

Credit market competition and the nature of firms¹

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Abstract

Empirical studies show that competition in credit markets has important effects on the entry and growth of firms in nonfinancial industries. This paper explores the hypothesis that the availability of credit at the time of a firm's founding has a profound effect on that firm's nature. I conjecture that in times when financial capital is difficult to obtain, firms will need to be built as relatively solid organizations. However, in an environment of easily available financial capital, firms can be constituted with an intrinsically weaker structure. To test this conjecture, I use confidential data from the U.S. Census Bureau on the entire universe of business establishments in existence over a thirty-year period; I follow the life cycles of those same establishments through a period of regulatory reform during which U.S. states were allowed to remove barriers to entry in the banking industry, a development that resulted in significantly improved credit competition. The evidence confirms my conjecture. Firms constituted in post-reform years are intrinsically frailer than those founded in a more financially constrained environment, while firms of pre-reform vintage do not seem to adapt their nature to an easier credit environment. Credit market competition does lead to more entry and growth of firms, but also to complex dynamics experienced by the population of business organizations.

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Introduction

Credit market competition is an important determinant of life-cycle dynamics in non-financial industries. A number of empirical studies have documented that more competition in credit markets means more firm entry, higher growth, and small-size organizations dominating the overall size distribution.²

In this paper, I suggest that the effects of credit conditions on firms are more profound than previously recognized. Specifically, I entertain the hypothesis that the conditions of availability of financial capital *at the time a firm is founded* leave an indelible mark on that firm's nature. This "genetic" mark, in turn, leads to complex population dynamics producing ever deeper effects on firms' life-cycle.

Organizational studies have long provided support for the idea that the core characteristics of a firm and therefore what constitutes its very nature are heavily determined by environmental conditions at the time of founding (e.g., Stinchcombe, 1965, Boeker, 1988, 1989, Carroll and Hannan, 2000). The conditions under which financial capital is available presumably constitute an important component of the environment. Yet we have no analysis of this relationship. In this paper I address this issue, exploring the following conjecture: if credit markets are non-competitive – and therefore external funding is relatively difficult to obtain - bidding successfully for this scarce input and/or evolving in such a way as to minimize reliance on it requires that organizations be constituted, all else equal, with solid business models. Conversely, in an environment where external finance is plenty, entrepreneurs may choose to establish new ventures with inferior organizational structures knowing that the costs of folding and perhaps starting anew are lower. In fact, more firms may be constituted by agents that in a tougher environment would have actually chosen not to undertake entrepreneurial activity at all. Consequently, firms born under credit-rich environmental conditions are likely to be innately more vulnerable to adverse shocks and to potential exit throughout their entire life-time. At the same time, firms born in times of constrained finance may remain true to their nature in a changing credit environment, or they may instead adapt into weaker organizations.

² See, e.g., Jayaratne and Strahan (1994), Beck, Demirguc-Kunt and Maksimovic (2004), Bertrand, Schoar and Thesmar (2007), Black and Strahan (2002), Cetorelli and Gambera (2001), Cetorelli (2004), Cetorelli and Strahan (2006).

To test the hypothesis that firms are shaped at birth by existing credit market conditions, I use confidential data of the Bureau of the Census on the entire universe of U.S. business establishments in existence from 1975 to 2005. I have matched this data with information on the process of reform of the banking industry that led U.S. states, at different point in time between the 1970s and the 1990s, to remove significant regulatory barriers to bank entry. As shown in previous studies, this process of deregulation is a very strong instrument capturing significant changes in credit market competition. The analysis suggests natal conditions strongly affect firms' nature and that this imprinting effect is long-lasting. Firms born in years after the reform are intrinsically frailer than firms born in the pre-reform environment, as evidenced by a consistently higher hazard of mortality throughout their life span. Firms of pre-reform vintages, founded in times of more constrained credit, do not seem to adapt to weaker standards in post-reform years, benefiting instead from an environment that makes financial capital more available.

This basic finding is robust to a wide array of alternative specifications, where I have controlled for state, industry and year common effects; added firm-specific, time varying characteristics or factors capturing state-specific cyclicalities and within-state, industry-specific cyclicalities. Likewise, the result is robust to estimation with alternative frailty models to further account for unobservable heterogeneity, and to focusing only on single-establishment firms. Finally, while I use a specific parametric hazard model, the results do not depend on it, and the basic differences in mortality patterns are also found in the raw data. Moreover, the results are also stable to small variations backward or forward in the exact timing of deregulation and they tolerate the existence of potential survival bias in the data.

This paper makes two contributions. First, I provide new empirical evidence on the effects of credit market competition on firms' life-cycle dynamics, underscoring a heterogeneous impact depending on firms' vintages and consequently suggesting a more complex impact than suggested in previous studies. Second, and more broadly, this paper proposes a methodological approach that allows for populations of economic agents (firms in this case) to *evolve* over time in response to environmental "shocks." The impact of credit market reform cannot be fully understood without charting the dynamic interplay between environmental conditions and populations. Existing firms can retain

their original nature or they can adapt to a changing environment; new firms can be born with a different “genetic pool” that reflects the new environment; or we can witness a combination of both (or none of the above!). The overall effect of the environmental change on population dynamics will be markedly different depending on which scenario is more likely to play out. My use of the language of evolutionary ecology is strategic, and serves to disaggregate dynamics that would otherwise be lumped together.

The conjecture I explore in this paper is very much consistent with recent contributions to the theory of firms, such as Rajan and Zingales (2001). The authors maintain that the changes experienced by financial markets in recent decades – of which the process of bank deregulation was an integral component – are tantamount to a true “financial revolution”. They posit that such environmental changes have a profound impact on the nature of firms. While sharing the notion that financial market conditions affect firms’ nature, Rajan and Zingales (2001) imply that such changes affect all firms, new and old, in a similar fashion. This paper, in contrast, argues for a differential impact between firms born before and those born after the change in the environment. Related to this contribution, Zingales (1998) studies the evolution of firms in the trucking industry in the years after an important piece of deregulation of the industry. He shows that firms with the best chances of survival in the new environment were those that were more efficient but also those that had ex ante stronger financial fundamentals (lower leverage). His work differs from mine in that his focus is on the post-deregulation impact on firms in existence in years prior to the deregulation, but he does not (indeed could not, given the nature of its data) focus on the nature of such firms as determined by imprinting characteristics at founding. Moreover, he does not address the potential effect of the changing environment on the nature of the firms born after deregulation.³ This work also relates to Schoar (2007), which investigates whether managerial style is affected by the economic conditions encountered by managers at the beginning of their career. She finds that to be the case and that such conditions affect managers’ career path throughout their entire life span.

³ In addition, in his study, the reform takes place within the industry of interest, while in my case the focus is on how a reform of the financial industry affects non-financial industries.

More broadly, this paper directly contributes to research on finance and the real economy. As mentioned above, much has been written about the effects of credit market competition on the real economy, but the basic limitation of previous studies is in the unavailability of a true micro-level database, which naturally leads to focus on more aggregate variables.⁴ The paper closest to this one is Kerr and Nanda (forthcoming), which uses the same data source. The authors highlight a “churning” effect associated with credit reform: the rate of exit of young (up to 3 year old) business organizations is higher after the reform. However, the authors limit their analysis to exit rates for this particular age cluster in the distribution and are not interested in the broader life-cycle issues that are central to my study.

Finally, this paper draws on the theoretical literature on firm dynamics (e.g., Jovanovic, 1982, Hopenhayn, 1992, Albuquerque and Hopenhayn, 2004, Clementi and Hopenhayn, 2006). This thread has probed the development of theoretical mechanisms to endogenously derive life cycle dynamics closely matching empirical observation, such as higher growth, higher growth volatility, and higher mortality during younger ages, larger average size at later stages, etc. For instance, in Jovanovic (1982)’s well-known model of firm selection, entrepreneurs learn with time whether they are sufficiently skilled in production, and as time goes by the weaker ones will choose to exit while the stronger ones thrive and stay in business. Jovanovic’s working assumption is that entrepreneurs are drawn from a fixed distribution of quality. My conjecture instead would imply that the distribution itself depends on the characteristics of the credit market, and that in a regime where credit is more easily available the whole distribution shifts, increasing mass toward the left tail.

Environmental imprinting

The conjecture I develop in this paper implies that firms’ nature is shaped by environmental conditions at founding. Such natal imprinting implies that there is an important component of inertia in the nature of an organization. These concepts and assumptions are widely applied in the field of organizational studies and in fact constitute

⁴ Certainly this is not the first economic study to use a micro-level panel of firms. The analysis conducted here is informed by prior contributions on firms’ population dynamics, such as – for instance – Caves (1998), Evans (1987a, 1987b), Dunne, Roberts and Samuelson (1988).

the basis of the field known as organizational ecology (Hannan and Freeman, 1977, 1984, Carroll, 1984, Hannan, 2005). This field argues that evolution in populations of organizations is propelled more through the creation of new firms of a different nature, more attuned to new conditions, than through continuous adaptations of existing ones. In fact, the theory argues that long-time survival *requires* structural inertia: organizations are successful if they are perceived as reliable and predictable, which leads to organizational forms that are resistant to change (Hannan and Freeman, 1984).⁵

The notion of environmental imprinting has been extensively documented empirically. In his seminal piece, Stinchcombe (1965) examined the organizational structure of U.S. industries from the time they were originally developed. He found that some distinctive traits recognizable in current times were directly traceable to the eras when they were formed. For instance, sectors developed prior to industrialization were still in the 1960s disproportionately populated by firms extensively employing unpaid family workers. Similarly, industries that developed after the “bureaucratization” era – involving written and filed communication in factory administration and the differentiation of managerial roles from family institutions – still employed many decades later a higher fraction of administrative workers than those formed in earlier periods. More recently, Jovanovic (2001) documented evidence of natal imprinting showing the existence of a positive correlation between firms’ current market valuation and their year of birth.⁶ This paper builds on these findings analyzing how natal imprinting operates in specific environmental circumstances, in this case related to credit, and how it affects population dynamics.

