Surviving credit market competition[1]

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Abstract

Empirical studies have documented that improvements in credit supply have important effects on entry in non financial industries. This paper shows that changes in credit supply conditions have much deeper effects on firms' population dynamics, well above and beyond the experience of entry. I explore the hypothesis that changes in credit supply have important effects on the demand side as well. I conjecture that when financial capital is difficult to obtain, while fewer firms may enter, those entering are drawn from a population with a better distribution of entrepreneurial quality. In an environment where financial capital is easily obtainable instead, the population of loan applicants changes as well, including those that in a tougher environment would not have tried entrepreneurship in the first place. These changes in the population of applicants imply significant effects on firms' life expectancy profile, and these effects are heterogeneous across firms of different vintage. Modifications in life expectancy are likely to affect firms' incentives in undertaking future capital investment and likewise investments in technological innovation. Hence, these changes in overall firms' population dynamics characterize an explicit mechanism through which finance can affect real economic activity.

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1. Introduction

Empirical studies have documented that more competition in credit markets enhances entry in non-financial sectors. This evidence has been recognized for its importance in supporting theories claiming that finance matters for real economic activity. Not only does it show that there are significant quantitative effects, but it sheds light on a specific channel through which improvements in financial conditions can affect the potentials for long-term economic growth.

The effects are quite large. Black and Strahan (2002), for instance, find that incorporation rates increased on average by 11 percent after U.S. states opened up to out-of-state banks. Kerr and Nanda (2008) presented significant qualifications to this evidence, documenting that out-of-state bank competition did lead to increased firm entry, but interestingly the higher birth rates were just about the same for doomed-to-fail enterprises (those dying within the first three years of life) and those that lived longer life (thus more likely to contribute to long term growth). Since both high and low quality firms were let in at the same higher rate, this evidence suggests that credit supply expanded, although allocated somewhat indiscriminately (this is what the authors define as the "democratization" of entry). Hence, the count of high quality firms increases, but at the cost of increased wasting of resources (their so-called "churning" effect).[2]

These qualifications are important, because they inform how enhanced bank competition affects credit supply and access to credit. However, in this paper I argue that there remain many aspects of the way financial conditions affect economic activity, related to the effect on entry, that are left unexplained. While new entry can in fact be, mechanically, a component of growth, ultimately what contributes to sustained, long term economic activity really depends on overall firms' population dynamics: more firms enter, but are they more or less likely to survive? Given the typically long gestation periods in the development of new products and the adoption of new, better technologies, long-term growth should really depend on firms' life expectancy. For instance, for given entry rate, firms' incentive to undertake investment and innovation are probably very different depending on whether there is a high or low chance to survive to maturity and

[2] Further evidence on entry, focusing on size at entry, was instead presented in Kerr and Nanda (2010).

rip the benefits of the original investments. So, should we expect that better access to credit changes firms' survival profile over their entire life cycle? If so, how and why? And how does a better credit environment affect survival of firms that had entered before, when credit was more constrained? After all, better ability to access funding, not just at birth but throughout a firm's life, should improve the chances of survival when, for instance, shocks are experienced. At the same time, the higher firm count resulting from easier finance may also translate in more competition and lower mark ups in firms' own markets, thus threatening survival.

Learning how firms' population dynamics evolve, and how the credit environment contributes to such evolution, is of first order importance to further our understanding of the finance-growth nexus. Changes in life expectancy should have a direct impact on firms' future investment choices and on their incentives to undertake innovation. This is, for instance, perfectly in keeping with models of Schumpeterian endogenous growth *a la'* Aghion and Howitt (1992, 1998). Hence, by underscoring this impact of changes in financial condition, I am suggesting a specific mechanism through which the financial side affects real economic activity.

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 mentioned above, that led U.S. states, at different point in time between the 1970s and the 1990s, to remove significant regulatory barriers to bank entry. This is essentially the same dataset used by Kerr and Nanda (2008), but as I provide details in the data section, I will highlight the differences both in scope and methodology.

Through the estimation of hazard models, I document that the same enhancement in bank competition – the decision by states to open up to out-of-state banking – that both Black and Strahan (2002) and Kerr and Nanda (2008) find to have such an important impact on entry, has broader and more complex effects on firms' population dynamics. First, I show that in times of constrained finance, firms face relatively high odds of mortality. However, experiencing at some point during their lifetime the credit reform, thus facing a new environment of easier access to funding, leads to better odds for a more prolonged life after that. At the same time, firms born post-reform are intrinsically frailer, as evidenced by a consistently higher hazard of mortality throughout their entire life span.

These findings are 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, factors capturing state-specific cyclicality and within-state, industry-specific cyclicality. Likewise, the results are robust to estimation with alternative frailty models to further account for unobservable heterogeneity. 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.

What kind of "model" regarding the effects of enhanced credit conditions on firms' population dynamics could yield the type of results just listed? I conjecture that while it is certainly the case that changes in credit conditions result in an impact on credit supply, the *demand* side is affected as well. More precisely, I posit that the distribution of quality of the population of loan applicants changes with improved credit supply: take a population of individuals, each endowed with a given level of quality should they choose to engage in entrepreneurial activity. Assume also that there is a cost to becoming an entrepreneur. It is the opportunity cost of doing something else, for example earning wages supplying labor services. If an individual does not have high enough entrepreneurial skills, he would be better off choosing the alternative activity, thus dropping altogether from the population of prospective entrepreneurs.[3] The cost of obtaining finance is part of this calculation. In an environment of constrained finance, individuals below a certain threshold of quality will not even enter in the pool of prospective entrepreneurs.[4] It thus follows that under constrained finance, with a left-truncated quality distribution, while the count of entrants is low, higher quality entrepreneurs are disproportionately more represented. This is a positive effect for the

[3] In this simplified description, individual quality is known, but the argument fits very nicely with the model of entrepreneurial choice of Jovanovic (1982), where individuals try entrepreneurship, learn their quality on the way, and then leave if the choice is too costly.

