Banking and Real Economic Activity

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1. Introduction

On December 12th, A.D. 56, Lucius Caecilius Jucundus recorded a transaction related to a loan for 11,039 sesterces, which he had extended for the completion of an auction sale that took place in the city of Pompeii.[1] Wax tablets recording this and 16 other similar loan contracts were found, charred but still legible, in an archeological excavation of his house, partially destroyed after the Vesuvius eruption on August 24th and 25th, A.D. 79.

L. Caecilius Jucundus was a very wealthy banker in Pompeii, the son of a freedman, who had become a banker himself. They were bankers in the basic definition of the term, in that they would accept deposits from clients and extend loans using part of the received deposits. The standard terms for such loan contracts implied a commission plus an interest rate that was normally about 2 percent a month, with a typical duration of up to one year but normally no more than just a few months.

Their main role, and the one documented in the above mentioned tablet, was to provide credit at auctions for the sale of property, harvests and slaves. In many cases they would arrange the sale of the very same collateral that had been pledged on past due

[1] As a term of reference, in that same period a laborer wage was 2-4 sesterces per day, and the average price for the purchase of a slave was 2,000 sesterces (Stambaugh, 1988).

loans that could not be repaid. They would also act as assayers of coins, provide foreign exchange services, extend types of loans other than those related to auction transactions, and engage in activities resembling what would currently be defined as trust management (Andreau, 1999, p.36).

While there is significant written evidence of banking activity in the first and second century A.D. in Rome, professional bankers were already in operation in Athens back in the fifth century B.C. and were found at about the same time in Egypt and Palestine (Andreau, 1999, p. 30-32).

Banks have thus been present even in the earliest instances of pre-modern, pre-capitalist societies, their role so pervasive and ingrained in the basic functioning of markets and economies that one almost wonders about the need to discuss the importance of banking institutions for the real economy. However, a debate on the basic determinants of the process of economic development has been alive and kicking at least throughout the entire twentieth century. Within this debate, the role of banks, and the financial sector in general, has been either readily dismissed (see, e.g., Robinson, 1952, Lucas, 1988) or alternatively recognized as “too obvious for serious discussion” (Miller, 1998, p.14). The start of the modern analysis of this issue is normally associated with the work of Joseph Schumpeter, who synthesized the idea that credit, especially bank credit, does create real value in his Theory of Economic Development (1911). However, Schumpeter himself drew on an even older debate when he challenged, for instance, the view of Ricardo that “banking operations cannot increase a country’s wealth” (Schumpeter, 1911, p. 98). This chapter first illustrates the reasons why the debate went on for so long, and goes on to make the claim that perhaps scholars have finally reached a consensus. Second, it presents the most current directions of research on this topic.

2. The causality debate

Perhaps the main reason for the persistence of this debate is that it has been very difficult to pin down the issue of causality. Anecdotal evidence from historical case studies, or even broader informal observations from cross-country data, will normally show a strong, positive correlation between any standard measure of real economic activity – per-capita output growth, per-capita capital growth and productivity growth – with standard measures of development of financial markets. For instance, in what is widely recognized as the first contribution to reignite the most current interest in this debate, King and Levine (1993) drew on data from 77 countries from 1960 to 1989 to show that a basic measure of the "depth" of the financial system – the aggregate value of currency demand and interest-bearing liabilities of banks and non-bank intermediaries – had a positive and economically very significant association with real economic activity.

The underlying idea behind this and related measures of the size of the financial sector is that a broader, deeper financial sector increasingly facilitates firms' access to capital. This is the basic "financial engine" of growth. More precisely, King and Levine show that if a country could increase the size of its financial sector from the bottom quartile to the top quartile of the distribution, the resulting superior access to capital would be reflected in an increase in per-capita income growth by almost one percent per year. Given that the difference in income growth between countries in the top and the bottom quartile of the distribution for the countries in this dataset over this sample period was about five percent, the change in financial depth would contribute to an impressive 20 percent reduction of such a gap (King and Levine, 1993, Table VII).