The credit reform

What is the specific nature of the “environmental change” invoked here? The impact of the deregulation of the U.S. banking industry initiated in the 1970s is difficult to overemphasize. Far from a run-of-the-mill new law, this reform represented the overhaul of a regulatory environment in effect since the 19th century. Prior to the reform, most

⁵ Arrow (1974) makes a similar argument, stating that “... the very pursuit of efficiency [in organizations] might lead to rigidity and unresponsiveness to further change.”

⁶ Interestingly, the notion of natal imprinting has been successfully applied to the wine industry, with a number of economic models predicting current wine prices based on vintage-specific environmental conditions (see, e.g., the same Jovanovic, 2001, but also Ashenfelter, 2008).

states effectively prohibited bank branching within a state (unit banking states) or imposed significant limitations to branching. At the same time, banks were prohibited from acquiring banks outside the state in which they were headquartered.⁷ But over the following twenty years, a deregulatory revolution ensued. At different points in time individual states removed branching restrictions and interstate barriers to entry. By mid-to-late 1990s, state boundaries were eliminated, effectively allowing banks headquartered anywhere to expand anywhere else. This deregulation process was therefore of “historical” proportions, and it was perceived as being irreversible as well. Hence, the reform captured a significant regime change.⁸

Since deregulation did not take place simultaneously in all states, it has allowed for quasi “natural experiment” conditions, whereby one can test the impact of changes in credit market conditions on variables of interest while still controlling for unobservable common factors. Many papers have adopted this approach and shown that the U.S. banking deregulation process has been a robust, exogenous instrument capturing the effect on real economic activity.⁹

Identification

Methodologically, how do we track the changing nature of firms? The paper makes inferences about the changing nature of firms but as mentioned in introduction, the empirical analysis is based on the estimation and comparison of hazard functions, and not on the analysis of changes in specific firm characteristics. For example, I cannot associate the credit reform with firms selecting different capital structure, labor intensity, human capital composition, etc. This is partly due to the data I have chosen to utilize. By virtue of its comprehensive nature, the dataset does not contain much economic information on each record. However, it also reflects the view that the effect of the changing

⁷ See, e.g., Amel (1993) for a complete overview of the state laws affecting the geography of banking in the United States prior to the reform.

⁸ The resilience of banking regulatory environments and their ability to shape business characteristics is not specific to the United States. Guiso, Sapienza and Zingales (2005), for example, make a convincing case that the business environment in Italy in the early 1980’s was very much shaped by the conditions of the banking industry in existence at the time of the passage of the Italian Banking Law of 1936.

⁹ The first paper that implemented this approach was Jayratne and Strahan (1996). See also, e.g., Black and Strahan (2002), Morgan, Rime and Strahan (2004), Cetorelli and Strahan (2006). For more details on the origin of the reform, see Kroszner and Strahan (1999) and for a discussion of endogeneity issues, Cetorelli (2009).

environment on firms' nature should be studied first at an even deeper level by asking whether it ultimately leads or not to different prospects for firms' life and death. Answering this question seems of first order consideration and this analysis was designed to address this issue head on. Given this priority, the dataset is ideally suited for the task.

Given this premise, the identification strategy is as follows. The credit reform brought with it a significant improvement in overall market efficiency and the relaxation of existing credit constraints. In essence, a more favorable environment to business creation and business growth and consequently to enhanced survival over the life cycle. Disregarding for the moment any purported impact on firms' nature, better credit conditions at the outset should guarantee more solid foundations to face the high uncertainty in infancy, better insurance against the potentials for distress during maturity, and/or the funds needed to undertake potential transformations to fight against obsolescence. Consequently, the odds of survival for a population of firms in years subsequent to the credit reform should improve. Put differently, if we estimated a hazard function for the population of firms after the reform, we should find it shifted down with respect to one estimated for years prior to the reform. Figure 1 depicts a hypothetical benchmark hazard function for firms under constrained financing (line M) and one under a more plentiful environment (line C).¹⁰

However, if the reform does affect the nature of the firms, then the odds of survival observed after the reform might actually worsen. The positive effect from an improved credit market could be matched, or perhaps even outweighed, by the worsening of the population of firms requesting credit. In terms of the figure, the hazard function estimated for the years after the reform could end up being not much different from the benchmark pre-reform one. If this were all we could do, identification of the effect on firms' nature would therefore be impaired by the existence of these two simultaneous effects. A raw comparison of the hazard functions pre and post reform (as it could be done with more aggregate data) would not help. We would not know for sure if the credit reform were simply not an important factor affecting firms' lives, or whether it were important but in a more complex way.

¹⁰ The functions are depicted as reaching a maximum at a very early age and then following a somewhat monotonic downward path. I will give extensive consideration to the shape of the hazard function in the results section.

Identification of the possible effect of credit market reforms on firms' nature requires a more data-demanding strategy. Ideally, we would like to be able to observe individual firms over time, introduce the "treatment," i.e. the credit reform, and then, controlling for confounding factors, analyze any difference in the patterns of mortality between firms experiencing the new environment but born before and firms born in the new environment. The dataset I have used in this study, a micro-level panel comprising the whole population of business organizations (details in the next section), permits precisely such a comparison.

Figure 2, Panel A-B, illustrates this identification strategy. If firms of pre-reform vintages were drawn from a population with stronger fundamentals, and if imprinting matters, they would take full advantage of the more relaxed credit constraints after the reform and their hazard function should be shifted down from that estimated using pre-reform records (Figure 2, Panel A). However, if the conjecture is correct, firms born under the new environment would instead be subject to both of the effects described above. The positive effect from more credit availability should be offset by their weaker nature. The hazard function for firms of post-reform vintages could therefore lie somewhere above that for pre-reform vintage firms in post-reform years (dashed line in Figure 2, Panel B). Any difference in hazard between the two subsets, as captured by the difference between the solid and the dashed line, will then be the result of firms being set up differently at birth, in response to different environmental conditions. If the conjecture is wrong, and there is no impact on firms' nature, or if imprinting is weak and firms quickly adapt to a new environment, then we should not expect to find any significant difference between the two sub-groups, and the dashed line should overlap with the solid line.

This difference-in-difference estimation approach can be seen more formally through the specification of the survival model. Let

$$L_{jisy} = \frac{\{S(t_{jisy} | (\beta'X; \Theta))\}^{1-d} \{f(t_{jisy} | (\beta'X; \Theta))\}^d}{S(t_{0jisy} | \beta'X; \Theta)}$$

be the generic likelihood functions for firm j in industry i located in state s in year y . In the expression, $f()$ is the density function of whichever distribution is going to be

assumed (issue to be addressed later), $S()$ is the corresponding survival function, and Θ is a vector of ancillary parameters associated with any given assumed distribution (ancillary parameters are also discussed more extensively later on). t_{jisy} is firm's survival analysis time (here corresponding to age), t_{0isy} is the onset of risk (here corresponding to birth) and d is the failure event (death).

Finally, X is a vector of covariates affecting survival, with β 's being the corresponding parameters. In particular:

$$\beta'X = \beta_1 \cdot Reform_{sy} + \beta_2 \cdot (Reform_{sy} \cdot Founding\ time_{jisy}) + \sum_{c=3}^n \beta_c \cdot Controls_{jisy}$$

where $Reform_{sy}$ is an indicator variable equal to one starting one year after interstate banking deregulation takes place in state s .¹¹ This term measures the overall impact on the hazard of mortality of the credit reform. $Founding\ time_{jisy}$ instead is equal to zero for firms that were born before the reform and equal to one for firms born after the reform. Hence, this second term of interaction captures the differential effect of the credit reform between the two subsets of firms born either before or after the reform. If the credit reform enhances credit availability, the odds of survival should improve after the reform, and therefore β_1 should be significantly different from zero. However, if conditions at founding matter, then the credit reform should have a differential effect on firms depending on their founding date, with the magnitude of β_2 picking up such difference.

What is in the *Controls* vector? Because of the state/time variability of the credit reform, and because of complete information on the population of individual firms, we can identify β_1 and β_2 and still include covariates with simultaneous firm, time, industry and state variation and indicator variables for time, industry and state fixed effects. Hence, the strategy allows identification of the main variables of interest while effectively "saturating" the model along industry, state and time dimensions and thus absorbing common factors of variability in the data. Details on the specific variables are in the results section.

¹¹ As customarily done, the indicator variable is switched to missing for the year of deregulation itself (see, e.g., Jayaratne and Strahan, 1994).

Alternative theories

Existing theories predict important effects on firms' dynamics, but they do not imply any effect on firms' nature. The question is whether they could still generate predictions on the hazard of mortality that could be confounded with those specific to my proposed conjecture. The basic textbook argument, for instance, states that with more credit competition the supply of financial capital goes up. However, if firms' nature is unchanged the implied assumption is that after the reform all firms should be better off, irrespective of firms' founding time. Therefore the hazard of mortality in post-reform years should be lower (as reflected in a significant β_1), but there would not be any difference between firms based on founding time (β_2 insignificant).

Alternatively, Petersen and Rajan (1995) argued that credit conditions are actually better for young firms under restricted competition, while mature firms are better off in an environment with competitive credit markets. Their conjecture has a natural implication for the survival function: odds are better at young age in a restricted credit environment while the reverse is true at more mature ages. This implies a crossing between the estimated hazard from the pre-reform sample and that from the post-reform sample. However, even this conjecture cannot predict any difference between the two subsets of existing firms in post-reform years. No matter under what conditions they were born, the conjecture simply implies that all younger firms should be worse off after the reform while all mature firms should be worse off before the reform. Again, this alternative conjecture does not offer confounding inferences with the main hypothesis under consideration.