[4] The cost is not necessarily just in terms of the interest paid on a loan, but also and perhaps even more importantly, on the likelihood to obtain a loan in the first place, which translates in the time needed to get funding (time that can be compared to the periods of lost earnings from the alternative labor activity).

chances of survival, as measured population wide. Of course, there is a counter effect: because finance is difficult to obtain, it is difficult to obtain along the entire life cycle of a firm. In the event of shocks the unconditional chance of a firm defaulting, and therefore dying, is relatively high because it is hard to obtain additional funding support.

Things change when finance becomes easier to obtain. For the argument just spelled out, firms should now be better positioned to weather adverse events, which should be reflected in unconditional better odds of survival in post-reform years. At the same time, however, the removal of financial constraints is equivalent to a shift to the left of the threshold of quality to becoming an entrepreneur. Individuals that in a tougher environment would have actually chosen not to undertake entrepreneurial activity at all are now joining the pool of new loan applicants. Hence, while now supply of funds has improved – and therefore higher entry rates are observed - the distribution of quality of those demanding credit has deteriorated. Consequently, the expectation of survival for this subset of the population of firms, those born in the new credit environment, deteriorates as well.

Conjecturing an adaptation on the demand side of the credit market in response to enhanced bank competition thus yields testable hypotheses on firms' population dynamics. The paper contributes innovative evidence and an expanded scope to the broad research on finance and the real economy. Beyond asserting the importance of financial conditions for economic growth (e.g. King and Levine, 1993, Jayaratne and Strahan, 1996, Rajan and Zingales, 1998) and presenting evidence on specific ways improved finance should be conducive to growth (e.g., Cetorelli and Gambera, 2001, Black and Strahan, 2002, Cetorelli and Strahan, 2006, Kerr and Nanda, 2008), this work zooms in on the behavior of the units of economic production, the firms. Figuratively speaking, while previous contributions have studied the relationship between finance and growth but with a focus tilted more on the financial side, this paper concentrates its attention more on the recipients of finance, the side where growth takes place. In this sense, the paper is also related to the broad literature that has studied firm dynamics (e.g., on the theory side, Jovanovic, 1982, Hopenhayn, 1992, Albuquerque and Hopenhayn, 2004, Clementi and Hopenhayn, 2006; while e.g., Caves, 1984, Evans, 1987, Dunne, Roberts and Samuelson, 1988, Evans and Jovanovic, 1989, Zingales, 1998, Carroll and Hannan, 2000 on the empirical side). This literature has developed theoretical mechanisms to endogenously derive certain observable patterns in firms' life cycle dynamics and has tested the importance of various determinants of such dynamics. This paper pushes on the importance of financial constraints among those determinants. To the best of my knowledge, this is also the first paper that directly explores the impact of changes in financial conditions on firms' survival dynamics.

2. The credit reform

Much has been written about the deregulation of the U.S. banking industry initiated in the 1970s. This process of credit reform 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 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.[5] 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 change in credit conditions.

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. [6] As said earlier, both Black and Strahan (2002) and Kerr and Nanda (2008), the reference points for this contribution, have used this

[5] 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.

[6] The first paper that implemented this approach was Jayratne and Strahan (1996). See also, e.g., 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).

methodological approach, and have emphasized in particular the role of interstate bank deregulation. Following in their footsteps, I will also focus on this state-specific event in my identification strategy.[7]

3. Identification

As mentioned in introduction, the empirical analysis is based on the estimation and comparison of hazard functions. The whole analysis is very much in the spirit of a corporate demography approach, thus concentrating its attention on individual firms, and following closely birth, life and death events. Identification of the possible effect of the credit market reforms, along the line of the conjecture described above, requires a 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, maintaining *all-else-equal* conditions, analyze any difference in the patterns of mortality among three different sets of firms: those that were born and lived in times prior to the reform, those that were born prior to the reform but that experience the new environment at some point in their life, and those firms born instead after the reform. The Census 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 1, Panel A-B, illustrates the identification strategy. What would survival look like in years before credit constraints are relaxed? We can estimate that by taking the records of all pre-reform vintage firms for the years lived before the reform (the word "vintage" refers to the experience of the birth event). Assume the corresponding hazard function for this set of firms is the one arbitrarily depicted as the dotted line in Figure 1, Panel A. The hazard estimates for this first set of firms serves as a convenient benchmark to compare the effects of the credit reform.

What should the hazard function look like for firms of pre-reform vintages, but estimated in years after the reform? Given the positive effects associated with easier

[7] In practice the distinction between intrastate branching deregulation and interstate banking deregulation is hard to tease out, as states have typically followed a common path of intrastate deregulation first but accompanied in short sequence by interstate opening. I have dealt with this issue and I discuss it in the robustness section.

access to credit, after the reform their hazard function should be shifted down from that estimated using pre-reform records (Figure 1, Panel A, solid line).

Now for the third set of firms, what would the hazard function look like for those born under the new credit environment? According to my conjecture, the positive effect on survival from more credit availability should be offset by the fact that this set of firms is drawn by a population of lower quality. The hazard function for firms of post-reform vintages could therefore lie somewhere above that for pre-reform vintage firms in post-reform years (middle dashed line in Figure 1, Panel B). Since the new credit environment is common to both post-reform vintage firms and to pre-reform vintage firms in post reform years, all else equal any difference in hazard between the two subsets, as captured by the difference between the solid, bottom line and the dashed line in the middle, should then capture the demand side effect, i.e. the fact that the reform also modified the quality distribution of the pool of applicants. If the conjecture is wrong, and there is no impact on the quality distribution, then we should not expect to find any significant difference between the two sub-groups, and the dashed line in the middle should overlap with the solid line.