In subsequent work, a more specific link between just *bank* credit and real economic activity was also confirmed using similar data and empirical methodology. Levine and Zervos (1998) show with a dataset for 42 countries between 1976 and 1993 that an increase in bank credit by one standard deviation results in an increase in real per-capita income growth of 0.7 percentage points per year (Levine and Zervos, 1998, Table III). The basic message of such studies is that the economic magnitude of increasing the overall scale of the banking industry is potentially very significant. However, despite the robustness and the strength of such results, skeptics in the underlying debate have always maintained that while this empirical evidence clearly indicates an important correlation between finance and real activity, it cannot address the fundamental issue at stake, namely whether banking activity – and by extension activity of the financial sector – is somewhat exogenously determined and if it is, whether it exerts an independent impulse on real economic sectors. Critics of the role of finance in real economic activity have always argued that characteristics of financial markets are *endogenously* determined, that is, the existence and the development of anything financial is simply a reflection of real economic activity. The empirical evidence mentioned above cannot disprove the argument that financial markets simply develop simultaneously to accommodate the expanding needs of a growing economy, or even that measures to deepen financial activity could be undertaken in anticipation of predicted future economic growth.

Much subsequent work, most of it by Levine and coauthors, has addressed specifically this issue of endogeneity and causality. It has done so by departing from basic cross-sectional analysis and embracing the more sophisticated econometric tools of dynamic panel estimation techniques (see, e.g., Levine, Loayza and Beck, 2000 and Beck, Levine and Loayza, 2000) and instrumental variables (Levine, 1998 and 1999, Levine, Loayza and Beck, 2000). In essence, the basic strategy involves trying to identify an exogenous component of financial development. This is achieved assuming that the level of development of the financial sector in a country is very much a reflection of the quality of the basic institutional setting that has developed in that country over time. In turn, such institutional setting (reflected in the degree of protection of property rights, in the quality of the legal enforcement system, in the overall level of trust, in the degree of corruption, etc.) is found to be highly determined by the legal origin of that country (see La Porta, Lopez-de-Silanez, Schleifer and Vishny, 1998). More specifically, the nature and quality of basic institutions appear to be highly correlated with whether the legal system of a country has roots in the British, German, French or Scandinavian traditions of rule of law. Since the basic activity of financial markets relies on the possibility of writing well-defined contracts describing transactions based on promises of future payments, financial markets will be more or less developed to the extent to which the legal system allows protection and enforcement of such contracts. And since the establishment of a given legal system in a country is to a large extent the result of past events, such as experiences of colonization, it is plausible to consider this feature as exogenously determined.

Thus, by either augmenting basic cross-sectional studies with instrumental variable analysis, or by using instrumental variables in dynamic panel models, the studies mentioned above came to conclusions that were remarkably similar to the earlier ones that relied on simpler identification techniques. That is, (the exogenous component of) financial development has a substantial economic impact on the real economy.

While this represented an important step in addressing the causality issue, questions could still be raised regarding the quality of the instrument and, perhaps most important, the fact that both the quality of the financial sector and that of other important institutions could all be determined by still other omitted factors. Hence, the doubt remains, following this approach, that an observed positive effect of financial sector variables could in fact be the reflection of something else affecting simultaneously the financial and the real side (see Zingales, 2003).

No less important, however, is the interpretation that we can give to studies that have used the measures of depth illustrated earlier to capture the importance of financial markets for the real economy. Depth, or size, is really an *outcome* measure, meaning that whatever it is that is done or could be done to improve the financial industry is then reflected in its relative size, which is what is observed. Yet by focusing on this end-result variable, we are at least one step removed from addressing causality in that we do not directly investigate *how* banks or other parts of the financial industry generate an independent impact on real economic activity. This leaves unanswered the above mentioned criticisms that financial markets evolve hand in hand with other economic variables and that those are the ones actually responsible for real sector growth. What is more, by maintaining the focus on the depth variables, the analysis is also much constrained in terms of quality of its normative content: if it is probably the case that going from the bottom to the top quartile in the banking size distribution is associated with considerably higher income growth, these studies are not able to prescribe exactly how deepening can be achieved.

3. Banks matter

Both the issue of causality and of normative content were directly addressed in another highly influential study, Jayaratne and Strahan (1996), which represented another significant leap forward in the quest for the ultimate word on the role of banking for the real sector. The authors narrowed the focus of analysis down from a cross-country perspective to a country-specific case study, that of the United States. While the choice may seem deficient with respect to the broader cross-country variability of previous studies, it actually comes with a tremendous pay-off for a study on the role of banking: as a result of decades of regulatory restrictions preventing or limiting bank expansion within and/or across states, up to the mid-1970s the United States featured what was in effect fifty separate banking markets (Morgan, Rime and Strahan, 2004), with state lines demarcating the boundaries of each individual market. Hence, studies limited to just U.S. banking still allows substantial cross-sectional variation. At the same time, the narrower focus on one country also reduces potentially important sources of unobservable heterogeneity that are more likely to plague multi-country data. Moreover, and most importantly, the end of the 1970's marks the beginning of an intense process of deregulation, in which individual states – *at different points in time* – removed regulatory barriers that had prevented bank entry. By the mid 1990s the process had concluded, allowing banks originally headquartered anywhere from that point on to expand potentially anywhere else without restriction.