The model specification also allows testing the alternative hypothesis that in fact there is no significant inertia and that pre-existing firms do adjust their nature at times of regime shifts. In this case, firms born prior to the credit reform might adapt to the new environment and weaken their nature in the face of more generous credit conditions. Rajan and Zingales (2001) actually is quite consistent with this argument. The implication of their reasoning is that such changes would be observable not just by new firms but by existing ones as well adapting to the new environment. If that were the case, we should find that pre-reform firms in post-reform years have worse odds of survival

than in pre-reform years (β_1 of opposite sign with respect to my main conjecture) and also that they do not differ from firms of post-reform vintages (β_2 not significant).

Finally, more competition may lead to more credit available but also to banks relaxing their lending standards. There is after all some evidence consistent with deteriorating credit portfolios in more competitive banking markets (Shaffer, 1999), so the question is whether we could obtain the same predictions just as a result of a change in supply conditions. The answer is no: relaxed lending standards would apply to the entire population of loan applicants. Hence irrespective of vintage (pre- or post-reform), firms of same age should exhibit the same odds of mortality. Moreover, the evidence has actually shown that after state deregulation bank efficiency did not deteriorate but in fact improved markedly (Jayaratne and Strahan, 1996, Stiroh and Strahan, 2002).

A related argument is that banks that were already in existence prior to the reform do not change, but the expansion in credit availability comes from new banks and these new banks are going to be of lower quality. If the new banks are disproportionately financing the worst among the new entrepreneurs (in a winner's curse scenario), then the population hazard for firms founded in post-reform years should deteriorate with respect to that of pre-reform year firms. However, in this case the prediction is that pre-reform vintage firms in post-reform years should not look different from the benchmark population of firms born and lived in pre-reform times. Moreover, the evidence has shown that the kind of banks moving in after the relaxation of barriers to entry are actually the more efficient and better run ones (see Evanoff and Ors, 2008, for a complete overview of the efficiency impact of bank entry after deregulation).

Hence, the predictions on the hazards of mortality of the different vintage groups within the population of firms seem to be unique to the conjecture under analysis. The next sections present the results of the empirical testing.

The data

The data used for this study is from the Longitudinal Business Database, a relatively new longitudinal dataset created by the U.S. Bureau of the Census. The database contains confidential information on the entire universe of business organizations with at least one employee ever been recorded in the United States from 1975 to 2005. By virtue of its

comprehensive nature, the dataset does not contain much economic information on each record. At most we know the number of employees and the total payroll, but in exchange the dataset has full demographic details, thus making it a unique tool to perform the type of analysis needed in this particular case. In fact, the type of research questions raised in this study could not even be conceived without the availability of such dataset.

For each individual establishment ever showing up in the dataset, birth, life and death is carefully recorded. Much pain has been taken to minimize instances of false births and deaths (see Jarmin and Miranda, 2002, for details). For the actual analysis I have introduced a number of filters from the original dataset. First, I have restricted my study to manufacturing sectors (SIC 20-39). The restriction to manufacturing was imposed to allow comparability with much of the existing literature, but also – as I argue more extensively in the next section - to minimize the potential distortions from unobservable sources of heterogeneity.

Within the manufacturing sectors, I have excluded records that were classified as having the following nature: government organizations, cooperatives, or tax exempt. Moreover, as customarily done to handle left-censoring issues, entities appearing in the *first* year of the data set but not marked as born in that year (“continuing” entities) were also dropped from the sample as it would not be possible to determine their age. After the application of these filters I was left with a dataset of about 1 million individual business organizations that were ever in existence over the 30-year time period, for a total of about 8 million records. Records for firms alive at time $t = T$, the last year of available data, were right-censored. The basic features normally identified in business populations are also observed in my dataset: very high mortality in early years followed by a decreasing trend as organizations mature. Almost 11 percent of all businesses in my dataset will fail in the first year of operation. By the end of the fifth year, when organizations could be considered as entering maturity, about 50 percent have been lost.¹²

Results

The first step was to determine the proper model to estimate hazard functions. Normally, when there is uncertainty about the shape of the hazard function, a flexible approach with

¹² Summary statistics are available upon request.

no parametric restrictions, such as that implied by the Cox model, is recommended. However, when there is some consensus a priori about the shape of the unconditional hazard, a fully parametric model produces more efficient estimates. There is substantial evidence from previous studies suggesting that the hazard of mortality for business organizations has a skewed, inverted-U shape, with a peak reached in early years, between infancy and adolescence, and then a monotone decreasing pattern (see, e.g., Bruderl, Preisendorfer and Ziegler, 1992). Building on a priori knowledge of the unconditional mortality profile thus suggests embracing a parametric model estimation approach.

There is also another reason to adopt a parametric model. The conjecture under study suggests that the credit reform has a differential impact on firms, depending on their founding date. The estimated coefficients of the corresponding covariates will pick up that differential impact, if any. However, and regardless of the model chosen for the analysis, parametric or not, the effect identified through the coefficient estimates would imply – in fact impose – a proportional effect, when instead it may be the case that the hazard for the two sub-groups of firms is not just different by a scale factor but it could in fact have a different shape altogether. Since the conjecture under study implies that the reform might actually change the whole distribution organizations are drawn from, its impact could be different for specific age groups. Adopting a parametric regression model we are able to test whether the sought-after differential effect of the credit reform is mainly a scale effect, an effect on the shape of the hazard function, or a combination of both.

The choice of a parametric model

I begin the analysis seeking confirmation of previous evidence regarding the shape of the unconditional hazard for business organizations. In order to do that, I fitted a step function of analysis time (organizational age) using a piece-wise linear exponential model and therefore letting the data telling us what the shape ought to be. In a first exercise, I generated discrete arbitrary steps at age = 3, 5, 10, 15 and older than 15. Figure 3, panel A, shows the estimated hazard function for such baseline specification.

The data confirms an inverted-U shape. In a second exercise, I did not select steps at specific ages but I instead allowed for each age year to have its own impact on the estimated hazard. The results are reported in Figure 3, panel B. While the figure is, as expected, choppier than that in the previous exercise, the basic pattern of the hazard function holds true.

Reassured by this finding I then moved on to the selection of the most suitable parametric function from the family of those ordinarily chosen for parametric survival analysis. By and large, while all of such functions can produce a monotonic decreasing pattern in the hazard function from a peak in early years, only the log-normal and the log-logistic can actually exhibit an inverted-U shape, under proper parameterization. In any case, I sought for a more formal confirmation performing a “horse race” among the standard models to select the one with the best Akaike or Schwartz Information Criterion score (Akaike, 1974, Schwartz, 1978). Table 1 reports the scores. As expected, the log-normal and log-logistic produced the best (lowest) scores. Although the log-normal had the lowest score overall, I chose the log-logistic parameterization since it is easier for computational purposes (its mathematical expression does not include the normal cumulative distribution function).

The hazard function under the log-logistic parameterization is:

$$h(t) = \frac{[\exp(-\beta' X)]^{1/\gamma} \cdot t^{[(1/\gamma)-1]}}{\gamma \cdot \{1 + [t \cdot \exp(-\beta' X)]\}^{1/\gamma}}$$

where t is survival time (here corresponding to organizations' age), X is the vector of covariates, and γ is the ancillary parameter of the log-logistic function.

Regarding the above argument on whether a covariate may have either a scale effect on the hazard or change the shape of the hazard altogether, in the log-logistic specification the scale effect is identified by the size of the estimated β 's, while the effect on the shape of a given covariate would be seen in covariate-specific γ 's. Figure 4 shows the effect of β and γ on the hazard function. A larger β lowers the hazard function proportionately, while a larger γ lowers the peak of the function but it also changes the overall shape.

As it transpires from its mathematical expression, in the log-logistic model a positive coefficient indicates a negative effect on the hazard, and vice versa. In this alternative formulation (accelerated failure-time metric), the covariates can be interpreted as factors that either speed up the aging process (if $\exp(-\beta'X) > 1$), or delay it ($\exp(-\beta'X) < 1$). Hence, in this analysis, a positive coefficient implies a lower hazard, and vice versa.

Estimation results

The following tables report results from a wide range of alternative specifications. The sample size is always about 8 million observations. In all tables I report standard errors in brackets. In a first basic regression I simply included the variable for the credit reform, varying by state over time, thus testing whether the hazard of mortality is different in years after the reform from that in years prior to the reform. This simplest specification does not look for a differential effect between firms based on their founding date and it does not incorporate controls. The results are in column 1 of Table 2. The coefficient on the indicator variable indicates a significant but rather small impact on the hazard function. Since the function is formulated in the alternative accelerated failure time metric, and because the function does not necessarily imply proportionality in the effects of the covariates, the point estimate – beside its sign - may not offer a full appreciation of the contribution of the corresponding covariate. For this reason, I resorted to computing the corresponding conditional hazard function and displaying it graphically. Figure 5, Panel A shows the effect of the credit reform indicator variable from the baseline case with no covariate other than the constant term.

In a second specification, I tested whether the impact of the reform could be seen in a change in the shape of the whole hazard function. As said before, this implies letting the ancillary parameter γ to be covariate-specific. Column 2 shows the results allowing for this separate effect of the covariate on the hazard. Figure 5, Panel B shows the plot of the computed hazard function for this alternative model specification.

The relative small effect of the variable could be an indication that the credit reform just does not have an economically significant impact on firms' mortality.

Alternatively, the result is also consistent with our prior that the credit reform has multiple effects that cancel each other out, as conjectured. All of the following specifications attempt to tackle directly this central point of the conjecture. The focus is going to be on the additional indicator variable that separates firms on the basis of their founding date. Column 3 reports the results of this specification, without imposing an additional differential effect on the shape of the hazard function. This more general specification is reported instead in column 4. The coefficients are markedly larger in both specifications, and the effect is not limited to a scale change only (picked up by the β estimates), as indicated by the significant difference in the estimated γ 's. The hazard functions for these two final model specifications are displayed in Figure 5, Panel C and D.

