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ENTRY BARRIERS AND FIRM DYNAMICS

Evidence from the Minimum Capital Requirement in Finland

Master's Thesis
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Abstract

In this thesis, I evaluate the effects of the minimum capital requirement on firm dynamics in Finland. Specifically, I look at the number, survival and employment growth of entrant firms. I also explore the dynamics of incumbent firms and overall employment. I find evidence that lowering the minimum capital requirement causes an increase in the number of limited companies created and a net increase in the number of jobs created by young limited companies. The effect is potentially very large, though significant uncertainty remains over the magnitude. As for the effects on incumbents and total employment, the evidence is inconclusive.

The minimum capital requirement is a regulation that obliges the founder(s) of a limited company to invest at least a certain amount towards the share capital of their firm. As such, it serves as a potential barrier for new entrepreneurs, especially considering the fact that most limited companies are founded using the minimum required amount of capital. Recent research has noted a declining trend in entry rates and the economic contribution of young firms across the developed world, and this thesis also provides descriptive evidence that suggests Finland is experiencing similar trends. Policy makers may look to the lowering of entry barriers as a response to such trends. Perhaps as an example of this, the minimum capital requirement has indeed been recently removed entirely in Finland, with an explicit goal of easing the setting up of businesses. With this in mind, I look at the effects of two previous reforms in the minimum capital requirement.

Between its introduction in 1980 and removal in 2019, the minimum capital requirement went through two major alterations: first an increase from 15 000 Finnish markkas to 50 000 Finnish markkas in 1997 and then a decrease from 8 000 euros to 2 500 euros in 2006 (after a conversion from 50 000 mk to 8 000 €). Comparing industries that I expect to have been more affected by the reforms to those industries for which I expect the effects to be relatively small, I estimate that the latter reform led to the creation of roughly a thousand new limited companies per year in 2007-2017 with little drop in average performance, but find no evidence for any effects of the former reform. There might be several reasons for the conflicting results, for instance data limitations in case of the 1997 reform and potential confounding factors inflating the estimates of the 2006 reform. However, some of the descriptive evidence shows patterns that are strongly suggestive of the existence of an effect in both cases.

Overall, the evidence suggests that lowering entry barriers, at least the minimum capital requirement, is a potential tool for increasing employment, though there might be other negative effects - for instance on productivity or the protection of consumers and debtors - that are not explored here. Furthermore, it alone is unlikely to turn around the trend of falling entry rates and economic contribution of entrants.

Keywords firm dynamics, entry barriers, minimum capital requirement, entrepreneurship

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Tiivistelmä

Tässä tutkielmassa arvioin vähimmäispääomavaatimuksen vaikutuksia suomalaiseen yritysdynamiikkaan. Tarkastelen uusien yritysten perustamismääriä sekä perustettujen yritysten selviytymistä ja työllisyyskasvua eri vähimmäisvaatimusten aikana. Lisäksi selvitän markkinoilla jo olevien yritysten dynamiikkaa sekä kokonaistyöllisyyttä. Tulosteni mukaan vähimmäisvaatimuksen alentaminen nostaa perustettujen yritysten määrää huomattavasti vähentämättä merkittävästi niiden suorituskykyä selviytymisen ja työllisyyskasvun valossa. Vaikutus on mahdollisesti hyvinkin suuri, joskin sen arviointiin liittyy paljon epävarmuutta. Markkinoilla jo olevien yritysten sekä kokonaistyöllisyyden osalta näyttö vaikutuksista on epäselvää.

Vähimmäispääomavaatimus on säännös, jonka mukaan osakeyhtiön osakepääomaan on sijoitettava vähintään tietty summa yritystä perustettaessa. Näin ollen, se toimii mahdollisena esteenä uusille yrittäjille, erityisesti ottaen huomioon, että suurin osa osakeyhtiöistä on perustettu vähimmäispääomalla. Viimeaikainen tutkimus on huomioinut monissa kehittyneissä valtioissa laskevan trendin uusien yritysten osuudessa taloudellisesta toiminnasta, ja tässä tutkielmassa esitetyn näytön perusteella trendi koskee myös Suomea. Vastauksena poliittiset päätöksentekijät saattavat koettaa madaltaa markkinoille tulon esteitä. Mahdollisesti esimerkkinä tästä vähimmäispääomavaatimus poistettiinkin Suomessa hiljattain kokonaan, ja poistamista perusteltiin yritysten perustamisen helpottamisella. Tätä kehitystä vasten tarkastelen tässä tutkielmassa kahta vähimmäispääomavaatimuksessa aiemmin tapahtunutta uudistusta.

Ennen kuin 1980 luvulla voimaan astunut vaatimus poistettiin heinäkuussa 2019, sitä ehdittiin muuttaa merkittävästi kaksi kertaa: vuonna 1997 kun se nostettiin 15 000 markasta 50 000 markkaan ja vuonna 2006 kun se laskettiin 8 000 eurosta 2 500 euroon (Suomen siirtyessä euroon vaatimus muuttui 50 000 markasta lähes samaa summaa vastaavaan 8 000 euroon). Vertaamalla toimialoja, joille oletan vähimmäispääomavaatimuksen olevan merkittävämpi este toimialoihin, joille puolestaan oletan sen olevan verrattain matala este, arvioin jälkimmäisen uudistuksen johtaneen vuosittain noin tuhannen uuden osakeyhtiön syntyyn vuosina 2007-2017. Vuoden 1997 uudistukselle en puolestaan löydä näyttöä minkäänlaisista vaikutuksista. Ristiriitaisille tuloksille on useita mahdollisia selityksiä, kuten vajaa tilastoaineisto ennen vuotta 1997 ja vuoden 2006 tuloksiin mahdollisesti vaikuttavat vääristävät tekijät. Osa esitetystä kuvailevasta aineistosta viittaa kuitenkin vahvasti siihen, että vaikutus on molemmissa tapauksissa olemassa.

Kaiken kaikkiaan tulosten valossa vaikuttaa siltä, että esteiden alentaminen – ainakin vähimmäispääomavaatimuksen tapauksessa – on mahdollinen työkalu työllisyyden kasvattamiseen, joskin tällä saattaa olla tämän tutkielman ulkopuolelle jääviä muita negatiivisia vaikutuksia esimerkiksi tuottavuuteen tai kuluttajien ja velkojien suojaan. On toisaalta myös epätodennäköistä, että tällaiset toimet yksinään kääntävät uusien yritysten laskevia trendejä.

Avainsanat yritysdynamiikka, markkinoille tulon esteet, vähimmäispääomavaatimus, yrittäjyys

Contents

1 Introduction

This thesis presents evidence on effects of the minimum capital requirement (MCR for short) on firm dynamics - the entry, growth and exit of firms - by making use of two reforms of the regulation during its history in Finland: a decrease in 1997 and an increase in 2006. The MCR is a specific kind of entry barrier which requires a certain amount of initial investment upon the founding of a company. In the Finnish context this has specifically meant investment into the share capital of a limited company. The evidence presented here broadly suggests that the MCR has been a meaningful barrier in the sense that lowering it increases limited incorporation. The overall impact of the regulation, though, is inconclusive, and it is not clear that the results can be extrapolated to other kinds of entry barriers.

Entrepreneurship is traditionally seen as an important part of a healthy economy, with young firms often hailed as major sources of job creation and innovation via Schumpeterian creative destruction. In this view, it is alarming that recent research on firm dynamics has found a trend of falling entry rates and job creation by young firms around the developed world. Decker, Haltiwanger, Jarmin, and Miranda (2014) observe a fall in the U.S. entry rate starting in the late 1980s, and find that it is not being offset by an increase in the size of the entrants. An OECD study shows that similar trends are also present at least for many European countries, Brazil, Canada and New Zealand (Calvino, Criscuolo, and Menon, 2015). Descriptive evidence presented in this paper suggests that Finland also shares these trends.

To spur entry in response to the falling trends, governments may try to bring down the barriers of setting up a business. One such barrier that has existed in Finland and multiple other countries is the MCR. First introduced in Finland in 1980, a recent development has indeed seen it removed entirely, starting July 1st 2019, with a stated purpose of making it easier to set up small businesses (Parliament Of Finland, 2018). In between, its level has been significantly altered in two reforms: in 1997 the requirement was raised from 25 000 to 50 000 Finnish markkas (corresponding approximately to a change from 2 500 to 8 400 euros according to the Parliament Of Finland (2018)) and in 2006 it was lowered from 8 000 euros to 2 500 euros. In this thesis, I make use of these two reforms and a long panel of Finnish firms stretching from 1988 to 2017 to evaluate the effects of the MCR on Finnish firm dynamics. Specifically, I look at the number of entrants and their post-entry performance in terms of survival and employment growth, as well as the performance of firms already in the market when a reform happens, and overall employment by both entrants and incumbents.

Using a differences-in-differences design with heterogeneous treatment intensity across industries similar to previous empirical research on entrepreneurial activity by Klapper, Laeven, and Rajan (2006) and Hombert, Schoar, Sraer, and Thesmar (2017), I find mixed evidence for the effects of the MCR. Following Hombert et al. (2017), I group industries based on a measure of treatment intensity - in this case the share of entrants with minimum capital - and compare the changes in several outcomes from before to after the reforms between the groups. The main estimates for the 2006 reform imply around a 1 000 new limited companies per year attributable to the reform, which is a significant number considering that in 2005 there were roughly 100 000 limited companies operating in Finland. However, there is likely bias in these estimates in both directions making the true magnitude of the effect highly uncertain.

When it comes to the performance of entrants, I find a slight decrease in survival but no systematic effect on growth. Meanwhile, for the effects on incumbents and total employment, it seems unlikely that the identifying assumption holds. Hence, while there appears to be a positive effect when it comes to the contribution of entrants, making definitive claims about the overall effects of the reform remains an elusive goal.

A question mark is also raised by the estimates for the 1997 reform, which one would expect to go systematically to the opposite direction from the 2006 case if the MCR was indeed the cause. However, this does not happen for any outcome, and the estimates are statistically insignificant at the 95% confidence level for nearly all of them. Due to data limitations, the treatment intensity measure used for the 1997 reform is less accurate than the one used for the 2006 reform, which might explain the failure to detect

effects, though it might also be that there is nothing to detect, which may or may not be a problem for the 2006 results depending on whether the lack of effect is due to the specifics of the 1997 economic environment or something more general. The latter concern, though, is alleviated by the fact that some of the descriptive evidence presented in section 2.5 strongly suggests that an effect exists in both cases, even if it's hard to convincingly say what the magnitude of the effect is. The same evidence unfortunately also suggests that the lowering of MCR-type entry barriers is an insufficient response to the falling trends of entry, insofar as one sees them as a problem in need of addressing.

This thesis proceeds in the following order: the next section establishes an empirical context, reflecting Finland's situation to trends in firm dynamics across the developed world and presenting previous research on the effects of entry barriers as well as some robust facts that are important to keep in mind in any discussion on firm dynamics. Section 3 reviews theoretical results on entrepreneurship and firm dynamics, drawing implications that both give predictions on what one would expect to observe in the data, and affect the way the findings should be interpreted. Section 4 explains in detail the strategy to identify the effects of the MCR on the outcomes of interest and presents the results of the estimation, evaluating them in the light of the theory discussed in section 3. Finally, section 5 discusses the implications, caveats and further questions arising from the results, and section 6 provides a concluding summary.

2   Empirical Context

This thesis adds mainly to two strands of literature: empirical research on entry barriers and descriptive research on firm dynamics. Previous empirical research on entry barriers has largely focused on static analysis of their effects on various indicators. Djankov, La Porta, Lopez-De-Silanes, and Shleifer (2002) describe the procedures required to set up a standardized[1] firm in 85 countries all over the world and use the data to evaluate theories of regulation. Especially they compare the public interest view, which posits that a government pursuing social efficiency counters market failures through regulation, against the public choice view, which sees the government as rent-seeking and hence regulation as inefficient. They find that stricter regulation of entry is not associated with higher quality products, better pollution records, better health outcomes or more competition, but is associated with higher levels of corruption and larger relative informal sector, and hence conclude that the evidence supports the public choice view.

However, van Stel, Storey, and Thurik (2007) find a lack of direct evidence for the effect of entry regulations on entrepreneurship and point out that "given the explicit link made by Djankov et al. between the speed and ease with which businesses may be established in a country and its economic performance - and the enthusiasm with which this link has been grasped by European Union policy makers - our findings imply that this link needs reconsidering". Examining the effect of four entry regulation variables - procedures, time, cost and minimum capital requirements - separately on the rates of nascent entrepreneurs (i.e. people actively taking steps to set up a business) and owners of young businesses for an unbalanced panel of 39 countries, they find that only the MCR has a significant negative effect. Specifically they identify the MCR as lowering the rate of nascent entrepreneurs and hence indirectly, though crucially not directly, the rate of owners of young businesses. This suggests that a higher minimum capital requirement turns potential business founders off from pursuing entrepreneurship altogether but does not act as an insurmountable obstacle for those already committed to the process of founding a business. Of course, it does not mean that the barrier is merely psychological, as the finding is readily explained by materially constrained people recognizing their situation before ever starting the process.

Klapper et al. (2006) make use of variation in the "natural" propensity for entry (proxied by entry rates in the United States) between industries to ask if the industries with higher natural entry propensity have lower entry rates in countries with higher entry costs. Further, they examine the effect of the entry regulations on the productivity growth of older incumbent firms, arguing that on the one hand indiscriminately screening out young firms may lessen the threat of Schumpeterian creative destruction faced by the incumbents and make them lazy, but on the other hand, if the regulations are an effective screening mechanism, the incumbent firms that have themselves passed through should be more competent. They find that countries with higher entry costs exhibit lower entry rates and lower productivity growth by incumbents in industries most affected by entry regulation (those that have low natural barriers to entry). They also find that high entry costs make entrants larger, suggesting that small firms are disincentivized from entering, or have to grow to a certain point without the protection of limited liability (in a state where they remain undetected in their data).

The evidence for the effects of entry regulation on entrepreneurship, then, is inconclusive. While Djankov et al. (2002) find evidence that high costs are associated with weaker economic performance of a country in broad terms, van Stel et al. (2007) and Klapper et al. (2006) disagree on whether entry regulations actually affect entrepreneurship rates, though even the former notes that the MCR does seem to have a negative effect on the entry rate.

Notably, the aforementioned research has focused on cross-country comparisons and has thus not been able to follow the post-entry performance of firms on a micro level. Hombert et al. (2017) on the

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[1]A standardized firm in Djankov et al. (2002) is a firm that performs general industrial or commercial activities, operates in the largest city of the country, is exempt from industry specific requirements, does not participate in foreign trade, does not trade in goods that are subject to excise taxes, is a domestically owned limited liability company, has the higher of (a) 10 times GDP per capita in 1999 or (b) the minimum capital requirement of capital subscribed in cash, does not own but rents land and business premises, has between 5 and 50 employees one month after the commencement of operations all of whom are nationals, has turnover of up to 10 times its start-up capital and does not qualify for investment incentives.

other hand consider the effect of a reform in entrepreneurs' unemployment insurance in France, taking into account not only the entry rate but also dynamic effects. They find that an improvement in the insurance - the lack of which can be viewed as an entry barrier even if it's not a direct cost - increases the number of entrants and overall job creation by entrants, but decreases job creation by small incumbent firms, though not large ones. Meanwhile, the reform does not seem to have a significant impact on the quality of the entrants measured as the probability of hiring an employee or the probability of exiting in the first two years. The methodology of Hombert et. al. serves as an inspiration for the identifying strategy in this thesis, and will be described in more detail in section 4.1.