Note that the identification strategy is 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 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. This separate estimation is feasible with the dataset used here because the credit reform occurs in different states at different points in time, so that I can find significant clusters of records at each year of age that had either not experienced the reform or that had experienced the reform. I present further robustness tests to potential survival bias in section 6.5.

More formally, this difference-in-difference estimation approach can be seen through the specification of the survival model. Let

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

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 (again, 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:

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

The first two terms on the right hand side capture the differential effects on the three sets of firms: $Reform_{sy}$ is an indicator variable equal to one starting one year after interstate banking deregulation takes place in state *s*.[8] 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

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

absorbing common factors of variability in the data. Details on the specific variables are in the results section.

4. 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.

Kerr and Nanda (2008) have made use of the same dataset, but there are fundamental distinctions in scope and methodology. In their work the focus is on entry, although they make use of the firm level information to construct the separate clusters of entrants on the basis of their ex-post length of survival. In this paper, instead, the focus is not just on entry but on survival analysis more broadly, and for that reason I make a much more intense use of the micro dimension of the dataset. 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.[9]

5. Preliminary investigation

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 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

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[9] Summary statistics are available upon request.

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.

5.1. 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 2, 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 2, 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:

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

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 3 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.

6. 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 4, 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 4, 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), but also by a change in shape, as indicated by the significant difference in the estimated γ's. The hazard functions for these two final model specifications are displayed in Figure 4, Panel C and D.

6.1. Economic significance

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. The effect of the reform is therefore reflected in large changes in firms' life expectancy, and significant throughout their entire life span.

Another way to appreciate the extent of the impact of credit market conditions on firms' population dynamics 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.

Overall, this evidence indicates that these changes in firms' life expectancy resulting from different credit conditions are big. If expectations on survival enter in the calculation to determine future capital expenses and innovation investments – as it should – then this evidence establishes a very clear channel through which financial conditions affect long term economic activity.

6.2. 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 5, 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 other studies (see, e.g., Cetorelli and Strahan, 2006, Kerr and Nanda, 2008). 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 Zingales (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,[10] 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 reform. We display the computed conditional hazards for these two more selected sub groups of firms in Figure 6. 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.

6.3. 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

[10] 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).

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 theta 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.

6.4. 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.

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.

6.5. 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, as said earlier, the identification strategy should be protected from basic survival bias, since we compare the hazard of mortality of firms in the three different groups but always for the same age. However, a possible concern is that there may still be a bias due to heterogeneity among pre-reform vintage firms. Consider the comparison of firms at age ten. 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 firms 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, 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 mis-classifying 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.

7. Alternative theories

There are existing theories predicting important effects on firms' dynamics. Could they also 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 that is all it is happening, i.e. there is no change in the composition of demand, then 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.

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.

8. Conclusions

To the best of my knowledge, this is the first paper that documents the role of credit conditions on the life expectancy of non-financial firms. The approach taken in this work and the empirical results illuminate the mechanic of the finance-growth relationship. Besides entry, expansion and growth ultimately really depends on who is staying in and who is transitioning out of production activities. The analysis suggests that the answer to this question is not that straightforward. Changes in financial conditions significantly impact the "engine" of growth; that is where – within a population of firms – productive investments and innovation is more likely to come from. This argument is well known in the growth literature, and it is indeed an important component of endogenous growth modeling in the spirit of Aghion and Howitt (1992) and its many offsprings.[11] However, this is, to my knowledge, a dimension of analysis related to the research work on *finance and growth* that has not been tackled before.

The evidence has shown that credit market reform has a deep impact on firms' life cycle dynamics. 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. The change is not just reflected in a proportional "shift" of the hazard function, but also in a significant change in its overall shape. This evidence is consistent with the credit reform – and its changes in credit supply - having an impact on the demand side as well, with important changes in the population of those who aspire to becoming an entrepreneur.

[11] The argument that life expectancy affects firms' productivity is also central in other fields. See, e.g., Melitz (2003).

The analysis also confirms the intuition of previous work, about the importance of entry (Black and Strahan, 2002) and the importance of the democratization effect highlighted in Kerr and Nanda (2008). Indeed, the results here do not contradict the fact that enhanced credit supply post reform 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.

In fact, 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.

At the same time, however, better credit conditions also allow deserving pre-reform firms to live longer lives instead of being exposed to premature death because of external causes linked to credit availability. This is another separate potential effect on growth.

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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	0.1407	0.4122	0.1978
	(0.0024)	(0.0022)	(0.0034)	(0.0055)
Reform$_{sy}$ • Founding time$_{jisy}$			-0.4163	-0.1483
			(0.0031)	(0.0054)
Constant	1.9407	1.8595	1.9196	1.8595
	(0.0020)	(0.0017)	(0.0019)	(0.0017)
gamma	0.5620	0.4538	0.5337	0.4498
	(0.0006)	(0.0021)	(0.0006)	(0.0021)
gamma_reform		0.6005		0.7558
		(0.0024)		(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	0.2330	0.2445	0.2426	0.1396	0.2266
	(0.0051)	(0.0052)	(0.0051)	(0.0051)	(0.0065)	(0.0063)
Reform$_{sy}$•Founding time$_{jisy}$	-0.1795	-0.1711	-0.1846	-0.1876	-0.3742	-0.1575
	(0.0050)	(0.0051)	(0.0049)	(0.0049)	(0.0051)	(0.0061)
Employment$_{jisy}$	0.0021	0.0011	0.0022	0.0022	0.0020	
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	
Share$_{jisy}$	1.2663	1.2493	1.0815	1.2938	1.5131	
	(0.0633)	(0.0630)	(0.0612)	(0.0635)	(0.0658)	
Employment at birth$_{jisy}$		0.0012				
		(0.0000)				
Industry share$_{iy}$			1.2158	1.0312	1.0473	
			(0.0325)	(0.0419)	(0.1036)	
Industry share in state$_{isy}$			-1.0290	0.1087	-0.0147	
			(0.0177)	(0.0313)	(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	1.8083	1.7845	1.7249	2.2146	1.9158
	(0.0018)	(0.0018)	(0.0031)	(0.0034)	(0.0124)	(0.0022)
State, Industry, Year FE	No	No	No	No	Yes	No
gamma	0.4416	0.4405	0.4410	0.4394	0.3998	-0.8198
	(0.0021)	(0.0021)	(0.0021)	(0.0021)	(0.0023)	(0.0021)
gamma_reform	0.7209	0.7239	0.7157	0.7138	0.6448	0.5012
	(0.0037)	(0.0037)	(0.0037)	(0.0037)	(0.0042)	(0.0040)
gamma_reform•founding time	0.5137	0.5143	0.5113	0.5106	0.4966	-0.3709
	(0.0034)	(0.0034)	(0.0034)	(0.0034)	(0.0036)	(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, 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.