As the results indicate, the credit reform appears to have a significant differential effect on firms' mortality depending on whether they were born before or after the reform itself. The hazard rates for post reform firms are much higher virtually throughout firms' entire life time. The peak is reached for both groups during adolescence. The hazard rate at the peak for post-reform firms is almost 60 percent higher than that for pre-reform years (0.139 vs 0.087). Hazard rates, however, remain very different even among older firms. If the effect of the reform had been just to let in firms drawn from the left tail of the quality distribution, we should have expected to see a spike in hazard rates among firms in infancy and perhaps adolescence years, but then no significant difference among firms at older ages, since the bulk of such firms would have been likely to die young. The fact that the difference in hazard persists is consistent with the idea that the whole distribution of quality shifts to the left, so that entrepreneurs of relatively higher quality may in fact assemble weaker, new organizations in post-reform years.

Another way to appreciate the extent of the impact of credit market conditions on firms' nature is by looking at the cumulative hazard function for the different sub-groups. The cumulative hazard can typically be interpreted as a measure of how many times a subject would be expected to experience failure if the failure event could occur multiple times. With populations of business organizations multiple failures actually is a meaningful concept, as we could conceive of the same entrepreneurs folding and starting businesses over time. Table 3 reports the estimated cumulative hazard for firms of pre-

reform vintages before and after the reform and for firms of post-reform vintages. The data suggest that over a 30-year period, a firm of pre-reform vintages would not even experience two failure occurrences, while a post-reform vintage firm experiences almost three death events over the same time period. This difference underscores a very significant effect on the composition of the population of manufacturing firms. Older, pre-reform vintage firms are now more likely to live longer, while post-reform vintage firms are intrinsically weaker and more subject to replacement.

Robustness to confounding factors

Although the identification strategy, based on the difference-in-difference approach is fundamentally robust to biases produced by unobserved confounding factors, we could still claim, for instance, that the passage of the reform in any given state really coincides with some major trending change that, unrelated to the credit markets, renders firms intrinsically weaker. For example, because of trending changes in technology, market innovation, etc., firms might tend to be constituted at a smaller scale, and markets are so much more active and competitive that it may be harder for a firm to grow in size over time. If size is negatively correlated with mortality, as it is normally presumed, and if the credit reform occurs in response to such overall changing trends in industries, then the reform variables might be picking up a difference among firms which is not necessarily attributable to a response to a changing *credit* environment. By the same token, the credit reform may just be happening when the relative balance of power among industries is changing and certain industries may be the drivers of the push for enhanced credit reforms. If that were the case, the observed response to the credit reform variables may just be underscoring these other changes. For instance, the higher mortality of post-reform firms may be concentrated among those industries that may be in decline in this hypothesized “battle” across industries. Organizational theories have shown, for instance, that firms’ mortality is affected by the level of “legitimization” of its own industry: the more affirmed the industry the better the life prospect among firms (Carroll and Hannan, 2000).

We can test the robustness of the basic results to the potential confounding effects of firm-specific or market/industry specific factors that could be proxy for the argument above. Results for these robustness tests are presented in Table 4. Column 1 shows a specification where I have added firm-specific employment size and the firm's share of total employment in its industry, in the state where it is located and over time. Adding them separately does not make any difference. The regression shows that larger firms and firms relatively larger in their own market have better odds of survival, but there is no direct indication that scale may have changed intrinsically for some external common factors that could also be responsible for the implementation of the state-level credit reform. As a separate control, I added the size of the firm at founding. Again, by the same reasoning, size at birth may be decided by industry or market specific factors that are also driving credit reform. As shown in column 2, size at founding mildly affects mortality, but it does not affect the separate effect of credit reform. In column 3 I have then added variables controlling for the relative importance of the industry the firms belong to, both in terms of overall manufacturing and specific to the conditions within a state. Both of these variables are time varying. The results indicate that firms' own mortality is affected by the relative strength of their industry, but again, there is no impact on the effect of the credit reform. Finally, I have added a simple measure of overall industry density and industry density within a state, as proxied by total number of firms in a given industry, and total number of firms in a given industry in a given state (again, varying over time). The first indicator of density should capture the legitimization of the industry, which is then reflected in better odds of survival for the firm. At the same time, local density should be more closely associated with market competition, and as such, higher local competition may be jeopardizing survival. These two controls add very little and their inclusion has no impact on the effect of the credit reform variables.

As a further attempt to control for common factors of variability in the data, in column 5 I then present the result of a specification including vectors of state, industry and year dummies. The estimated coefficients on the dummies are not reported. The coefficients on the other covariates, including the credit reform variables, are different with this specification, and the estimated γ 's are different as well. However, if anything the results indicate an even somewhat stronger difference between pre and post-reform

firms. The computation of the overall hazard function, as displayed in Figure 6, confirms this and confirms that there is no other relevant change in the relative patterns of the conditional hazard functions, thus strengthening the finding on the effect of the credit reform on the nature of firms.

As a last test, done in the spirit of refining the identification strategy even further, I have estimated the differential effect of the credit reform not just between firms born before and after the reform, but within these two groups I further subdivided the population in firms that - for sector-specific reasons - are highly dependent on external sources of finance for capital investment, from those that instead are less dependent. The concept of external financial dependence is that presented in Rajan and Zingales (1998) and extensively adopted in various studies on firm dynamics (see, e.g., Cetorelli and Strahan, 2006, Kerr and Nanda, forthcoming). By looking at the differential impact along this additional dimension I am basically performing a triple difference estimation that should minimize even further the potential biasing effect of (still unaccounted) factors. Because I have firm-specific data for which I know the age, I can actually refine the standard measure of dependence proposed by Rajan and Zinagales (1998), by conditioning the value of such indicator on firms' age. More precisely, typically the same value of external dependence is applied to all firms irrespective of their age, simply because in standard application data is aggregated across firms at the industry level. However, in my case I have constructed a composite index of external dependence,¹³ the result of calculating a separate one for firms less than five years old, one for firms between five and ten years old and one for firms older than 10. This composite index is then applied to each firm taking the appropriate value depending on the specific age reached by the firm. The results of this additional estimation are in column 6 of Table 4. The effect of the credit reform should be felt more so on firms that depend more on external sources of finance, but following the identification approach adopted so far, we want to know if firms in dependent sectors that were born after the reform display different hazard from firms also in dependent sectors but that were born prior to the

¹³ External dependence is obtained from Compustat data as the median value across all firms of a given age group of total capital expenditures minus cash flow from operations divided by total capital expenditures. For more details on the calculation and for the relevance of this index to non-Compustat firms, see Cetorelli and Strahan (2006).

reform. We display the computed conditional hazards for these two more selected sub groups of firms in Figure 7. As the picture shows, the basic pattern of higher hazard for the first sub group that was identified earlier remains still evident in the data.

Unobservable heterogeneity

Unobservable heterogeneity is a well-known cause of misleading estimations of the odds of survival in a population. If there are subgroups of records under study with heterogeneous degrees of frailty due to characteristics that cannot be observed, the progressive faster-rate exit of the most frail will make the population during mature years look stronger than the actual odds of survival for each individual member of the population. This will be reflected in a mis-estimation of the shape of the hazard function.

Lacking in social science studies the luxury of performing controlled scientific experiments, where by definition everything is accounted for, there are three ways to attempt to minimize the impact of unobserved heterogeneity in the data. First, select a population to analyze that by its own nature is more homogeneous. The decision in this study to confine the analysis to manufacturing industries was driven partly by this consideration. A suitable research design is another effective way to minimize unaccountable factors. The strategy followed in this study – based on the identification of a difference-in-difference effect - raises the bar for the potential biases due to unmeasured heterogeneity. If such unaccountable factors are common across the sub-groups of the population, then they cannot be responsible for any differential impact of the “treatment” found in the data. Having said that, one could still argue that there are unobservable factors affecting the sub-groups of the population in a differential way. Hence, a third solution is to handle unobservable heterogeneity implementing the estimation of the various baseline specifications using a more general parametric approach where unobservable heterogeneity is explicitly modeled. Table 5, columns 1-4, present the results of log-logistic estimations with unshared heterogeneity, where the hazard is equal to the base one multiplied by an unobserved, observation-specific factor, which is assumed to be distributed according to a gamma distribution. The table reports the conventional estimates for the parameter theta, measuring the dispersion of the

unobserved factor. As the tests of significance for the parameter θ indicate, there is unobservable heterogeneity in the data. However, the impact of the credit reform variables does not seem to be different in this more general model. Columns 5-8 show instead the results from models with shared heterogeneity (in essence a random-effect model), where the assumption is that the heterogeneity is common across all the observations for the same firm. Again, the results indicate the presence of unmeasured factors, but there is not a significant impact on the credit reform variables.

Focus on single-establishment firms

So far I have treated the population of firms without giving any consideration to the distinction between single-establishment firms and those that are instead parts of a multi-establishment organization. Firms can be founded as single-establishment entities or they can be founded as an additional component to an already existing company. Also, during their life time, single-establishment firms may become part of more complex organizations. Since the conjecture under analysis is that firms are shaped at founding by the environment they face, it seems plausible to argue that firms constituted to be part of an existing organization, or firms that constitute themselves with multiple establishments simultaneously, might be of a different nature than those constituted as single-establishment organizations. In particular, the process of founding of a new entity that joins a multi-establishment organization would be characterized at least in good part by the nature of the existing organization.

A separate battery of estimations was therefore performed focusing on single-establishment firms. In order to do that, I have excluded all entities that were born as part of a multi-establishment organization. The results, in Table 6, indicate that the effects of the credit reform do not vary from the basic specifications presented earlier without any exclusion from the population. Perhaps the lack of significant difference in the results is due to the fact that single-establishment births represent by far the most common form of organizational founding. Of the entire number of individual organizations in the dataset, about 90 percent of them were in fact single-establishment at birth.

A further round of tests were performed not just removing organizations that were founded as part of multi-establishment entities, but also censoring the records of those organizations that were founded as single-establishments and then later became part of a multi-establishment organization (I do not remove the records for these firms in their entirety, but only those for the years since the transition). The results (not reported) indicate that even with this additional refinement the effect of the credit reform remain unchanged.

Additional robustness tests

In addition to the large battery of alternative model specifications reported above, I have performed additional robustness tests. In the interest of space, the results of the additional tests are not reported but are readily available.

