in effect, among the qualities of the abbot, we find a capacity to assess the value of things as well as of acts and, by extension, of the men carrying out the acts. The abbot was the one who, because he understood their value, could convert the terrestrial things into spiritual benefits, acting on several planes as a true "banker of God" (Toneatto 2010, 2012). The monks in general appear as true virtuosi of this particular operation and this arrangement is similar to their managerial capacities, whereas the same spiritual and intellectual qualities, *discretio*, *diligentia*, *cura*, *sollicitudo*, could be found with managers of material commodities as well as pastors. The functions of spiritual and temporal management of the monasteries are here largely converging if not confused: being able to assess the value of things is not so far from the operation which enables one to ascertain the value of acts (Toneatto 2012: 299-342 ; Devroey 2006 : 576-577).

The capacity for assessment exhibited by the best abbots of the early Middle Ages is on a par with their capacity to manage their domains according to the information available which, though often scarce and incomplete, enables rational decisions. Their knowledge does not lie only on a spiritual plane, but also includes practical know-how which enables them to enumerate the men, plots of lands and royalties so as to make decisions relating to the structure of the abbey's possessions in a rational way. The polyp-

tychs thus appear to be the fruits of a convergence between the political will of the Carolingians, a reasoned application of the Rule of Saint Benoît, which ordered the abbots to give written accounts, and the capacity of members of the aristocracy of the 9th century to grasp things through the use of numbers. The latter is accompanied by a clear awareness of the concrete functioning of economic life, of the organisation of production and of the building of incomes.

Going into further detail, we can see that certain economic and financial mechanisms have been assimilated perfectly, possibly intuitively, by the actors of the 9th century. A classic example is the description, by the bishops gathered into a council in Paris in 829, of a downward speculation. In order to define what immoral gain is (*turpe lucrum*), they imagine a loan in grains made during an expensive period. A borrower who cannot pay cash commits to refunding in kind the following year, not by volume but the value of corn at the moment of the loan. In other words, if he bought a muid of grain worth two denarii in 829, he did not repay a muid in 830, but what two denarii would buy at that time. The gamble of the lender is that, if the 830 harvest is good, prices may decrease and the debtor may be compelled to give a much greater quantity of corn than what he obtained in 829 (Feller 2011b).

Another example is supplied by the way Adalhard of Corbie, in Northern Italy in 813, assessed the value of two plots of land by including all the factors enabling him to do so: surface area, but also fertility, remoteness from the town, distance to a means of communication, relative value of the buildings for exploitation. This rare case shows a Carolingian aristocrat in a situation of expertise: he per they want to do. Thus, in the exchange of 813 noted above, Adalhard introduced arbitrary modifications into his assessment criteria (while explicitly noting he was doing so) so as to “finalise” his case and to make possible the permutation of property. When he requested the average unit price of a plot of land near Brescia and near Nonantola, price ranges were suggested to him. In one case, the maximum price of the jugerum is five solidi (i.e. denarii), and in the other it is eight denarii. If we find the arithmetic ratio existing between the maximum prices, we arrive at a multiplying coefficient of 7.5. At the highest price, a jugerum of land near Brescia is worth 7.5 times that of land near Nonantola. Evidently, accepting this ratio would have established an excessive distortion between the surface areas in question. Adalhard modifies the prices arbitrarily so that he reaches a ratio from one to three: he therefore assesses the surface unit

near Brescia at three solidi (36 denarii) instead of five and near Nonantola at one solidus (i.e. 12 denarii) instead of eight. We thus arrive at a ratio from 1 to 3, which corrects the inequalities caused by the functioning of the market, but which implies an assessed 60%-price decrease for the plot of Brescia and a 66% increase for that of Nonantola (Bougard 2008). This is still a sophisticated game of proportion: to arrive at a ratio from 1 to 3, 2/3 of the value should be increased and decreased. We shall not dwell on figure 3, but rather highlight the arbitrariness of the abbot who, knowing the market prices and integrating the elements contributing to its formation, decides to leave them aside and establishes a fictitious harmony by playing with the values, which enables him to say under what condition the surface areas in question can balance each other out and thus be exchanged. Playing with words, we can say that the exchange value is here voluntarily disjointed from the market price: the aim is however to interchange two pieces real estate belonging to ecclesiastical institutions and, in this case, the notion of market price has no relevant bearing. The notion of fairness, conversely, should be respected at all costs. The value, therefore, is but one of the aspects of the question and probably not the main one.

It happens that the observations of the actors on the value of things are simply common sense, even if they inform on important management decisions. Thus, in the 12th century, the monks of Cluny knew perfectly what they had to do to manage their supply rationally. Rather than organising costly cart transport from the peripheral domains to the abbey, they sold the products of their remotest dependences and, with the money obtained, bought the goods which they may need and which would not be produced close to the abbey (Duby 1973). The ideal of self-reliance is here combined to an intelligent use of the market and a fine awareness of what prices are. The administrators of Cluny understood how to compare costs and integrate transport in their calculation of the costs of the foodstuffs they consumed and of the clothes they wore. We are here in a monetary economy, far from the type of exchanges practiced by Loup de Ferrières and undoubtedly quite close to the ways of the speculators of the 9th century.

The vocabulary used by the authors illuminates their reasoning. Adalhard, for example, often justifies what he is doing and cites, even in the acts of practice, *opportunitas*, *utilitas* and *ratio*. Frequently, on the other hand, he reasons in terms of *quantitas* and of *qualitas*, seeking to establish a link between both, logically involving figures and, quite often, currency. We have just seen that conversions

may entail a degree of arbitrariness: *discretio*, one of the main qualities of an abbot, effectively enables him to set a price or to establish the link existing between two things of the same or different nature. He knows the value of things, but also how to assess and how to act according to their usefulness. Accordingly, the actor may choose to realise an operation or not, whether it is a commutation of goods or a mercantile exchange.

We said above that monks were like the “bankers of God” and that they acted as virtuosi of conversion. This should also be understood literally. The game between payments in kind and payments in money, as well as the central issue of the functioning of markets in the supply of towns via the seigniorial commission, gives rise to variations of an extreme diversity perfectly grasped by the actors.

The owners of land and of power sense the major movements of economic life and are in a position to make their decisions according to these perceptions. The second half of the 12th century in Tuscany is a good illustration of this (Dameron 1986; Conti 1965; Feller 2008b). As urban markets are developing and the supply of Florence is becoming an economic as well as political stake, the ecclesiastical lords, in this instance the abbot of Passignano and the bishop of Florence, convert their peasant tenure collection systems. Up to the 1190s, they used a complex system including money, a minimal proportion of the harvest, customary gifts, duties, bread, wine and poultry. The quantities required tended, during the 12th century, to become symbolical. Still, between the beginning of the 1190s and the end of the 1120s, the renewal of the agrarian contracts causes major turmoil. The abbey of Passignano as well as the bishop of Florence now require their tenant farmers, at the execution of contracts, to pay a high tenure entrance fee in money as well as a fixed quantity of cereals annually. The royalties in kind and the duties have admittedly been abolished, but these new requirements set the lords into a new economic system, dominated by commercial elements. The aim is to be present on the food market of Florence and to be able to influence the quality of harvests. Holding fixed quantities of cereals each year enables them to influence prices, to anticipate increases or decreases, and to place secure bets, regardless of whether the harvest is plentiful or scarce. The system provides insurance against production risks, mainly in the form of passing them entirely onto the producers who must cope with the variations in the quantities produced. The peasants were not fooled and endeavoured, as far as possible, to resist that new system which took them away from the

market by setting aside its profits for their lord. The issue of market access is the stumbling block here. On the other hand, the profits that derived from the rate variations are henceforth the monopoly of the lord. Moreover, the peasants are no longer able to direct their production and choose the commodities they are going to produce, which deprives them of the opportunity of taking part in the economic game by anticipating needs or taking into account the mechanisms of demand. This tendency to substitute fixed royalties in kind for proportional royalties, like *terraticum*, seems to be frequent in Italy at the end of the 12th century. It can be observed in the Padouan as in the Florentine *contado* and for the same reasons (Rippe 2003: 466-472): the aim is to derive the highest profit from the price increases and the growing use of currency in daily life transactions. The conversions of royalties thus show the capacity of the land-owners and power-holders to integrate economic calculation in their general policy of social domination.

Conclusion

The categories developed by economic anthropology and notably those opposing mercantile exchanges and non-mercantile exchanges are illuminating. They enable us to understand what is at stake in the wording of the values and in the different exchange procedures attested to in the medieval documentation. By introducing distinctions between the types of relations established by exchanges, files which are normally difficult or even impossible to interpret become more meaningful, as in the case of the texts surrounding the pontificate of Meinwerk of Paderborn. Postulating that the exchanges establish relations between things and that this relation is also a relation between people and a relation between people and things, strengthens the consideration of prices and requires a renewed interrogation of their meaning. Medieval economic actors have left us documentation which is difficult to understand and which frequently masks the reality of the enterprises established between people through things they circulate. The examination of the value of things, as expressed or hidden, enables us to come as close as possible to the way interpersonal relations, whether friendship, domination or subjugation, are intertwined with strictly economic relations by allocating things and men relations which throw greater light on reality.

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Interviews

Philip Scranton interviewed by Pierre François

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1 Your work deals with the birth and development of US capitalism in 19th Century, as do several economic sociologists, such as Neil Fligstein or William G. Roy. How would you contrast your approach of the topic with the one they developed?

First, a clarification. My research work started in the early 1980s with asking questions about the character and trajectories of US textile manufacturers, particularly those in Philadelphia, from the late 18th century through the 1880s. That research generated the book, *Proprietary Capitalism* (Cambridge, 1984). For the rest of the 1980s, I extended the Philadelphia project, tracing the regional industry's course through the end of the Great Depression. The bulk of that monograph, *Figured Tapestry* (Cambridge, 1989), dealt with 20th century phenomena, including the suburbanization of production, labor militancy, the introduction of synthetic fibers, and collective responses to crises of war and market collapse. *Endless Novelty* (Princeton, 1997) stretched beyond textiles and Philadelphia to attempt a national-scale reconstruction of what I've termed "specialty production," a durable industrial alternative to America's much over-hyped "mass production," addressing the last third of the 19th century and, roughly, the first third of the 20th. Since 2000, I've moved forward in time, researching aspects of post-World War Two specialty manufacturing, particularly machine tools and jet

engines, ca. 1940s-1980s, focusing on technological innovation and uncertainty.

So, on balance, only a portion of my research has been located in the discourse of 19th century capitalism's 'birth and development,' much like Neil Fligstein, who moved from *Transformation of Corporate Control* in the 1990s to drafting *The Architecture of Markets: An Economic Sociology of 21st Century Capitalist Societies* (2002), then recently policy and theory works – *Euroclash* (2008) and *A Theory of Fields* (2012). It seems to me that Fligstein's research arc bends away from history and toward theoretical synthesis and toward influencing present practice, whereas mine remained historical, pursuing the course of specialty sectors, a critical dimension of production (and by extension, services), both through the Cold War era and beyond the United States. William G. Roy's course has been more eclectic, shifting from *Socializing Capital's* analysis of the great merger wave, its antecedents and implications (1997) to an overview of social constructionism, *Making Societies* (2001), and just last year *Reds, Whites and Blues: Social Movements, Folk Music and Race in the United States*, which explores the ways in which Old Left and Civil Rights activists performed culture from the 1930s through the 1960s. Highlighting these research tracks briefly is the first step to responding to your question, as Fligstein has used his historical efforts to ground economic sociology interventions with direct contemporary impact, whereas Roy moved to other themes indirectly linked to his major work on 19th century US capitalism. I have remained in some respects a traditional historian, an "archives rat" seeking documents and images that can help frame narratives which, in turn, aim to highlight long-term industrial dynamics in modern capitalism.