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

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

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

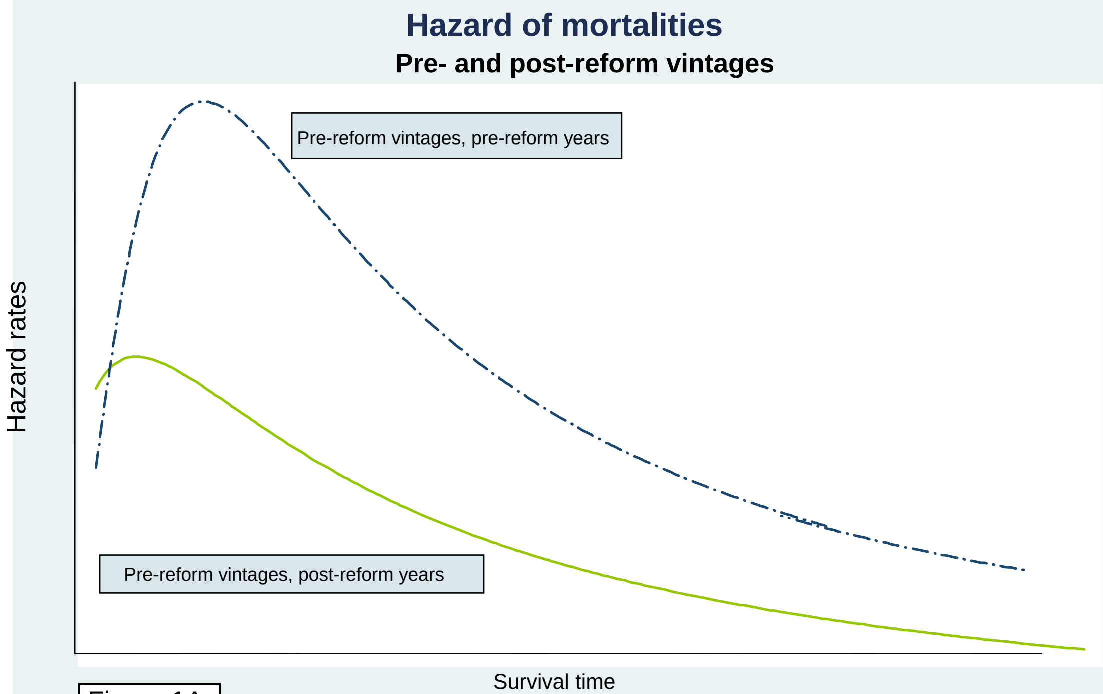
Hazard of mortalities
Pre- and post-reform vintages
Pre-reform vintages, pre-reform years
Pre-reform vintages, post-reform years
Hazard rates
Survival time