As a result of this process of deregulation, banking markets have become increasingly more competitive and efficient. This should in turn translate into more credit availability, hence a clear and direct effect on the real economy. This is exactly what Jayaratne and Strahan (1996) tests. The simultaneous existence of cross-sectional and over-time variation concerning individual states' timing of deregulation represents a unique opportunity to conduct analysis in conditions that approximate those of a "natural experiment," a scenario notoriously hard to achieve in social science inquiry. More precisely, it is possible to measure the impact of bank deregulation – and the associated changes in competition and efficiency - comparing state-specific real economic variables *before and after* deregulation. In the language of natural experiment analysis, the control group is represented by observations in a state before deregulation, including observations in other states that have not deregulated yet, while the treatment group is represented by all observations in years following deregulation. Because deregulation is not implemented at the same time in all states, unobserved state-specific omitted factors and time-specific events common across states that could explain the dependent variable can be absorbed by state and time indicator variables, still leaving sufficient variation to identify the specific effect of deregulation.

This identification strategy makes an important step forward in dealing with causality for at least two reasons: first, it does not capture developments in the financial sector, banking in particular, by looking at an ex-post outcome performance such as credit size, capturing instead the effect of a specific event, bank deregulation, that is supposed to generate developments in the sector. And because theory would suggest that the resulting improvement in competition and efficiency should be associated with more and better allocated capital, the causal link is now much more direct. Second, the event in question can be plausibly considered to be exogenous and occurring independently of current or expected developments in the real economy. For example, studies have indicated that small banks had been very influential in establishing tight restrictions to expansion as early as the 1930s and that their influence remained strong through the early 1980s (Economides, Hubbard and Palia, 1996, White, 1998). Also, the extensive failure of thrifts in the 1980s has been considered another cause of deregulation, as large, better-diversified banks were allowed to acquire the failing banks (Kane, 1996). Finally, Kroszner and Strahan (1999) find that technological changes in both deposit and lending activity were among the leading factors behind deregulation.

With these premises, Jayaratne and Strahan (1996) were able to find evidence confirming a causal link between banking deregulation and state income growth. In particular, using a panel from 1972 to 1992, they find that income growth in a state was more than half a percentage point higher, per year, after deregulation of its banking industry. Their contribution should be recognized both methodologically, for the important tightening in the strategy to address causality, but also because it focused on a specific characteristic of the banking industry, thereby bringing the data closer to theory and at the same time enhancing the normative content of the analysis. The evidence presented in this paper represents, in my opinion, the closest to a nail in the coffin of the causality debate. After this paper, it has become very difficult to counter Schumpeter's assertion that “bank credit does create value,” or at the very least the burden of proof has shifted squarely to the other side of the debate.[2]

4. *How* do banks matter?

From this point on, the research frontier advances forward. No longer is it necessary to expend effort making the point that banks are important for the real economy. Taking that as a given, scholars can now focus on the perhaps richer and more satisfying quest of fully understanding the *mechanisms* through which banks can affect the real economy. The operative questions at stake are what specific characteristics of banks and of the banking industry are likely to matter the most for real output variables, such as income or productivity growth? And similarly, from the other end, what specific elements or features of the real sectors of the economy are really affected by banks activity, so that ultimately such activity is reflected in an impact on real output? Delving deeper into the *micro* details governing the banking-real economy relationship, it is now possible to really put to the test specific theories of banking. Moreover, and as mentioned earlier, the normative value of the newest studies of banking and the real economy increases tangibly. As economists fine tune what works the most, policy makers are increasingly able to navigate the sometimes turbulent waters of banking regulatory activity.

4.1. The role of bank competition

[2] A parallel paper that should also be considered as a turning point, although focusing on the broader relationship between overall financial development and economic growth, is Rajan and Zingales (1998). Their contribution is described in detail in the previous chapter.