More directly, there are sharp differences between my three wholly – or partly – 19th century volumes and Fligstein's *Transformation* or Roy's *Socializing*. The latter pair are anchored in the analysis of sophisticated data sets, Roy delivering a "quantitative test of efficiency theory" (21-40) – a core element of Alfred Chandler's heralded arguments for the economic/managerial logics of giant American corporations – and Fligstein utilizing multiple sets, some adopted and others constructed, to address location, survival and related longitudinal characteristics of merged firms and of America's top 100 manufacturers

through the 1980s – also presenting challenges and emendations to Chandler’s findings. Both books take up Naomi Lamoreaux’s reinterpretation of the “great merger movement” at the turn of the 20th century¹ as an axial moment defining American capitalism, though they take different paths in addressing it. Both rely heavily on published primary and secondary sources, with relatively little archival work, as may well be appropriate for studies treating big historical questions by using social science tools and theories. Notably, Fligstein explains that his approach relies on “organizational theory” from sociology, supplemented by economic and historical methods, which bring the state and legislators into play, actors Chandler had sidelined (5-11). Roy notes that his proposal for “an alternative analysis of the institutionalization of the large, publicly traded manufacturing corporation in America” draws upon several decades work in the “new economic sociology”, emphasizing “power theory” and the socialization of property while critiquing customary notions of corporate efficiency (9-16). Roy thinks Chandler is simply wrong, whereas Fligstein suggests he incompletely specified the field of action.

My approach in *Proprietary Capitalism, Figured Tapestry*, and *Endless Novelty*, was to start by noting the silences in the received narrative of American industrialization. Thus, big business was peripheral. Still, this was not an effort, as many commentators have erroneously stated, to emphasize small and middling size firms, but instead, to survey the industrial landscape to determine who had been excluded. The answer turned out to be firms at all scales that took a different path to profit than seeking standardization, cost-reduction, market share, and dominant competitive pricing. As I was employed in the early ‘80s at a small “college of textiles and science” in Philadelphia, the local industrial landscape beckoned. It featured an array of ruins, scattered across a 125 square mile city (ca. 325 sq. km.), that suggested the considerable prosperity of a vast manufacturing system. Familiar industrialization narratives used New England and the South to anchor tales of Massachusetts’ proto-mass production in textiles being superseded by the Carolinas’ cheap, displaced agricultural labor and fast, early 20th century technology – spinning frames, looms, and knitting machines. Chandler had retold the antebellum Lowell story as a preface to introducing genuine big businesses, the railroads, but I wondered what these Philadelphia companies had done to make serious money for generations (judging by the size and number of the city’s mills) and why they weren’t included in the American textile narrative or the wider industrialization

tale. Starting from silences is in a way the inverse of starting with big questions, though filling in those silences can in time generate big questions.

So long before any theoretical frames were installed, my research commenced with an empirical double move. I commenced driving and walking around the city’s decayed textile districts, taking photos and notes, and soon making copies of the 19th century insurance maps that, at intervals from the 1850s, detailed Philadelphia’s manufacturing, commercial and housing sites. This helped me populate imaginatively the spaces in which hundreds of firms, some employing thousands of workers, had practiced their versions of industrial capitalism, versions I couldn’t then specify, but which produced city-scapes utterly distinct from Lowell, Fall River or the South’s mill towns.² Second, about 1978, I was fortunate to secure research access to the Philadelphia Social History Project’s census data files, which in addition to large samples of population information, 1850-1880, included a comprehensive computerization of the parallel manufacturing data, firm by firm in all sectors. However, when examining binders of the manufacturing printouts, I encountered so many anomalous figures that I began checking them against the original manuscript census forms. So many transcription errors surfaced that I set aside the data files, dropping any data analysis that would have used them. Instead, I went back to the manuscripts to extract enterprise information, double-checking every entry to reduce transcription errors. Crosschecking to city directories allowed me to establish the spatial distribution of firms over time, as address did not appear on the census manuscripts. Thereafter, the spiral of source gathering scrolled outward, to industrial directories, litigation case files, trade and industrial journals, personal and company archives, government publications and the like. In a sense, what I attempted was to reconstitute the Philadelphia textile industry’s historical dynamism through layering documents contemporary actors created and through consulting its material residues in multiple neighborhoods.

For several years, my questions were rudimentary: who were these folks and what were they doing? Where did they come from and how/why did they succeed and fail? But the answers were distinctive. In large measure “my” textile capitalists were immigrant entrepreneurs (chiefly British, Scottish and German) running family firms at scales from microscopic to immense. None were incorporated, unlike the Lowell mills; instead they were partnerships and proprietorships. But most important, the work they undertook was not in standard goods, but in up-market special-

ties for fashion (styled and figured fabrics and knits) or for households (carpets, lace, upholsteries), made on order in batches by higher-skilled workers than found in staple goods plants. They also operated in networks of production, some specializing in fine yarns, others in designing, dyeing, finishing, weaving or knitting, rather, in general, than in integrated facilities. Moreover, they established reputations for flexibility, fast deliveries, and opportunistic pricing, the source of sizable, though uncertain, profits that derived from 'hitting the styles.' Their activities replicated patterns found in European textile manufacturing, featuring dis-integrated and interactive specialists who founded their own banks, eating clubs, and trade societies. It gradually dawned on me that these vigorous Proprietary Capitalists had been excluded from the US industrialization narrative because their practices did not fit the teleologies leading to positing big business and mass production as the core American industrial story. At that point larger questions began to bubble up, and theoretical resources from the neo-Marxist (or at least post-neo-classical) economic geography I'd been reading gained value and purchase, especially the urban studies debates triggered by David Harvey's *Social Justice and the City* (1973).

Much the same bottom-up, archive-combing practices informed *Figured Tapestry* and *Endless Novelty*, systematic narrative volumes that enlarged the scope of industrialization's history, emphasizing contingency and complexity, along with a claim that high-value added trades contributed as much to the structure of industry as did the giants of standardization and commodification, making small unit profits on huge volumes. This work perhaps also created back-stories for the later vogue of studying industrial districts/networks as America's Chandler-style corporations crumbled one by one with the secular decline of US manufacturing and the financialization of the international economy. The data I used was presented in descriptive statistics, not regressed in relation to models or hypotheses, and there is very little about any of the three monographs that would resonate with quantitative social science methods – a significant contrast with Fligstein's and Roy's approaches.

2 How would you describe the relationship between economic sociologists and economic historians in general? What is the relevance of sociological tools for your own work? And what has your work been received

and read in the community of economic sociologists?

Again a clarification, as I'm not an economic historian, at least given the field's methodological boundaries since the 1970s. As the preceding text suggested, I do not build/analyze data sets, create proxies, or devise ways to generate correlations/regressions that might argue for, if not nail down, causal relations. Indeed, I am deeply skeptical about the nomothetic viability of the hard-science-seeking aspects of the social sciences, epitomized in the mathematized interiority of contemporary mainstream economics. Such objections hardly matter to practitioners, of course, not even when root-and-branch critiques are delivered by colleagues who can "do the math", like Deirdre McCloskey or Donald Mackenzie. Instead, I work idiosyncratically, in ways that may resonate with Lucien Karpnik's economics of quality/singularities (*economic sociology_the European electronic newsletter* 15.1), as a historian of industrialization, or fieldwise, as a business historian and a historian of technology. Both disciplines have a critical, discursive, and basic-research-centered tradition, and both have gradually moved away from studying iconic objects (firms, artifacts) toward engaging broad processes of institutional, cultural, and practical change. For a more adequate perspective on the relations between economic historians and economic sociologists, I'd suggest you ask my colleague Naomi Lamoreaux at Yale. From scanning several issues of your newsletter, my sense, quite preliminary, is that economic sociologists have begun welcoming historians' perspectives that reflect qualitative research developed outside data-anchored projects. Economic historians, at least in the US, have been little interested in such work over the last two generations.

As for the use of "sociological tools" in my work, this of course turns on the contents of that phrase. One webpage lists the discipline's tools as including "field observation, scientific experiments, questionnaires and surveys, interviews, statistics, and studying primary and secondary resources," which is helpful if not fully satisfactory.³ Plainly, historians cannot use fieldwork (except archeological), experiments, surveys, or interviews (except to document the recent, through oral histories), but do actively work with statistics and both primary and secondary sources, as do I. Crucially, though, historians are impelled by our training to adopt a critical stance toward all sources, given that the information we recover is "always already interpreted" (an insight from Derrida, by way of Heidegger and Gadamer). This is a commonplace in sociology, I expect, but

historians have no techniques to achieve a defensible, “objective” viewpoint that could correct our sources’ twisted “vision” to 20/20-normal. Instead, we “triangulate” in order to secure multiple angles of approach to a phenomenon or process from varied sources, employing a peer-referenced subjectivity to assign relative values to each. As a recent commentary on a US Tax Court decision opined: “If conflicting information is provided in multiple sources, one must consider the hierarchy and reliability of such sources.”⁴ True indeed, and a fundamental challenge when it comes to debating hierarchies and reliability in contexts of power and class stratification. Moreover, with Joan Scott,⁵ when confronting statistics, historians ask who created them and for what purposes? What omissions can we detect and how might they be salient to the original statistical project, much less our appropriations of quantities? These questions and the frequent incommensurability of statistics gathered at different times can undermine database-driven projects in ‘social science history’. My sense that sociological theories are also significant research resources is not reflected in discussions of tools/methods that I’ve run across, but as you’ve asked about using theory separately, I’ll address that shortly.

As for the third query, concerning the reception of my work by economic sociologists, here I am quite clueless. Google Scholar shows some citations of my studies in scattered ec-soc journal articles, but I’ve not encountered a review of or engagement with the main monographs, or with the recent volume, *Reimagining Business History* (Hopkins, 2013), that Patrick Fridenson and I completed last year. Economic sociologists have crowded agendas, certainly, so I’m not surprised at this. Yet it would be lovely were there uses or critiques of my work that I’ve not discovered, or were your questions and my responses to provoke some.

3 One typical way to contrast history and sociology is to underlie the uneven weight of explicit theoretical insights in both disciplines. What is the role of theory in your own work?

OK, it’s good to take up the question of “explicit theoretical insights”. My sense is that the “role of theory” in the social sciences is deeply connected to scholars’ diligent search for rigor, predictability, and scientific standing. Rigor can, in this domain, involve setting aside or abstracting away the situational or non-quantifiable elements of social and historical situations, or, at other sites, it can

invite as comprehensive a depiction as possible of all the knowable actors and elements in motion.⁶ Whose rigor, whose theory, and for implementation toward what ends? These are potentially valuable questions to discuss, and have been current since the 1970s among historians of culture and politics, as well as societies, economies, and technologies (at least in North America).

My own engagements with theory commenced in graduate school at the University of Pennsylvania, where the 1970s history Ph.D program featured a full-year “History and Theory” colloquium with readings stretching across three centuries from Hobbes to Foucault, emphasizing Marx, Durkheim, Weber and Parsons – masters of social and economic analysis. Still, the relationship between theory and “history work” wasn’t obvious; a deep reluctance to “apply theory” was general, as was a deep unfamiliarity with what “application” meant for social scientific colleagues’ research. Our student cohort had encountered grand theory, compelling systems and conceptual arrays, but had little exposure to mid-range theorizing about, say, population trends, urban clustering, or capital flight. Once we finished our degrees and dispersed to teaching jobs, we were on our own, theory-wise. Not long after, Penn History dropped the “Theory” colloquium, signaling disengagement.

For me, the theory “bug” became a long-term infection, not least because, for about a dozen years, I coordinated running a leftist (and used) bookshop here in Philadelphia. Reading *New Left Review* while sitting by the cash register kept me up to date on current controversies along one vector of theory and politics, but college teaching and beginning the textile history research pushed me to gather usable concepts in urban economics and geography, for example. After leaving the shop to others’ care in the early 80s (it’s still in business!), my readings broadened to include most works by Giddens, Beck, Sennett, Latour, Bauman, Bourdieu, and Foucault, and parts of Baudrillard, Douglas, Castells, Goffman, Geertz, Habermas, Weick, Mirowski, and Gadamer, among others. Throughout, as now, I read theory for pleasure, scribbling reactions and cross-references in volume after volume.