Figure 1A

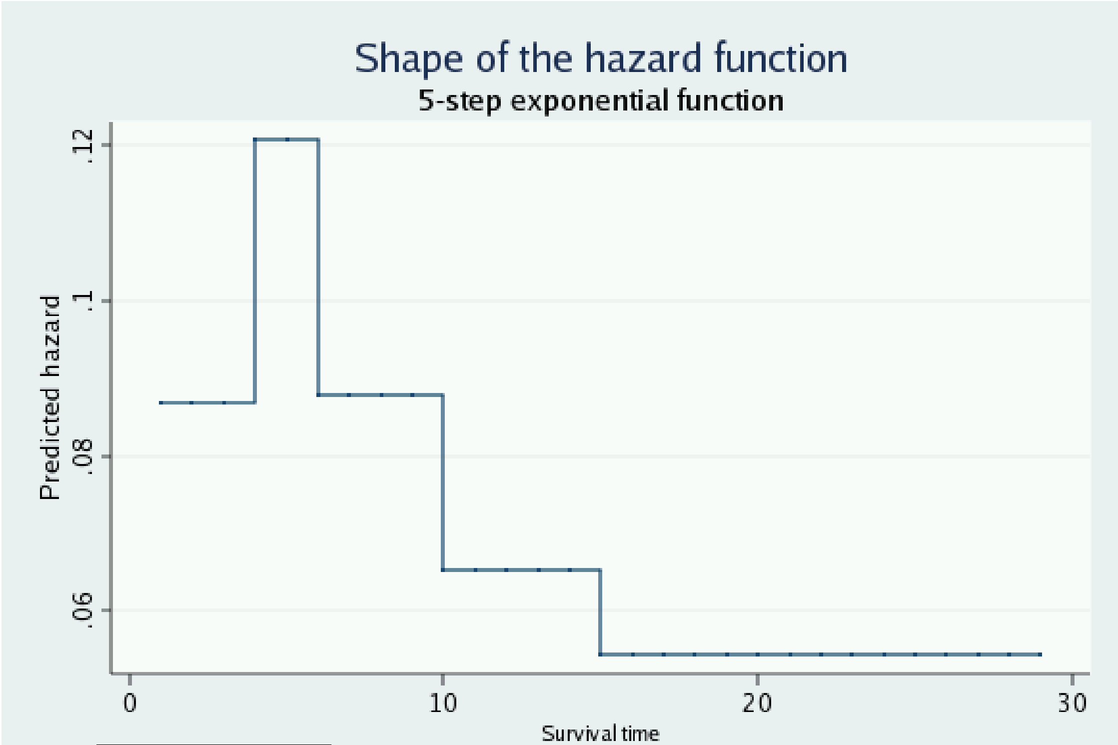

Figure 2A

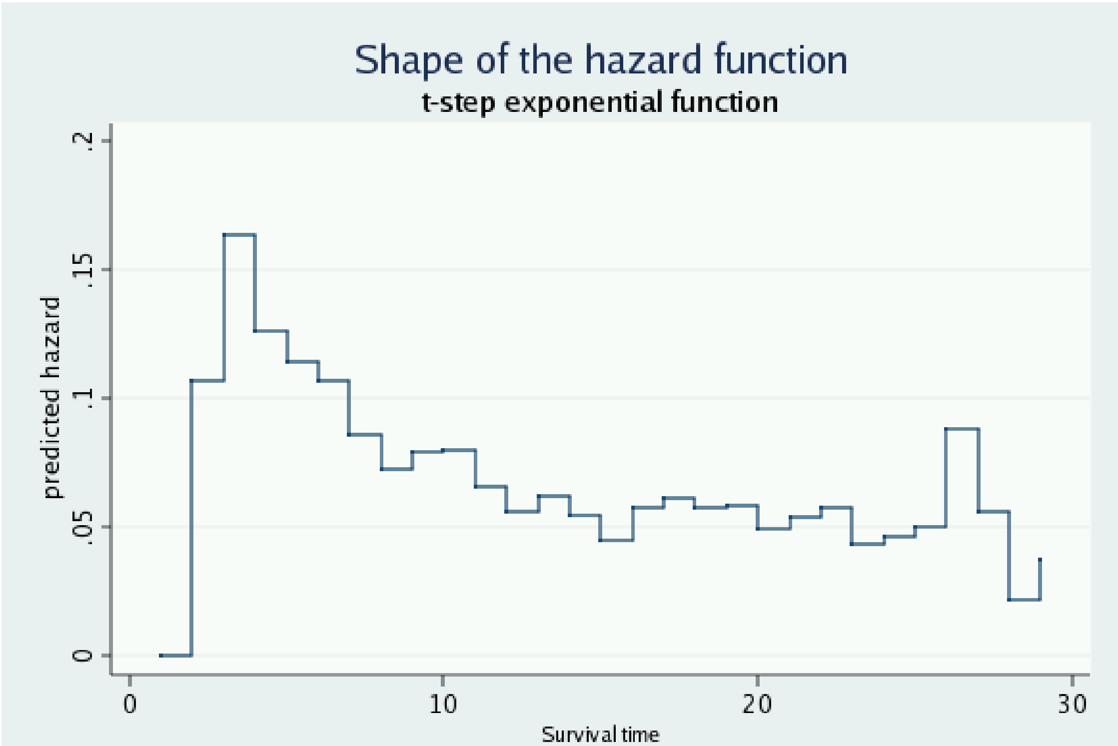

Figure 2B

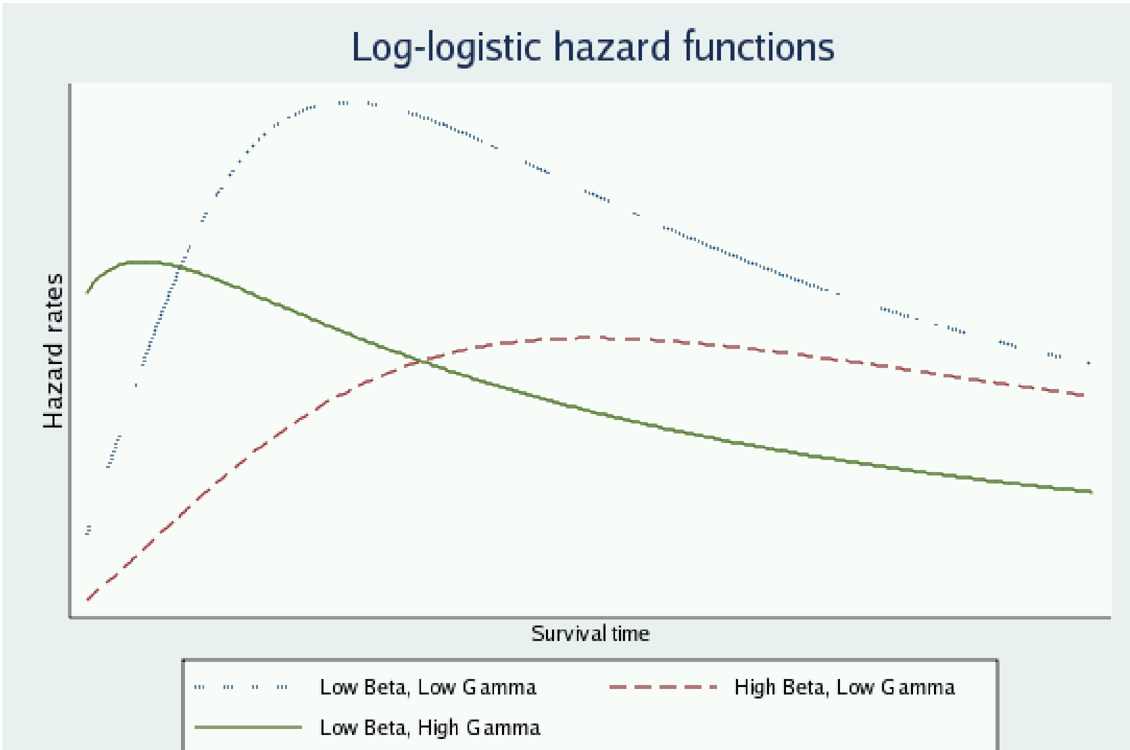
Log-logistic hazard functions
Hazard rates
Survival time
Low Beta, Low Gamma
High Beta, Low Gamma
Low Beta, High Gamma