In sum, while several studies have considered the effects of entry barriers on the aggregate economic performance of a country, or the entry rate, the empirical evidence for their dynamic effects on the firm level is still lacking. The contribution of this thesis then is especially to add to the latter, while specifically studying the MCR, a barrier highlighted by van Stel et al. (2007) as having significant negative effects on the rate of nascent entrepreneurship and by extension, if not directly, the entry rate. While evaluating the dynamic effects of the MCR is the main focus, an additional contribution is to explore the trends in Finnish business dynamism from the early nineties almost to the present day and set them to international context. The rest of this section presents recent descriptive research on firm dynamics - as well as some well established stylized facts - comparing the trends around the world to those calculated for Finland from the data used in this thesis. Before proceeding to that, though, a primer on the methodology of measuring firm dynamics as well as a description of the data used and the institutional background are in order.

2.1   The Data

The core of the data used comes from the business register database of Statistics Finland. For the years 1988 to 2012, the business register includes yearly observations for Finnish business entities that are either employers or liable for the value added tax (or both). To be recorded in the data, a business has to have operated for at least six months in the statistical year in question, and employed more than a half employees (in full time equivalent units) or exceeded the minimum turnover limit. The minimum turnover is set separately for each year and has grown monotonically from 8126 euros in 1995 to 10595 euros in 2012. The period includes a few major changes in the data gathering process. First is the move from the turnover tax system to the value added tax system in 1994, which causes a break in the series of legal entry and exit years. Next, in 1999, a business identifier has been given to natural persons, replacing their personal ID as the primary identifier in the statistics. This causes a major spike of exits before and entries after 1999, a problem that is mostly remedied by retrieving the old identifiers from the 1999 cross section (though a slight spike remains). Finally, and most importantly from the perspective of this thesis, in 2006 the coverage of primary production and real estate businesses has been extended (Statistics Finland, 2012), leading to a significant increase to the number of businesses with their first observation in the year immediately following the second minimum capital reform. To mitigate the possible problems this causes for the entry rate, these industries are excluded for the entire study period.

2013 sees a change in Statistics Finland's information systems and data gathering process to the extent that the statistics are warned to not be comparable to the previous years. However, as the major source of the turnover and employee figures is the same (i.e. the tax administration), the available years 2013 to 2017 are included with this caveat in mind (as a robustness check, all estimations are performed also without these years, which does not substantially change the results - see appendix A.3). A major change in the recording of the statistics is that in addition to the employee and turnover limits, a balance sheet limit has been added in 2013 (Statistics Finland, 2017). Hence it is possible for a firm to be included in the data even if it employs less than a half employees and stays below the turnover limit, as long as its balance sheet exceeds 170 000 euros. To make the entry rates more comparable, all firms that do not fulfill either the employee or the turnover requirements are excluded.

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With the above modifications, the core data becomes an unbalanced panel of 792 227 companies and 6 319 325 observations. In addition to the readily compiled Statistics Finland data, which provides the figures for employment and turnover, as well as the numbers of businesses, their legal entry and exit dates and classifications for legal forms and industries, financial statement data directly from the tax administration[2] is used to obtain information on the share capital of the firms. The share capital data is available for 2002-2016, a period that notably includes only the second of the reforms under study, leading to the need to take an indirect approach when considering the 1997 reform. It covers 93.6% of the limited companies entering between 2002 and 2016 - in total 117 854 firms. However, for 19 784 of these firms, the observation for share capital in the entry year is missing. Since most firms (around two thirds in the data) never change their share capital, I use the next available observation for those missing the first share capital.

2.2 Institutional Background

The institutional changes that are considered are two reforms of the law on limited companies, both of which included an alteration in the level of the minimum capital requirement. To help the reader understand the meaning of those alterations, this section provides a brief introduction to the legal forms under which one can operate in Finland and the larger context of the reforms.

Legal forms of business entities in Finland

The Statistics Finland classification identifies 22 separate categories of legal forms for business entities in Finland (Statistics Finland, 2012). Based on the classification used by the tax administration, Statistics Finland's classification combines some rare legal forms under the same category. However, most of these legal forms are relatively few in numbers and do not represent what one usually considers a competitive firm. Hence, in this thesis attention is restricted to the four most common forms which are identified separately in the Statistics Finland data, and account for 98.4% of the observations in the core data. By far the most popular of these are natural persons (toiminimi, tmi) and limited companies (osakeyhtiö, oy). Less popular, but still significant, are general partnerships (avoin yhtiö, ay), and limited partnerships (kommandiittiyhtiö, ky).[3] Figure 1 plots the relative shares of these legal forms over the study period. Notably, limited companies have gained popularity over both types of partnerships with the share of natural persons staying quite fixed.

The main differences between the four largest legal forms relate to the number of owners, the liability and representability of the owners and the costs of setting up as well as running a business under a given form. Natural persons are individual entrepreneurs who are personally liable for both the agreements the business enters into and the debts of the business. Natural persons have no capital requirements and do not necessarily have to register in the Trade Register, unless they operate in a licensed trade, have permanent premises or employ people outside their immediate family. If they do decide to register, they have to pay a handling fee of 110 euros. They register by filling out a notification form (Y3), and do not need additional documents apart from the receipt showing that they've paid the handling fee. The natural person is their own representative.

General partnerships meanwhile must have at least two owners, called "partners". The partnership can enter into agreements as a separate legal entity, but the partners remain personally liable for the partnership's debts. General partnerships still have no capital requirements, but they must register into the trade register, paying a handling fee of 240 euros. Their bureaucratic cost is also slightly increased by having to include the original Partnership Agreement as an enclosure to the notification form (Y2).

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[2]The tax administration data has been accessed via the VATT Institute for Economic Research

[3]The names and abbreviations inside the brackets correspond to the Finnish legal terms. The English translations are given as used in the Statistics Finland data. However, there might be some differences in the interpretation of the English terms across countries. Furthermore, "natural persons" might also be called "sole proprietorships" and "general partnerships" just "partnerships".

The partnership is represented by the partners. Limited partnerships are otherwise the same as general partnerships, except that they can have "silent partners" who are not personally liable for the partnership's debts. The silent partners have to invest capital into the firm, but there is no minimum requirement for the amount. A limited partnerships must have at least one general and one silent partner. Only the general partners are representatives of the partnership.

Limited companies must have at least one shareholder. The shareholders are not personally liable for the obligations of the company, and the company is represented by the board of directors. Upon registration, the owners of a limited company must pay a fee of 380 euros, and include the original Memorandum of Association and a copy of the Articles of Association as enclosures to the notification form (Y1 and appendix form 1). (Finnish Patent and Registration Office, 2018)

Figure 1
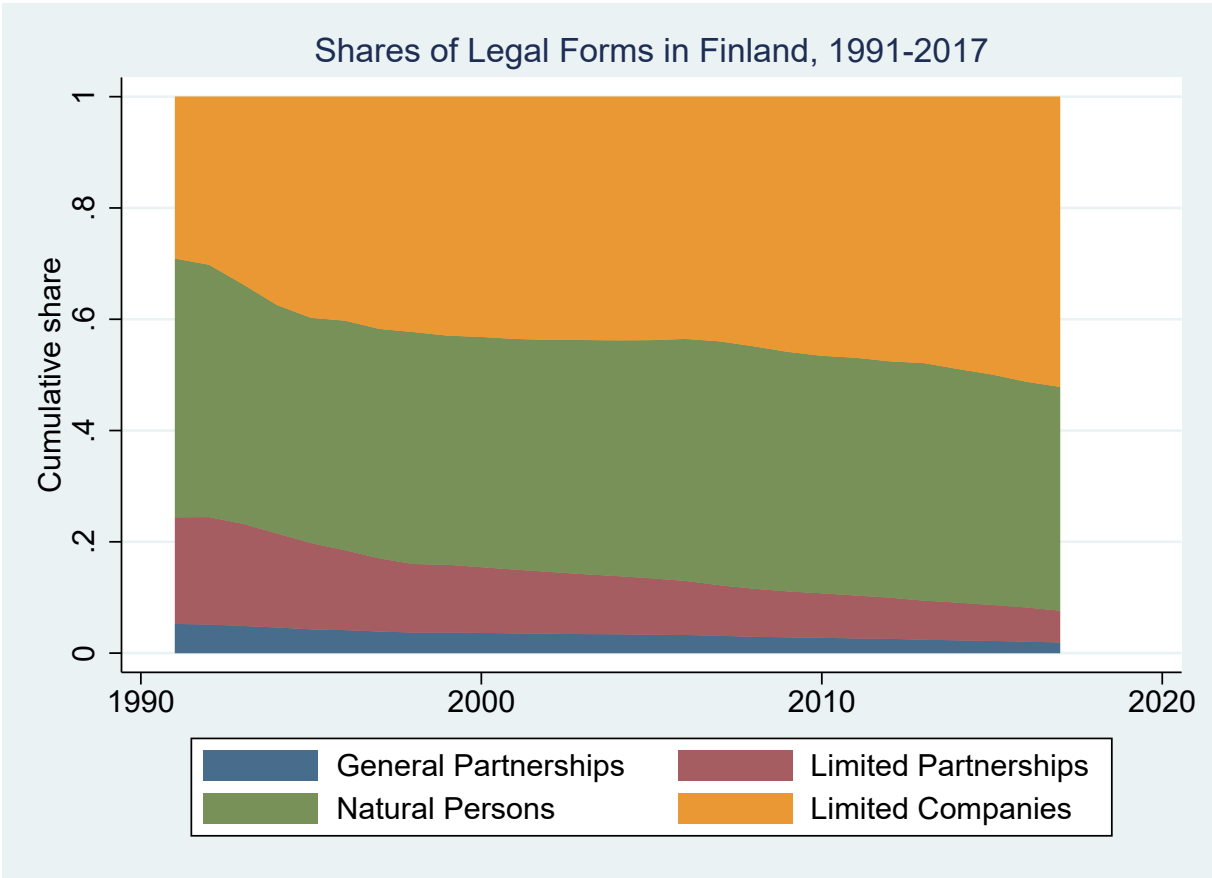
Limited companies used to have a requirement for a minimum amount of capital to be invested in the company, which is the next topic of discussion. There are, however, a few more differences between the legal forms that should be pointed out. Firstly, natural persons are exempted from having to apply double-entry bookkeeping, unless they fill at least two of the following three conditions: over 100 000 euros of total assets, over 200 000 euros of turnover or comparable income and average personnel amounting to more than three (Ministry of Economic Affairs and Employment, 2017). This somewhat lowers the bureaucratic cost associated with setting up a business under the natural person form. Another significant differentiator of the legal forms is their treatment in taxation. Specifically, limited companies are independently liable for income taxation, while the other legal forms are not (Tax Administration, 2016). This means that, in practice, the shareholders of a limited company are taxed twice for the money the company pays out to them: first the profits of the corporation are taxed, then the dividend incomes of the shareholders. Finally, it should be mentioned that the classification of limited companies includes as a subset public limited companies (julkinen osakeyhtiö, oyj), which are limited companies that can (though do not necessarily have to) be traded publicly, and are subject to more regulation than their

private counterparts. Typically though, firms only "go public" after having already existed and grown significantly as private companies, so the distinction will be ignored here as the focus is on the incentives for setting up an entirely new company.

The minimum capital requirement

While the cost of setting up a business as a natural person or a general or limited partnership consists of small registration fees and the time invested to complete the bureaucratic process, limited companies have traditionally also been required to invest a certain minimum amount towards the share capital (osakepääoma) of the firm.[4] Share capital belongs to the firm's restricted equity capital (sidottu oma pääoma) and can be distributed to the shareholders only via a specific process called reduction of the share capital. However, under an MCR regime, the firm must always have the specified minimum amount of share capital, so that the only way to distribute the minimum share capital is via the dissolution and deregistration of the company. (Ministry of Justice, 2012).

Note that share capital is simply an item belonging to the firm's equity (i.e. on the liabilities-side) on the balance sheet. It is usually invested as cash, but it does not have to remain so. Hence, the firm can use the cash generated by the share capital investments as it sees fit, so long as it always has (at least the minimum amount of) share capital, i.e. its assets minus non-equity liabilities must cover at least the share capital (which has to be at least the specified minimum amount).

The MCR was introduced in Finland in 1980 and set to 15 000 Finnish markkas (corresponding approximately to 2 500 euros). When the law was reformed in 1997, the requirement was adjusted for inflation, leading to a new minimum of 50 000 Finnish markkas (approximately 8 400 euros). Then, as Finland was moving from the Finnish markka to the euro, the minimum was converted from 50 000 mk to 8000 € in 1999, and with the new law on limited companies of 2006, lowered to 2 500 € (Parliament Of Finland, 2018). Most recently, starting July 1st 2019, the MCR has been entirely removed (Muilu, 2019).

Hence there have been five changes in the MCR regime in Finnish history: once introduced, twice reformed, once converted from one currency to another and once removed. The data described in the previous section covers three of these changes: the raise from 15 000 mk to 50 000 mk in 1997 and the fall from 8 000 € to 2 500 € in 2006, as well as the conversion from 50 000 mk to 8 000 € in 1999. Though the last mentioned may have introduced a slight change in the MCR in real terms, it will be ignored in what follows for the relatively tiny nature of that change. The 1997 and 2006 reforms on the other hand are quite significant, and provide an excellent opportunity to evaluate effects of changes in entry barriers of a very concrete nature. Evaluating the effects these reforms had on the Finnish economy also provide valuable insights from the perspective of the latest change - the 2019 removal of the MCR - though much data for it is not yet available.

Before proceeding, though, it must be noted that whatever effects are estimated are potentially inescapably confounded by other factors included in the reforms that introduced the changes in the MCR. The 1997 change was a part of a larger reform of the law on limited companies, while the 2006 change belonged to a complete overhaul of the law on limited companies, with a new law replacing the old one. Detailing every single change that these reforms included is far beyond the scope of this text, but the government proposals given at the time provide a reasonable idea of the main objectives.

The proposal that led to the 1997 reform (Finlex, 1996) was said to include necessary changes to execute directives of the European Communities as well as several changes stemming from national interest. It introduced as new terms the division into public and private limited companies (oyj and oy) and proposed extensions to the officially recognized set of financial instruments available to limited companies, namely option privileges, preferred shares and capital loans (note that the fact that these instruments hadn't been coded to law before does not mean they hadn't existed as concepts in the Finnish

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[4]Whenever referring to the minimum capital requirement (MCR), I mean specifically the Finnish version in which the capital must be invested as share capital. The specifics of the requirement may vary across countries.

financial markets). In addition, the proposal aimed to simplify regulations about decision making relating to the gathering and distribution of companies' assets, and extend their disclosure duties. Hence, there were a few smaller changes, some potentially easing and others potentially complicating the operations of a limited company. However, the change in the MCR seems to have been the biggest single piece of the reform.

The 2006 reform is a more complicated case, since in it the entire law was replaced, and the original government proposal (Finlex, 2005) did not even include the MCR change. The main objectives seem to have been to decrease and lighten formalities, and increase the dispositive aspects of the law. There was also a stated special focus on the state of small limited companies, with proposals for both lightened bureaucracy and material easing for small companies. These kind of elements may very well work to inflate the estimates for the effects of the MCR. The identification strategy used in this thesis, described later in section 4.1, is tailored to be sensitive to changes in the MCR especially, but it is likely that the firms that benefit from the lowering of the MCR are mostly small and also benefit from the other reforms aimed at small firms.

That the MCR changes in 1997 and 2006 were parts of larger reforms complicates things, but at the same time emphasizes why it is extremely interesting to have access to two reforms of the same regulation, with the added bonus of them moving into opposite directions, and - nominally speaking at least - with similar magnitudes. Observing significant effects in both cases would increase the likelihood that the MCR is the cause, while only having such results in one or the other would raise the question of whether it is some other regulation that either independently causes the effect or is needed in combination with the MCR.