And early on, I came across a comment from Anthony Giddens that captured what I’d view as the “role of theory” in my research. I’ve just gone back and recovered it:

“The concepts of structuration theory, as with any competing theoretical perspective, should for many research purposes be

regarded as sensitizing devices, nothing more. That is to say, they may be useful for thinking about research problems and the interpretation of research results.”⁷

Sensitizing devices, nothing more. This unplanned stew of theoretical work has provided me with materials that are good to think, as Mary Douglas put it long ago.⁸ The result is that, with some exceptions, theoretical conjunctions are noted in, but not axial to my publications.⁹ It is not my aim to employ theory to test explanatory hypotheses, nor to expect that theoretical frames will help model cause-and-effect relations – two alternative approaches, both of which lean toward the explicitness pole. Instead I seek to “match” concepts and historical situations/processes, looking among the many options for what strikes me as a plausibly effective means for organizing and interpreting historical evidence. It’s not any kind of scientific practice, but given a richly-populated theoretical terrain, such an approach can invigorate a narrative while broadening the value of a theoretical perspective. In this vein, my articles have sometimes introduced insightful theorists, like Giddens, Beck, or Bauman, to historians who’ve not previously encountered their work. Last, along this line, Patrick Fridenson and I undertook to share set of broadly-theoretical reflections with colleagues and students, through last year’s *Reimagining Business History*, a cluster of 43 short essays highlighting themes and concepts that may spark future research projects. Think of this perhaps as marketing sensitizing devices.

4 How would you describe the typical relationships historians and sociologists carry with economics?

I can’t speak for sociologists at all, of course, but I wouldn’t claim that there are any “typical” relationships among historians and economists, either. Still, it seems that the majority of US economic historians working in the quantitative vein are linked strongly to contemporary economic literatures, as honorific citations in articles’ literature sections would suggest. There are, however, non-conforming economic historians in the US and Europe, for whom the neo-institutionalist literature is salient, many of whom straddle the fuzzy border between blended qualitative/quantitative economic history and narrative-based business history. Some of them publish in the leading business history journals: *Business History Review* (Harvard), *Business History* (UK), *Entreprises et Histoire* (France) and *Enterprise and Society* (US-Oxford UP), the last of which I edit for the Business History Conference. Still, very few articles in any of these

four journals will utilize equations with ‘Sigmas.’ “The firmer the commitment to quantitative social science, the thinner the relationship with qualitative research” might be a plausible rule of thumb... Indeed, a third group of historians, at least in the English-language domain, take a broadly-cultural approach to socio-economic relations and have next to no connection with economics as a discipline, for they regard its abstract formalism, reductionism, and predictability goals as useless when exploring/explaining historical processes in context. Economists’ dismissals of such research as irrelevant to their concerns is wholly appropriate, as it’s not science, and not intended to be. Meanwhile, economists’ aspirations to scientific standing, sometimes cruelly described as “physics envy,” don’t seem to be working out all that well, despite their quasi-Nobel prizes? Last, at work more in business schools than history departments in the UK and North America, a cluster of business historians has found Nelson and Winter’s evolutionary economics provocative and productive as a means to locate institutional practices and problems in the flow of political, economic, and cultural activities.¹⁰ This seems a very promising vector.

5 How and why would you advise a junior researcher in economic sociology to work with historians?

I think this is best phrased “when and why,” because working with historians would surely be a situational prospect. Perhaps there’s a economic sociology project being developed concerning how 21st century information technologies are intersecting with lifeways in Europe’s remaining rural districts, with questions about pathways opened, customary practices challenged, outmigration triggered, or generational (or richer vs. poorer household) tensions enhanced or assuaged. An historian’s involvement might well be judged useful were the project leaders interested in contextualizing this phenomenon in light of earlier information technologies that intensified information flows and altered economic possibilities, such as postal services, rural delivery of newspapers, telegraphy, telephony, wireless radio (for coastal emergencies, weather), commercial and state broadcasting, etc. One could learn how island communities, for example, or rural enclaves in the Midi or Macedonia, experienced these repeated info “invasions” and responded, over the last two centuries, putting a long historical arc, not behind, but amid current activities.

Now a junior economic sociology researcher generating such a 21st century IT topic could not be expected to go

"looking for history" unless in his or her training and dissertation efforts long-term patterns of social relations and cultural practices or medium-term cycles of political and economic initiatives had been given some emphasis. History does 'matter' to many writers in this newsletter, but it's not clear to me how this concern involves much more than scenery-setting, whereas many historians would aver that the power of culture, for example, arises from its status as congealed, and frequently-unacknowledged history or that the capacity of institutions to constrain and facilitate stems from the absorption of historical conflicts into unquestioned routines.¹¹ (For the value of history, see also Kenneth Zimmerman's commentary on Neil Fligstein's discussion [*economic sociology_the european electronic newsletter 11.1*, 41-44] of the 2008 market smash. Zimmerman's text, at *economic sociology_the european electronic newsletter 11.2*, 48-52, especially 50, offers a critique of restricted visions among economic sociologists. Fligstein's rejoinder at 53-54 is valuable, though I'd think it's the Bruno Latour of *Aramis, or the Love of Technology* (1996) who would be most relevant to historicizing transformations in economic institutions.)

So, presuming that our junior scholar has an emergent project and that s/he recognizes that history matters, yet isn't trained in historical research methods, what to do? Two things I'd not recommend. First, don't consult a near-to-hand historian with a request to "pick his/her brain." This presumes the historian is a data-source, like a reference book, and is unlikely to be effective. Second, don't invite a historian of, in my imagined scenario, the Faroe Islands, Sicily, or Macedonia, OR of France's PPT or Greek radio and television, to become a research partner. The chance that their interests would mesh with your research challenges, indeed that they would comprehend the project's conceptual foundations is fairly remote, as many of us are both specialized and ill-read in "outside" literatures like economic sociology. Instead, first, I'd ask senior economic sociologists for advice about historians they've encountered who've done compatible research on earlier eras, whether or not directly informed by economic sociology. Second, I'd suggest getting in touch and involved with one of the European/global online academic networks among, say, communications scholars or the history of technology to begin thinking with colleagues about what sort and what scope of historical reading and research would build a sufficient foundation for the contemporary initiative. These online linkages frequently are productive and connect junior scholars with one another, a valuable outcome. (One group I've found welcoming and respon-

sive is the Tensions of Europe collective, run out of the Foundation for the History of Technology at the Technical University Eindhoven, but there are others, of course.) Last, and only if history looks to matter seriously to the project, I'd seek out a relevant summer seminar in historical practice and method to learn closer at hand how historians work and think, how they ask questions and zero in on sources that can provide paths toward answers, and how they build contexts within which those answers can have durable meaning. Ultimately, the most useful situation would be to find a historian intrigued by economic sociology who would join a project team both to become familiar with a neighboring discipline's ways of proceeding and to share the somewhat different practices historians have devised, seeking intersections and interactions.

Endnotes

1Naomi Lamoreaux: *The Great Merger Movement in American Business, 1895-1904*. New York: Cambridge, 1988.

2This visual survey appeared in book form as Philip Scranton and Walter Licht: *Work Sights: Industrial Philadelphia, 1890-1950*. Philadelphia: Temple University Press, 1986. Paperback 1988.

3http://www.ehow.com/facts_5714343_tools-used-sociology.html#ixzz2xw7bLZMD

4<http://www.marketwatch.com/story/ira-rollover-ruling-stuns-advisers-and-savers-2014-04-04?pagenumber=2>

5A Statistical Representation of Work: La Statistique de l'Industrie à Paris, 1847-1848. In: Scott, Joan (ed.), *Gender and the Politics of History*. New York: Columbia, 1999, 113-38.

6See Bruno Latour: *Reassembling the Social: An Introduction to Actor-Network Theory*. New York: Oxford, 2005.

7Anthony Giddens: *The Constitution of Society*. Berkeley: University of California Press, 1984, 326.

8Mary Douglas: *How Institutions Think*. Syracuse: Syracuse University Press, 1986. Consider her provocative claim that "institutions perform the same task as theory. They also confer sameness." (59) See also M. Douglas and B. Isherwood: *A World of Goods: Towards an Anthropology of Consumption*. New York: Basic Books, 1979.

9One exception is a just-recently-released article that directly explores the value of historical ethnography in technological history research. Scranton: Histories and Historical Ethnographies of Technical Practice: Managing Jet Propulsion in the US and France. In: *Entreprises et Histoire*(73), December 2013, 108-43.

10See Richard Nelson and Sidney Winter: *An Evolutionary Theory of Economic Change*. Cambridge: Harvard University Press, 1982.

11See Michael D. Cohen et al.: Routines and Other Recurring Action Patterns of Organizations: Contemporary Research Issues. In: *Industrial and Corporate Change* 5(1996): 653-698.

John Padgett interviewed by Pierre François

John F. Padgett is professor of Political Science at the University of Chicago, with U of C courtesy appointments also in the Sociology and History departments. In the past, he has held professorial appointments at Harvard University in Sociology, at the University of Trento in Economics, and at the Santa Fe Institute. Padgett also has held visiting appointments at the Princeton Institute for Advanced Study, the European University Institute, Villa I Tatti, the Center for Advanced Study in the Behavioral Sciences, and at the Italian universities of IMT-Lucca, Bocconi, Bologna, and Modena. Appearances notwithstanding, he comes from tobacco country in rural Maryland.

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1 You have been trained as an engineer. How did you get to work on Florentine Renaissance?

My career trajectory from electrical engineering to the Florentine Renaissance has been detailed in a number of short articles on my website: home.uchicago.edu/~jpadgett. In particular, the 1998 Santa Fe Institute profile posted there is amusing – no doubt because it was written by someone who writes better than I do. In brief, the explanation of my unusual biographical turn is twofold: (1) First, the Vietnam War led me to reevaluate as immoral my otherwise interesting undergraduate summer employment, which was designing and building (sonar systems in) nuclear submarines. A series of “do good” political activist jobs following that disillusionment eventually led to my post-undergraduate work for the mayor of downtrodden Trenton, New Jersey. There, Princeton University scales fell from my eyes. Life up close, I found, was very different than what my professors and I had imagined it to be in the classroom. (2) Second, I co-taught with Harrison White for four years during my first academic job at Harvard University. Harrison taught me not about “network analysis” per se but more powerfully about his structuralist-cum-constructivist worldview. Woody Powell and I in our recent book, *The Emergence of Organizations and Markets*, have codified that Harrison White vision as “In the short run, actors make relations, but in the long run, relations make actors.” The second half of that mantra blew my methodologically individualist mind, which had been molded at Michigan graduate school by the writings of Herbert Simon. Instead of one worldview, I now had two competing worldviews in my single boundedly rational head, contentiously and vibrantly

debating each other. Harrison taught and theorized inductively, not deductively. He didn’t teach other peoples’ theories; instead he trained his students (and me) to build their own. With my own diverse students, I have tried to do the same. Harrison and I tore through history book after history book, always looking for rich and deeply studied empirical foundations with which to challenge our own priors and to demand us to think. Books, Harrison always said, were to eat – not politely but voraciously, tearing them apart with multicolored scribbles throughout. Harrison never read anything I am aware of about Renaissance Florence, but he infected me with a passion for history that I had never experienced before. “History” in Harrison’s non-historian hands did not mean the (dry) past. It meant social dynamics that constructed and reconstructed itself, ever afresh. (3) I guess I should add the final step: I continued to pursue the Harrison White agenda at Chicago through teaching. Florence in specific emerged out of teaching a comparative-history course for “fun,” once that teaching made me aware of just how rich those Florentine archives really were – if only I could learn to tap into them. Getting tenure on the basis of my Michigan-style formal modeling freed me up to “waste” the time necessary belatedly to acquire hitherto missing linguistic and archival skills.

2 How was your work received in the community of historians working on the quattrocento? What was the most exotic dimensions: the quantitative aspect of your work, or its sociological dimension?