In the decade following Jayaratne and Strahan (1996), research work in this field has evolved in multiple directions, in which emphasis was directed at the explicit features and characteristics of the banking industry. A relatively large amount of work, in particular, has been - and continues to be - dedicated to studying the role played by bank *competition* for the real economy. The reasons for the attention paid to this characteristic of the industry are twofold. First, in contrast to most industries, where the default is that market structure and competitive conduct evolve endogenously, the banking industry has historically been heavily regulated, and for the most varying reasons, both in the U.S. and in other countries. Hence it is plausible to make the case that this is an exogenously determined characteristic of the industry when studying the impact on the real economy. Second, and not less important, there is a fascinating contrast of theoretical conjectures that can be formulated about the effects of bank competition. Petersen and Rajan (1995) expressed very clearly the essence of this contrast. The authors challenged the conventional view that enhancing bank competition necessarily leads to better loan terms and better access to credit. The theoretical argument is that in fact banks need at least some degree of market power to have the right incentives to undertake the proper investments in screening and monitoring necessary to resolve uncertainty about the quality of new entrepreneurs. The intuition is that in the absence of some ability to "capture" the client firm over time, a bank anticipates that an entrepreneur that turns out to be successful has the possibility in future periods to seek better terms from competing banks that would not need to incur any additional cost of screening and monitoring (or would spend just a fraction of what the original bank had to). Hence, in a highly competitive banking environment, banks would be required to charge loan terms reflecting the high intrinsic risk of the entrepreneurs. A bank with market power could instead offer better initial terms knowing that any upfront cost in starting such a lending relationship could be recuperated at later stages. The unconventional prediction that follows is that firms, especially young ones, might have better access to credit if they operate in more concentrated banking markets.[3] Petersen and Rajan (1995) test this prediction using U.S. data for more than 3,000 respondent firms to the 1988 National Survey of Small Business Finance, matched by geographic location using FDIC Summary of Deposits information on the location of bank offices. The bank-specific data allowed them to construct measures of local market concentration, which they used as a proxy for market power.

As they specified in the paper, there can be multiple sources of market power, but local market concentration seeks to capture market power derived from spatial location. The idea is that firms, especially the small ones they focus on, are bound to have very local relationships with a bank and therefore their proximity to banking centers, and the related density of such centers in the firm's location, give an idea of how "captive" the firm can be (Petersen and Rajan, 1995, p. 417-418). The authors find that young firms operating in markets with high bank concentration are more likely to access credit. They also find that loan terms are more advantageous (lower lending rates) than in less concentrated markets. This term differential disappears and in fact reverses as firms become more mature.

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[3] The basic ideas behind the Petersen and Rajan contribution were already present in Schumpeter (1911) and formulated in Mayer (1988). For some additional theoretical work suggesting positive effects associated with banking market power, see Shaffer [26], Cao and Shi (2000), Dell'Ariccia [10], Manove, Padilla and Pagano [20] and Cetorelli and Peretto [8]. For more conventional viewpoints, see, e.g., Pagano [22] and Guzman [12].

Another paper to focus on the role of bank competition for the real economy is Cetorelli and Gambera (2001). This paper explored the empirical relevance of the market structure of the banking sector for industrial growth. The authors took the basic cross-sectional work initiated by King and Levine (1993) and then asked: if it is agreed that the size of the banking industry is important to capital accumulation, does it matter whether the underlying industry structure is unconcentrated, thus approximating perfectly competitive conditions, or whether instead market power is concentrated among few banking institutions? From a theoretical standpoint, Cetorelli and Gambera played with the same antagonism of conjectures presented in Petersen and Rajan (1995). Their methodological approach built on the contribution of Rajan and Zingales (1998), in that they used a cross-country dataset but looked at the differential impact of bank concentration in a country *across* industrial sectors that for their own idiosyncratic reasons display varying degree of dependence on external sources of finance for capital investment. The identification strategy is then based upon the intuition that if bank competition has a role, it should matter more for firms in sectors that are highly dependent on external finance availability. As in Rajan and Zingales (1998), by seeking such a differential effect, the identification strategy raises considerably the bar for potential objections on ground of endogeneity, omitted variable biases and reverse causality. The findings suggest a non-trivial impact of bank concentration on industrial growth and in fact simultaneous support for both sides of the theoretical controversy. First, there is evidence that bank concentration has a first-order negative effect on growth. This finding is consistent with the theoretical prediction that higher bank concentration results in a lower amount of credit available in the economy *as a whole*.