Figure 3

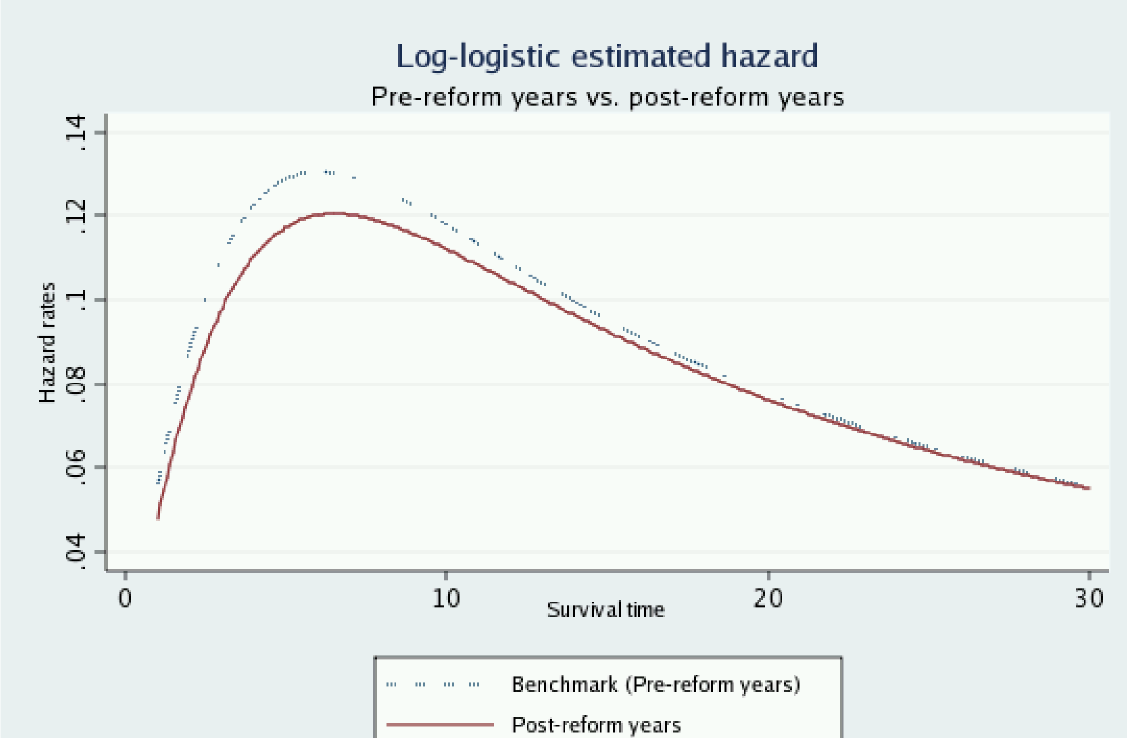

Figure 4A

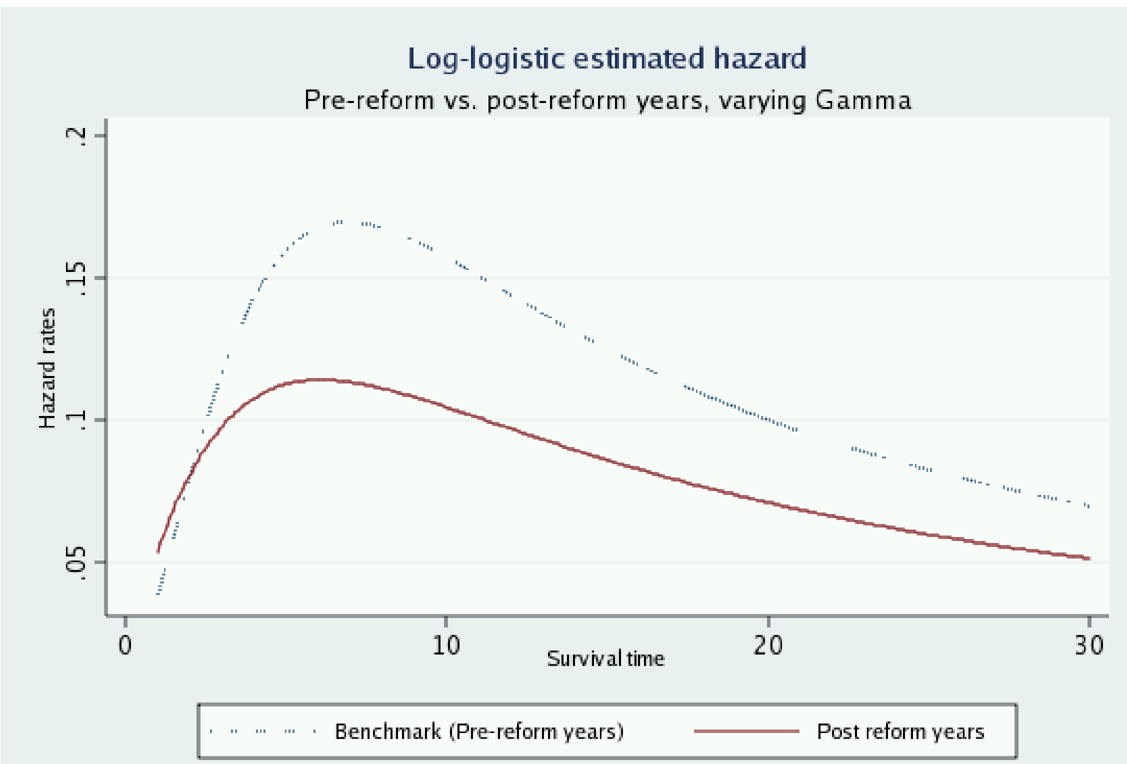