2.3   Measuring Firm Dynamics

Measuring firm dynamics, specifically the entry and exit of businesses, is notoriously troublesome. A degree of arbitrariness is already included in the decision to focus on the yearly number of entries instead of, for example, quarterly or multiple-year figures. Also, it is by no means clear when a firm enters a market or when should it be counted as having exited. Is the proper starting date the date a business is officially registered, the date any economic activity is first observed or the date the date the business hires its first employee or crosses some arbitrary turnover threshold? Has the business exited only once it goes bankrupt or is officially removed from the register, or when economic activity is no longer observed? What if a business goes on a hiatus and continues economic activity after two years of silence - should it be counted as one entry or two entries and one exit? To muddy the picture further, one has to ask what is a true entry and a true exit: how should one treat mergers, acquisitions and spin-offs, for example, and how can one even detect these in the data?

The literature has taken several approaches to deal with these problems. Decker et al. (2016) for instance use establishment level data to assign an age for each new firm identifier they observe in the data based on the oldest establishment said firm operates (where the startup year is defined as the first year the firm hires an employee). Hence a new firm is only a true entrant if all its establishments are new too. The firm is then allowed to age one year at a time as long as the firm identifier is observed in the data, regardless of any mergers and acquisitions along the way, and firm growth rates are calculated as averages of establishment level growth rates in order to only account for "organic" growth. Azoulay, Jones, Kim, and Miranda (2019) use the same data set to identify true exits and exits by acquisition. This is possible by tracking whether the establishments of a ceasing firm also disappear from the data. Arguing that "the owner(s) of a successful venture might decide to exit by selling their idea and the assets embodied in their firm", they categorize all acquisitions as "successful exits". It may be, though, that some owners are forced to sell even if they'd prefer continuing, in which case it is questionable to call an acquisition a successful exit. However, short of asking the owners themselves, this kind of distinction can be very hard to identify, further illustrating that the interpretation of exit especially is not always

straightforward.

Another novel approach to measuring firm dynamics was proposed by Bendetto, Haltiwanger, Lane, and McKinney (2009). It utilizes employer-employee data to identify entry, exit, mergers and acquisitions via the movements of clusters of employees between firms. Four conditions[5] are formed and their combinations used to identify movement categories. An obvious drawback of this method is the necessity of setting up arbitrary thresholds for the number of employees whose movement is considered significant and the time period in which the transitions need to be observed. It is also practically necessary to leave out the smallest firms (i.e. the majority of firms), since, for instance, observing one individual transitioning from a one-employee business to some other business is hardly evidence of an acquisition - it could just be for example that a solitary entrepreneur exits and gets employed by another company.

Figure 2
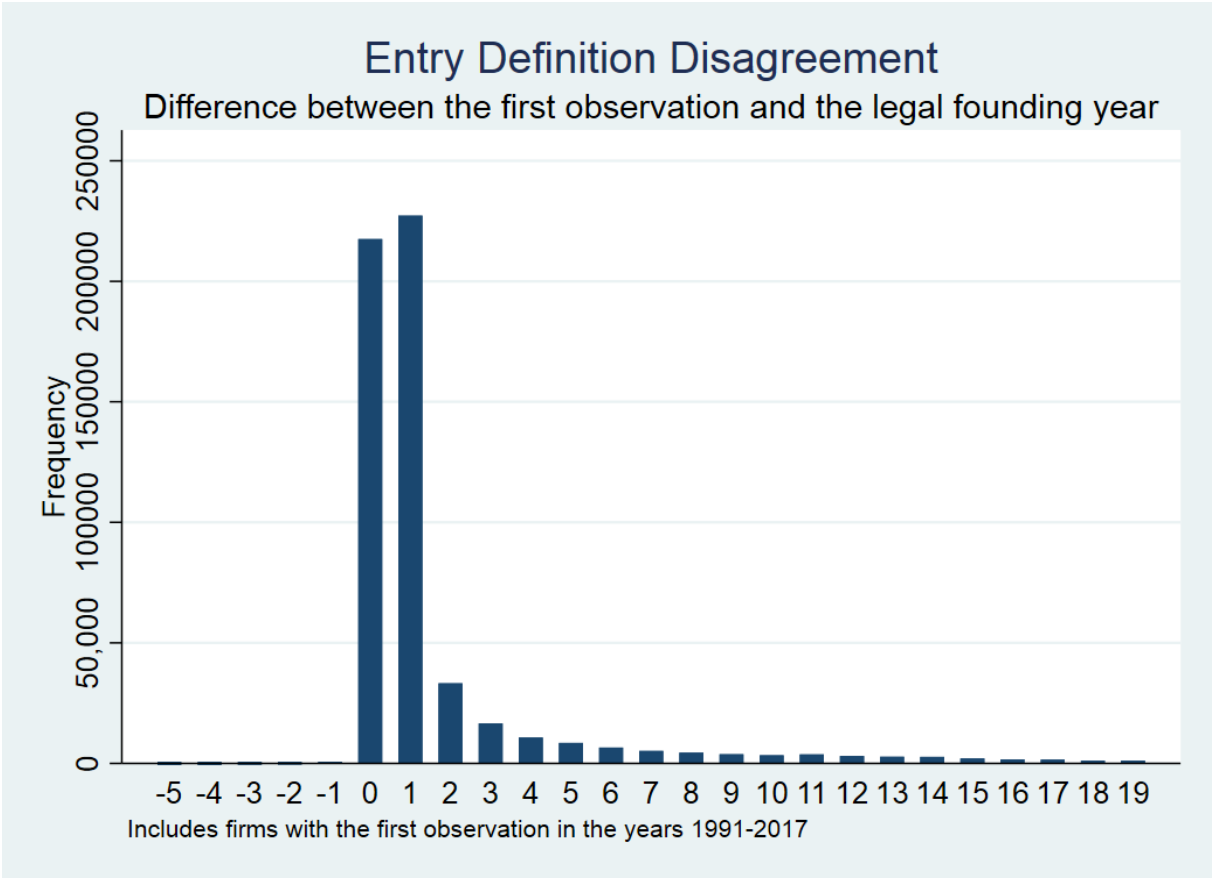
The Statistics Finland data provides information on the firms' legal founding dates. Figure 2 shows a bar chart of the frequencies of disagreements between entry defined as the first time a firm is observed in the panel and the legal starting year assigned to it. A disagreement of zero means that the first appearance is the same as the legal starting year, while a disagreement of 1 implies that the legal starting year is the year before the first observation (and vice versa for -1). For most of the firms the two definitions either agree or the first observation happens in the year following the legal founding, which also makes sense taking into account the fact that a firm has to have operated for at least six months to be included in the panel. Given the definition, one would expect to see no negative disagreements. However, there are some - though relatively few - firms like this. These might simply be erroneous codings of the entry date. On the positive side the disagreements drop sharply after one. However, it takes them a while to completely

[5]1: The predecessor exits (i.e. falls below the employment threshold) and the average employment at the predecessor over the chosen time period is less than 10% of the predecessor's employment prior to the transition, 2: 80% of the predecessor's current employees transition to the successor, 3: The successor is an entrant (i.e. rises above the employment threshold), and the average employment at the successor over the chosen time period is less than 10% of the successor's employment after the transition, and 4: 80% of the successor's employees after the transition came for the predecessor.

die off - in fact, though the graph cuts off at 19, there are a few firms with more than a hundred years of positive disagreement in the data.

Also included in the Statistics Finland data is establishment level information. This allows for the calculation of entries and exits following the methodology mentioned above (Decker et al., 2016; Azoulay et al., 2019). A problem with imposing these additional requirements on the definitions is the question of how one should deal with the firms that do not fulfill these requirements: should they all be considered incumbents? Following the principles of partial identification famously promoted by Charles F. Manski (see e.g. Manski (2015)), one can consider what can be concluded if nothing is assumed about them. In that case, only intervals for the outcomes of interest can be identified: the lower bound is obtained by treating all the firms in question as incumbents, and similarly the upper bound is obtained by treating all of them as entrants or exiters, with the true value guaranteed to lie somewhere in between of the bounds (though, without additional assumptions, the true value is equally likely to lie anywhere within the bounds). Still, it should be noted that this identification region is certain to hold the "true" rate only in the given sample. In order to generalize it to the entire population of interest, one needs to further assume away any selection bias when it comes to missing data.

In what follows, the first appearance of a firm as a statistical unit (i.e. exceeding the thresholds mentioned in the previous section) in the panel is used as the baseline definition of entry. The firm then ages one year at a time until it is never observed in the panel again, at which point it is considered to have exited. The year 1988 is discarded as it is the first year any firm can appear in the panel and hence all firms are by definition entrants. The years 1989 and 1990 are also dropped as they exhibit very high - probably mechanistically inflated - counts of entrants compared to the other years. The last year in the data, 2017, is not considered when describing exits, but is included for other purposes.

The magnitude of the potential error stemming from the definition of entry is illustrated in section 2.5's figure 3, which graphs the entry rate in Finland over the study period. The bolded line corresponds to the baseline definition, i.e. the upper bound of the identifying region. The shaded area is the identifying region, with the lower bound corresponding to a stricter definition of entry where two additional requirements are imposed: all the entrant's establishments must also be observed for the first time, and the disagreement between the legal founding year and the first observation is either zero or one. Note that the denominator - the total number of firms - stays the same between these two ways of defining the entry rate. For most of the study period, the correlation between the upper and lower bounds is nearly perfect, though the difference in magnitude is not negligible. Only during the first years the definitions completely disagree not only on the magnitudes but also on the direction of the trends. This is most likely explained by the fact that a merger of information systems at Statistics Finland has left a lot of empty establishment identifiers in the data for the years 1989-1995 (Statistics Finland, 2015). Because the difference in magnitudes between the baseline and the stricter definition is so large, the stricter definition is used for purposes of robustness checking (the estimates presented in section 4 are mostly quite robust to the definition of entry - see appendix A.4).

For exits, only the baseline definition is used. One reason is that legal exit dates are available only for relatively few of the firms that exit according to the baseline definition. A potential explanation is that many firms may stay in existence on paper for a long time after ceasing economic activity, since the cost of doing so is not high and it leaves open the possibility of restarting the business, or using the same legal entity for entirely different business activities in the future. Even so, one could still make the definition stricter by imposing only the establishment-level criteria. However, in that case, with survival probabilities and growth rates one would also need to obtain another firm identifier that is considered to be the continuation of the firm that doesn't exit by the stricter definition, as the original firm identifier has no more observations. This would require making some arbitrary assumptions. For instance, if a firm with a single establishment is acquired by one with ten establishments, the latter should probably not be treated as a simple continuation of the former. Similarly, one would need to decide what happens to a multi-establishment firm that is broken up and merged into several other companies.

Finally, there are multiple measures one could use when determining the size of a firm. The most common measures are the turnover and the number of employees. The latter will be adopted as the measure in this thesis for a couple of reasons. First reason is technical: since the panel of data used spans nearly three decades, as well as two currencies, inflation becomes a major question for the comparability of size in terms of turnover over time. While inflation adjustment is certainly possible, employment provides a more directly consistent and easily interpretable measure of size. The second reason is more subjective: job creation is a widely shared objective among policy makers, and though I'll remain agnostic as to whether this should be the case, I believe most readers will be more interested in the potential employment effects that might be uncovered than the effects on firms' turnovers. The third an final reason is practical: employment is usually the measure of choice in the previous research used to contextualize Finland's situation below, and hence adopting it makes comparison more straightforward. Thus, from now on, when discussing the calculations made with the Statistics Finland data, "size" and "growth" will refer to the number of employees.[6]

2.4   Stylized Facts on the Growth of Firms

Before exploring the trends highlighted in recent studies on firm dynamics, it is useful to establish a few empirically robust observations which provide crucial context for interpreting everything that will be discussed afterwards. Coad (2007) provides a comprehensive review of the literature on firm growth. This section briefly summarizes a few of the most relevant empirical findings presented therein. All the studies mentioned up to the subtitle "Transformational and subsistence entrepreneurship" are presented as cited in Coad (2007).

The first thing to note is that the size distribution of firms is positively skewed. While there is disagreement on the exact form of the distribution with some authors (e.g Gibrat, 1931; Prais, 1956; Simon and Bonini, 1958) suggesting a log-normal distribution and others favoring the Pareto distribution (e.g. Steindl, 1965; Ijiri and Simon, 1964; 1971; 1974) with some finding that disaggregating the data actually reveals messier multimodal distributions (Bottazzi and Secchi, 2003; Bottazzi et al., 2005), the observation that there are few very large firms and a lot of small firms is not in question.

Relatedly, the relative growth distribution of firms has robustly been observed to be fat-tailed, with high probabilities of both extremely high growth and contraction. Some studies (e.g. Stanley, 1996) find a fit to the symmetrical Laplace distribution, while others (e.g. Reichstein and Jensen, 2005) prefer the right-skewed exponential distribution. Generally speaking, it seems that the median firm exhibits little to no growth while some firms grow and others contract very fast.

Furthermore, there appears to be a negative correlation between firm size and growth - small firms grow (on average and in relative terms) faster than large ones (e.g. Kumar, 1985; Dunne and Hughes, 1994; Bottazzi and Secchi, 2003). Since age and growth are highly correlated, there is also a negative correlation between age and growth. In fact, some authors (e.g. Fizaine, 1968; Evans 1987b) argue that the causal relationship is actually between age and growth rather than size and growth.

Transformational and subsistence entrepreneurship

Focusing especially on the impact of startups in the U.S. economy, Decker et al. (2014) argue that the majority of the growth contribution of young firms is driven by a few very high growth firms while the median entrant exhibits practically no growth, even conditional on survival. This phenomenon is not surprising in light of the stylized facts discussed above. Part of the explanation probably lies in the proposition of Schoar (2010), who, summarizing a wealth of evidence especially from the developing world, proposes that a distinction should be made between at least two different kinds of groups: subsistence and transformational entrepreneurs. The former group are self-employed people or business founders

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[6]Specifically, when speaking of the Statistics Finland data, employees are expressed in full time equivalent units. This means that, for instance, two half-time-employees constitute one full-time-employee.

whose main objective is to provide a living for the entrepreneur and possibly their family, but who have no ambitions of growing their business further. The latter meanwhile go into entrepreneurship in hopes of growing a business beyond the subsistence needs of the entrepreneurs themselves, providing employment for others in the process.

While Schoar's argument is mainly focused on the developing world, I share the view pointed out by Decker et al. (2014) that the distinction is relevant for the developed world as well. Significant support for this view comes from the survey evidence of U.S. business founders reported by Hurst and Pugsley (2011) who find that only around one third of new businesses have a product or service that they want to bring to the market, while most business founders cite non-pecuniary motivations such as "being their own boss" and the flexibility of being a small business owner. While there is a further distinction to be made between subsistence entrepreneurs who set up a business because they have no alternative source of income and non-pecuniary benefit -seeking entrepreneurs who might be quite well of working as employees if they didn't opt for setting up a small business, in terms of employment growth the categories are so close that I will mainly use the term "subsistence entrepreneur" as shorthand for referring to both.

2.5   Is Business Dynamism in Decline?

Evident from the previous discussion is that "creative destruction" indeed seems to play a significant role in the development of modern economies, with young high-growth firms contributing a disproportionate share of economic activity. This casts an ominous light on some of the recent findings in empirical literature presented next, and motivates the search for potential remedies - for example the removal of entry barriers such as the minimum capital requirement.