First, despite the arguments presented earlier about the appropriate choice of a hazard model, the evidence in support of an inverted-U shape and the selection of a log-logistic specification, there may still be lingering doubts that the results depend on these specific modeling choices. This is not the case. First, the identification strategy does not impinge upon the shape of the hazard function per se, but on shifts of the function. In other words, the identification would have been the same even in the case of, say, a monotonically decreasing hazard function (provided that it was the correct parametric specification). At any rate, as additional evidence, I looked at the raw data and estimated unconditional hazard functions for the three sub groups of firms in the population. The ordering in terms of mortality rates are the same as those obtained with the more refined parametric specifications shown above.

Second, concerns may be raised that despite the number of controls, I may still not be capturing basic market or industry factors that could be directly related to the surviving of firms. Consequently, I constructed a measure of state-specific economic cycle as the growth rate of total employment in a state, and a more in-depth measure of industry-specific cycle within a state, as the growth rate of total industry employment in a state. These two variables should effectively absorb state-level variability and industry-

by-state variability. The results indicate, as expected, that the hazard of mortality is counter-cyclical, but the addition of any of these variables affect the basic results.

Third, as an additional refinement to the idea of conditioning at founding, I have constructed indicator variables capturing a vintage effect: a dummy variable common to all firms born in the same calendar year. This is another way to control for confounding events occurring at the time of birth. The results are robust to these additional controls.

Fourth, the identification strategy should be protected from basic survival bias because the essence of the test is to compare the hazard of mortality of firms in the three different groups but always for the same age. That is, the model compares, say, a firm that is ten year old in a pre-reform environment, with a ten year old firm that experienced the reform at some point during its life, with a ten year old firm that was instead born after the reform. However, a possible concern is that there may still be a bias due to heterogeneity among pre-reform vintage firms: a ten year old firm that lived eight-nine years before the reform occurred may be a different firm from one of same age but that experienced the reform when it was just two or three: by virtue of surviving in the previous environment for a relatively long period of time, the first firm may be selectively stronger, and therefore the conditional hazard rates for pre-reform vintage firms in post-reform years may turn out to be “excessively” low because of the pooling together of such firms without consideration to *when* they experienced the reform. The frailty models should correct for this potential issues, but as an additional robustness test I have run estimations separating pre-reform vintage in post-reform years in a group of “young” at reform and “old” at reform, as firms that were seven years and younger or older, respectively, when they experienced the event. The results do indicate that the older pre-reform vintage firms exhibit indeed the lowest hazards in post-reform years, but the basic results stand: pre-reform firms that were still young at reform time (and therefore perhaps really more similar to post-reform vintage ones) still exhibit lower hazards than the benchmark group while the post-reform vintage firms higher hazards.

Fifth, as said in the identification section, throughout the analysis the deregulation event is captured by the decision of a state to open up its banking market to out-of state competition. But this occurrence was normally preceded, albeit by only a relative small period of time, by the decision to remove barriers to branching within a state to banks

already in existence in a state. One could then argue that perhaps by the time the state is removing interstate barriers, competitive conduct has already begun to change, but by setting the event date at the passing of the interstate deregulation I might be misclassifying firms born just around that date. In order to allow for some flexibility on the exact time of the environmental change, I ran alternative specifications setting the reform time back for up to three years prior to the effective time a state allow for interstate entry. The results did not change.

Sixth, and by a somewhat similar argument, one could also claim that deregulation takes time before banks can take in all the changes and effectively reach a new, long-run equilibrium. If this were the case, hazard rates in the period immediately after the reform may turn out to be higher than normal but only for reasons specific to the temporary adjustment of the industry. Consequently, I ran another specification where this time the reform variable was artificially shifted forward up to three years, and I still found the basic result unaffected.

Conclusions

Imagine the introduction of a medicine that if taken regularly would lower significantly the occurrence of the common cold. Individuals born before this innovation might minimize catching the disease by making a habit of proper preventative measures (staying home when sick, minimizing contact with sick individuals, washing hands frequently, etc.). These habits might become ingrained with these individuals and stick with them even after the introduction of the medicine. However, individuals from generations born after the medicine may be less likely to engage in prevention. In fact, they may be more likely to take up riskier actions knowing that they can rely on the existing medicine. Populations of organizations, I argue, may not behave much differently than the human population in this metaphor.¹⁴

¹⁴ This, it turns out, is more than a made up metaphor. A 2008 report by the Center for Disease Control on HIV-AIDS among male homosexuals, documented a marked increase in infection rates among the youngest age groups, in contrast to negligible and even negative rates for older age groups. AIDS service organizations claim the successful introduction in the last decade of anti-retroviral therapies has led to “treatment optimism”, a diminished fear of HIV infection among younger individuals and an increased likelihood of engaging in risky behavior. (referenced in: www.washingtonpost.com/wp-dyn/content/article/2008/06/26/AR2008062603521.html).

This paper has shown that credit market reform has a deep impact on firms' life cycle dynamics. Evidence indicates that life expectancy is significantly altered as a result of the change in the business environment. Irrespective of vintage, odds of mortality are lower after the reform. However, the impact is heterogeneous within the population of firms. Organizations of pre-reform vintages have a clear improvement in their life chances, both in absolute terms and relative to firms of post-reform vintages. This evidence is consistent with the concept of structural inertia in firms' nature. Firms of post-reform vintages, on the other hand, seem to select into an innately more fragile nature, even in an environment that should in fact enhance life chances.

The ultimate effects of credit market reform in this regard contradict conventional wisdom. A market reform such as that under analysis is normally assumed to bring with it an influx of new blood and produce a generational makeover. My results suggest that firm creation may indeed be enhanced with more credit competition, but they also suggest that survivorship of these firms may be impaired. Moreover, since organizations do not have a natural life-ending age – as in human population and living organisms in general – the more resilient, pre-reform cohorts of firms are destined to dominate the overall firm distribution. This paper thus suggests that credit market reforms may in fact lead to enhanced aging of firm populations, perhaps an outcome contrary to original intentions.

This paper does not, however, take any normative stance, for example in suggesting that constrained credit markets leads to better social outcomes. The main goal here is instead to inform the complexity of the dynamic process of business formation and the contribution to this process of financial variables. The conjecture proposed in this paper and the associated evidence are perfectly consistent with the idea that credit competition is in fact welfare enhancing. Credit competition allows deserving pre-reform firms to live longer lives instead of being exposed to premature death because of external causes linked to credit availability. At the same time, it allows the entry, among a perhaps increasing population of lesser entrepreneurs, of truly exceptional firms that are more likely to drive the process of technological innovation and growth. Nothing in the arguments presented in this paper precludes the possibility that in a more favorable credit

environment there is a better chance for good business ideas to be undertaken, for top prospect firms to be created, and for them to thrive over time.¹⁵

The paper is also not suggesting that banks' default rates are necessarily higher in post-reform years, because while firms born after the reform display higher hazard rates, at the same time firms born prior to the reform have lower hazard rates. The aggregate impact on overall population hazards will therefore depend on the actual composition by firms' vintage.

As mentioned in the identification section, the paper does not analyze what specific firm characteristics may be changing in response to the credit reform but instead draws inference on population dynamics looking more deeply to changes in life chances. However, speculating on what might be changing in firms' nature, likely candidates may be characteristics of their financial structure, and a theoretical starting point could be the model by Evans and Jovanovic (1989), describing the choice of entrepreneurs under liquidity constraints. Their model predicts, for instance, that in environments where finance is constrained, wealthier individuals, who can rely on larger internal sources of funding, are more likely to become entrepreneurs. Put it in a broader perspective, their model implies that in a credit constrained environment firms are started with lower leverage. Hence, after the reform, these firms may maintain a higher level of resilience to income shocks (this is also related to the arguments in Zingales, 1998, mentioned earlier), *unless* they adapt to the new environment and take up structurally higher levels of debt. In post-reform years instead, given the relaxed credit constraints, it is not just wealthy entrepreneurs that could start a business, but these new firms, with higher leverage, would be intrinsically more fragile. More in general, identifying what specific characteristics of firms can explain the patterns in life cycle dynamics documented here is a natural follow up question for future research.

Finally, the work presented in this paper has a connection with path-dependence theories and with institutional economics in general.¹⁶ A standard policy experiment in

¹⁵ Casual observation from the data confirms this point: I calculated firm-level yearly growth rates over the entire sample period and calculated basic population statistics. The data shows that the distribution of growth rates for the population of post-reform firms has a higher variance but most importantly it has a skewness an order of magnitude larger than that for the distribution of firms of pre-reform vintages, thus indicating that the largest-growth organizations were indeed founded in post-reform years.

¹⁶ See, e.g., Acemoglu, Johnson and Robinson (2002) and Engerman and Sokoloff (2002).

economic development involves cross sectional comparisons of economic systems and the prescription that backward economies can shift to a path of development and convergence if they embrace the institutional environment observed among developed systems. The evolutionary approach adopted in this paper, and the related evidence in support of imprinting and inertia within the population of business organizations, instead suggest a more complex impact of institutional reforms. In fact it gives substance to why initial conditions matter and the rationale for why development paths may not be replicable. As indicated in this paper, population responses to institutional changes may come through adaptation or through self-selection, or both. Both the new steady state equilibrium and the dynamic path to the equilibrium point itself may be vastly different from expected depending on the evolutionary impact of the reform. Regardless of outcome, the concepts of evolutionary ecology may help us to model these dynamics and predict their outcomes in more complex ways.

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Table 1
 Comparison of parametric models
 Akaike information criterion (AIC) and Schwartz's Bayesian information criterion (BIC)

DISTRIBUTION	Number of parameters	AIC	BIC
Exponential	1	2301319	2301360
Weibull	2	2237836	2237891
Gompertz	2	2300707	2300762
Log-normal	2	2099385	2099440
Log-logistic	2	2140027	2140083

The table reports the scores for both Akaike and Schwartz information criteria. Both criteria penalize the log likelihood based on the number of parameters each model needs to be estimated and population size (Schwart's only). Both criteria identify the best-fitting models as those with the lowest score.