Historians I respect as a very persnickety bunch. They are not easy for social scientists to get to know, mostly because historians assume that social scientists do not know what we talking about. And mostly they are right. The austere reductionism of generalizing models and the rich contextualism of particularizing narratives truly are in deep tension. [I am currently creating a Working Group on History and Evolution at the Social Science Research Council in New York to further pursue this conversation and tension.] The saving grace from epistemological fog, however, for most traditional historians is simple – how many hours have you spent at the archives? When I first started studying Renaissance Florence, I was quite irritated that only one historian, David Herlihy (bless his deceased soul), would talk to me. Everyone else thought I was a naïve idiot, which of course I was. After three hot summers of tedious labor in the Florentine archives, however, people noticed that I was not going away. They must have gossiped be-

hind my back enough to start to go to lunch with me. They all knew my *Robust Action* article in *Sociology*, but still never considered me a bona fide historian until I started to publish in history journals (*Renaissance Quarterly* and *Journal of Modern History*). Historians make you earn their respect, and my work is the better for it. To answer your question directly: The “sociological dimension” is not incomprehensible to historians, because they know about social history. The “quantitative aspect” is also not inconceivable to them either, because some of them anyway are aware of demographic and economic history and the *Annales* school. These two features are often considered out of date, since the history profession as a whole made its “cultural turn” in the 1980s. The real barrier is not those two features; the real problem is the social-science aspiration for “theory” in general. I try to argue as vigorously with my social science colleagues as with my history colleagues that “theory” should not be conceptualized as deterministic “laws” – since I too would oppose “theory” if that is what it meant. “Theory” should be conceptualized as “searching for generative processes” not as “searching for prediction.” In my Santa Fe Institute world, this is science as a biologist would define it, not science as a physicist would define it. I self-consciously persist in my stubborn naivete that others likewise will come to see this lack of contradiction between science and history, if only I can prove it to them not through hand waving but through real research.

3 You carry an important theoretical enterprises, in parallel to your florentine job: is Florence just an empirical setting to test your theoretical assumptions, or does it carry its own autonomous agenda?

The Padgett and Powell book definitely presents Florence in the first rhetorical style – “just” as one among many cases that illustrate the theory. But that presentational style is misleading. I studied and published about Florence long before I ever heard about autocatalysis. In biographical fact the autocatalysis and multiple network theory in that book inductively emerged out of Florence. Or in deference to my co-author, I should say the theory in that book inductively emerged out of our two primordial cases of Florence and biotechnology. Because of this induction, Florence and biotechnology cannot be considered “tests” of the theory – the theory itself was generated by them! Rather all the other empirical cases in the book – ranging from medieval Tuscany to early modern Amsterdam to nineteenth-century

Germany to twentieth-century Soviet Union and China – should be considered “tests” of the theory. My next book will be written in a more narrative manner – namely, understanding the co-evolution of Florence networks and institutions on their own terms. “On its own terms,” however, does not mean “with no theoretical assumptions.” Any interpretation requires theoretical assumptions, explicitly stated or not. Most historians I know agree with this; they are not mere antiquarian collectors of curious bables.

4 How would you describe the typical relationships historians and sociologists carry with economics?

Here is the other side of my world – talking not just with “complexifying” historians but also with “simplifying” modelers like economists and physicists. My interaction with economists has been heavily structured by the two lodestars of my training: Herbert Simon and Harrison White. In other words, to the extent that economists are interested in and tolerant of bounded rationality, not just in perfect rationality, and to the extent that economists are interested in dynamics, not just in fixed-point equilibria, I find interaction with them valuable and provocative. The mental discipline that comes from formalization is powerful for sharpening questions and thinking through complicated causal chains. The engineer still in me likes that. Unfortunately those are not the two types of economists that I mostly find in my home environment at the University of Chicago. Because of a shared interest in either evolution or behavioral economics, my experience with economists at the Santa Fe Institute and at the various Italian universities with which I have been affiliated has been more cooperative than it has been at home. Speaking beyond my personal experience, the “social capital” approach to networks is a third front for possible fruitful interaction between economists and historically oriented social scientists. That approach in my mind is just fine for “in the short run, actors make relations,” but it is quite weak for “in the long run, relations make actors.” This important difference in temporal perspective, however, surely does not foreclose valuable potential synergies. Add dynamics and more such synergies might develop in the future than have emerged to date.

5 How and why would you advise a junior researcher in economic sociology to work with historians?

As for why: Because this is the goal of science – not only deep data, data, data, but also deep process, process, process. Appearances notwithstanding, I am convinced

that the substantive goals of hard scientists and historians are really the same – namely “to explain” means to understand the generative processes that produced the outcome of interest. This is different from the “just so stories” one too often finds in social scientists’ cross-sectional comparative statics. As for how: That also is simple – just spend many hours in the archives.

Book Reviews

Book: Friedman, Walter E., 2014: *Fortune Tellers: The Story of America's First Economic Forecasters*. Princeton: Princeton University Press.

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Nearly a quarter of a century ago, Donald McCloskey (1991: 287) bemoaned the extent to which “the press and public treat the economist as a soothsayer...to the point of believing that economics intends chiefly to forecast.” Public faith in the forecasts made by professional economists may have waned somewhat since the most recent financial crisis – but the future-orientated nature of both capital markets (Knorr Cetina 2011) and macroeconomic management (Holmes 2009) have ensured that the forecaster remains a pervasive public presence. What then, were the conditions under which forecasters became legitimate, and indeed recognizable, public figures in the first place? In *Fortune Tellers*, Walter Friedman seeks to provide an answer to that question, via a series of exacting intellectual biographies. Friedman traces the occasionally intertwined lives of seven men who – more as entrepreneurs than as intellectuals – made the forecasting industry possible. In the process, Friedman reveals that it was these first forecasters who did the most to create “the idea that capitalism was both logical and understandable” – an achievement enabled by the production and circulation of business cycle charts that depicted “the economy as a separate phenomenon, divorced from labor strife, cultural difference, and even events like war” (p. 127).

Fortune Tellers is set in the first few decades of the twentieth century, a period of expanding stock ownership, a booming railroad bond market, and rampant market manipulation. This was a time of anxiety in America, not only regarding when the next crisis would occur, but over the sustainability and desirability of capitalism itself. Only one in a series of spectacular crashes, the Banker's Panic of 1907 proved to be the moment around which the careers of Friedman's first three subjects pivoted. Roger Babson, who sold bond listings to professional investors, was unimpressed by the turbulent Wall Street “casino.” He was, however, inspired by his sympathy for the “thrifty” investors who had lost their savings in 1907 to “move from merely reporting business information to analysing its im-

plications for the future” (p. 22). Babson drew on meteorological metaphors and his obsession with Newtonian mechanics to develop the *Babsonchart*, a graphical representation of equal and opposite economic reactions organized around a putative line of equilibrium. This first economic barometer enabled forecasting by marrying models borrowed from physics with a theory of economic passions. The natural reaction to adversity, said Babson, is thrift and temperance, which drives economic recovery; while prosperity makes men “wasteful, self-indulgent and careless” (p. 30), calling forth the next inevitable depression.

Friedman argues that economists “justifiably regard much of Babson's science as suspect” (p. 13, also p.150). Nonetheless, he includes Babson among the systematic scientific forecasters (pp. 4-7), rather than the astrologers and fortune tellers whose opinions were equally sought after (a confusing distinction to make in light of the work's title!). This is perhaps because of the significant influence Babson had, not only as the first of many “business prophets” (p. 50), but as the inventor of the “business activity” category, which captured economic processes beyond specific product lines or localities. Babson and his chart in effect made it possible not only to forecast boom and bust, but to *visualize* a “national, interconnected economy” (p. 30) – what would later come to be known as the macroeconomy.

From Babson, *Fortune Tellers* moves to consider Irving Fisher's role in the development of the forecasting industry. Babson's obsession with Newton clearly inspires scepticism in Friedman, but his admiration for Fisher shines through, in an almost hagiographic introduction to the economist's academic legacy. There are, however, curious parallels between the lives and careers of Fisher the “mathematical prodigy” (p. 74) and Babson the “oracle” (p. 74). Both men suffered from TB and conducted most of their work in leafy suburban retreats; both had a curious and distasteful admiration for eugenics and its supposed economic significance (pp. 30, 66); and both were successful authors of early “self-help” manuals (pp. 48, 64). Like Babson, too, Fisher conducted an aggressive marketing campaign for his forecasting service, travelling and speaking widely, and trying to enrol the support of the President. His forecasting work grew out of his inquiries into interest rates and the forward-looking character of capitalism, expressed in his “hydrological” (rather than meteorological)

logical) modelling of the economy, and his famous Equation of Exchange (pp. 57-59). While Fisher might have brought Ivy League prestige and mathematical sophistication to the forecasting industry, still dominated by Babson when he entered it (p. 74), he was embarrassed personally and professionally by his much-commented upon failure to predict the crash of 1929 (a crisis which Babson seemed to have correctly foretold).

Friedman is explicit in his admiration for Fisher, but his own style of writing and analysis shares more with that of his next subject, John Moody. Moody's vision of the market was neither meteorological nor hydrological, but "Dickensian" (p. 87), expressed in his "ability to tell a sweeping historical narrative from the biographical perspective of business and industry leaders" (p. 111). Lacking the wealth and education of Babson and Fisher, Moody (like his father) lost everything on the stock market, and the 1907 Panic forced him to sell his railroad bond manual service – and the "Moody's" name – to Babson (p. 99). Moody may have "failed to develop, like Babson and Fisher, a theory intended to rationalize the workings of capitalism" (p. 113), but his desire to create a level playing field on Wall Street laid the foundations for what might now be called fundamental analysis (p. 110). His interest in the future-orientation of capitalism led him to disseminate information about the expectations held by business leaders, and his visualizations of the economy focused on "influences" and "alliances," resembling something like a "family tree of capitalism" (p. 97). Among Moody's heirs today we could therefore count executive confidence surveys, and boutique services like "clan analytics" for emerging market investors. Friedman's focus, however, is on Moody's legacy as an evangelist for transparency, and as the first of the forecasters to recognize that confidence itself can be used as an indicator of the economic future.

From the three men whose forecasting careers were triggered by the 1907 Panic, Friedman moves on to investigate the role played by two institutions – the Harvard Economic Service (through the lives of C. J. Bullock and Warren Persons), and Herbert Hoover's Department of Commerce (focusing on Wesley Mitchell's career) – in the development of forecasting. Where Fisher's forecasts were based on a causal claim about the "timeless relationship between price levels and the volume of money in an economy" (p. 59), and Babson's rested on the idea that the market oscillated inevitably around a putative "normal" level of activity, the *ABC Curve* that Persons designed for Harvard was informed by the idea that "analogous patterns of econom-

ic change found in past periods of depression and expansion would reemerge in the future...In forecasting, Persons believed that history repeated itself, though imperfectly" (pp. 139-140). Although Friedman stresses Babson's use of meteorological influences, Persons' method in fact had far more in common with contemporary weather forecasting (cf. Buchanan 2013: 206). Persons also differed from Fisher in his attempt to remove "disturbances" and produce a purified visualization of the underlying business cycle (p. 107); Fisher felt the cycle was a mere illusion, an epiphenomenon of the monetary processes he had described. The Harvard Economic Service was inspired by Bullock's suspicions about the profit motive guiding private, non-academic forecasters, but the Service was just as entrepreneurial in their marketing campaigns, and still found themselves accused of pandering to their clients when they failed to predict the 1929 depression (p. 159). Friedman's depiction of the debates entered into by the Harvard Economic Service and contemporary economists from Keynes to Tinbergen to Morgenstern (pp. 144-155) is a fascinating piece of intellectual history, and a good deal more engaging than some of the painstaking biographical detail recounted in the first three chapters.