The main point of this section is that recent studies have noted trends in firm dynamics which suggest evolution towards an increasingly stagnant business environment across the developed world. Below, these trends are presented and subsequently compared to the specific case of Finland, using the data described in section 2.1.
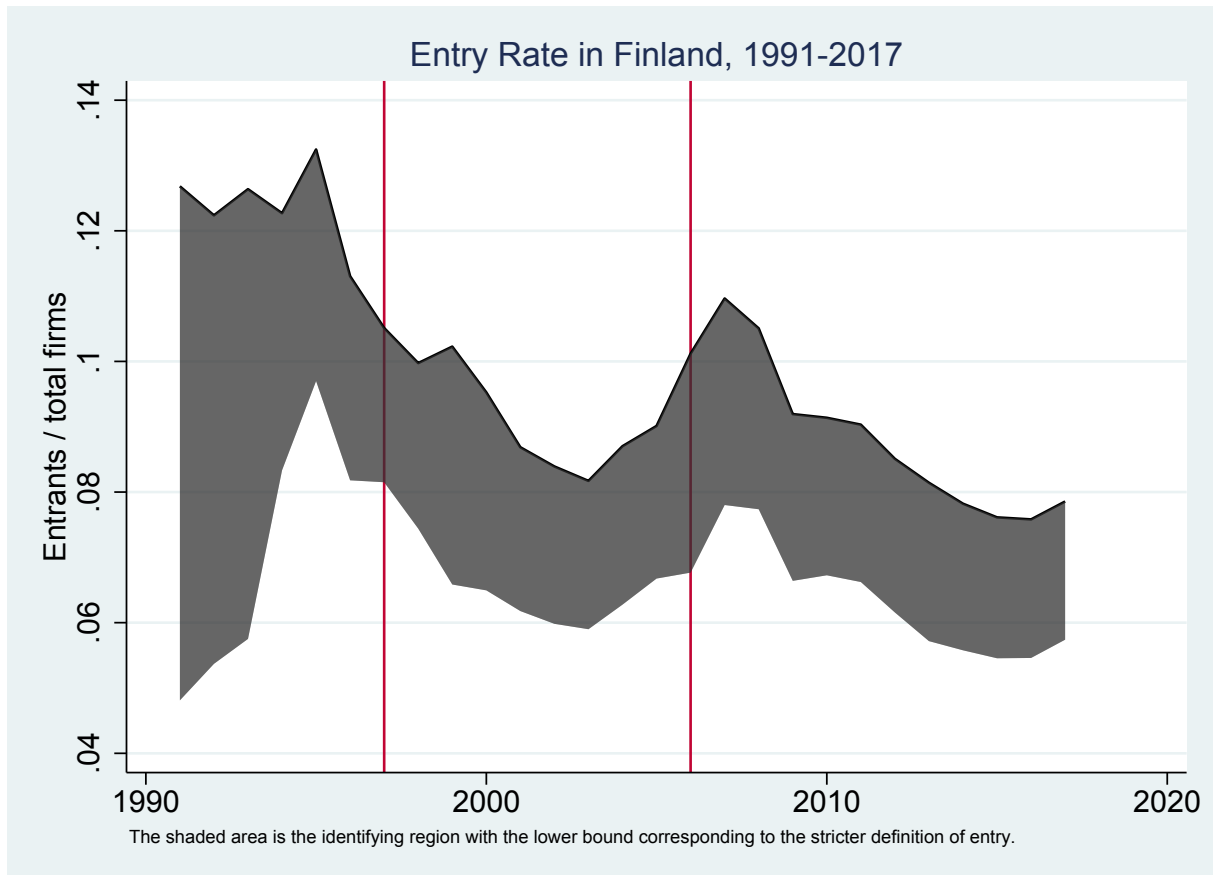
The entry rate

The first sign of the dynamism of an economy is the entry rate, i.e. the ratio of entrant firms to all firms. Generally speaking, a high entry rate is seen as a positive phenomenon, since it suggests a supportive business environment for entrepreneurs to try new ideas. Hence questions have been raised on what appears to be a global (at least across the developed world) phenomenon of a recent decline in entry rates. Decker, Haltiwanger, Jarmin, and Miranda (2014) observe a fall in the U.S. entry rate from 12.0 percent in the late 1980s to 10.6 before the Great Recession, after which it drops sharply below 8 percent. An OECD study shows that similar trends are also evident at least for many European countries, Brazil, Canada and New Zealand (Calvino, Criscuolo, and Menon, 2015).

Figure 3 plots the entry rate for Finland over the study period. The bolded line corresponds to the baseline definition, while the shaded area is the identifying region with its lower bound expressing the entry rate if all the firms not fulfilling the additional requirements described in the section 2.3 are considered incumbents. In case one trusts the baseline definition, the evolution of the entry rate is very similar to that observed in the U.S. by Decker et al. (2014). The entry rate starts above 12%, drops below 10% after 1997 and makes a modest return just before the Great Recession, after which it sinks to its lowest points at below 8%. This is not only qualitatively but also quantitatively close to the U.S case. The overall downward trend doesn't change much when considering the lower bound, though the levels drop quite a lot below the U.S. figures. The massive width of the interval in the first few years is most likely due to the problem in the older establishment-level data already mentioned in section 2.3.

Figure 3

The shaded area is the identifying region with the lower bound corresponding to the stricter definition of entry.

The entry rate is broken down by legal form in figure 4 (using the baseline definition). An interesting observation emerges from this picture: while all legal forms exhibit downward trends, limited companies go through a curious dip with the fall and rise happening around the times of the reforms in the minimum capital requirement. Are the reforms responsible for these shifts? A few pieces of evidence suggest that they play a role, though do not explain the whole pattern. First, consider the first reform in 1997. The steep fall of the entry rate for limited companies begins already in 1995, that is, clearly before the reform. However, at the same time the other legal forms also experience a downturn, suggesting some larger change in the macroeconomic environment. Then, as the rate for limited companies keeps falling, the other legal forms level off in 1997. This would be consistent with entrepreneurs choosing to incorporate with one of the other legal forms as barriers to setting up a limited company get higher. Remarkably, the opposite pattern is observed near the second reform in 2006. All legal forms turn to positive growth already in 2004, but the rest experience a slight downturn in 2006, while limited companies continue high growth for another year, before joining the rest in the Great Recession.

The entry rate for limited companies can be further broken down by the shares of companies founded at or above the minimum capital requirement threshold in the years for which data is available. Taking an average of firms starting in the years 2002-2005, one finds that, before the 2006 minimum capital requirement reform, around 60.5% of limited companies were set up at the limit, that is, with a share capital of 8000 euros. Repeating the exercise for the years 2007-2016 reveals that as the requirement is lowered to 2500 euros, the share of firms set up at the minimum jumps up to 75.0%. Unsurprisingly, this is driven by a significant increase in firms founded at the minimum requirement - while the number of firms founded above the threshold increases slightly, the number of those founded at the threshold roughly doubles compared to the pre-reform years. Meanwhile the number of firms founded around the old threshold of 8000 (plus or minus a thousand) euros drops by an order of magnitude. This suggests that not only are more entrepreneurs willing to set up a limited company when the minimum capital

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requirement is brought down, but also that most of those who might have been willing to do it at 8000 euros prefer the new minimum of 2500 euros.
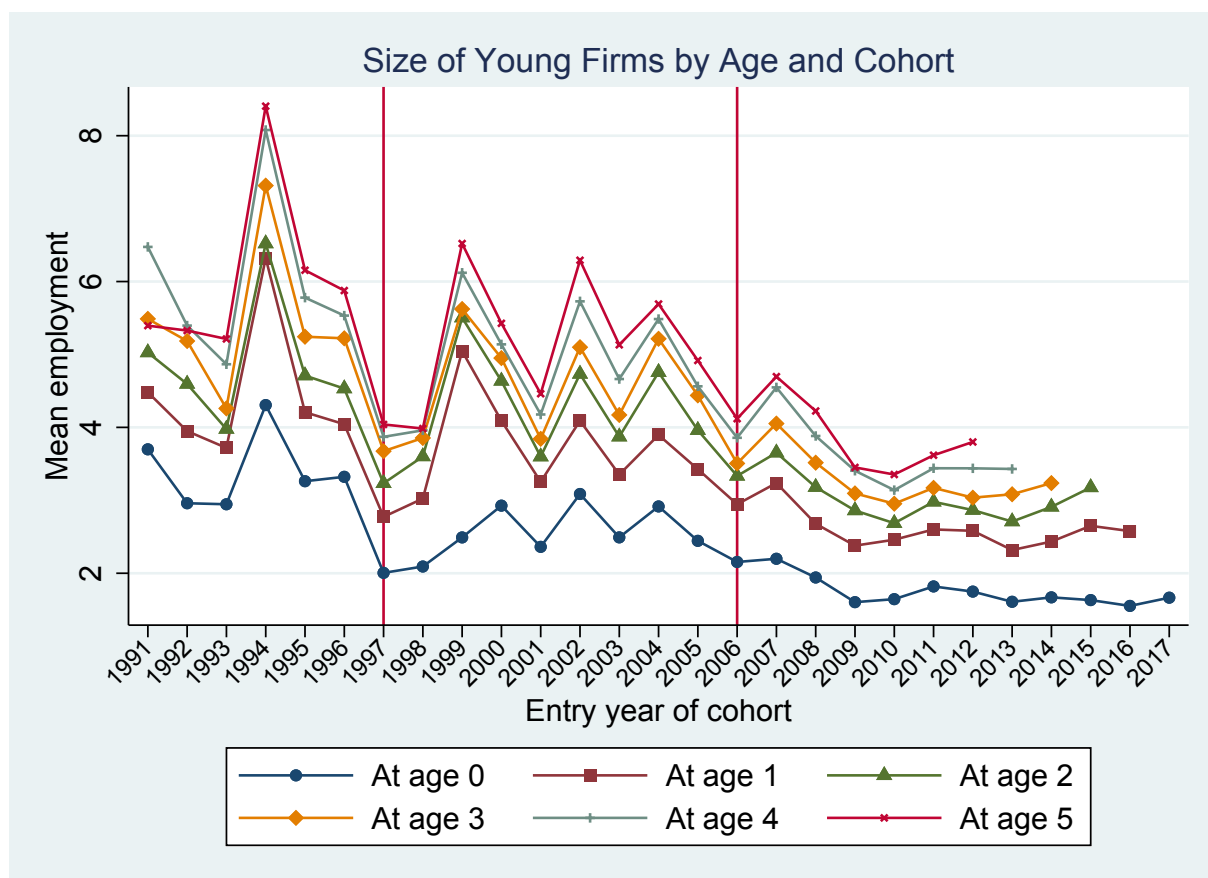
Figure 4

Entry Rates by Legal Form
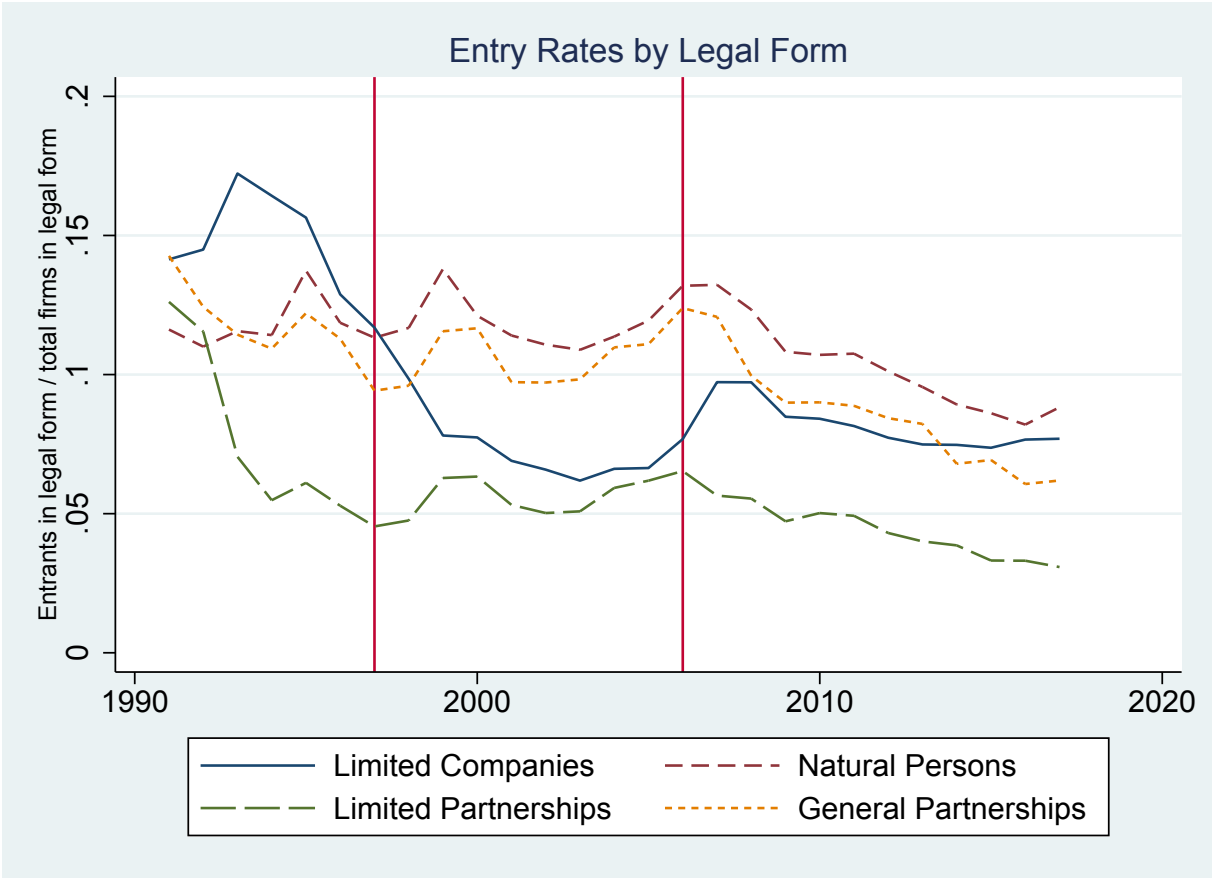
The size and growth of young firms

While the entry rate is an important indicator, it alone does not suffice to draw conclusions about the contribution that young firms are bringing to the economy, since it might be that the firms not entering anymore would mostly have been run by the subsistence entrepreneurs who had no intentions to grow anyway. Only when observing the development of the size of the entrants as well as their growth some years after entry can we begin to evaluate the total contribution.

Decker et al. (2014) note that the average size, measured in employees, of U.S. startups has remained approximately the same or declined over the three-decade period from early 1980s to early 2010s depending on the data source. The same flat or downward trends hold for all the countries in Calvino et al. (2015) as well, albeit with a significantly shorter study horizon (2003-2012 at best). As a consequence, not only the share of young firms but also their overall contribution to economic activity is declining.

Figure 5

As elsewhere, the declining entry rate in Finland is not being offset by larger entrants, at least in terms of employment. Figure 5 shows the mean employment of entrants in their first six years of operation. A few interesting observations arise. Firstly, the mean employment of a cohort at entry seems to predict its mean employment each year after that quite well. Closely related is the fact that the downward trend is present not just in the employment of entrants, but persists as they age and, notably, as less successful firms drop out. Finally, the biggest growth seems to happen in the first year. It should be noted that here the growth in the mean employment can be due both to the actual growth of the surviving firms and the dropping out of firms at the bottom of the size distribution.