Table 2
Effect of credit reform on hazard of mortality
Differential effect by time of founding

Independent variable	1	2	3	4
Credit reform _{sy}	0.0561 (0.0024)	0.1407 (0.0022)	0.4122 (0.0034)	0.1978 (0.0055)
Reform _{sy} • Founding time _{jisy}			-0.4163 (0.0031)	-0.1483 (0.0054)
Constant	1.9407 (0.0020)	1.8595 (0.0017)	1.9196 (0.0019)	1.8595 (0.0017)
gamma	0.5620 (0.0006)	0.4538 (0.0021)	0.5337 (0.0006)	0.4498 (0.0021)
gamma_reform		0.6005 (0.0024)		0.7558 (0.0038)
gamma_reform•founding time				0.5202 (0.0034)

The table reports estimates of hazard functions based on a log-logistic parameterization. Estimates are obtained from the entire population of for-profit manufacturing business establishments in existence between 1975 and 2005. The failure event is business establishment death. Credit reform is an indicator variable that turns equal to one for the years after the state in which the business establishment is located allows interstate banking. Founding time is an indicator variable equal to one for business establishments born in years after the interstate banking reform. The table reports estimates of the ancillary parameter of the log-logistic function, gamma. The ancillary parameter in the various specifications is either kept constant or allowed to be different for the sub-populations of establishments born before or after the credit reform. The reported coefficients are expressed in the accelerated failure metric, therefore a positive (negative) coefficients indicates a negative (positive) contribution to the hazard of mortality.

Table 3
 Predicted cumulative hazards
 By vintage and by pre- or post-reform years

	Cumulative hazard					
	Age 5	Age 10	Age 15	Age 20	Age 25	Age 30
Pre-reform vintages, pre-reform years	0.53	1.35	2.04	2.58	3.02	3.40
Pre-reform vintages, post-reform years	0.41	0.80	1.13	1.40	1.64	1.84
Post-reform vintages	0.50	1.18	1.75	2.21	2.59	2.92

The table reports the estimated cumulative hazard for the different sub-populations of business establishments. The statistics are calculated using the specification in column 4 of Table 2

Table 4
Effect of credit reform on hazard of mortality
Additional controls

Independent variables	1	2	3	4	5	6
Credit reform _{sy}	0.2389 (0.0051)	0.2330 (0.0052)	0.2445 (0.0051)	0.2426 (0.0051)	0.1396 (0.0065)	0.2266 (0.0063)
Reform _{sy} •Founding time _{jisy}	-0.1795 (0.0050)	-0.1711 (0.0051)	-0.1846 (0.0049)	-0.1876 (0.0049)	-0.3742 (0.0051)	-0.1575 (0.0061)
Employment _{jisy}	0.0021 (0.0000)	0.0011 (0.0000)	0.0022 (0.0000)	0.0022 (0.0000)	0.0020 (0.0000)	
Share _{jisy}	1.2663 (0.0633)	1.2493 (0.0630)	1.0815 (0.0612)	1.2938 (0.0635)	1.5131 (0.0658)	
Employment at birth _{jisy}		0.0012 (0.0000)				
Industry share _{iy}			1.2158 (0.0325)	1.0312 (0.0419)	1.0473 (0.1036)	
Industry share in state _{isy}			-1.0290 (0.0177)	0.1087 (0.0313)	-0.0147 (0.0340)	
Industry density _{iy}				0.0000 (0.0000)	0.0000 (0.0000)	
Industry density in state _{isy}				0.0000 (0.0000)	0.0000 (0.0000)	
External dependence _i						-0.1109 (0.0027)
Reform _{sy} •dependence _i						-0.0731 (0.0085)
Reform _{sy} •founding time _{jisy} •dependence _i						0.0293 (0.0083)
Constant	1.8137 (0.0018)	1.8083 (0.0018)	1.7845 (0.0031)	1.7249 (0.0034)	2.2146 (0.0124)	1.9158 (0.0022)
State, Industry, Year FE	No	No	No	No	Yes	No
gamma	0.4416 (0.0021)	0.4405 (0.0021)	0.4410 (0.0021)	0.4394 (0.0021)	0.3998 (0.0023)	-0.8198 (0.0021)
gamma_reform	0.7209 (0.0037)	0.7239 (0.0037)	0.7157 (0.0037)	0.7138 (0.0037)	0.6448 (0.0042)	0.5012 (0.0040)
gamma_reform•founding time	0.5137 (0.0034)	0.5143 (0.0034)	0.5113 (0.0034)	0.5106 (0.0034)	0.4966 (0.0036)	-0.3709 (0.0037)

The table reports estimates of hazard functions based on a log-logistic parameterization. Estimates are obtained from the entire population of for-profit manufacturing business establishments in existence between 1975 and 2005. The failure event is business establishment death. Credit reform is an indicator

variable that turns equal to one for the years after the state in which the business establishment is located allows interstate banking. Founding time is an indicator variable equal to one for business establishments born in years after the interstate banking reform. Employment is the establishment's employment size. Share is the establishment's share of total industry employment in the state where the establishment is located. Employment at birth is establishment's employment size at the time of founding. Industry share is the share of total manufacturing employment of the establishment's industry. Industry share in state is the share of total state employment of the establishment's industry. Industry density is the total number of business establishments in the establishment's industry. Industry density in state is the total number of business establishments in the establishment's industry in the state where the establishment is located. External dependence of industry i is a composite index of the dependence from external sources of finance varying by firms' age, for age less than 5, between 6 and 10, and older than 10. The table reports estimates of the ancillary parameter of the log-logistic function, γ . The ancillary parameter in the various specifications is either kept constant or allowed to be different for the sub-populations of establishments born before or after the credit reform. The reported coefficients are expressed in the accelerated failure metric, therefore a positive (negative) coefficients indicates a negative (positive) contribution to the hazard of mortality.

Additional specifications including a broader set of state by year fixed effects and firm and industry time varying controls were also ran using a discrete logit model on a random sample of the original dataset. The results, not reported, were consistent with the other model specifications.

Table 5
Effect of credit reform on hazard of mortality
Unshared and shared frailty models

Independent variables	Unshared frailty				Shared frailty			
	1	2	3	4	5	6	7	8
Credit reform _{sy}	0.3145 (0.0035)	0.4122 (0.0034)	0.3123 (0.0036)	0.4116 (0.0034)	0.3303 (0.0029)	0.4122 (0.0034)	0.3297 (0.0030)	0.4116 (0.0034)
Reform _{sy} •Founding time _{jisy}	-0.3302 (0.0033)	-0.4163 (0.0031)	-0.3292 (0.0034)	-0.4079 (0.0030)	-0.3452 (0.0029)	-0.4163 (0.0031)	-0.3511 (0.0029)	-0.4079 (0.0030)
Employment _{jisy}			0.0006 (0.0000)	0.0023 (0.0000)			0.0011 (0.0000)	0.0023 (0.0000)
Share _{jisy}			1.2260 (0.0533)	1.3552 (0.0658)			1.0125 (0.0514)	1.3552 (0.0658)
Industry share _{iy}			0.7337 (0.0399)	1.1042 (0.0436)			0.6294 (0.0395)	1.1042 (0.0436)
Industry share in state _{isy}			0.1910 (0.0297)	0.0530 (0.0322)			0.1609 (0.0294)	0.0530 (0.0322)
Industry density _{iy}			0.24•e-6 (0.1•e-7)	0.31•e-6 (0.1•e-7)			0.31•e-6 (0.1•e-7)	0.31•e-6 (0.1•e-7)
Industry density in state _{isy}			0.31•e-5 (0.1•e-6)	0.49•e-5 (0.1•e-6)			0.31•e-5 (0.1•e-6)	0.49•e-5 (0.1•e-6)
Constant	1.6028 (0.0019)	1.9196 (0.0019)	1.5371 (0.0034)	1.7825 (0.0036)	1.6083 (0.0018)	1.9196 (0.0019)	1.5395 (0.0033)	1.7825 (0.0036)
gamma	0.3390 (0.0007)	0.5337 (0.0006)	0.3463 (0.0007)	0.5225 (0.0006)	0.3417 (0.0007)	0.5337 (0.0006)	0.3516 (0.0007)	0.5225 (0.0006)
theta	1.1153 (0.0049)		1.0304 (0.0051)		1.0619 (0.0047)		0.9587 (0.0048)	

The table reports estimates of hazard functions based on a log-logistic parameterization. Estimates are obtained from the entire population of for-profit manufacturing business establishments in existence between 1975 and 2005. The failure event is business establishment death. Credit reform is an indicator variable that turns equal to one for the years after the state in which the business establishment is located allows interstate banking. Founding time is an indicator variable equal to one for business establishments born in years after the interstate banking reform. Employment is the establishment's employment size. Share is the establishment's share of total industry employment in the state where the establishment is located. Employment at birth is establishment's employment size at the time of founding. Industry share is the share of total manufacturing employment of the establishment's industry. Industry share in state is the share of total state employment of the establishment's industry. Industry density is the total number of business establishments in the establishment's industry. Industry density in state is the total number of business establishments in the establishment's industry in the state where the establishment is located. The table reports estimates of the ancillary parameter of the log-logistic function, gamma. The ancillary parameter in the various specifications is either kept constant or allowed to be different for the sub-populations of establishments born before or after the credit reform. The unobservable frailty parameter follows a gamma distribution. Theta is the estimated variance of the unobservable parameter in the frailty model. In the shared frailty models, observations are grouped by business establishment. The reported coefficients are expressed in the accelerated failure metric, therefore a positive (negative) coefficients indicates a negative (positive) contribution to the hazard of mortality.