Keynes had fallen out with the Harvard forecasters over their lack of interest in controlling the business cycle, but Mitchell, under Hoover at the Department of Commerce, was motivated by the understanding that "If we could foresee the business cycle, there would be none" (p. 166). This was no Keynesianism, however, and Hoover was frustrated by his failure to foresee 1929 and protect what he saw as American capitalism against the New Deal. In his retirement, Hoover reflected on a problem that is central to today's state-sponsored forecasters, and which goes to the heart of any sociological understanding of forecasting in a reflexive world: "How could one promote a reasonable sense of optimism without creating a zealous overconfidence?" (p. 192)

Fortune Tellers provides a biography of the figure of the forecaster, and in doing so, reveals that it was forecasters who "popularized one of the greatest inventions of the century: the idea of an autonomous "economy" that followed decipherable rules" (p. 197). In this it acts as a superb complement to Breslau (2003) on the role that Fisher and Mitchell's work played in contemporary economies becoming "disembedded." The gallery of charts and visualizations used by early forecasters (pp. 118-127) will certainly be of interest for those concerned with the material devices through which economic processes and partici-

pants are formatted. If *Fortune Tellers* is likely to leave the sociological reader with a niggle, it will probably relate to Friedman's at times unconvincing distinction between soothsayers or oracles (among whom it seems he would be happy to place Babson), and the professional forecasters that he takes as his focus. Perhaps, given the many parallels between the social logics of divination and economic forecasting (Zeitlyn 2012), it might be possible to draw on Friedman's rich historical data in order to develop a "symmetrical" sociology of the role that fortune tellers of all persuasions (and their technologies) play in formatting economic subjectivities and agencies?

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Book: Renate Mayntz (ed.), 2012: *Crisis and Control: Institutional Change in Financial Market Regulation*. Frankfurt: Campus Verlag.

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Financial Crises and Institutional Change: Some Observations and Critical Remarks

Many observers of financial reform are puzzled by the inadequacy of changes in the financial system after 2008, especially in comparison to the magnitude of existing problems, and despite having one of the worst financial

crises in the history. Many of them accept that the recent global crisis has revealed some of the major problems in the structure of financial systems, incentive mechanisms, as well as the weaknesses of the regulatory framework that was supposed to prevent moral hazard problems and the building up of systematic risk. Not surprisingly, the emergence of these problems in 2007/2008 increased the political salience of the topic and stimulated efforts to change the existing regulatory framework. It also created a strong political pressure for democratically elected officials, to take further steps in order to appease public frustration. However, after more than six years since the emergence of the crisis, one can better see retrospectively that the changes are inadequate to address the fundamental problems of financial systems, and to meet democratic demands.

The edited book by Renate Mayntz, reviewed in this article, provides a comprehensive account of the post-crisis regulatory changes in the financial area. The book is mainly guided by empirical concerns: it aims at documenting what has changed, and also not changed, in the aftermath of the recent global financial crisis at three levels: national, European Union, and international level. It asks whether we observe a profound shift from the regulatory practices of the pre-crisis era (self-regulation) to a much stricter public regulation after the crisis, or instead a bleak transformation with perfunctory changes. It involves twelve chapters and is structured along these three analytical levels. After a detailed introductory chapter by Mayntz, five chapters (from chapter 2 to 6) are devoted to regulatory changes in advanced post-industrial economies, involving the US, the UK, France, Germany and Switzerland, respectively. The seventh and eighth chapters cover the regulatory changes at the European Union level. Whereas the former provides an overall account of changes, the latter focuses on the hedge-fund regulation, specifically. From chapter 9 to 12, the regulatory efforts to change the international financial system are analyzed by focusing on the international accounting standards, the Basel committee, the Financial Stability Board, and the broader assessment of the international financial architecture. In order to give full credit to the chapters and to present their detailed arguments, I will separately discuss them in the following lines, and will rely on the analytical organization of the book. In the end, however, I will share my general comments about the book.

Financial Reform at the National Level

The country case analyses start with the second chapter by Wooley and Ziegler, which examines the American case in detail. The authors recognize the structural changes in financial regulation in the aftermath of the crisis, with the establishment of the Financial Stability Oversight Council and the Consumer Financial Protection Bureau, as well as changes in the procedures and practices such as enhanced prudential regulation of derivatives activity and systematically important firms. However, they also observe that financial policy elites supported the status quo and the preferences of the existing lobby groups in the financial arena. For these policy elites, it was not enough to delegitimize the existing financial institutions despite the magnitude of the crisis. Hence, the post-crisis institutional change in the US was incomplete and unable to address the most significant weaknesses of the financial system. What is further interesting in this chapter, especially for the economic sociologists, is the role assigned to 'policy entrepreneurs'. The chapter presents a detailed account of how knowledgeable and skillful policy entrepreneurs such as Elizabeth Warren, Paul Volcker and Gray Gensler were able to change the course of intellectual debate and policy formation process. Given that most accounts of financial crisis and institutional change solely deals with structural factors such as interests, institutions or ideas, I found this emphasis on agency particularly important.

The third chapter by Johal, Moran and Williams discusses the post-crisis changes in Britain. Similar to the American case, the changes documented in Britain were either delayed, incomplete or inadequate. As usually agreed, the British financial system is very similar to that of American one with respect to the size, that of being one of the centers of the global finance, that of the centrality of financial innovation and public reliance on self-regulation. However, arguably in a larger scale than the US, according to the authors, the British crisis not only called into the question the structure and weaknesses of the financial regulation, but also a whole economic strategy based on financial activities. The authors demonstrate how the City of London, before the financial crisis, was commonly pictured as the motor of British economic revival, in a context of decaying manufacturing industries. Even after the crisis, the British efforts to replace the existing framework were extremely cautious not to jeopardize the role of the City in the British economy. Beyond documenting the inadequate of changes, this chapter also nicely deals with the discussions around the reform proposals and presents the coun-

tervailing forces that are in favor of further reform. What I found especially interesting in this chapter is the emphasis on the changing institutional and sociological structure of the Bank of England. The authors argue that the Bank of England has traditionally been closer to financial markets and ideologically supportive of self-regulation. However, in the last generation, and at an accelerating pace in recent years, this character has changed. The Bank became increasingly dominated by economic professionals who are relatively autonomous from the interests in the markets, and whose legitimacy depended on their professional accreditation as economists. Andrew Haldane, according to the authors, was an important case in point. As the deputy director, he has been outspokenly critical towards the bonus payments systems, the threat of financial institutions to relocate their activities, and to the grand narrative that the financial system is the major contributor to the British economy. I think this story can be further analyzed with the sociological perspective of economic professions provided by Marion Fourcade (2009). It would be interesting to see how increasing professionalism and central bank authority contributes/inhibits market perspective or interests in the governance of financial affairs.

One can easily understand the continuity, rather than change, in the Anglo-Saxon financial markets with reference to path dependency of the political economy model, or the power of finance capital in shaping political outcomes. But, what is the situation in the continental European countries that either have not completely adopted the financialized model, or kept their relative autonomy from the political power of finance capital? Surprisingly, the changes in these countries were also limited. For example, the fourth chapter by Nicolas Jabko attempts to explain the disparity between the critical rhetoric for the global regulation and the absence of overhaul reform in France. The French government officials were extremely critical to the international financial governance, after the initial signs of crisis emerged in 2007. But Jabko also documents that there is a large gap between this ambitious rhetoric for transforming the finance, and the actual changes that have been made in recent years. According to him, cognitive biases in the decision making process are more explanatory to make sense of the French puzzle, in comparison to other accounts that either focus on the French incapacity to set the EU agenda, or domestic theories of interest. In his analysis, Jabko pays attention to the sociological composition of the French financial policy elite. Following a very similar career trajectory (graduating from ENA, starting their careers in the Ministry of Finance

as financial inspectors and recruited by private banks in their late career), according to Jabko, these elites are well connected to each other, and they have held the belief that most problems did not originate or manifest themselves in France. Also, they have shared the belief that they have effective informal ways of dealing with the problems of France's biggest banks, largely due to informal connections between them. Therefore, political motivation for comprehensive reform was insufficient in France.

The chapter written by Handke and Zimmermann on Germany, on the other hand, emphasizes the role of institutional barriers for reform. The authors are similarly puzzled by the fact that Germany has been very hesitant in adopting full range of reforms after the crisis, even though, Germany was among the most internationally active proponents of reforms prior to the crisis. Authors argue that the German financial system is closely linked to political structures at the federal, regional and local level. This creates numerous veto opportunities for actors with a stake in existing institutional structures, and makes it harder to accomplish comprehensive reforms.

Switzerland is another coordinated economy with a corporatist tradition, but also with higher level of financialization and with its financial system's unique integration to the international finance. In chapter 6, Steinlin and Trampusch argue that the changes in the financial realm in Switzerland, in the aftermath of the crisis, cannot be adequately understood without examining the interaction between the national and international levels. For them, most of the changes in Switzerland were made possible with the international community's pressure on banking secrecy and self-regulation. The authors document how Switzerland managed to protect its financial system's peculiar features until the crisis. They also explain how three central elements of Swiss political economy – namely right wing party dominance, weak state capacity and preference for self-regulation – helped this peculiar system to survive. Similarly, they argue that these factors also help explain the inadequacy of the changes in Swiss financial system, even though there were major international pressures. This chapter is perhaps noteworthy for two other reasons. First, the authors' emphasis for the interaction of national and international levels should be the way to be followed. One common mistake of comparative political economists or country experts is to focus too much on domestic political developments. On the other hand, international political economy scholars pay too much atten-

tion to international dynamics, perhaps neglecting how domestic political factors shape international politics. Secondly, this country chapter illustrates a country with a very different trait. Unlike the other four countries covered in this book, Switzerland exemplifies a small country with higher level of international vulnerability. As the authors rightly stress, even though towards the very end of the article, this country case provides a valuable opportunity to test the insights of Katzenstein (1985). Focusing on the national-international nexus, and taking the country size seriously, one might say more about the possibility of changes in the financial regulation.

Efforts at the European Level

The seventh chapter by Quaglia documents the regulatory changes in the European Union. She rightly observes that financial regulation in the EU was mainly a technical policy area prior to the crisis, however, heads of states and governments – most notably Sarkozy and Merkel – became interested in financial regulation after the crisis erupted, and at times adopted populist stances to appease public opinion. Nevertheless, the regulatory response of the European Union to the global financial crisis was still incomplete, and involved an incremental change rather than a major break with the past. Quaglia points out three reasons for this incomplete change: interlocking mechanism of the EU, different member states perspectives, and lobby power of financial groups. Particular attention is being paid to the different interests of the opposing countries – the UK, Ireland, Luxembourg and some Nordic countries- on the one hand, and the countries that are in favor of further regulatory response – most importantly Germany, France, Italy and Spain. This chapter, as the preceding one on Switzerland, reminds how the interaction of different levels is essential to the analysis of financial regulatory change.

The next chapter by Woll focuses on the hedge fund regulation in the EU. Similar to the previous chapter, this one, too, emphasizes the centrality of national positions. However, it puts more emphasis on the 'interest perspective' and the inter-connections between governments and business. In particular, this chapter attempts to unpack the positions of the French, British and German governments and shows that each nation state defended the interests of their national industries with respect to the hedge fund regulation. Not surprisingly, the final directive, AIFM, emerged as a political compromise between the two opposing forces, namely the UK and France. While the for-

mer had to accept that alternative investment would be regulated at the supranational level, the latter had to agree on the exclusion of off-shore activities from the scope of the regulatory directive.

International Financial Architecture

The chapters that discuss the reform at the international level deal with different aspects of financial system and institutions. In chapter 9, Lagneau-Monet and Quack examine the reform proposals to change the international accounting standards. Unlike others in this edited volume, this chapter adopts a public policy analysis approach and traces the political process in which the multiple reform proposals unfolded and co-evolved. Borrowing from Kingdon (1995), authors discuss the financial crisis as a 'window of opportunity' for financial reform. However, at the same time, they also observe that actors have never converged on a single reform project at any stage of the process. Instead, multiple political agendas and reform proposals co-existed. What is more important, according to the authors, is the centrality of evolving problem definitions in the policy formation process, rather than describing the process as a transformation from one equilibrium point (pre-crisis) to another one (post-crisis).