Seeking evidence for the effects of the minimum capital requirement, figure 6 plots the average size in the first five years of limited corporations versus the other legal forms with entry years divided in three periods: before 1997, 1997-2006 and after 2006. Here the mean is calculated with respect to the original size of the cohort, i.e. firms that exit stay in the denominator while contributing zero to the numerator. This emphasizes the importance of the entry rate: on average, once a cohort has entered, its employment growth stays flat - the growth of the growing firms only manages to offset the jobs lost by the firms that shrink or exit. It is also clear that limited companies are much more prone to create jobs, a possible motivation to incentivice setting them up. However, the MCR doesn't appear to have an immediately obvious effect on the mean employment growth of a cohort. The employment of limited companies is on average between six and eight for firms starting before the first reform and between the reforms. However, after the second reform there is a dramatic drop to around four. This reflects the declining trend already observed in figure 5.
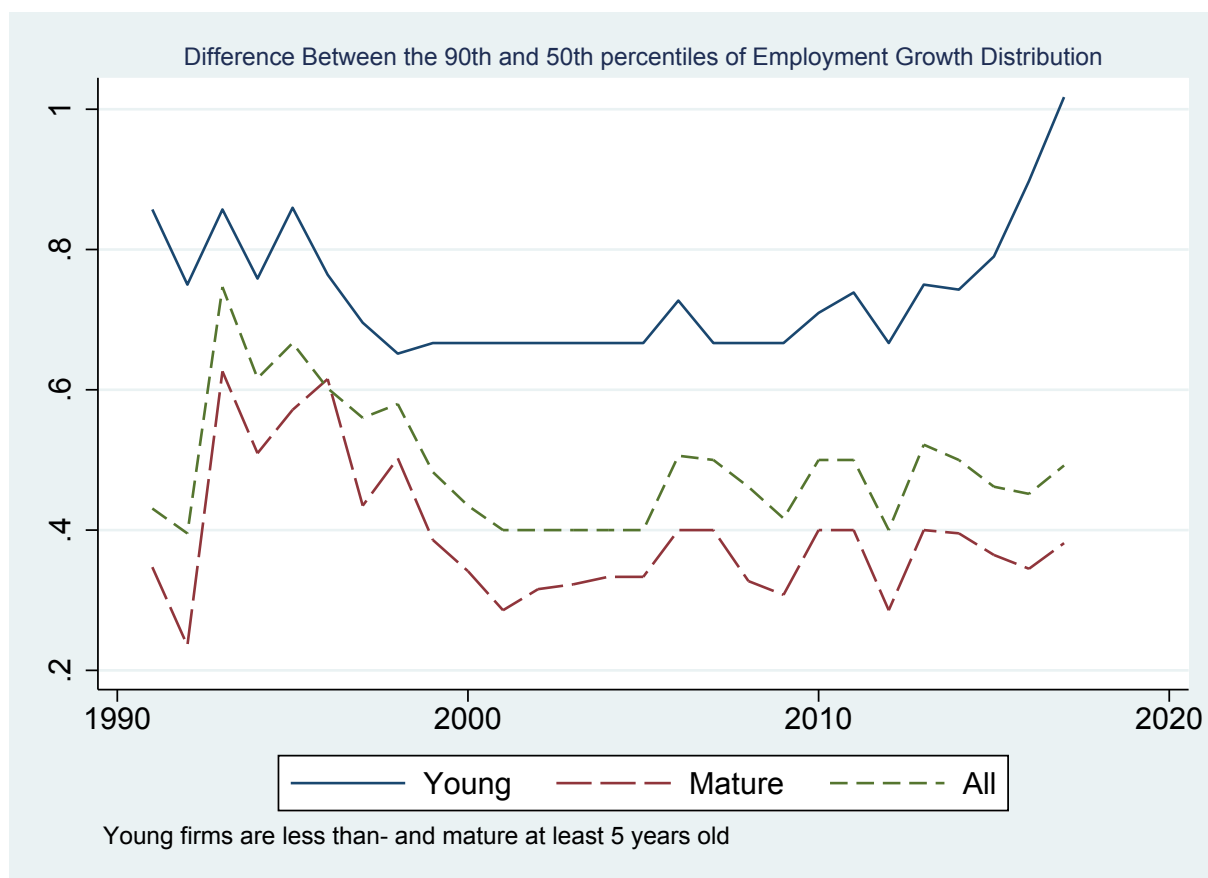
Figure 6
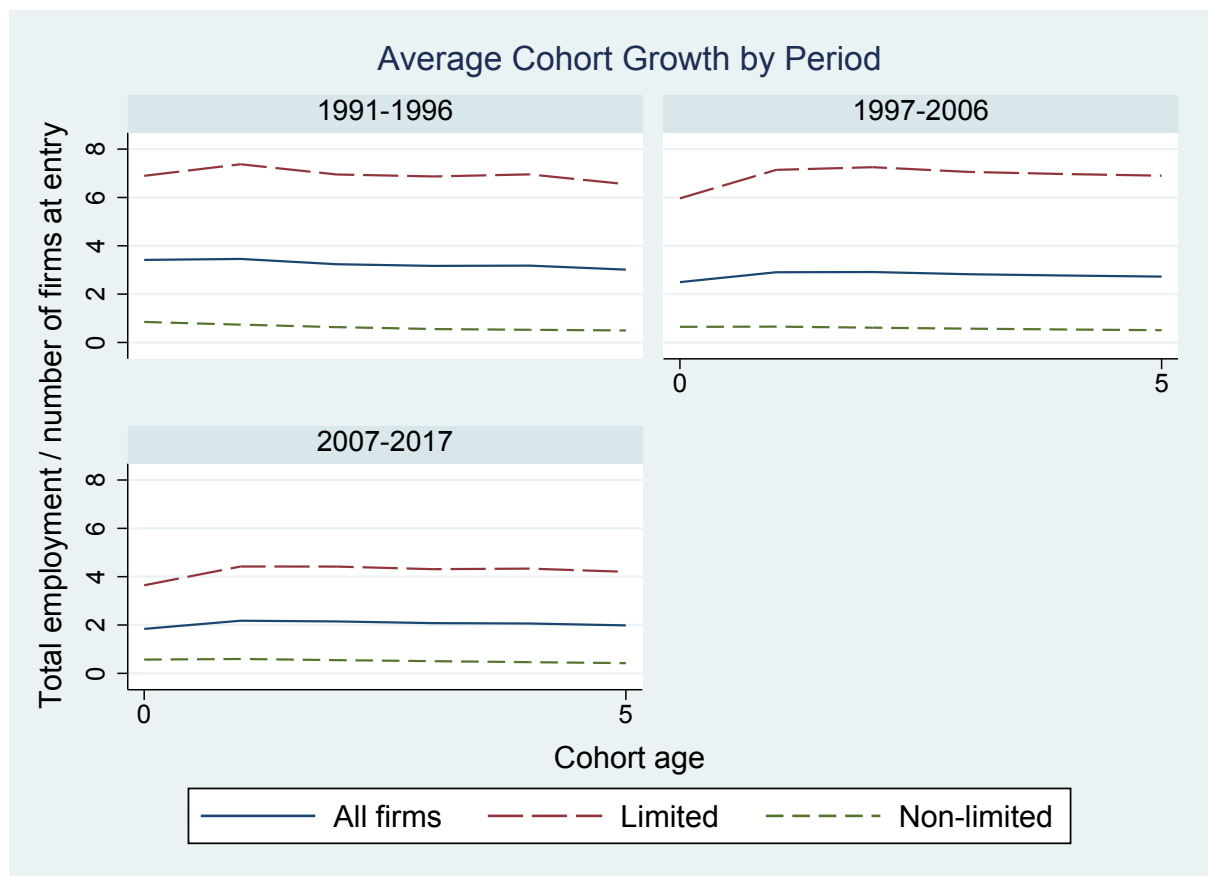
If the entry rate was declining, but it was only the subsistence entrepreneurs who no longer went into business, there might be less cause for concern. The declining size of entrants, however, points potentially to less transformational entrepreneurs. Decker, Haltiwanger, Jarmin, and Miranda (2016) present further worrying evidence from the U.S. They find that the skewness in the growth distribution of young firms has also been declining since 2000, reflecting a sharp decline at the 90th percentile, since the median firm continues to exhibit very little growth. While the entry rate and average size of entrants exhibit similar trends in Finland as in the U.S., the distributional development of the growth rates seems to diverge somewhat. Following Decker et al. (2016), figure 7 plots the evolution of the difference between the 90th and 50th percentiles of the employment growth rate distribution for young and mature firms. Letting E_{it} denote the employment of firm i at time t, the growth rate is defined as

$$\gamma_{it} = \frac{E_{it} - E_{it-1}}{0.5 * (E_{it} + E_{it-1})} \tag{1}$$

As in the case of the U.S., the 90-50-differential for young firms in Finland declines when approaching the year 2000, but then levels off at the turn of millennia and rises sharply towards the end of the study period in contrast to the findings of Decker et al. (2016), who observe a continuing decline for the U.S. all the way to the 2010s (though their data only extends to 2012). Digging deeper into the 90-50 differential, one finds that, since the median firm exhibits little to no growth, this measure of skewness almost perfectly mirrors the growth of the 90th percentile firm (this is true for both young and mature firms, and in both the U.S. and Finland). Hence while the rate of entry and average size of entrants have decreased, there appears to be no similar decline in the growth of the highest growing young firms.

Figure 7

Difference Between the 90th and 50th percentiles of Employment Growth Distribution

Young — Mature — All

Young firms are less than- and mature at least 5 years old

Exit and survival

The final piece in the dynamism-puzzle is the development of the exit of firms. This can be viewed from two angles: the exit rate, i.e. the ratio of exiting firms to all firms in a given period, and survival rates, i.e. the share of businesses of a given age that continue to the next period. The interpretation of exit and survival rates as signs of the health of an economy is somewhat less straightforward than that of entry rates and growth rates of young firms, increases in which can be generally viewed as positive developments. On the one hand, high survival and low exit rates imply success and stability on the level of individual firms, which surely is desirable. On the other, as Calvino et al. (2015) for instance point out, they can be imbued with a contrary interpretation: short survival times are a sign of healthy dynamism, with companies allowed to easily enter the market with risky business strategies and then exit quickly or grow depending on their success.

Looking at the overall exit rate - the ratio of exiting firms to total firms - in the U.S., Decker et al. (2016) find that it does not exhibit a similar secular decline as the entry rate, instead varying around the 9% level from 1979 to 2011. Meanwhile Calvino et al. (2015), focusing on the exit rate of young (less than three years old) firms find that in the 2002-2012 period the trend has varied somewhat more across countries than the entry rate or the size of entrants, though in most cases it has remained flat or increased slightly, with a few countries (e.g. Spain, New Zealand and especially Belgium) exhibiting more pronounced increases.

Figure 8

Young firms are less than- and mature at least 5 years old

Figure 8 plots the exit rate in Finland separately for young and mature firms, as well as for all firms pooled together. Interestingly, one observes an initial fall until around the first reform,[7] followed by a leveling-off and then an increase after the second reform. The trends are similar for all firms, though more pronounced for the young ones. This may suggest that, under the high MCR, there is less competitive pressures driving firms to exit, or the firms operating in the market are more stable as they have had to make higher initial investments (though this shouldn't matter for the early post-reform-years of the mature firms).

To look behind the exit rate, one can investigate the survival of firms as a function of age. Doing this in the Finnish case reveals another reason why policy makers might want to incentivice entrepreneurship in the form of limited companies. This is illustrated in figure 9 which plots Kaplan-Meier survival functions separately for limited corporations and the other legal forms pooled together, showing clearly superior performance by limited companies in terms of longevity. Even if the policy makers have preferences for dynamism rather than stability, though, one might still expect them to ease the barriers of limited incorporation, that being the legal form that allows higher risk-taking.

Figure 9 pools together all available time periods to estimate the survival as a function of age. To look at its development over time, figure 10 plots the Kaplan-Meier estimates separately for three periods, corresponding with the three different MCR regimes. One might expect higher minimum capital requirements to also raise the survival times of limited companies. In figure 10a this seems to be the case for the first eight years: companies founded in the years of high capital requirements (1997-2006) have higher survival probabilities for the first years, but though the firms founded after the second reform have lower probabilities for all available ages, the firms founded before the first reform have higher probabilities for ages beyond 10 years. This might reflect larger changes in the business environment (e.g. the 2008 crisis)

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[7] There is a spike after the reform, but some of this this may reflect the problem discussed in section 2.1 that in the data the coding of firms identifiers changed from 1998 to 1999 when natural persons' personal identifiers were replaced by firm identifiers.

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with the older established firms being more resilient to them.

Meanwhile, if managers substitute for limited incorporation by choosing to incorporate with the other legal forms, one might expect an effect to be observable also for non-limited companies: as capital requirements rise, some managers that would have otherwise set up a limited company choose non-limited incorporation instead. If we assume that these "marginal managers" are on average more talented than those who would have set up a non-limited company in any case, then the effect would go in the same direction as for limited companies. Indeed, figure 10b shows that the pattern is the same as in the case of limited companies with those firms starting in the era of higher capital requirements showing larger survival probabilities, in this case up to 13 years and with more pronounced differences than in the limited company case.