Regardless of their external financial dependence, this effect is common to all industrial sectors. However, the paper also finds evidence that bank concentration has a *heterogeneous* effect across industries. In particular, sectors where young firms are more dependent on external finance enjoy a beneficial effect from a concentrated banking sector, which could actually more than compensate the first-order, negative effect. This finding supports the basic argument in Petersen and Rajan (1995) predicting that concentration of market power in banking facilitates the development of lending relationships, which have in turn an enhancing effect on firms' growth.

4.2. Banks and industry dynamics in product markets

As the research agenda on bank competition and the real economy picked up momentum, it also became more ambitious. Much current work has gone, for example, into the understanding of how bank competition can actually affect the life-cycle dynamics of industrial sectors of production. For instance, does more bank competition mean more entry in non-financial industries? And what is the related impact for *incumbent* firms? Would changes in bank competition lead to *structural* changes in other industries, such as affecting average firm size, or the whole firm size distribution?

The role of bank competition on these characteristics of non-financial industries had not really been explored before, at least in the mainstream economic literature. Scattered evidence is found in the work of history scholars. For example, in his study of Italian industrialization in the late nineteenth century, Cohen (1967) describes how a quasi-monopolistic banking industry "...led to the emergence of concentration of ownership and control in the new and rapidly growing sectors of the industrial structure."

Capie and Rodrik-Bali (1982) note that the intense process of consolidation and increase in concentration that characterized British banking in the early 1890's preceded that experienced later on by manufacturing industrial sectors. Similarly, Haber (1991) reports over a century of Mexican history, between 1830 and 1930, a very close connection between bank and industry concentration. The general impression from historical studies that bank concentration should be associated with concentrated industries is finally expressed by Cameron (1967) in his renowned study on banking in the early stages of industrialization, where he states that "...Competition in banking is related to the question of competition in industry. In general the two flourish – and decline – together. Whether this phenomenon is a joint by-product of other circumstances, or whether it results from the decline or restriction of competition among banks, is a matter worthy of further research. It is a striking coincidence, in any case, that industrial structure – competitive, oligopolistic, or monopolistic – tends to mirror financial structure." At the same time, while important as analyses of countries' economic development, the empirical evidence presented in these studies is limited by their focus on specific countries, periods and socio-institutional circumstances. Lacking in the literature had been broader empirical analyses apt to make general assertions about the role of banking market structure on industries' market structure (Cetorelli, 2004, p. …).

A lack of systematic empirical evidence on this relationship is also accompanied by scattered formal theoretical modeling to guide the implementation of empirical identification strategies. Nevertheless, we can delve into the existing literature on the economic role of banking market structure to formulate alternative theoretical conjectures. To this end, the framework proposed by Petersen and Rajan (1995) described above represents a good foundation from which to ponder the role of bank concentration on industry concentration. In their reasoning, banks with market power fund young firms with the expectation that they will be capable of extracting future rents once those firms eventually become profitable. Consequently, one could then argue that banks with market power, following their goal of profit maximization, should always attempt to select the best available pool of entrepreneurs, thus favoring new entrants along the entire life cycle of an industry. This is because new entrants are potentially endowed with higher return projects and more innovative technologies that would guarantee ever increasing profit-sharing opportunities for the banks. Thus, according to this logic, bank concentration should continuously foster entry and therefore contribute to enhance industry competition. This is a testable hypothesis. Yet, maintaining the same premises in the Petersen and Rajan model, it is also legitimate to envision a completely different set of economic forces at play that could lead to the opposite conclusion. Consider, for example, a nascent industry where a bank with market power may indeed facilitate credit access to young firms. Once lending relationships are established, however, at later stages the bank may have an incentive to preserve its ties with the older clients - now industry incumbents - and constrain access to credit to newer entrants. The argument is that by increasing market competition, the newcomers would undermine the market power and therefore the profitability of industry incumbents and consequently the profitability of the bank itself. In essence, this argument follows from the recognition that market power gives banks an implicit equity stake in the firms they are already financing, thus potentially distorting their incentive to extend credit in product markets. This theoretical argument would then suggest that bank concentration should enhance industry concentration. Judging by the formulation of these alternative conjectures, the effect of bank concentration on industry market structure is therefore theoretically ambiguous.