The next chapter by Goldbach and Kerwer discusses the changes in the Basel banking standards. They observe more continuity than change after the financial crisis. Even though higher standards such as capital requirements and definitions have been introduced, the pre-crisis decision making structure has been left untouched. Much more importantly, this chapter shows that the committee members still adhere to the previous approach to the banking risk. They still hold the conviction that the uncertainty in financial markets can be transformed into calculable risk, even though, the recent crisis questioned this approach and the recent work suggested instead that financial markets entail unknown and incalculable risks (Taleb 2007). I find this observation especially valuable given that most of the changes we observe after the crisis occur at the level of settings and instruments, rather than policy goals (Hall 1993).

Another chapter on international financial changes focuses on the transformation of the Financial Stability Forum (FSF) to the Financial Stability Board (FSB). Shawn Donnelly shows that the Board is more institutionally developed compared to its predecessor with a higher internal capacity to handle work load, better institutional capacity to coordinate external activities and members, as well as with

a better framework to deal with national jurisdictions. Similarly, the newly established board is much more successful in developing concrete common goals, enhance transparency and generate peer pressure for reform. However, Donnelly observes that the Board still preserves most of the self-regulatory practices and does not challenge the status quo in a comprehensive way.

In a similar vein, in the very last chapter, Underhill & Blom evaluate the transformation in the international financial architecture as incomplete. For them, the extension of the membership of the Board from G7 to G20 was a major step to increase the involvement of different voices. However, they are still skeptical of the influence of these new members into the building up of the new architecture. The authors argue that private financial interests and their strong relations with the G7 state elites still affect the formation of financial policies at the international level. Borrowing the 'input v. output legitimacy' concept from Scharpf (1999), the authors illustrate that whereas the input side has improved, there are no observable changes at the output side.

Some Critical Remarks

This edited book provides one of the most comprehensive accounts of the post-crisis regulatory response both at the national, European and international level. The chapters are written by well-informed country and subject specialists, and clearly document what has changed – also not changed- in the aftermath of the global financial crisis. In this respect, I think the book accomplishes its most important task: providing an accurate account of post-crisis financial regulatory changes. However, like any good work, it has strong and less strong sides. The readers would have definitely benefitted more from the book had it provided a better conceptual and theoretical framework (i) to delineate the scope of changes/non-changes in the financial regulation, (ii) to make sense of variations between nation states, between different analytical levels (e.g. national v. international), and between issue areas, and (iii) to explain why regulatory change is inadequate compared to the expectations. It is true that it might be unfair to criticize an edited book – with mainly an empirical focus- on the grounds that it does not provide a clear argument or a theoretical framework. However, I hope these comments will be taken more as suggestions for future research on the topic, rather than as a direct criticism for the book.

First of all, I think we need a better conceptualization to evaluate and to describe changes in the financial markets. The book, as outlined in the introductory chapter by Mayntz, borrows the 'incremental v. radical institutional change' conceptualization from the institutionalist theory in order to determine the confines of change. However, it is not totally clear from the text – both in the first chapter and as it was applied in the empirical chapters – if the emphasis is on the pace of change, or the scope of change. Evidently, as discussed in the institutional theory, radical change refers to a major policy and/or institutional change that come immediately after an external shock. And empirically, what we currently observe in the financial markets is not a case of radical change. On the other hand, incremental change refers to small adaptations and gradual change that may have large impact in the long term. Given that we are still experiencing the post-crisis context, one can hardly be sure if the current situation refers to a limited change – in terms of the scope of changes – or rather to an intermediary step that can be perceived and theorized with the concept of incremental change. I find this conceptualization neither necessary, nor helpful to make sense of events happening in the financial markets. Instead, I believe, a framework that takes the kind of changes into account, would be more useful to evaluate the recent developments in financial regulation. Here, I am directly referring to the seminal article by Peter A. Hall (1993), where he identifies three distinct kinds of change: settings (levels), instruments (techniques) and policy goals. My reading of the empirical chapters is that we do not observe a fundamental 'policy goal change' in any of the cases. Many chapters stress that policy elites still hold similar views towards financial risk and the operation of financial markets. On the other hand, the changes are rather about settings and instruments of policy. For example, as discussed in chapter 10, changes in the Basel accords increase the capital requirement ratios and introduce stricter definitions for capital, but do not alter the way financial risk is calculated or perceived. Needless to say, one does not need to use this framework, or any other similar one. But the point is that we need better theories to evaluate the nature of regulatory changes in the financial markets in order to better assess what has changed, and what has not.

Secondly, even though being fully aware that it would be unfair to expect it from an edited book, I think we need better arguments to explain variations in the financial change between different nation states, between different levels of analysis, as well between different issue areas.

Even though not a systematic effort was put in the empirical chapters, the introductory chapter states that financial reform was more observable at the national level compared to the EU and international level. One reason put forward by the author is that the severity of threat was much more explicit at the national level. Similarly, country chapters explain why reform was delayed or inadequate in each country with reference to domestic political factors, but the reader would have benefitted more had the introductory chapter told more about these factors, which might explain cross-national diversities. Moreover, the reader would have enjoyed the book more if they had read more about why certain issue areas were transformed more than others. Even though not covered extensively in the book, we know from other studies that the efforts to introduce macro-prudential regulation to address systematic risk were appreciated by the policy makers more in comparison to other reforms. This was both due to the severity of problems and the impact of the earlier intellectual work by Martin Hellwig, Avinash Persaud, Charles Goodhart as well as the research department of the BIS (Baker 2013).

As a last point, and in a similar vein with the previous comment, I think the future research on the topic need to have a more comparative focus to explain the barriers in front of financial regulatory change. Each individual chapter in this book provides an excellent account of these barriers – either being the political power of finance capital, the role of domestic institutions, or the cognitive factors shaping elite perceptions. This directly engages with the 'interests, institutions and ideas' debate in comparative politics and political economy. Therefore, the future research should aim testing the explanatory power of these perspectives in comparative settings. The politics of post-crisis financial reform provides an enormous opportunity to test our theories about politics and society. We should not miss this opportunity.

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"Cliometrics" was always controversial, and self-consciously so. Launched as the "new economic history" in the 1960s and 1970s, of course in struggle with an "old economic history", the cliometricians wanted to apply economic (neoclassical) theory and the quantitative toolbox of the economist to economic history. Like with other forms of "economic imperialism" – the spread of economic perspectives and techniques to other disciplines – this has met with resistance, in this case by historians more focused on contextualization and inductive methods.¹

Francesco Boldizzoni's *The Poverty of Clio* is a *streitschrift* against cliometrics and for a different kind of economic history, which Boldizzoni labels "creative" economic history. Essentially, his objections to cliometrics boils down to two arguments. One, cliometrics by applying neoclassical theory to the past untenably presupposes the homogeneity of the economy over time, seeing a modern capitalist market economy everywhere, even where there isn't one. Cliometrics misrepresents the actions of historical agents by presupposing that they are economically rational in the sense of profit-maximizing. Two, cliometrics uses the wrong methodological approach, using theory-driven deductive designs when really too little is known for a deductive approach, and being too hasty in applying quantitative methods when the historical data are not good enough. I am sympathetic to the first argument but less so to the second one. I will go through the contents of the book to clarify how Boldizzoni supports his claims, and then get back to the two fundamental arguments at the end of the review.

Chapter one presents Boldizzoni's definition of cliometrics and some main criticisms. To Boldizzoni the purpose of cliometrics "is to create narratives of the past compatible with neoliberal economics" (p. 5) and its definition is the application of neoclassical economic theory to history rather than the use of statistics and econometrics (p. 10). In chapter two Boldizzoni criticizes the market focus of neoclassical economics, including so-called neo-institutionalism (e.g. Douglass North) which tried to make economics more realistic but according to Boldizzoni still sees a market as the norm and other institutions as replacements (p. 39). In this chapter there is also some material which feels scattered and unnecessary, where Boldizzoni criticizes methodological individualism with an array of references (Durkheim–Mary Douglas–Bourdieu–Giddens–Margaret Archer, pp. 41–45) and also the idea of "the selfish gene" (pp. 45ff.) The rather rapid mix of subjects reveals one of the weaknesses of the book: sometimes it hurries from one topic to another without leaving the reader feeling really satisfied with the conclusions on any of the topics. Chapter three continues the criticism of the market-centeredness of cliometrics, including a hard-hitting critique of the Stanford economic historian Avner Greif's seemingly completely misleading analysis of Genoese traders during the 13th century. Again, however, the chapter is a bit unfocused with too many different critiques – the criticism of market thinking in analyses of love, religion and the antique economy seems apt to me, but mixing this with equally unforgiving criticism of using modern GDP growth models on historical data and of technical details such as which proxies may be used to measure historical living standards, makes the reading fragmented. While I am convinced that market-centeredness is an essential tenet of cliometrics (as defined in chapter one) and therefore worth thorough discussion in the book, I don't feel that the discussion of living standards proxies and growth models is necessary for the book's purposes.

After these two scattered chapters, we reach two more coherent ones. Both have the header "The world we have lost" and present Boldizzoni's ideals for economic historians; chapter 4 deals with micro and chapter 5 with macro economic analysis. Chapter 4 centers on analyses of households which do not assume modern capitalist market rationality: Witold Kula's *Economic Theory of the Feudal System* (1962) and Chayanov's peasant economy analysis are key references here. Chapter 5 focuses on the Annales school. Compared to chapters 2 and 3 which seem unnecessarily shrill and unfocused, chapters 4 and 5 are much more coherent, focusing on the key issues for an

economic history that wants to avoid the neoliberal market bias of cliometrics (as defined by Boldizzoni): how to understand human action in different economic-historical contexts without resorting to the simplistic notion that actors always follow a modern capitalist rationality.

Chapter 6, "Building on the past", aims to present the coherent alternative, what Boldizzoni calls a "creative" approach to economic history (as distinct from narrative history and cliometrics). Again, the chapter like chapters two and three partially lapses into very short relapses of lots of empirical research that Boldizzoni finds exemplary – here for example on culture, consumption, markets, money, capitalism and capitalists, families, fiscal history, history of ideas, and environment. Boldizzoni writes well and I find many interesting reading tips here, but again I find that the short recaps of research are not enough to convince me in every single case that the preferred interpretation of Boldizzoni is indeed the right one; since he skips along so quickly between different topics, it feels like none of the debates are conclusively and convincingly settled. One of the blurbs of the book claims that "few works in historiography can muster the scope and learning of this book", which is a good point, but this reader would prefer a more narrow focus, on topics essential to the book's key argument, so that each discussion could be more conclusive.

So, I find some problems with the structure of the book, but let me emphasize that it is still always interesting and that while the book is less focused than I would have wanted, it is also a treasure trove of references. So let's get back to the argument: that cliometrics is wrong in applying a market model to all times and contexts, and that its use of a deductive approach and quantitative methods is misleading. I agree with the first point: as Boldizzoni shows in the case of Avner Greif's analysis of medieval Genoese traders, economic historians often are too hasty to presuppose that actors follow a profit-maximizing rationality. But the other part of the argument

is less convincing to me. Boldizzoni claims in one place: "The use of deductive reasoning should be avoided, because the work of the historian is inductive by definition." (p. 151) At another place, he claims that since theories must be built from empirical observations, historians must not work from theory, since without empirical observations there could be no theory in the first place. To me, the problem seems so much easier: one of "scope conditions"². The historian must know his/her context well enough to know which kind of (hypothesis-generating) theories and models are applicable to the present context. The fault of some (but not all!) of the cliometricians criticized in *The Poverty of Clio* is to this reader not that they employ a deductive approach, use theory to generate hypotheses, when studying history, but rather that they use a theoretical model of profit-maximizing capitalist rationality that is simply not applicable to the periods studied, but rather, as Boldizzoni says, serves to "create narratives of the past compatible with neoliberal economics" (p. 5). It is the specific theory, or rather its just outside of its scope conditions, that is the problem, not the use of theory *per se*.

In all, while I do not find the argument of the book completely convincing and also have some objections to the presentation and structure of the book, I still found *The Poverty of Clio* an interesting and stimulating read, commendable for anyone interested in economic history.