Whether the aforementioned developments are caused by the chances in the MCR or some other changes in the business environment (or both) remains inconclusive, but the patterns of the survival functions are consistent with what one might expect to see. In any case, they document a recent decline in firms' survival probabilities: survival probabilities in the post-2006 period are the lowest they've been during the study period for firms of all (available) ages. For both limited and non-limited companies, the young firms born under the high MCR regime between 1997 and 2006 have higher survival probabilities than in the low MCR periods, but whereas the 1996-1996 cohorts seem to catch up with the high MCR cohorts as they get older, the post-2006 cohorts are left lagging behind.
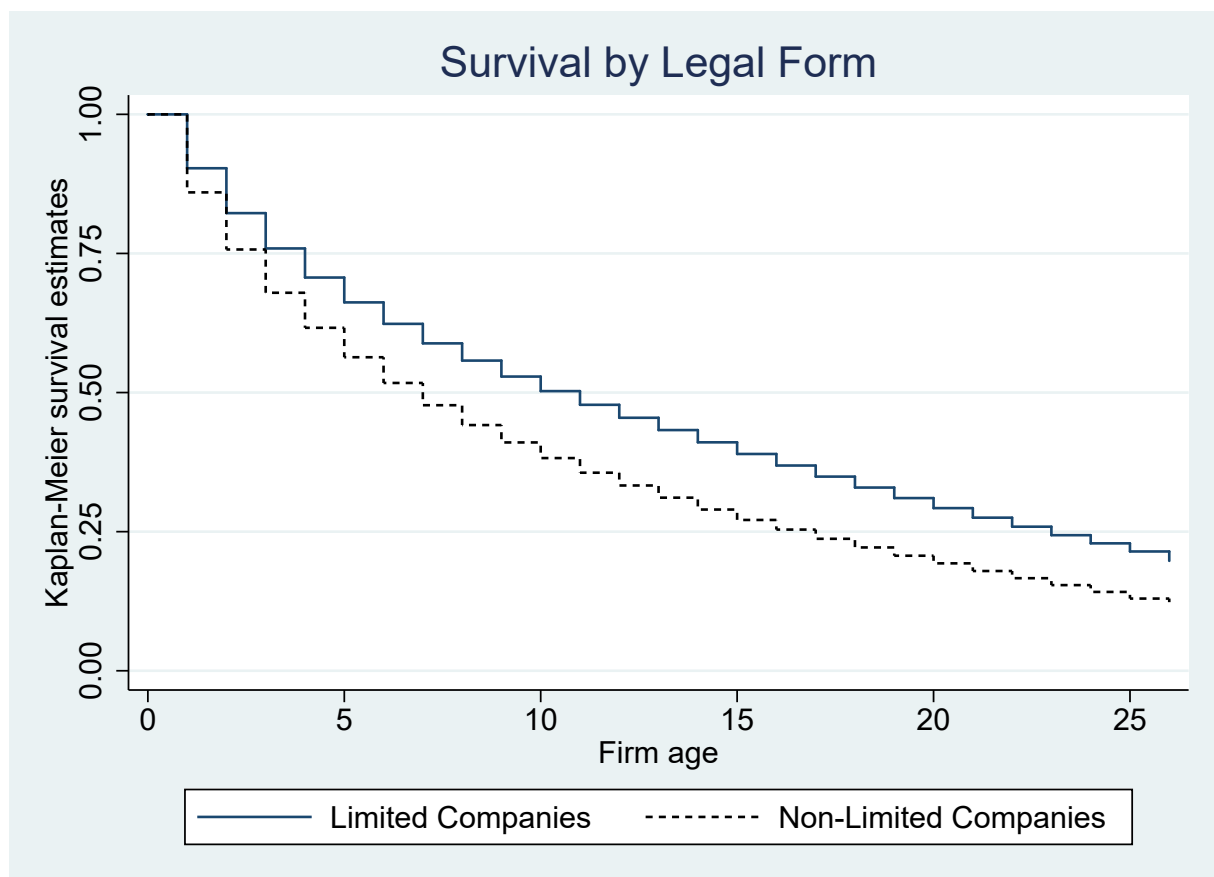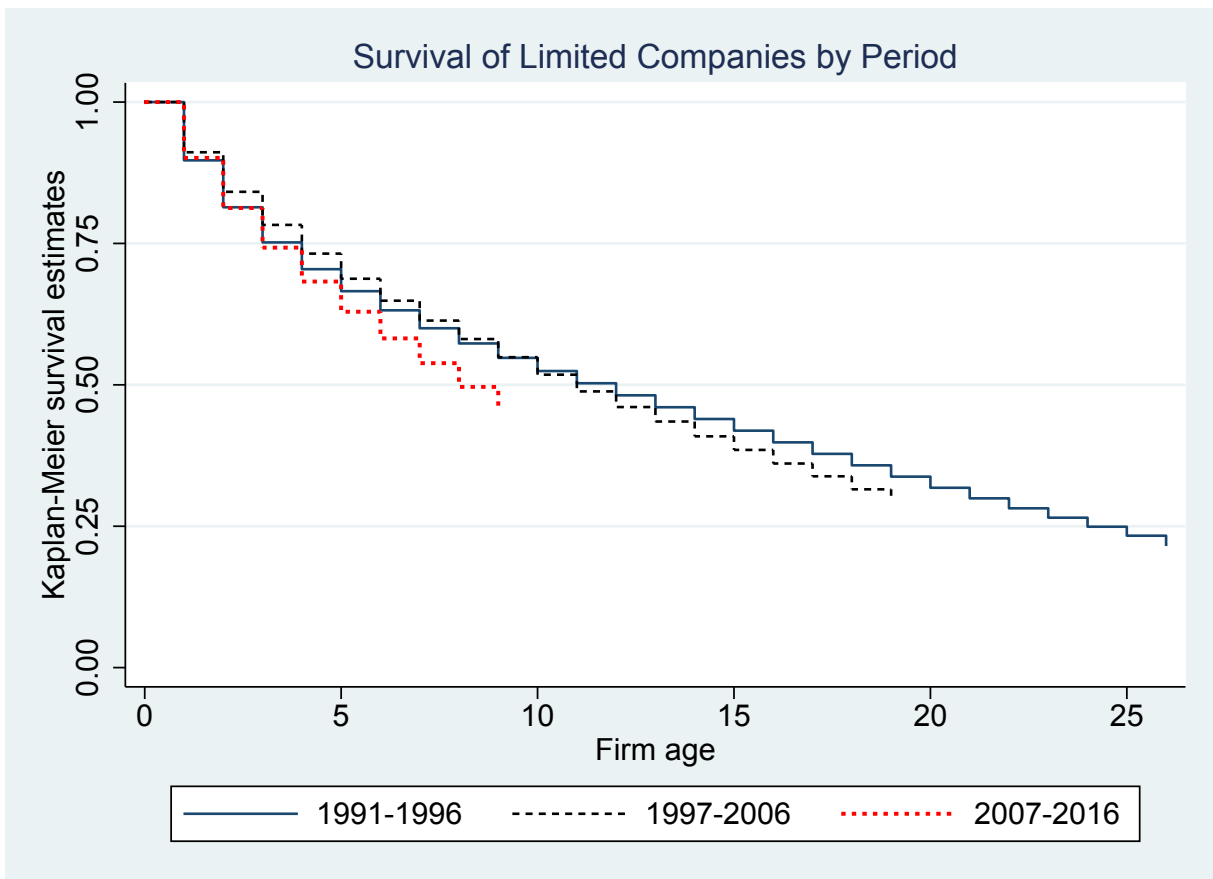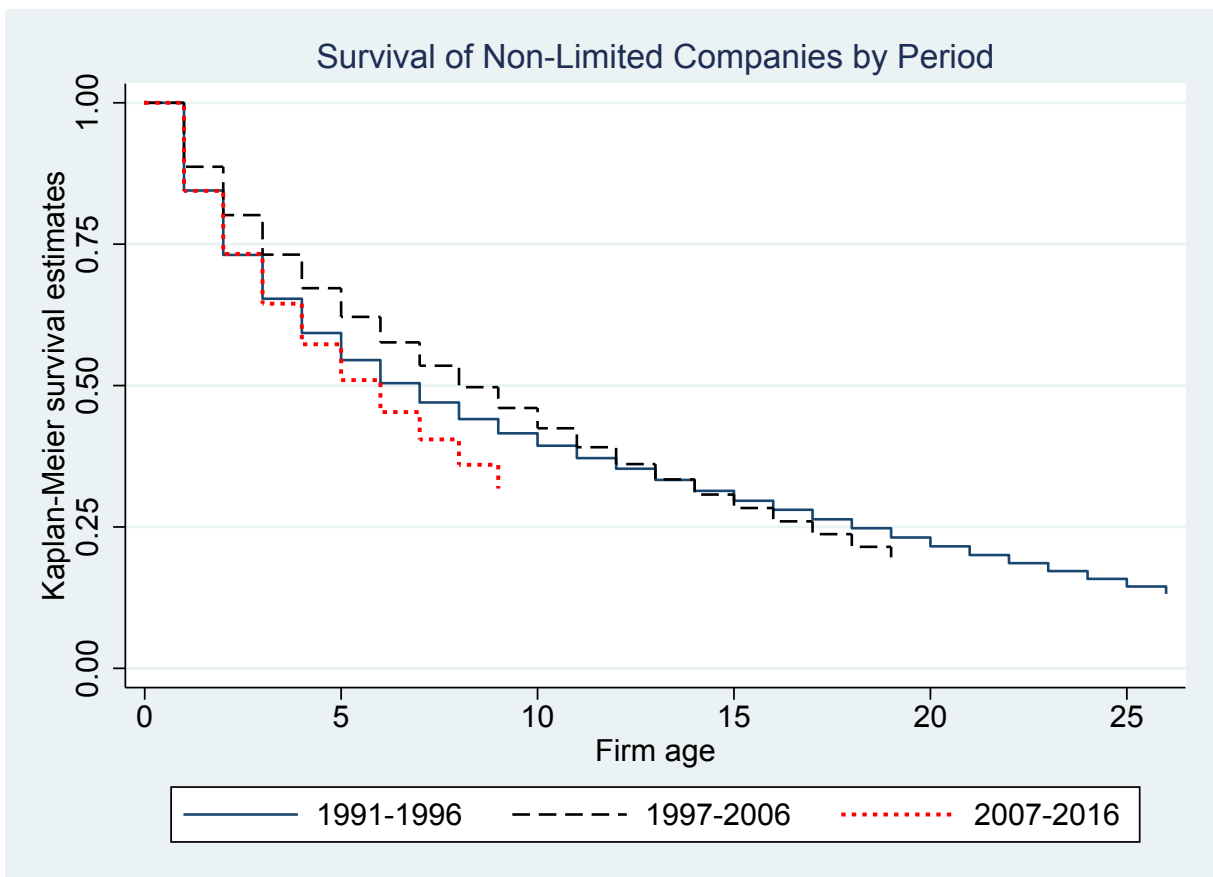
Figure 9

Figure 10

(a)

Survival of Limited Companies by Period

(b)

Survival of Non-Limited Companies by Period

Industry concentration

With the entry rate declining, the size and growth of entrants staying the same at best and their survival probabilities falling, the overall contribution from young firms is indeed on the decline. Hence the "engines of creative destruction" are slowing down, and insofar as the performance of mature incumbent firms' isn't falling to the same degree, this implies a decline in business dynamism beyond any aggregate shocks affecting the entire economy (young and old firms alike). One way this can be expected to materialize is in the concentration of industries, with a small number of firms commanding an increasingly large share of the market in any given industry. Recent studies have found that this seems to be the case, at least for the U.S.

Autor, Dorn, Katz, Patterson, and Van Reenen (2017) document a rise in average product market concentration on the four-digit level across a variety of measures and industries. Depending on the industry, the rise begins somewhere from the late 1980s to the late 1990s, and is especially high in finance, services, utilities and transportation and retail, with manufacturing and wholesale showing slightly less marked increases (though they exhibit higher levels to begin with). The authors note that the trends are more pronounced when measured in sales rather than employees (though still clearly visible using the latter measure in most cases), implying that firms are increasingly able to achieve higher shares of industry sales with fewer employees. De Loecker, Eeckhout, and Unger (2017) go beyond concentration, exploring the evolution of markups, which they argue is a better measure of market power, since concentration usually relies on arbitrarily setting the boundaries of a market and might hence reflect product differentiation rather than actual market power. They find that markups have also risen sharply since the 1980s from about 20% to 30% above marginal cost to around 60% in 2014.

Figure 11
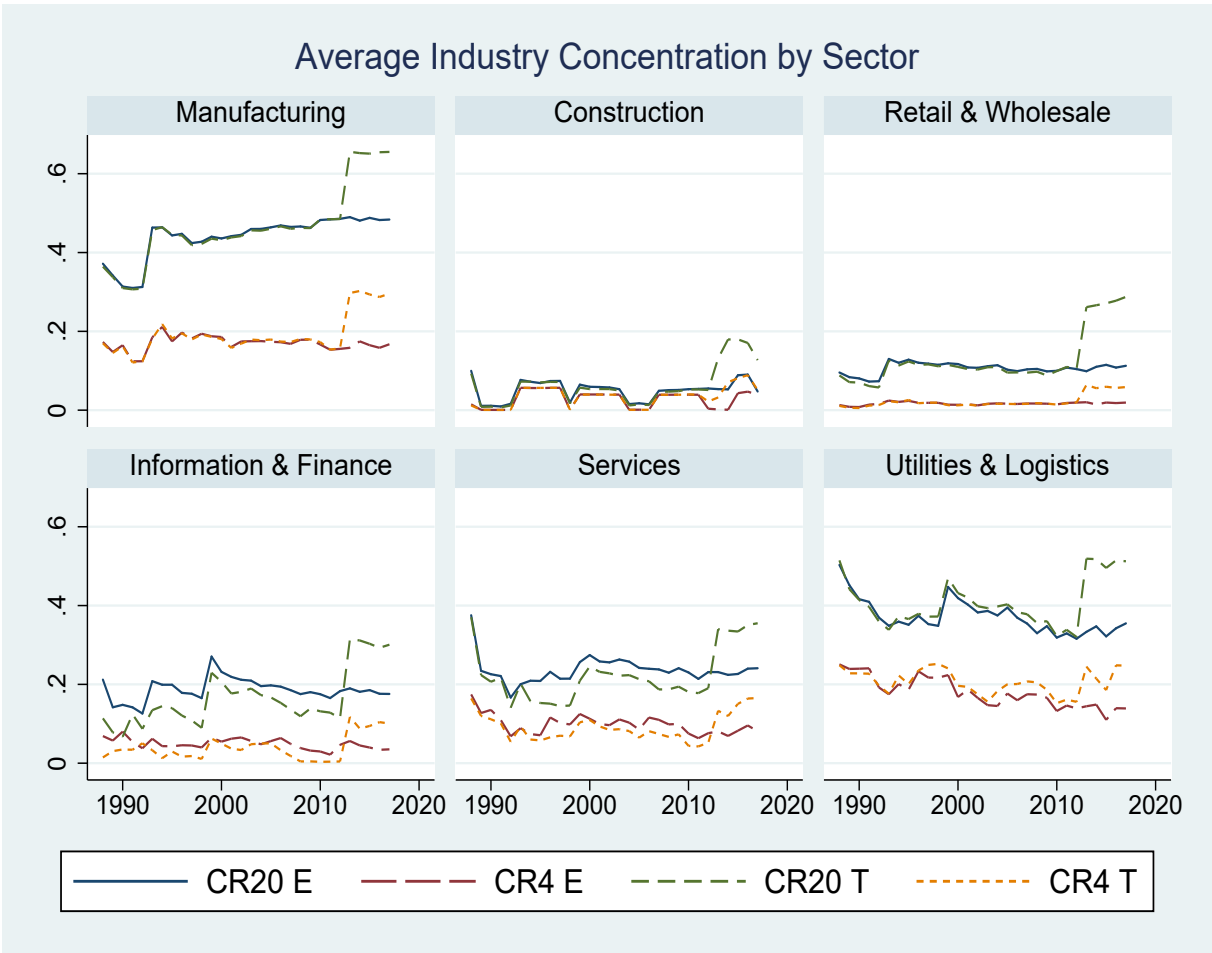
Similarly to Autor et al. (2017), figure 11 plots the concentration in Finnish industries, measured as the average CR4 and CR20[8] of industries in larger sectors,[9] using both turnover (CR4 T & CR20 T) and employment (CR4 E & CR20 E) as measures of size. In contrast to the U.S. case, Finnish industries seem to exhibit little increase in concentration when measured in employment, although there is perhaps a slight upward trend in the CR20 of manufacturing (the levels are mostly similar, though they should not be considered very comparable due to different data sources and industry groupings). When measured in turnover, the trends are similarly flat for most of the study period, except for the very end where each sector seems to undergo a sudden jump around 2013. However, recall from section 2.1 that this corresponds to a major reform of the statistics gathering at Statistics Finland, and since the correlation between the employment and turnover measures up to that point is nearly perfect, and there is no such rise in the employment measures, this jump is most likely explained by the discontinuity in the gathering of data.

Summarizing the Finnish trends

In conclusion, it seems that Finland shares some of the trends of declining business dynamism observed all across the developed world. The entry rate as well as the size and growth of entrants is on a downward path, with the survival of young firms also declining over time. Taken together, these developments imply a fall in the overall economic contribution of young firms. The development of the overall exit rate, meanwhile, seems to correspond well with the reforms in the MCR with higher rates of exit under the lower requirements.

Where Finland diverges at least from the U.S. is in the growth distribution of firms and the concentration of industries. There is no apparent decline in the growth rates of the highest growing Finnish startups, even if the averages of the entrants seem to be declining. This suggests that Finland's "problem" may be a higher share of subsistence entrepreneurs rather than declining growth on the part of transformational entrepreneurs. There also appear to be nowhere near as drastic hikes in industry concentration in Finland as in the U.S, suggesting that the declining performance may be more symmetrically experienced by established firms as well.

These trends together raise the question of whether or not it makes sense for the Finnish policy makers to lower the barriers of entry. The decline in the entry rate seems to be a secular trend shared by many nations, and lowering entry barriers will most likely provide only brief relief. This is evident for instance in figure 4, where the overall trend for all legal forms is going downwards despite the jumps potentially caused by the altering of the MCR. Meanwhile, if the falling size and growth of entrants is driven by an increasing share of subsistence entrepreneurs rather than falling growth among transformational ones, lowering entry barriers is only going to amplify this trend. Also, there appears to be no evidence that the higher MCR would be protecting incumbents to the extent that industries would increase in concentration.

At the same time, though, it's not clear what potential negative consequences a lower MCR might have. One reason worth mentioning is that the MCR may protect consumers and debtors from fraudulent companies. Furthermore, a lower MCR may make it easier for black market operators to set up front companies, a concern raised by the tax administration following the recent removal of MCR in Finland (Muilu, 2019). These questions are beyond the scope of this thesis, however. To explore what the potential consequences - positive or negative - may be in terms of firm dynamics, the next section reviews some theoretical frameworks dealing with entrepreneurship and firm dynamics.