Table 6
Effect of credit reform on hazard of mortality
Focus on single establishments

Independent variables	1	2	3	4	Frailty	
					Unshared	Shared
					5	6
Credit reform _{sy}	0.1930 (0.0059)	0.2054 (0.0058)	0.2153 (0.0057)	0.2098 (0.0057)	0.2964 (0.0038)	0.3156 (0.0032)
Reform _{sy} •Founding time _{jisy}	-0.1321 (0.0058)	-0.1399 (0.0057)	-0.1465 (0.0056)	-0.1521 (0.0055)	-0.3078 (0.0035)	-0.3322 (0.0031)
Employment _{jisy}		0.0015 (0.0000)	0.0016 (0.0000)	0.0017 (0.0000)	0.0000 (0.0000)	0.0003 (0.0000)
Share _{jisy}		1.0869 (0.0767)	1.0229 (0.0755)	1.2452 (0.0787)	0.8959 (0.0595)	0.9282 (0.0604)
Industry share _{iy}			1.7575 (0.0342)	1.4030 (0.0440)	0.8124 (0.0413)	0.7304 (0.0409)
Industry share in state _{isy}			-0.9414 (0.0184)	0.2333 (0.0334)	0.2472 (0.0309)	0.2311 (0.0307)
Industry density _{iy}				0.0000 (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)
Industry density in state _{isy}				0.0000 (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)
Constant	1.7953 (0.0018)	1.7748 (0.0018)	1.6997 (0.0033)	1.6403 (0.0036)	1.4639 (0.0034)	1.4671 (0.0034)
gamma	0.4382 (0.0022)	0.4333 (0.0022)	0.4321 (0.0022)	0.4296 (0.0023)	0.3303 (0.0007)	0.3351 (0.0007)
gamma_reform	0.7382 (0.0040)	0.7261 (0.0040)	0.7191 (0.0040)	0.7163 (0.0040)		
gamma_reform•founding time	0.5059 (0.0036)	0.5058 (0.0036)	0.5032 (0.0036)	0.5023 (0.0036)		
theta					1.0342 (0.0050)	0.9773 (0.0048)

Business establishments that were founded as part of multi-establishment entities were not included in these regressions. The table reports estimates of hazard functions based on a log-logistic parameterization. Estimates are obtained from the entire population of for-profit manufacturing business establishments in existence between 1975 and 2005. The failure event is business establishment death. Credit reform is an indicator variable that turns equal to one for the years after the state in which the business establishment is located allows interstate banking. Founding time is an indicator variable equal to one for business establishments born in years after the interstate banking reform. Employment is the establishment's employment size. Share is the establishment's share of total industry employment in the state where the establishment is located. Employment at birth is establishment's employment size at the time of founding. Industry share is the share of total manufacturing employment of the establishment's industry. Industry share in state is the share of total state employment of the establishment's industry. Industry density is the total number of business establishments in the establishment's industry. Industry density in state is the total number of business establishments in the establishment's industry in the state where the establishment is located. The table

reports estimates of the ancillary parameter of the log-logistic function, γ . The ancillary parameter in the various specifications is either kept constant or allowed to be different for the sub-populations of establishments born before or after the credit reform. The unobservable frailty parameter follows a gamma distribution. θ is the estimated variance of the unobservable parameter in the frailty model. In the shared frailty models, observations are grouped by business establishment. The reported coefficients are expressed in the accelerated failure metric, therefore a positive (negative) coefficient indicates a negative (positive) contribution to the hazard of mortality.