Endnotes

¹Boldizzoni's criticism is very similar to for example the criticism of neoclassical economists by institutional and other heterodox economists; see Geoffrey Hodgson's *How Economics Forgot History: The Problem of Historical Specificity in Social Science* (Routledge, 2001) for a good example.

²As introduced in many a research design textbook, such as Bob Hancké's *Intelligent Research Design: A Guide for Beginning Researchers in the Social Sciences* (Oxford, 2009).

Ph.D. Projects in Economic Sociology

The Transformations of a Political Market: the Case of the French Railways

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While states may become involved in the functioning of economic exchanges in a variety of ways (see Block and Evans, 2005), some economic activities seem particularly political. French railways definitely fall into this category. Since the mid-19th century, the railway market has been strongly shaped by the state, which quickly organized it into regional monopolies. Furthermore, railway companies were nationalized in 1938, and until a few years ago the French state-owned company SNCF (for French National Railway Company) benefited from a total monopoly of exploitation.

But these highlights do not give an accurate picture of how the sector really functions. Firstly, even if the state has been particularly present throughout the history of the railway, this does not mean that its economic regulation has always followed the same trajectory. Railway policy has changed over time, and was strongly influenced by the dominant state ideology at a given time. Secondly, it would be wrong to assume that the French state has been building the railway economy alone for several decades, since firms, even when they belong to the state, are economic entities that seek to defend their own interests (Bourdieu, 2005). Thirdly, even when it is a legal monopoly, an economic sector has to be considered as a competitive market since a good can often be substituted for another (Chamberlin, 1933). Thus, during the last century railway companies had to deal with the significant increase in road and air transport that offered alternatives to their clients.

My dissertation is a contribution to the sociology of firms and the state applied to markets (see Fligstein, 2001). I analyze the functioning and transformation of the French railway policy during the 19th and 20th century, as well as the economic strategy of SNCF, the main French railway company since its creation in the 1930s. My study relies on different types of sources: legal materials, newspaper articles, official reports, books and journal articles written by

state engineers, SNCF archives and railway workers' union documents. I have also conducted interviews with various actors in the sector (e.g. senior officials, managers and union leaders) to analyze the recent period.

In the first part of the dissertation, I focus on the relationship between the state and the railway industry. In line with Dobbin's analysis (1994), I show that the state has increasingly shaped the market over time with the help of state engineers from the "grands corps d'Etat" that dominate the sector. More specifically, I study the role of public authority in expanding the railway network and in the birth of a railway transport as a "public service". This leads me to question the reasons for the central place of the state in the market. To a certain extent, French political culture may account for the prevailing market architecture in France. But to understand the specific development of the national railway policy, the physical features of rail technology need to be considered. Railways make transport faster, more widely used, and cheaper: in this way they can be a strong lever of economic development, as well as a tool for promoting national political cohesion. Additionally, the high fixed costs and economies of scale that characterize this industry help explain why it has long been considered a "natural monopoly", and therefore a market that has to be strictly regulated by the state. In order to further interrogate this, I then focus on the railway policy in the second half of 20th century and show that, contrary to some preconceived notions, railway nationalization didn't necessarily reinforce the public service principles governing market regulation. Instead, during this period there was a neoliberal turn in the ideologies of successive French governments (Denord, 2007), with significant effects on the railway industry: some parts of the network were abandoned; SNCF was ordered to become financially independent; etc.

In the second part of the dissertation, I examine the economic strategies of SNCF. I first explain why economic changes encounter resistance within the state-owned company. The firm is known for its organizational culture linked to belief in public service, for being deeply rooted in combative unionism, and for its lack of commercial spirit. However, the replacement of management direction over time altered the nature of the power struggles within the organization and promoted change. I illustrate this point through an analysis of the transformation of the rail fare

system (see Finez, 2014), in a way that echoes the debate on economic performativity (MacKenzie et al., 2007). The historic fare system, based on the principle of a uniform rate per kilometer, was steadily abandoned during the post-war decades in favor of pricing indexed to marginal costs. At the turn of the 1980s-90s, this paradigm was itself replaced by a real-time pricing mechanism aimed at obtaining as much surplus per consumer as possible. I show that the transformation of SNCF fare models was the result of work by a few leading actors who relied on tools from the discipline of economics, as well as their own influence, to present their innovations as possible, necessary and legitimate. Finally, I show however that the economic strategy of the firm cannot be reduced to its behavior in the national rail transport market. To cope with the rigidity of rail transport, particularly with the problem of breaking bulk, SNCF began very early to extend its activities into road transport. More recently, as part of the European market liberalization, the firm built a corporate group dedicated to transport and logistics, composed of about 1000 subsidiaries that have achieved market shares all over Europe and beyond.

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From family to investor capitalism? The Swiss machine, electro-technical, and metallurgy industry during the 20th century

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Many authors have shown the persistence of family capitalism in most developed countries during the contemporary period (see e.g., Colli 2003; James 2006; Colli/Rose 2008). The issue remains, however, relatively neglected in the Swiss case. Most contributions consist of case studies, and thus do not allow a general interpretation of Swiss family capitalism. Studies that take into account a larger group of firms (Frey et al. 2004; Ernst & Young 2005) tend to neglect the historical perspective. The aim of my research is to fill these gaps by showing how families resisted the so-called managerial and financial revolutions that were supposed to put an end to family capitalism. For this purpose, I analyze the evolution of corporate governance in twenty-two large firms in the Swiss machine, electro-technical and metallurgy (MEM) sector – the largest industrial employer and exporter of the country from the inter-war period (Billeter 1985) until the end of the 20th century – whose board of directors and executive managers have been identified for five benchmark years across the 20th century (1910, 1937, 1957, 1980 and 2000). This thesis relates to business history and the sociology of elites, and uses different methods such as network analysis and prosopography. It is articulated around three main axes of analysis.

The first one shows the evolution of corporate governance in the MEM firms. I examine the distinction between *family-owned* and *family-controlled* firms (Casson 2000: 199) in order to trace the evolution of both dimensions during the 20th century. My results show that family ownership and control prevailed until the 1980s. The existence of a very minimal legal framework concerning corporate governance allowed founding dynasties to keep control and ownership of their firms, even when the latter became publicly quoted. The arrival of “new” families in existing firms also contributed in an important way to the resistance of family capitalism to the so-called “managerial revolution”. However, during the last decade of the centu-

ry institutional investors became more important among Swiss shareholders and better protection for minority shareholders was implemented. We thus observe a shift of Swiss corporate governance towards investor capitalism and shareholder value ideology, although many families were able to resist these changes and to keep their firm in their family's hands until the end of the century. For example, most of them continued to issue different categories of shares, an old practice that was clearly against the shareholder value ideology and that allowed families to keep the majority of the voting rights in the firm without holding the majority of the capital.

The second part of my thesis focuses on interfirm coordination. During most of the 20th century, Switzerland was a coordinated market economy, as opposed to liberal economies (Hall/Soskice 2001). In this context, interfirm ties, identified through a network analysis showing interlocking directorates, were an important mechanism of coordination among business elites. My research allows me to demonstrate that family firms were clearly integrated into the Swiss corporate network, holding a very central position along with banks. This is an important finding, as corporate networks are usually considered to have supplanted family ties (Windolf 2009); in the Swiss case, on the contrary, families – along with bankers – contributed to the creation and the consolidation of a dense Swiss corporate network that allowed the corporate elite to organize business transactions, but that also contributed to reinforce the cohesion of the Swiss capitalist class. Moreover, MEM elites, including members of family firms, were strongly represented in business interest associations. By the end of the century however, interlocking directorates declined strongly, and corporate elites began to withdraw from business interest associations.

My analysis ends with a prosopography of the business elite leading the MEM firms, as corporations cannot be understood apart from the corporate elite, and vice versa (Useem 1980: 43). It is worth noting that the family managers' profiles showed no major differences when compared to the non-family managers' profiles. Family managers in particular were highly educated and therefore were in fact professional managers, contradicting the rather artificial distinction often made between the two groups (see e.g., Chandler 1977). The profile of the MEM corporate elite remained in some respects very stable during the 20th century. In particular, women clearly had no access to the leading positions in the firms. They played, however, an invisible but nonetheless key role in the transmission of

these positions among family firms, through marriage and family alliances. Moreover, the corporate elite usually came from the upper class, although some self-made men managed to build successful careers that were sometimes handed down to the following generations. In other respects, the MEM elite profile showed deep transformation at the end of the 20th century. First, more and more foreigners were sitting on the boards of MEM firms between 1980 (2%) and 2000 (22%), and this affected the cohesion of the Swiss corporate elite. Second, more and more of the MEM elite earned masters in business administration degrees (MBA). This played a role in the shift of Swiss firms towards a shareholder value ideology by the end of the century.

To sum up, my thesis shows that no managerial revolution happened in Switzerland – at least not in the MEM sector. Although some important changes that took place by the end of the 20th century confirm a transition towards investor capitalism and more competitive interaction among firms and the corporate elite, the persistence of family control in several companies calls for a more nuanced conclusion arguing for the coexistence of both forms – family and investor – of capitalism.

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Farm advisors. The love of progress during the "silent revolution" (1945-1983)

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This research focuses on the history of a professional group: agricultural advisors, whose role in French agricultural modernization after 1945, known as the "silent revolution" of the French peasants, is still unrecognized. It offers a new reading of one of the largest upheavals of the 20th century: the postwar rural transformation. In so doing, it examines the institutional mechanisms leading to social and economic changes, internal dynamics in the agricultural and para-agricultural sectors, and the moral values and perspectives of the actors involved in this "big bang". The main goal of this work is to clarify the farmers' accession in shifting to large investments and changing their lifestyles, while considering them neither as pure rational actors, nor as manipulated agents. Agricultural advisors and farmers worked together within a political and economical framework which formed the foundation for national negotiations between the State and farmers' organizations, but they also appeared locally as co-producers of the modernization policy. This study on the history of agricultural advisors aims at decentralizing common view turned of the era of modernization, from after the Second World War until the early 1980s. It distinguishes three periods, around which the three parts of the thesis are articulated.

From the Liberation of France until the late 1950s, a new political project took place: to increase the productivity of agricultural labor through the promotion of family farms. Agricultural advisors emerge as essential interlocutors for policy makers on one side, and farmers on the other. Re-

construction provided the opportunity to try new devices for managing agricultural investments. Further, French productivity missions to the United States or northwestern Europe, led by officials and agricultural union leaders, brought back new methods of agricultural extension. During the 1950s, several local initiatives allowed experimentation with these methods, paired with the ongoing presence of a technical advisor working closer to the farmers and covering a smaller area. In these closely controlled areas, advisors worked with small groups of volunteers, trying to generalize new production techniques and to improve standards of living of farmer families. After tense negotiations, the role of agricultural advisors gets its first official recognition in 1959, when the implementing responsibility for the extension service for agriculture was gradually transferred from the State to professional agricultural organizations. The efficiency of the French agriculture modernization policy was therefore based on the mobilization of a group of farmers from small and middle peasantry, who would agree to invest in their production tools in order to increase their standard of living. Agricultural advisors played a key role in this work of mobilization. Often sons of small farmers themselves, they were able to quickly build relationships with the members of the modernizing peasantry, with whom they shared social benchmarks, values and ambitions.

In the next period, from the early 1960s to the beginning of the 1970s, agricultural advisors, only men at first, and then women recruited to work with female groups, tended to form a relatively independent professional group. They operated under more homogeneous frameworks: the Chambers of Agriculture emerged as the main employer, capturing public funding for agricultural extension, and federating most of the local initiatives of the 1950s. Agricultural advisors were all trained in national centers where they would learn not only modern production techniques, but also accounting, management, group dynamics and meeting facilitation as well. They formed specific unions, trying to forge their professional identity around common values: independence from the State, devotion to the social promotion of the family farms, and a belief that progress inevitably depends on technology, economics, and social issues. But the socio-historical analysis of this professional dynamic reveals a constant tension between on the one hand the roles assigned to them (their function), and on the other, the meaning they gave to their activity (their business). Professionalization appears here as an unfinished process: if the advisors are able to form a distinct group from both agricultural teachers (employed by the State)

and commercial agents (employed by industries and large cooperatives), they are never truly autonomous from their employers, the leaders of agricultural professional organizations. This constant tension did not prevent the forming of a specific professional *ethos* shared by most of the advisors, men as well as women, based on the dedication to the cause of farmers engaged in the modernization, and a sense of resourcefulness comparable to the *métis* of the ancient Greeks.