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[8] The sum of the sizes of the four in case of CR4 and twenty in case of CR20 largest firms in an industry divided by the total size of that industry

[9] Autor et al. (2017) use four-digit industries and six sectors, whereas figure 11, consistently with the rest of this thesis, uses five-digit industries. The sectors are combinations of the one-letter level sectors of the standard industrial classification of 2008 used by Statistics Finland (see Statistics Finland (2008)). Appendix B lists the one-letter sectors belonging to each sector-title used in the text.

3 Theoretical Predictions for the Effects of the MCR

This section briefly presents a number of theories that help to predict and contextualize the effects the minimum capital requirement might have on firm dynamics. In the following discussion the MCR will be treated as belonging to a firm's entry cost. One might argue that it should more appropriately be viewed as an investment instead since it stays in the firm. However, the fact that an overwhelming majority[10] of limited companies are founded with the minimum required amount suggests that this investment is unnecessary for most firms, and treated like a cost by potential entrepreneurs. At the very least, the MCR constitutes an opportunity cost for the entrepreneur, as it restricts the choices one can make with their disposable wealth.

To help organize the various implications arising from the discussion, I'll consider the predicted effects of a reform that lowers entry costs for all firms - such as the 2006 MCR reform[11] - on firm dynamics piece by piece. Calvino et al. (2015) break the economic contribution of young firms down to four components: the entry rate, the average size of entrants, the survival rate and the average growth of survivors. In addition, one needs to consider the size, growth and survival of incumbent firms to arrive at the total economic contribution of all firms.

3.1 Entrepreneurial Traits and Preferences

The question of how changes in the MCR will reflect in the number and performance of entrants crucially depends on who the potential entrepreneurs are and what is driving their decisions. In simplified models the main driving force is usually a single parameter, whether that be talent, risk aversion or preference for entrepreneurship. In the real world, however, the decisions may also reflect behavioral biases that make them less than optimal.

Ability and the role of information

A significant view within the theory of entrepreneurship considers the entry decision and subsequent performance of firms as a function of the entrepreneurial ability of a firm's founder. Within this view, contrasting two canonical models - those of Lucas (1978) and Jovanovic (1982) - illustrates an important distinction arising from the awareness of the agents. In both models, potential entrepreneurs are heterogeneous in their ability[12], with more talented individuals performing better in the market after entry. The difference comes form whether the potential managers know their ability or not: Lucas' entrepreneurs are perfectly informed while Jovanovic's only learn about their efficiency from market signals after entering.

Starting with the number of entrants, there isn't much disagreement on what the qualitative effect of the cost-lowering reform should be. In both models, the agents maximize expected utility, and a higher entry cost will reduce the expected utility from entrepreneurship regardless of the outcome,[13] which means that less agents will want to become entrepreneurs. Lowering the MCR, then, should unequivocally yield higher entry numbers. It is in the post-entry performance where interesting differences start to appear.

If the potential entrepreneurs are fully aware of their ability, a higher entry cost excludes only those who are not talented enough to achieve net positive value under it. Thus the Lucas-model implies a cut-off level in ability, with everyone above that ability going into entrepreneurship and everyone below it choosing wage-employment, and a trade-off between the quantity of entrepreneurs and their average ability. Since ability drives performance, one would then expect to see that the "marginal managers", who would have chosen wage-employment under a higher MCR, end up running smaller businesses and exit quicker, so that the averages of survival and employment fall.

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[10] 60-75% depending on the time period according to the data used here (see section 2.5)

[11] Note that strictly speaking the 2006 MCR reform lowered entry costs for all limited companies rather than all firms, a distinction that is ignored for brevity in this section but should always be kept in mind when interpreting the results

[12] Note that Jovanovic speaks of firms rather than entrepreneurs or managers and efficiency and costs rather than talent, but the different labeling doesn't change the role these parameters play in the model.

[13] It should be noted, though, that Lucas does not explicitly model an entry cost while Jovanovic does.

In the other extreme where the entrepreneurs learn nothing about their ability before entry, the entry cost will have no bearing on the average ability of any given cohort. Note, though, that there may still be a fall in the average performance stemming from the fact that there is now more competition in the market. However, in contrast to Lucas, the entrepreneurs that are encouraged to enter are no worse than those who would have entered in any case. Recalling the empirical fact that the size and growth distributions of firms are highly skewed, a high MCR in a Lucas-world would then cut out firms from the low-end of those distributions, while in the world of Jovanovic it would randomly cut out firms from all across the distribution, leading to a much greater negative impact in total. This brings us to a point that is worth emphasizing: the impact of the reform is largely determined by the extent to which those entrepreneurs that are going to be successful know that they are going to be successful! If success is highly correlated with the preconceptions of the entrepreneur, the most promising entrepreneurs will become entrepreneurs regardless of the costs, so that the reform will have little impact. Meanwhile, if entrepreneurship is more a process of trial-and-error, even modest cost-lowering reforms can have big effects as the group of people choosing entrepreneurship under the new regime will include a few high-impact entrepreneurs with some probability.

It seems plausible that reality is somewhere in between the Lucas and Jovanovic worlds: potential entrepreneurs probably have gained some signals from other activities, but are not quite certain of their abilities. Thus, one would expect the average performance of entrepreneurs to fall with the entry cost, with the fall being greater the more entrepreneurs are aware of their ability, and the total impact being more positive the less aware they are.

Preferences: risk aversion and non-pecuniary benefits

Another standard explanation for the entry decision is heterogeneity in the degree of risk aversion among the workforce. Kihlstrom and Laffont (1979) develop a general equilibrium model based on this idea. In their model the agents can choose to become an entrepreneur or a worker with entrepreneurship providing a risky profit while employment pays a risk-free endogenously determined wage. In the equilibrium, more risk averse agents choose the safe employment while less risk averse ones become entrepreneurs. Similarly, of those that become entrepreneurs, the less risk averse ones end up running larger (in terms of employment) firms. In this scenario, then, the decision to become an entrepreneur as well as to grow ones firm is a function of the preferences of the agents with no need for some agents to be better at running a business than others. Whereas in the Lucas- and Jovanovic-models those firms that became large were run by more able managers, here success is a matter of luck with willingness to take a gamble a necessary - but, importantly, not a sufficient - condition.

A notable feature of the Kihlstrom-Laffont model is that its equilibrium is in general inefficient with respect to the number of firms and allocation of labour. There are two opposing forces at play behind the inefficiency: on the one hand risk aversion causes too few agents to choose entrepreneurship, while on the other hand risk aversion among those who do become entrepreneurs causes them to hire sub-optimally which in turn lowers the equilibrium wage and creates an incentive for too many agents to become entrepreneurs. Hence there are generally too few or too many entrepreneurs, depending on the specification of the parameters. This highlights the notion that more entrepreneurship is not necessarily better.

Assuming risk aversion to be the driving factor behind entry does not really change the predictions for the number of entrants. A decrease in the MCR still raises utility for all outcomes if one chooses entrepreneurship, leading to a higher expected utility from entrepreneurship regardless of ones risk attitudes, so that more agents will choose entrepreneurship. Furthermore, note that the agents on the margin that are encouraged choose entrepreneurship under a lower MCR will be more risk averse also in their decisions to grow the company. Thus one would expect to see smaller entrants and lower growth. The effects on survival are more ambiguous and depend on the options the entrepreneurs have. In the

Kihlstrom-Laffont-model, an entrepreneur that exits simply becomes a wage-employee, i.e. unemployment does not exist and firms do not go bankrupt. With this sort of specification, highly risk-averse agents should also exit quicker, because upon receiving a negative shock they will rather switch to employment that guarantees a certain income than wait for their luck to turn. On the other hand, if there is a possibility of bankruptcy or unemployment, more risk averse agents could be expected to make less risky moves and hence survive for longer.

In addition to different preferences for risk, individuals may also vary in their preferences for other dimensions of entrepreneurship. Recall from section 2.4 that survey evidence suggests most entrepreneurs to be motivated by factors other than money, such as flexibility and "being their own boss" (Hurst and Pugsley, 2011). In other words, entrepreneurs gain non-pecuniary benefits from being an entrepreneur - entrepreneurship is a good in itself. Inspired by this, Hurst and Pugsley (2015) present a model of entrepreneurial choice based on the preferences for business ownership. In addition to the preference-heterogeneity, there are different industries which are defined by their fixed costs. In each industry, a good may be produced by a small firm owned by an agent, or a corporation that employs agents. The agents decide whether to use their labour as employees in the corporate sector or small business owners, in which case they also decide which good to sell (i.e. which industry to enter). In each industry the fixed costs determine an efficient scale of employment, and the agent-operated firms are restricted to one employee (the owner) while the corporate firms can employ as many agents as they want. As all entrepreneurs are looking to get their non-pecuniary benefits with as little cost as possible, and the industries do not differ in their ability to generate these benefits, the result is a cutoff-point for fixed costs, below which all goods are produced by small business owners and above by corporations.

It's again not hard to see that a decrease in the MCR - a reform that lowers the fixed cost for all industries - will result in an increase in the number of entrepreneurs (i.e. the share of industries operated by agent-owned businesses). However, it is important to note that the Hurst-Pugsley-model is explicitly not trying to explain differences in ex-post performance of firms. In fact, it completely abstracts away the dynamics of small business formation and growth, treating all entrepreneurs essentially as sole proprietors without employees. The implications of non-pecuniary motives for the post-entry performance of firms then mainly depends on how important one believes them to be relative to monetary motives that make entrepreneurs seek growth.

If their relative importance is high (as evidence suggests, see section 2.4), most of the new entrants resulting from the reform would be concentrating in small scale industries with little intentions of growing. If we suppose that the growth-seeking transformational entrepreneurs are high-ability individuals who are at least to some extent aware of their abilities, we'd expect them not to be discouraged by slightly higher entry costs. The result of lowering costs, unless with a large amount, would then mainly be an influx of subsistence entrepreneurs to the economy. On average, we could then expect smaller entrants exhibiting lower growth. The effects on survival could conceivably go to the other direction though, as survival is still an objective for subsistence entrepreneurs, perhaps even more so than transformational entrepreneurs, who may be more likely to follow an up-or-out strategy.

Note that while the existence of non-pecuniary benefits thus seems to have broadly negative implications for the performance of firms, as Hurst and Pugsley point out, this does not need to imply losses in utility in a world with non-pecuniary benefits. In a utilitarian sense, the decreased performance can be acceptable if the entrepreneurs are in fact sufficiently happier even if their productivity may be substantially lower than it would be if they worked as employees for someone else. While this is important to keep in mind when interpreting the results, it should also be once more emphasized that the Hurst-Pugsley model specifically models the decision of the so-called subsistence entrepreneurs and stays silent on transformational entrepreneurs - the ones who are looking to grow in the first place and hence likely to have a bigger economic impact. The welfare effects will critically hinge on how the latter group is affected by entry barriers.

Variety in traits and preferences

While the discussed theoretical frameworks agree that lower entry costs lead to an increase in the number of entrants, and most point to a decrease in their average potential, they may have different implications on the magnitude of the effect. An important factor here is the extent to which the parameters driving the decisions take on different values. Hombert et al. (2017) consider the implications of different talent distributions for the standard Lucas model. They point out that if the potential entrepreneurs are very heterogeneous with respect to their abilities, raising the expected returns from entrepreneurship[14] has a small positive effect on the entry rate but a large negative effect on the average quality of the entrants, while the case of homogeneously talented potential entrepreneurs leads to opposite conclusions. This is because, in the former case, there are few "marginal managers" just below the threshold who are encouraged to enter due to the reform, but on the other hand the ability of these marginal managers is substantially lower than of those already in the market. On the contrary, in the case of the homogeneous distribution, there is a large mass of managers on the margin who will choose entrepreneurship due to the reform, and the ability of these managers is not that much lower than of those already in the market. Note that this argument only applies if talent is known - the distribution should not matter for managers unaware of their ability.[15]

Thus, if known talent is the driving force, the impact of the reform depends a lot on its distribution. However, the same argument applies just as well if the driving factor is (known) risk aversion or non-pecuniary utility gained from entrepreneurship instead of ability: a lot of variation in either one is going to lead to a small response in terms of entry numbers as long as the entrepreneurs are making rational calculations with perfect information. This, of course, is a dubious assumption in the real world, where the decisions of entrepreneurs may reflect biases and heuristics. Åstebro, Herz, Nanda, and Weber (2014) provide a review of some insights from behavioral economics research on entrepreneurship, which are important to take into account when considering the potential effects of the MCR on one hand, and justifications for such regulation on the other. The next subsection briefly summarizes and comments on this review. The other studies mentioned are presented as cited in Åstebro et al. (2014).

Insights from behavioral economics: overconfidence

Åstebro et al. (2014) introduce a few observations that are especially pertinent to motivating behavioral concerns in the context of entrepreneurship. One is a study by Hall and Woodward (2010), calculating that the very low probability of success should make the expected utility of entrepreneurial ventures negative for normal degrees of risk aversion. Another is the fact that, despite this, entrepreneurship not only exists but is relatively prevalent (Parker 2009). Finally, entrepreneurs also exhibit the kind of persistence that an objective observer might deem irrational - they keep running their businesses for long times despite low returns (Hamilton 2000; Åstebro 2003; Moskowitz and Vissing-Jørgensen 2002). Åstebro et. al. also cite experimental evidence by Holm, Opper and Nee (2013) that finds no difference in risk attitudes between entrepreneurs and non-entrepreneurs, suggesting that the puzzle is not explained to a satisfactory degree by the proposition that some subset of the general population has very different attitudes towards risk. This clearly casts doubt on the predictive power of the Kihlstrom-Laffont-model.

The non-pecuniary benefits discussed in the previous subsection provide one potential explanation for the above observations, especially the "irrational" persistence, and are indeed brought up by Åstebro et. al.. While Hurst and Pugsley (2011; 2015) were mainly interested in the question of why some entrepreneurs are content staying small, Åstebro et. al. also point out potentially utility producing factors that seem especially relevant for the entry decisions of the so-called transformational entrepreneurs:

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[14]Specifically, they consider a guaranteed allowance for all entrepreneurs in case of failure, i.e. insurance. However, an entry cost is just a negative allowance regardless of success, so its effect is to lower the expected returns and the conclusion is qualitatively the same.

[15]A caveat to this is that if the entrepreneurs are risk averse but unaware of their ability while still knowing the distribution of the ability parameter, less variance in the ability distribution would imply less risk and hence more entrants under the new regime.

pride in bringing one's own business idea to market success, competitiveness, and - related to the previous paragraph - risk itself.

However, arguably the most relevant contribution from behavioral economics to research on entrepreneurship is the concept of overconfidence, which does a lot to explain why entrepreneurs exist despite the well-known weak odds of success. Åstebro et. al. follow Moore and Healey (2008) in differentiating three more precise biases that are often referred to using the general term of overconfidence: overestimation, overplacement and overprecision. The first refers to a general tendency to view one's own ability as higher than it is, while the second differs in that it requires a direct assessment of one's own ability relative to some comparison group - here you place yourself higher in the relative skill distribution than you really are, whereas in overestimation you exaggerate your "absolute" skill level. Finally, overprecision refers to excessive certainty about one's ability. Note that one can underestimate or underplace their skill while being overprecise: you might for instance be very sure that you are a lot worse at something than other people, while in reality having quite average abilities.