Figure 4B

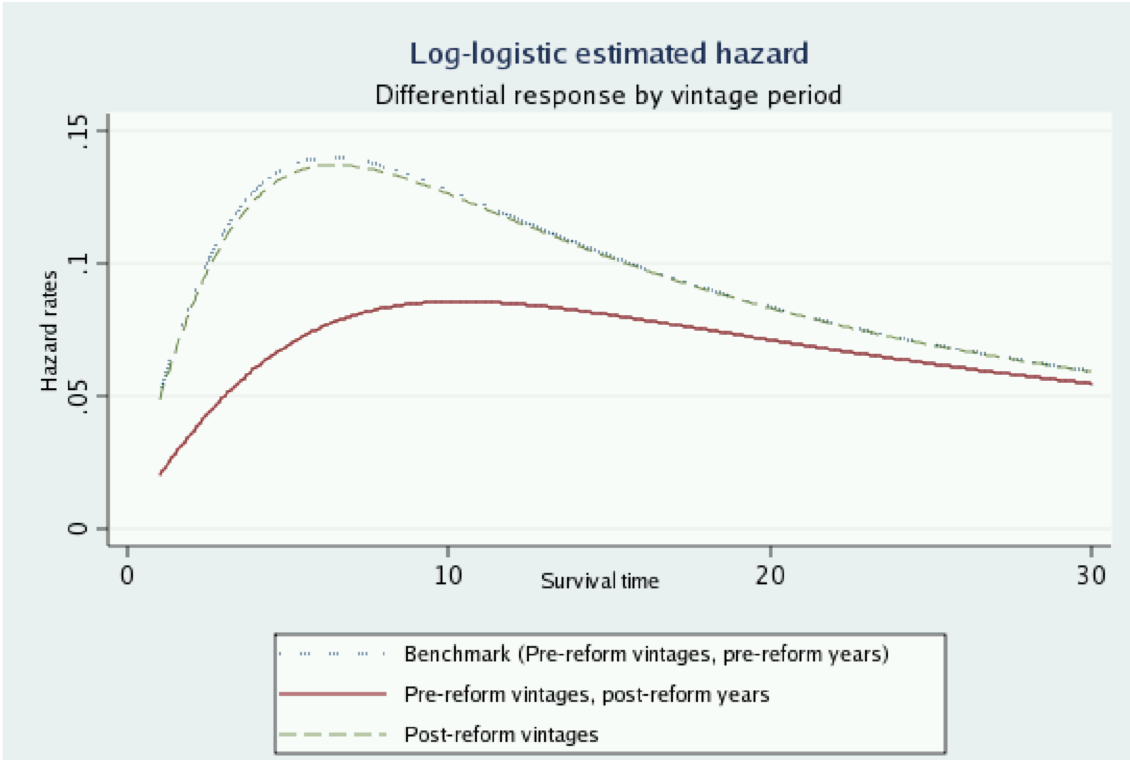

Figure 4C

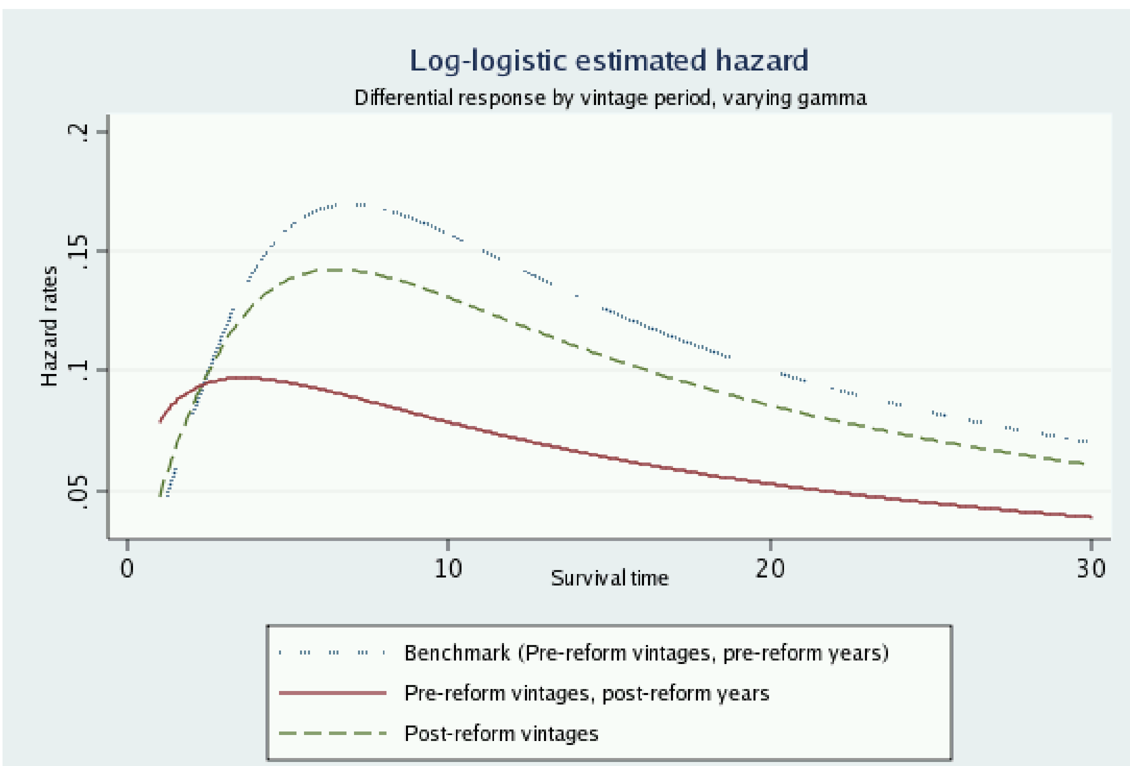