Finally, from the early 1970s to the early 1980s, agricultural development policy suffered a crisis: funding problems, a divergence of views between agricultural professional organizations, and economical difficulties encountered by farmers who had invested a lot during the two previous decades. That crisis legitimated the introduction of new management methods to reframe the work of agricultural advisors. This managerial turn weakened the position of the advisors by imposing new bureaucratic requirements, which ran counter to their very conception of their business (valuing autonomy and dedication). More generally, it questioned the modernizing project, undermining confidence in “progress”. Diversification of departmental agricultural policies, including the promotion of quality labels, appears as a possible response to the economic impasse of the race for productivity. In mountain territories particularly, agricultural advisors play a decisive role in promoting alternative models of development based on a better valorization of local products. A statistical survey carried out in 1982 on the occasion of the States General of Agricultural Development shows the progressive invisibilization of agricultural advisors. Although the first advisors' moral values remain essential references for all agents involved in the agricultural development policy, the transmission of the collective professional identity elaborated during the previous three decades is no longer assured. Agricultural advisors tend to lose importance in the process of modernization. The modernizing project itself is fading little by little.

Sharks and peaches: an economic sociology approach to the birth of the market for small loans in Georgia, 1904-1928.

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The “loan shark” starts to appear as an “evil” public figure in the United States during the 1870s. The term “shark” itself has a long history relating to the shortness of cash money in the XIXth century. It came to describe more directly “salary buyers” in a booming industrial society¹: a network of loan companies which started to lend small amounts of money to industrial wage earners working for large firms in the late XIXth century². Earning a stable wage was seen as a sufficient collateral for those who didn't have anything else, and they could get access to cash money without having to wait until payday through small loans whose interest rates highly over passed the limits set by Usury Laws (Easterly, 2009).

The State of Georgia had a loophole in its civil code, it acknowledged the difference between a “loan” and a “sale” when it came to that particular commodity that are wages. The “salary buyers” could therefore argue their way out of common Usury Laws, and hence flourished. This particular legal feature enabled a nation wide network of loan companies to develop, known as the “Big Four”, who were based in Atlanta and operated hundreds of agencies all over the country, giving rises to innumerable judiciary conflicts relating to the usurious nature of their operations. The legal loophole was probably not the only reason for this, but Georgia was always at the center of the reformist campaign: it was believed that if the situation could improve in this State, the rest of the country would follow.

Small loans and their highly volatile collateral (wages, small property, etc.) required efficient procedures of debt settlement for the lenders, and as the Law was very much subject to interpretations, it is interesting to look at the different instances who played a part in that process. Loan sharks and small loans were central issues in the debates about the reform of the judiciary system at the time, the most typical example being Louis Brandeis's Justice for the Poor where the author strongly argues in favor of the abolition of the Justices of the Peace courts, because of their

close relationships with sharks. The Justices of the Peace were indeed the principal legal channel used by sharks to collect their debt, through foreclosures, garnishments or assignments of wages, and the latter almost always judged in favor of the sharks. However, sometimes borrowers would contest the decision, on their own or strongly encouraged to do so by reformists and Legal Aid activists, and the conflict would move one step up the judiciary ladder. Some cases even reached the Georgia Supreme court, and we can analyze the structure of the market through the capacity of actors to impose their reading of the law and their understanding of the nature of credit and credit mechanisms at different legal levels (JP courts, municipal and county courts, penal courts, Supreme courts). Are transformations of future wages in hard cash money just buying and selling of specific commodities, a one shot operation hence regulated by common exchange laws or are they actual loans of money, entailing a specific lasting relation of debt and obligation between a money holder and his client?

Our object is defined by its legal looseness, the businesses we study are neither legal nor illegal or informal, but on the boarder of the legal system, being a “necessary evil” they are never regulated firmly. Moreover, their practices rely on both formal and informal elements, resorting to formal contracts and standardized procedures but also using methods whose traditions are grounded in older and more personal forms of credit. As the law is unclear, the different actors rely on their understanding of existing social relations to enforce debt settlement, they judge on their ability to weigh on the judicial decision making, according to their place in the social system. The *sharks* and their small loans offspring represent a transitional form of credit intermediary, between an old economy of personal credit relations and purely contractual credit forms. But one should not reify this distinction, as we often observe that contracts are used as a means to convince actors of the seriousness, the impersonality, the bureaucratized aspect of the business rather than just to show for the legality of their practices.

Between 1904 and 1920, numerous anti-*sharks* campaign were carried in Georgia to uphold the interpretation according to which salary sales were actual loans of money, they organized around philanthropic institutions such as the Russell Sage Foundation and the Legal Aid Societies. The reformers tried to transform their political objective into a 'public' problem, in the words of contemporary American sociologist John Dewey (1927). They would

strongly rely on the press and on *Grand Juries* to issue indictments and hope that the situation would be regulated. The passing of the Uniform Small Loan Laws (Caruthers/Guinnane/Lee, 2012) was an important step towards a regulated market for small loans, but the section which was supposed to make all salary sales bona fide loans was never included in the draft. Many buyers therefore accepted the regulation and institutionalized their business, but the “salary buyers” never disappeared and the campaign waged on. However, we observe that in the late 1920s the landscape of personal loan finance settles down in Georgia, and the actors who occupy the market at that time are still the one we can identify today.

One of our aims is to describe and understand the various conflicts which helped define the market for small loans in the US in the 1920s, the legitimacy discourses (Boltanski/Thévenot, 1991) resorted to by the different actors as well as the various regulations set up to discriminate between different types of institutionalized lenders. Among those we can identify five principal ones: salary buyers, small loan lenders, industrial lenders, industrial banks, and household finance companies. We are arguing that this allocation operated mostly through legal distinctions, although they were not competing for the exact same pool of clients, we wish to show that the “market share” was not the major element that can help us understand how the market set up. The various lenders were allocated different sectors of the regulation spectrum, mostly as exceptions to Usury Laws – they could set relatively higher interest rates. The second defining variable was the type of collateral required: some companies accepted only chattel property, others required the signature of various members of the family, colleagues, wealthier patrons, etc. while others asked for multiple guarantees. We believe that if we cross the interest rates applied with the choice of the collateral required to get a loan, we can map the way market ideologies and morals interacted at the time (Forbade and Haley 2007).

One example describes the type of actors we can find. On the one hand, the Household Finance Corporation, originally created by Frank Mackey, named the “King of Sharks” and operating mostly in Illinois; at this death, the company was taken over and institutionalized, and its new directors decided to set the lowest interest rates on the market, 1,5 % monthly. It was a lot smaller than the rates permitted by the USLL (3 ½%), and made them the cheapest lender on the market. However, the rationale behind this business model was not entirely economic, the HFC

wanted to orientate small loans towards households, not individual wage earners, giving birth to the world of 'family loans'. Only the man could subscribe to a loan, but he needed his wife's signature to do so, it was believed that this way the money borrowed would be used to a proper motive, and not unsound deeds. The rhetoric was actually a lot more complex, it was believed that relations to credit threatened communication between husband and wife and put too much stress on the husband, hence favoring deviant behavior such as alcoholism or prostitution. The risk of default due to low interest rates and the risk of moral hazard were also believed to be compensated by the soundness of investments made by borrowing couples.

On the other hand, "industrial lenders", were also former salary buyers who decided to go in the regulated small loans business. They coined the term "industrial" to show that they were still targeting the same population (at least in terms of work status) as before, and as such emphasized a lot their long experience in the business. But they didn't offer salary sales or loans any more, they only provided small loan services, and required some sort of chattel collateral, along with the individual's signature (and no one else's), going back to an early and standard form of chattel lending, only now regulated and organized in a trade corporation, the National Association of Small Loan Lenders.

Salary buyers still remained, but after 1928 they started dealing only with the poorest classes of the population. Indeed, with every wave of regulation and institutionalization we see an exclusion of the most marginal borrowers from access to small loans. And this strongly impacts the racial and gender relations which unfold through credit mechanisms. To better understand this process we will carry an ethnographic fieldwork in contemporary Georgia: the comparison with today's "payday loans" or "Refund Anticipation Loans" is interesting because it directly relates to questions of urban poverty and exclusion. The loan sharks' clients are a much more homogeneous population today than they were one hundred years ago, and one of the major reason for this is the massive development and regulation of consumption credit (Calder 2009, Marron 2009, Hyman 2011). In order to show this, we use data gathered from various archives to understand who the sharks' clients were at the beginning of the 20th century, and the evidence suggests a highly heterogeneous population: albeit a majority of industry workers or employees, we also find a significant amount of domestic workers, daily laborers, or street musicians, and on the other end of the spectrum engineers, senior civil servants or even physi-

cians and dentists. Whereas today, exclusion from regular banking and credit services concerns more directly poor wage earners or welfare beneficiaries, and the discourses (economic, social, scientific or philanthropic) are more concentrated around the struggle against poverty.

Endnotes

1As well as some chattel lenders.

2Mostly railroad, steel and mining agency

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Making Brownfields Green: Commodification and Valuation of Urban Spaces

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European metropolises are increasingly constrained by international climate agreements to reduce global warming. Since the 1990's, protocols have defined a set of solutions to reduce urban energy consumption. Making inner cities denser and greener is one of the common remedies identified for curbing urban sprawl, decreasing car gas

emissions and transforming city-dwellers' habits. Indeed, over the past ten years, Sweden and Germany have developed renowned green housing programs in their town centres. Following these urban trends, French cities have recently launched eco-neighbourhood projects that are essentially made up of green buildings (housing, businesses and shops), green equipment (renewable energy technologies, public transportation networks, green heating systems, recycling facilities, among others) and parks. Most of them are extremely extensive urban projects that are often built upon former industrial sites. The scarcity of available land in the centre leads local authorities to set up eco-neighbourhood projects on transitional areas.

Turning these "brownfields" into green real-estate programs is a complex and contentious process. Many public and private actors are involved in the renewal of industrial lands: land owners, city councils, state ministries and agencies, real-estate developers, urban developers, architects, experts, industrial firms and construction companies are among the main players engaged in the valuation of these particular spaces. They use diversified resources to requalify and commodify lands, to reconfigure the urban environment, and to define and singularize the features of the real-estate supply.

Based on long-term fieldwork among real-estate developers and organizational interviews with the above-mentioned players, our research questions the way they use environmental issues to create economic value on land and housing markets. In fact, environmental concerns are far from anecdotic and are increasingly used as principles of commodification and valuation in urban economies. For example, both landlords and buyers now consider soil pollution as a key criterion to set the price of land. As sanitary

issues gain momentum, environmental experts have developed sophisticated tools to economicize pollution. Indeed, remediation is central to the requalification process of industrial lands and its cost has deep implications on the valuation and configuration of eco-neighbourhoods. Another example illustrates the way environmental issues and urban economies are intertwined. In the past five years, institutional investors specialized in business real-estate have translated green accreditations into investment financial models. Architects, real-estate developers, construction firms and engineering companies promote greener practices, while investors consider green building a safer investment.

The dissertation studies the political and economic implications of eco-neighbourhoods. On the one hand, institutional actors see these projects as popular and convenient political measures that combine urban policies defined at different organizational levels. While examining the instruments deployed by central and local public actors in eco-neighbourhoods, we found evidence that these devices are increasingly based on market mediation strategies. Furthermore, our work shows that these high-profit programs are embedded in broader industrial policies. Indeed, they foster both the construction and real-estate sectors, and encourage technological innovation. As such, they encourage the development of green industrial devices and the growth of markets for environmental expertise.

Note from the editors: We decided to publish again this PhD project, which was incorrectly edited in the previous issue.

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