Hazard of mortalities

Pre- and post-reform years

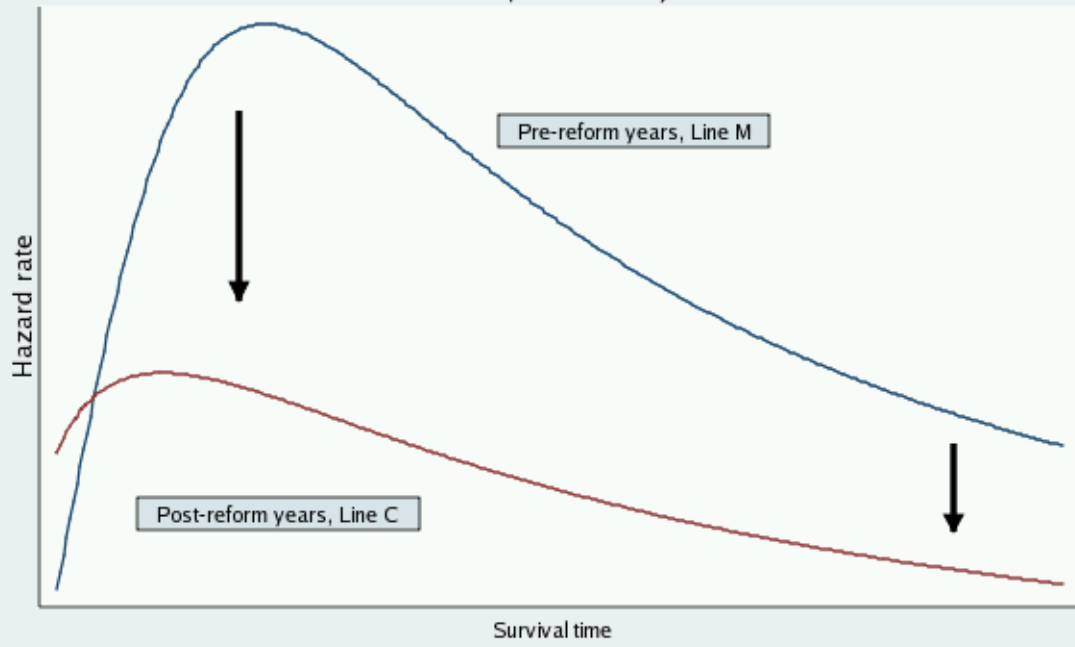
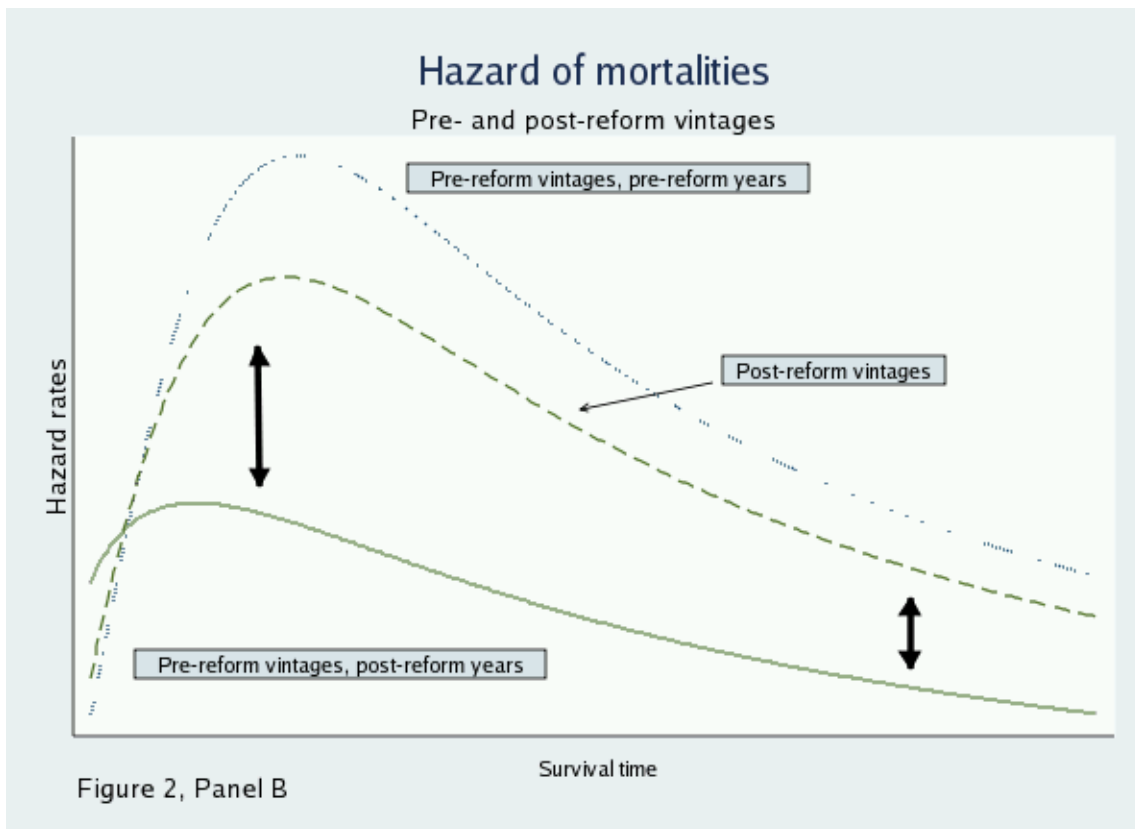
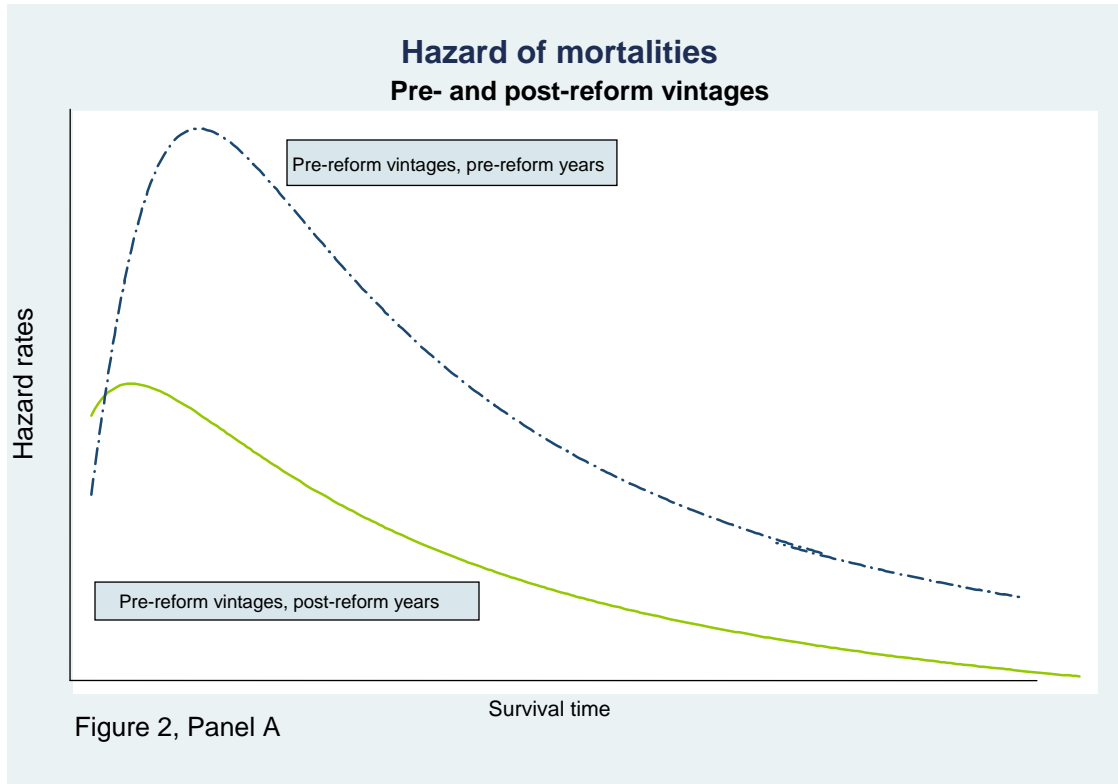
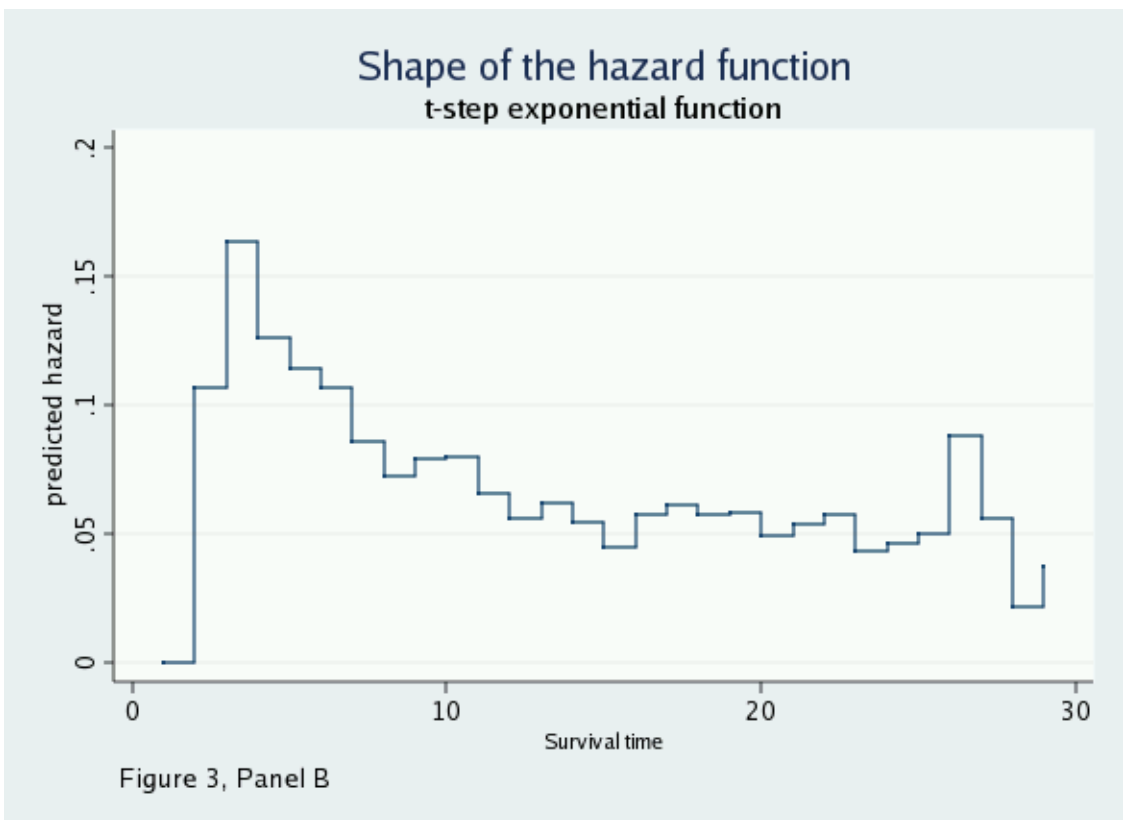
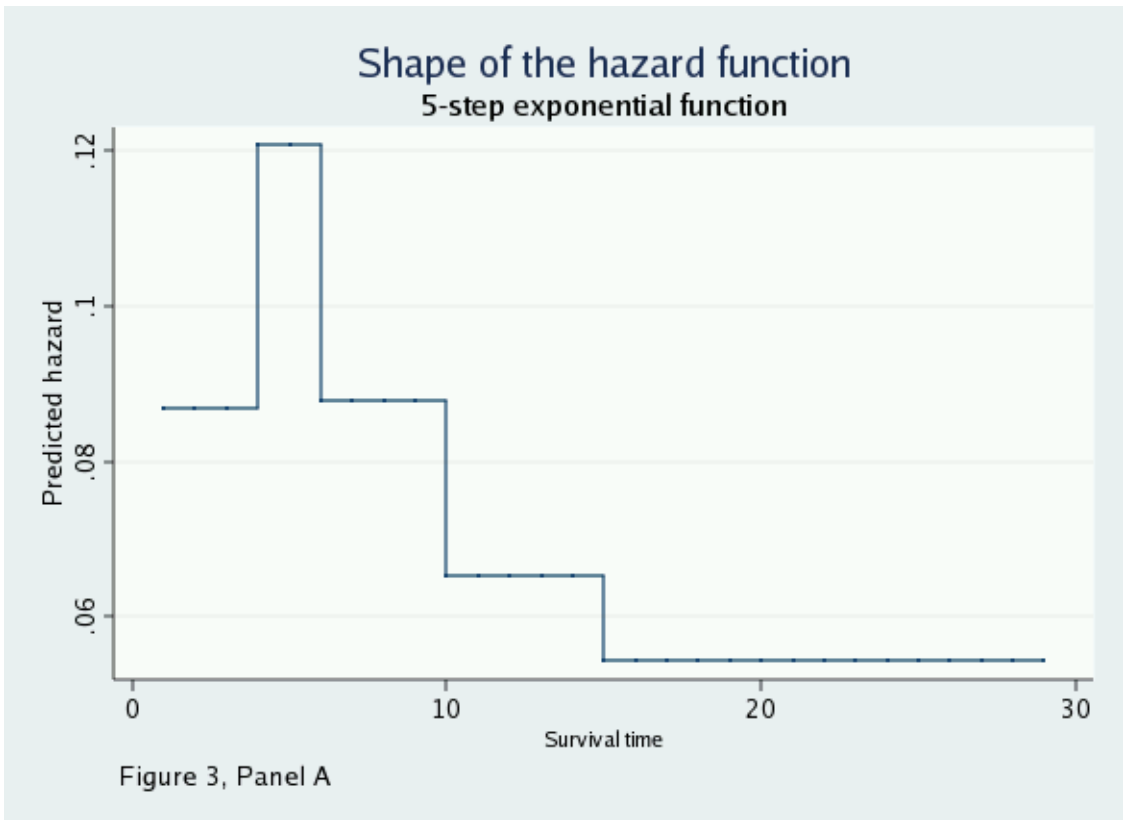


Figure 1





Log-logistic hazard functions

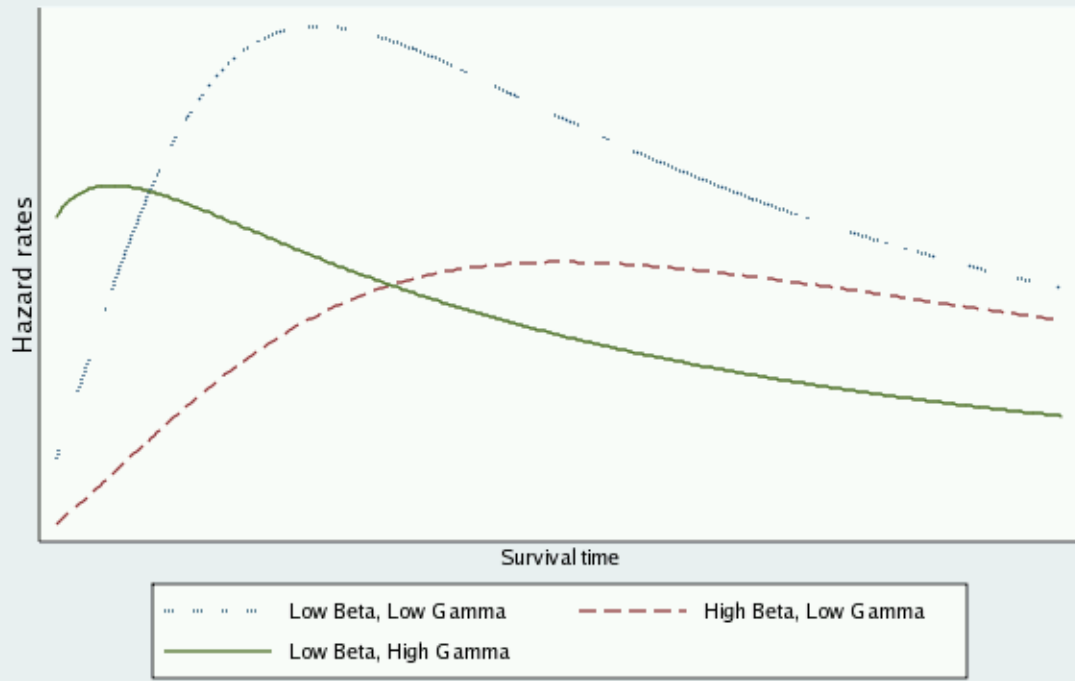


Figure 4