These alternative conjectures have been brought to the data. A first example is represented by Black and Strahan (2002). Regulatory action that removes entry barriers and leads to a more efficient and competitive banking industry should be reflected in a direct effect on entrepreneurship. More precisely, the entry rates of business entrepreneurships should be higher following the kind of deregulation illustrated above. Using data on new business incorporations between 1976 and 1996, Black and Strahan compare the number of new incorporations, and the growth rate of new incorporations before and after deregulation, using the same identification methodology in Jayaratne and Strahan (1996). The authors find that after deregulation that first allowed banks to freely branch within a state, the number of new businesses increased by almost 10 percent, and its growth rate between 3 and 4 percent. Subsequent to deregulation that also allowed banks to expand across state lines, the number of new businesses increased further by about 6 percent, while there was no significant effect on the growth rate. The economic impact on new business creation of opening up banking markets is therefore very large. Moreover, allowing more businesses to be in operation may be reflected in increasing output accumulation. Hence, analyzing the relationship between banking deregulation and business formation puts substance in the original goal of understanding the mechanism through which banking activity can affect real economic activity and ultimately long-term economic growth.

At the same time, going back to the theoretical conjectures outlined earlier on, while it is certainly consistent with theory that bank competition enhances business entry, it may not be inconsistent to speculate that banks may also have, as noted earlier, diminished incentives to screen and monitor entrepreneurs. Consequently, more entry may also be associated with higher mortality of young firms. A first take on this issue was presented in Cetorelli (2003). The author used a public version of the U.S. Bureau of the Census dataset on business establishments that contains information on age categories to test – among other things - the impact of banking deregulation on entry but also on the persistence rates of young businesses, as measured by rates of job destruction. The evidence was consistent with that presented by Black and Strahan (2002) on entry. Moreover, it suggested that the persistence of younger businesses was actually higher after deregulation (hence, lower job destruction). In contrast, using the more detailed, confidential version of the Census data, Kerr and Nanda (2007) find that the failure rates (complete business shutdown) of enterprises 3 years and younger are actually higher after deregulation. Since the authors also find evidence consistent with more entry, they interpret the combination of results as suggesting that improvements in bank competition favor a "democratization" of the entry process. That is, potential entrepreneurs have a greater chance to start a business but they do not necessarily have a greater chance of surviving and remaining in business.

More work has been conducted related to the effects of bank competition on life-cycle dynamics. In the above mentioned paper, Cetorelli (2003) also looks at the growth rates of incumbent enterprises and at their own persistence rates. The evidence there suggests that *less* bank competition meant *delayed exit* of incumbent firms, a finding consistent with the idea exposed before that monopolistic banks may have distorted incentives in favoring their older clients, thus preventing a healthier process of creative destruction.

Still with a focus on life-cycle dynamics, Cetorelli and Strahan (2006) attempted a broad analysis of the impact of more bank competition on a complete set of metrics of the market structure of non-financial industries. The authors use U.S. Census data between 1977 and 1994 on the number of business establishments and their size – measured by employment level – located in the different states and operating in any of the 20, 2-digit SIC manufacturing sectors. Consistent with previous findings they show evidence that more vigorous competition leads to more firms in operation. In addition, they also find that the average firm size decreases as banks become more competitive. Lower average firm size is consistent with the finding of more firms in operation, and both add strength to the idea that more bank competition favors entry and allows entry at a smaller scale.

Finally, they find that the whole firm size distribution is shifted, with an increase in mass toward smaller size firms. The additional evidence on the size distribution adds content to the conjecture that the impact of improved competition may be felt differently by different firms – young or old, small or large – and in different sectors. More firms in operation and smaller average size could reflect entry by very small establishments. If that were simply the case, one would expect an increase in mass at the smallest end of the size distribution and declines in mass elsewhere in the distribution. If better bank competition also helps the existing small firms grow (due to an overall increased supply of financial resources), then we ought to see a greater proportion not only of the smallest but also of midsized establishments as well. Moreover, testing for shifts in the whole size distribution allows us to compare how the shares of small - and mid-sized (presumably bank dependent) establishments behave relative to another sort of control group, namely, the share of the very largest establishments. These establishments (those with 1,000 or more employees) should not be affected by banking conditions because very large firms have access to nationwide (and competitive) securities markets. Thus, their fortunes should not vary with local credit conditions (Cetorelli and Strahan, 2006, p. 455).