Figure 4D

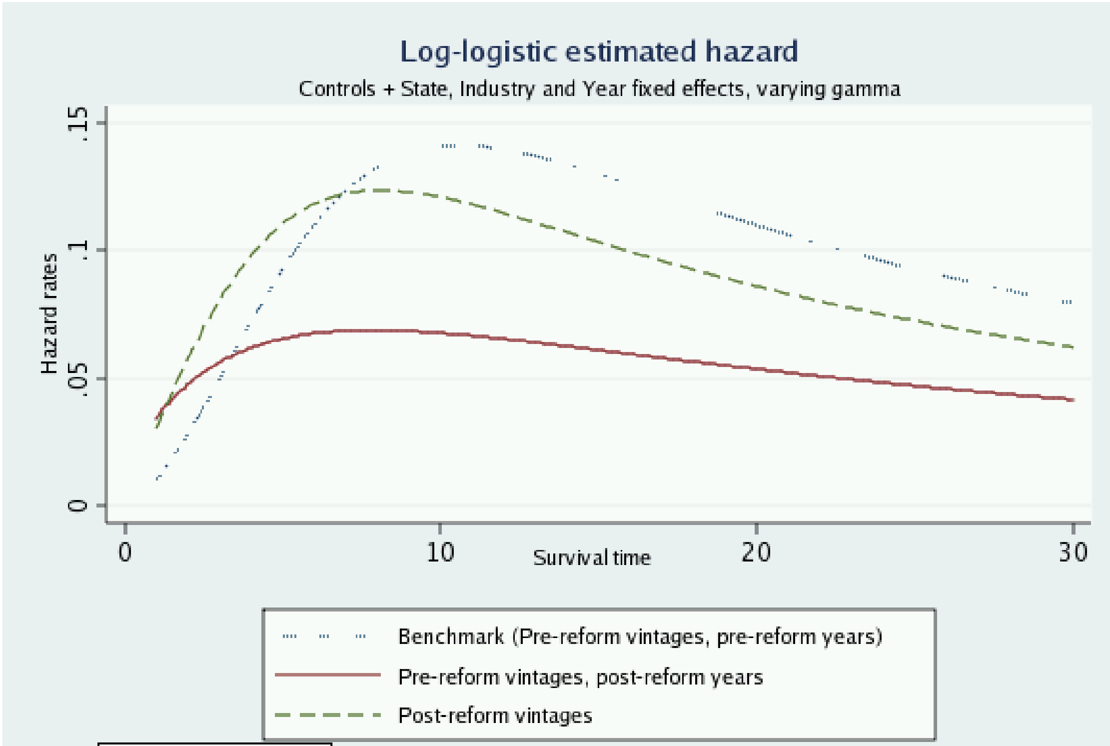

Figure 5

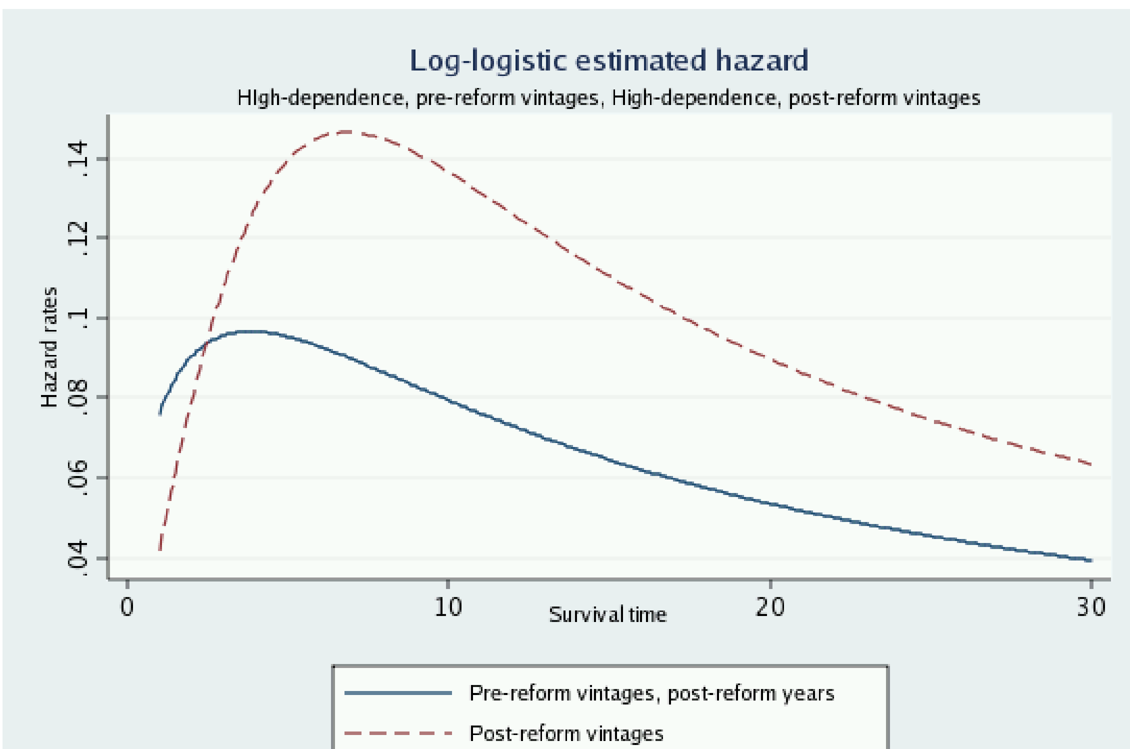

Figure 6