Finally, and still on the relationship of bank competition to industry life-cycle, Cetorelli (2008) investigates whether the process of banking deregulation could be so important as to render firms after deregulation intrinsically different from those that started their operations prior to deregulation. This idea, commonly referred to in corporate demography as “imprinting” effect, posits that in a time when external capital was relatively harder to obtain, prospective firms needed a set of organizational and managerial characteristics that would increase their chance not only to obtain (scarcer) financing, but also to survive in the event of constrains to obtaining additional credit. Conversely, after deregulation, and the removal of important frictions to credit supply, new firms may not need to develop that set of characteristics that were previously requierd, thus resulting in a group of intrinsically more fragile units. Cetorelli (2008) finds preliminary evidence consistent with this conjecture. Firms born prior to deregulation seem to have a “thicker skin” than firms with similar characteristics born after deregulation.

Analyzing the role of bank competition for the real economy has thus proven to be a quite rich investment. One last contribution that followed a similar thread, but with a significantly different focus is Morgan, Rime and Strahan (2004), which tackled the very relevant issue of the impact of bank deregulation for macroeconomic stability. The issue is potentially debatable. On the one hand, the opening up of markets should have a beneficial effect on market stability as it enhances opportunities for risk-sharing across markets. On the other hand, as mentioned earlier, banks in a more competitive environment may be less apt to efficiently allocate credit. Also, as suggested by Keeley (1990), increased competition may have a perverse effect on banks' risk taking behavior because competition reduces banks' franchise value. The authors inquire about this issue and test the response of state-specific measures of business cycle volatility to the deregulation events. They find that volatility drops substantially, between 30 and 40 percent, after deregulation.

The material reviewed in this chapter has been developed using datasets drawn almost exclusively from U.S. banks and U.S. sectors of production. A legitimate criticism could be made about whether these conclusions have broader, international validity. Recent work based on specific country studies or cross-sections of non-U.S. countries seems to confirm the findings examined so far. A very ingenious paper drawing extensively on the comparison of many decades of economic history is Haber (1991). The author essentially presents a horse race between Mexico, Brazil and the United States starting from the earliest stages of industrialization and then following the evolution of industrial structure in response to developments in capital markets. Haber focuses his attention on the evolution of the textile industry between approximately the 1840s and the 1930s. The reason to focus on such an industry is that, aside from the fact that it had a much higher relevance in the past, it also had specific characteristics (including low entry barriers, divisibility of capital and scale efficiencies exhausted at small size) such that the industry was not naturally prone to encourage concentration. The only substantial barrier to entry was the ability to access external capital. Focusing on such industry represents a good case study to understand the finance-real economy relationship because any change in the financial sector that may have occurred in the three countries could convincingly be argued not to be endogenously determined by events in the textile industry.

Haber documents very persuasively the relationship between efficiency, regulatory reforms – or lack thereof – in the financial sector and the structure of the textile industry. Specifically, he shows that the U.S. adopted important reforms of both the banking industry and capital markets early on. In particular, the National Banking Act of 1863 produced important entry in the banking industry that facilitated access to credit in the production sector. As Haber remarks, "by the end of World War I the textile industry was awash in finance and many companies took advantage of the swollen credit markets to float numerous securities issue" (Haber, 1991, p. 564).

The experience of Mexico was just the opposite. The textile industries, and production in general had no access to either bank finance or capital markets throughout a large part of the nineteenth century. When banks eventually emerged, they actually developed as institutions tightly connected to a limited number of entrepreneurs, who were themselves connected to government officials. Hence, external finance remained severely limited and differentially available in the market. The result was a textile industry that evolved as highly concentrated. Brazil had a very similar experience and developmental trajectory as Mexico at least initially. Financial markets were extremely underdeveloped, but in 1890 important reforms sowed the seeds for significant expansions of the banking sector and capital markets. As a result, the structure of the textile industry in Brazil in the first decades of the twentieth century looked a lot more like that of the United States than that of Mexico.

Another example of a non-U.S. country study is the experience of France in response to the reform of the banking industry of 1985, analyzed by Bertrand, Schoar and Thesmar (2007). The French reform reduced significantly government intervention in banks' lending activity. This reform lead naturally to a boost in efficiency in private banks and enhanced competition in the credit market. The reforms to the banking industry, the authors document, generated important changes in the microeconomic behavior of firms and had a related strong impact on the structure of industries. In relation to the topics analyzed in this chapter, the authors show that the regulatory reform of the banking sector in France had a positive effect on firm entry and exit rates and a negative effect on product market concentration.

Still on baking reforms, but with a focus on Europe, Cetorelli (2004) investigated the effect of the implementation of the Second Banking Directive of the European Union – a regulatory reform that essentially created the conditions for an integrated European banking market – on industrial structure. Cetorelli used a panel of manufacturing industries in 29 OECD countries, both EU and non-EU members. The evidence showed that enhanced competition in EU banking markets lead to markets in non-financial sectors characterized by lower average firm size. This conclusion is consistent with the findings in Cetorelli and Strahan (2004) and points at a beneficial effect of bank competition on credit access to young firms.

Finally, Beck, Demirguc-Kunt, and Maksimovic (2004) use data from a World Bank survey of firms in a large cross-section of countries. One of the questions the survey asked was whether firms had difficulty in obtaining credit. Matching this and other information with the specific market structure of the banking sector where those firms were located, the authors found that higher bank concentration was associated with more financing obstacles, especially for smaller firms.

Two important points can be made regarding the role of bank competition in enhancing real economic activity. First, there does not seem to be a Pareto-dominant policy regarding the optimal banking market structure: competition in banking does not necessarily dominate monopoly, and vice versa. Second, regulation of the financial industry is intimately related to industrial policy. Depending on the level of concentration of the banking industry, ceteris paribus, individual sectors will grow at different speeds. Therefore, banking market structure plays an important role in shaping the cross-industry size distribution within a country.

4.3. Importance of ownership structure

As mentioned earlier, by delving deeper into the mechanics of the banking-real economy relationship, scholars have been able to broaden significantly the policy implications associated with their findings. In addition to the numerous, separate threads of research related to bank competition, a good deal of emphasis has also been placed on the characteristics of banks' ownership structure and on the ownership connections between banks and firms. A well-established view in corporate finance stresses the value of close ties between banks and borrowers. Bankers that are represented on the borrower's board of directors and are able to follow closely the day-to-day operations of the borrowers are also able to perform careful assessments of the firm's investment strategies, growth opportunities and overall risk exposure. Access to such information allows the bank to make efficient credit allocation decisions. An alternative view stresses the possibility that related lending may be subject to important conflicts of interests, such that a bank may have incentives to allocate credit to a firm with which it shares close relations not necessarily on the basis of standard risk-return considerations.

Empirical evidence has offered support for both views. Gorton and Schmid (2000) found a positive effect on performance for German firms with close bank ties. A positive effect on liquidity needs and resolution of financial distress is found for Japanese firms (Hoshi, Kashyap and Stein, 1990). However, a negative effect on firm performance, firm growth and cost of capital is found on Japanese firms by Weinstein and Yafeh (1998). A negative effect on default rates is found for Mexican firms by La Porta et al (2003) and a negative effect on growth and on capital allocation is found by Maurer and Haber (2004) using nineteenth century records for Mexican firms. Finally, Krozner and Strahan (2001) find that US firms with close bank ties are not treated differently from similar firms without bank ties.

5. Conclusions

It is not by chance that Lucius Caecilius Jucundus established himself and prospered in the city of Pompeii. Pompeii was a well-developed center with close proximity to the sea and where markets were held on a regular basis. It is well recognized that bankers like him were instrumental in facilitating and developing commercial activity. And while the contours of entrepreneurship in ancient Rome may not fit a modern profile, it is clear that

Jucundus and others still played a role in assisting productive activity (Andreau, 1999, p. 145-152).

The tale of Lucius Caecilius Jucundus serves as a good example to illustrate the crux of the debate around the role of banks for the real economy. It is certainly the case that banks – and financial activity - follow where real activity goes. The direction of causality from economics to finance, in other words, has never been seriously questioned. Much harder to prove is that banking can develop independently of what goes on in the real economy and that developments in the banking industry can in fact alter economic activity.

The impulse to map these dynamics has inspired a lively body of literature, one that reflects both the intrinsic intellectual interest on the issues at stake but also their vast policy implications. After all, the pervasive nature of policy control of the banking industry rests on certain assumptions about banks fundamental role in the real economy. After more than a decade of rigorous research we are probably now in a position to assert with a significant degree of confidence that banking does matter for real economic activity. We have not only learned that banking activity has a large impact on various measures of output growth but have also made important progress in understanding exactly how that happens. Developments in the way banks operate, as reflected, for example, in their competitive conduct or on specific ownership structures, bring with them far-reaching implications for economic activity. Pushing research further in this direction is expected to continue to yield significant results with no diminishing returns in sight yet.

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