Overestimation and overplacement bias one's expected returns to the positive direction and hence can intuitively be expected to increase the likelihood of becoming an entrepreneur. Following Weinstein (1980), Åstebro et. al. further distinguish the related, often observationally equivalent trait of optimism, which refers to a general attitude of "good things will happen" (in contrast to overestimating or overplacing oneself in a specific situation). The effects of overprecision meanwhile are not as straightforward, as it might make some people undervalue exploration ("It's not worth trying, I know I'm not good enough" as opposed to "Maybe it will work or maybe not but I won't know without trying").

The implications of overconfidence on the effects of a cost-lowering reform then depend on the specific form of the bias as well as, to a large extent, on the prevalence of such biases among the different groups under consideration. For instance, one might conjecture that if entrepreneurs are more overestimating or optimistic (a hypothesis with some empirical support according to Åstebro et. al.), they would be less sensitive to variation in the entry cost, since they would be expecting to succeed anyway. Overplacing on the other hand - if prevalent in the whole population - might work to reinforce the gain in the entry rate following a lowering of costs, as the increased competition would be dismissed in the calculations of potential entrepreneurs.

The different forms overconfidence takes also have implications for whether or not it is desirable to try and curtail the "excessive" entry they it may cause - for instance by increasing entry barriers. As Åstebro et. al. point out: "while overestimation and optimism may trigger the pursuit of breakthrough innovations with strong positive externalities, overplacement may primarily lead to entry into already contested markets, and it may be associated more with imitation. Hence, while overplacement may still indirectly foster innovation through increased competition within a market, the potential positive externalities are much less clear." Furthermore, higher entry barriers may deter individuals who overprecisely underestimate their ability, even if they may potentially be very capable entrepreneurs.

One can also raise an evolutionary question of how overconfidence is selected for in the market: do such a traits increase or decrease the entrepreneur's chances of survival? If overconfidence is highly correlated with the decision to go into entrepreneurship but detrimental in the long run, the incumbents would be on average less overconfident,[16] and the entrants that drop out quickly would be the most overconfident. Such mechanics would provide an alternative explanation for the up-or-out dynamics of young firms and provide an avenue for interesting future research, but for current purposes the main implication is that, if this indeed is the case, the lowering of entry costs might bring in slightly less overconfident entrepreneurs with better chances to survive and grow

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[16]Though such an effect might just as well be the result of learning.

3.2 Aggregate Effects and Conditions

While the traits and preferences of individual entrepreneurs can help explain their decision to enter and subsequent performance, and thus have implications on the effects of a reform that changes the incentives of entry, further investigation is needed to investigate the potential aggregate effects such a reform might have. After all, it might be that any gains made by entrants are negated by losses on the side of incumbent firms. Furthermore, the broader economic conditions in play at the time of a reform may affect the effects of the reform itself.

Entry and exit in a stationary equilibrium

Drawing from two stylized facts from empirical studies - that firm dynamics are dominated by firm-specific uncertainty and that the entry and exit rates are highly correlated across industries - Hopenhayn (1992) presents the classic long run equilibrium model of firm dynamics in which firms face a series of individual productivity shocks and then optimize the timing of their exit. New firms must make a nonrecoverable investment before they observe their first shock, which is qualitatively equivalent to them being unaware of their ability - thus entrants face conditions similar to the Jovanovic-model. The novel contribution of Hopenhayn is to derive a stationary equilibrium which includes entry and exit - in contrast to previous models where they had converged to zero in the long run and been in that sense only a transitory mechanism on the way to the equilibrium. This provides a tractable framework for analyzing the long run effects of changes parameter such as the entry cost. The drawback is that the analysis is necessarily simplistic: for example, the entry and exit rates are equal by definition in the stationary equilibrium.

Nevertheless, the Hopenhayn model has a few interesting predictions for our discussion. The first results are very intuitive: a higher entry cost decreases entry - and hence also exit - and increases the expected lifetime of firms through decreasing selection. The effect on the size distribution of firms is less straightforward as there are two opposing forces at play: an increase in the entry cost has a price effect and a selection effect. The former refers to the fact that with less competition the output price increases leading to higher output and employment decisions across the possible shocks a firm might receive. On the other hand the decrease in competition means that there will be a higher fraction of firms receiving lower shocks (i.e. decreasing their output and employment) since they won't be selected out. The relative strength of the two effects is determined by the parameters of the shock process and the production function.

Aggregate fluctuations

Whereas the previous models have mainly considered uncertainty as firm-specific, Clementi and Palazzo (2016) extend the Hopenhayn-framework to an economy with aggregate fluctuations by letting the productivity of an individual firm be the product of an idiosyncratic shock and an aggregate shock common to all firms. Their modeling choices are in part motivated by the empirical observation that the entry rate is procyclical while the exit rate is countercyclical. Intuitively this makes sense: a positive aggregate shock makes entry more appealing and exit less appealing. It is what follows from the modeling choices made that has notable implications for the MCR reforms.

Whereas Hopenhayn's interest was in providing a stationary equilibrium framework that could be used for what essentially amounts to analysis of comparative statics, Clementi and Palazzo aim to model the impulse responses of an economy with entry and exit. Their main result is that, compared to an economy without them, entry and exit propagate the effects of aggregate shocks, boosting the persistence and unconditional variation of aggregate quantities. To see why, consider what happens with a positive shock to aggregate productivity. Initially, firms with worse idiosyncratic shocks will now find entry profitable, increasing the number but decreasing the average productivity (which equals the aggregate productivity in this case, since there is a unit mass of firms). The immediate effect is small, however, due to the fact that the output share of entering firms is small. In any cohort of entrants, some firms grow while others

exit, owing to their idiosyncratic shocks. Since the distribution of the idiosyncratic shocks is the same regardless of the size of the cohort, a larger cohort implies more young firms that will keep growing. Add to this the fact that, for a given amount of capital, firms with higher idiosyncratic shocks (i.e. firms that will grow more) are on average younger (since if a firm with a low shock has a lot of capital, it has had to have the change to build up that capital earlier when it had a higher shock), and the result is that the effects of the aggregate shock persist for a long time via the disproportional contribution of the cohort(s) born under the positive circumstances. Opposite arguments apply to a negative aggregate shock, with smaller cohorts of entrants resulting in more sluggish growth relative to normal times, compared to the case of no entry or exit. Indeed, the authors argue that an exceptionally large drop in the entry rate following the 2008 crisis is a major explanation for the slow recovery from it.

As in Hopenhayn's model, Clementi and Palazzo also include a fixed cost that each firm must pay upon entry. While they don't focus on exploring changes in that cost, in light of the propagation effects being mainly a function of the size of any entrant cohort, it's quite clear what the qualitative effect is: a higher cost implies less entrants for any shock, and hence less propagation of positive and more propagation of negative aggregate shocks. It's important to recognize how the sort of propagation the Clementi-Palazzo-model proposes can confound any estimation of the effects of entry barriers: exactly the same change in the level of the MCR, for instance, might have different causal effects under different macroeconomic conditions!

In sum, the only truly uncontested prediction derived from the theories discussed in this section is that the number of entrants moves to the opposite direction with the level of the entry cost. Another quite clear implication is that the survival probability of incumbent firms should fall. All the other components are up to debate, though the size and growth of entrants are unlikely to move to the opposite direction as the level of the entry cost. The survival of entrants and the growth rates of incumbents could go either way, and the total effects of changes in the entry cost will depend on the magnitudes of the individual changes in all the components relative to each other. Furthermore, they may depend on other aggregate shocks experienced by the economy as highlighted by the Clementi-Palazzo model, which makes the external validity obtained from the investigation of any individual reform at least quantitatively uncertain. For example, if the economy is hit by a negative shock shortly following the reform, it may recover substantially faster than it would have in absence of the reform, even if in "normal times" the reform would have had little effect. This becomes especially interesting for our specific discussion considering the fact that the 2006 reform was shortly followed by the 2008 financial crisis.

4 Estimating the Effects of the MCR

To test the predictions explored in the previous section, and evaluate the impact of changing entry barriers, I will make use of a long panel of Finnish firms, encompassing two reforms in the minimum capital requirement of limited companies. The data was presented in section 2.1, and the reforms as well as other necessary institutional background detailed in section 2.2. An important point regarding the reforms is that they were national reforms, affecting all firms in Finland - limited companies directly and other companies indirectly - at the same time. This makes distinguishing the potential causal effects of the reform from other shocks affecting firm dynamics complicated, as it is difficult to construct a credible control group that would not be affected by the reform. The identifying strategy explained in detail below tries to get around this problem by making use of the varying extent to which different industries are affected. That is, the basic idea is to compare changes in industries that are expected to be more affected by the reform to changes in industries that are expected to be less affected. The results of the approach are presented in section 4.2.

4.1 Estimation Strategy

To distinguish the potential causal effects of the MCR reforms from other factors affecting firm dynamics in the study period, I make use of a differences-in-differences strategy with heterogeneous treatment intensity across industries similar to Klapper et al. (2006) and Hombert et al. (2017). The idea of the estimator is simple: industries are ranked according to the treatment intensity (i.e. some index that reflects how much a given industry is expected to be affected by the reform), and the estimates are then obtained by comparing how the outcomes of interest change with the explanatory variable under study depending on the level of treatment intensity.

Following Hombert et al. (2017), once the treatment intensity rankings are obtained, the industries will be divided in groups based on the rankings. While they define industries on the 4-digit level and group them into treatment intensity quartiles, with Q1 expected to be the least affected by the reform and Q4 the most affected, I choose to define the industries at the 5-digit level instead to have the firms in the identified industries be as close competitors as possible. This is especially important when considering the effects on the incumbents - the looser the defined industries, the harder it will be to detect such competitive mechanisms. To maximize within-group precision in the outcomes and between-groups variation in treatment intensity, as discussed in more detail below, I also divide the industries into three groups instead of four, with G1 containing the third of industries with the smallest and G3 those with the largest treatment intensities.

The identification strategy can be expressed as the following equation:

$$Y_{it} = \alpha + \lambda_t + \mu_i + \sum_{k=2}^{3} \beta_k \left(G_i^k * post_t \right) + \epsilon_{it} \tag{2}$$

where Y_{it} is the outcome of interest for industry i and the cohort of firms starting in year t, α is a constant, λ_t refers to entry year- and μ_i to industry fixed effects. G_i^k are indicators equal to one if the industry belongs to the kth group (G1 being the reference group) and $post_t$ is a dummy equal to one for the post-reform period. β_k are the coefficients of interest, giving the average treatment effect for the industries in the kth group of treatment intensity. The main effects for $post_t$ and G_i^k are excluded, since they are linear combinations of λ_t and μ_i respectively. Note that the estimates of treatment effects obtained in this manner are necessarily relative to G1 industries. They can only be treated as absolute estimates of the effects by assuming that any changes in G1 industries are wholly unrelated to the reform. Thus, the estimates obtained for the effects on the number of entrants, for instance, are likely to understate the actual changes in entry numbers attributable to the reform.

Treatment intensity, assumptions and measurement error

The reform Hombert et al. (2017) study was aimed at unemployed individuals with limited capital who are more likely to start low-scale firms, so they expect industries with a larger fraction of sole proprietorships to be more affected. Hence, they define treatment intensity as the fraction of sole proprietors in an industry. Meanwhile Klapper et al. (2006) use the industry's "natural propensity of entry" (proxied by the entry rate in the U.S. where entry costs are assumed to be the lowest) as the treatment intensity index: industries with higher natural entry are expected to be more affected by higher entry costs. As the interest here is in what happens when one alters the MCR, the amount of firms founded at that threshold provides a natural measure for an industry's treatment intensity. Specifically, I define the treatment intensity as the average pre-reform ratio of the number of entrants founded with the minimum required share capital to the total number of limited company entrants. Due to the fact that the share capital data is available only for the years 2002-2016, the averages are taken over 2002-2005 and straightforward estimation is limited to the 2006-reform.

The intuition behind the treatment intensity measure is that the MCR is less of an obstacle for firms in industries where a higher share of entrants invest more than required in any case. The identifying assumption is then that, without the reform, the changes across industries would not have been systematically related to this measure of treatment intensity, i.e. the outcomes of interest would have followed parallel trends across the treatment intensity groups. As will be discussed in more detail later, depending on the outcome, the plausibility of this assumption ranges from imperfect to nonexistent. However, the imperfection does not seem too drastic for the most interesting results - namely those regarding the number and performance of limited company entrants - not to be at least qualitatively credible.

As the industries are defined at high specificity, many of them become quite small. This increases the likelihood of extreme deviations from true treatment intensity in the measure used. For instance, a lot of industries with few limited entrants per year may end up having measures of one or zero, even if the actual intensity they face is nowhere near so extreme. This decreases the accuracy of the treatment intensity measure, and makes detecting effects less likely. For this reason, all industries with less than 50 companies on average in the 2002-2005 and 2007-2017 periods are dropped out of consideration (this leaves around three hundred industries for estimation). However, there is always a possibility that such systematic dropping of observations biases the results. Importantly, the results are not robust to including all industries (see appendix A.4). Thus, if one wants to generalize the results to the entire set of industries and not just the set of industries used for estimation, one needs to assume that the lack of robustness is due to the measurement error and not due to selection issues (assuming of course that the main identifying assumption of parallel trends holds for the set of industries that are included).

Some additional assumptions are needed to make use of all the available information in the data at hand. In the case of the 2006 reform, if one assumes that the ranking of industries based on the treatment intensity is stable over time, one can also include the years 1998-2001 to the pre-reform period. This is done in the main results, though the estimates are also mostly robust for including only 2002-2005 (see appendix A.3).

Finally, note that in both cases, the reform went into effect in the beginning of September. Hence, as observations are made on a yearly level, both the 1997 and 2006 cross sections include firms founded both before and after the respective reforms. In most estimations then, the reform years 1997 and 2006 are ignored. This will also somewhat alleviate worries of expectation effects, where potential entrepreneurs might wait on entering until the MCR is lowered, or rush to enter before the MCR is raised.

The 1997 reform

The case of the 1997 reform is slightly more complicated, as there is no share capital data for the close years either before or after the reform. Furthermore, the ranking constructed using the pre-reform period of the 2006-reform corresponds in turn to the after-reform period in relation to the 1997 reform.

However, one can construct a slightly different treatment intensity measure, the ranking of which should nevertheless highly correlate with that of the measure defined above. This is done by taking the cross section of limited companies in 2002, keeping only those that were founded between 1991 and 1997, and dividing the number of firms in an industry that have the pre-reform minimum amount of share capital[17] with the total number of firms in that industry. Note that there is no need to assume that the firms founded with minimum capital are not less likely to exit than those founded with more, as long as they exit at similar rates across industries. Using this proxy is possible because the 1997 reform did not force existing firms to raise their share capital (although some did so[18] - thus another implicit assumption made is that the raising happened at similar rates across industries). Because the treatment intensity measure for the 1997 reform is less reliable, the estimates will mainly serve as a robustness check for the 2006 results, and a more detailed discussion will focus on the latter. Even so, one would expect the effects to be at least qualitatively opposite to each other. As discussed later, this is not quite the case, though there are several potential explanations, not all of which invalidate the results for the 2006 reform.

Outcomes

To holistically evaluate the effects of a change in the MCR on firm dynamics, it's important to consider not only how many new firms (if any) are created as a result, but also how the quality of those new firms changes, and how the incumbent firms react. Only with that information can one conclusively say what the total effect of the reform is. The outcomes described below attempt to unearth all of the aforementioned information, but in practice results with any information value are only obtained for the first two questions. However, it is still in itself informative to see how the other outcomes fail to be informative, and discuss why this might be the case.

To begin, the effects on the employment contribution of entrants are evaluated. Following Calvino et al. (2015), this can be broken down to the number, size and survival of entrants as well as the growth of surviving entrants. As in their report, I will specifically consider the three-year survival and growth of new firms. This is ultimately an arbitrary decision with a trade-off between the number of cohorts that can be included and the number of years each cohort can be followed. As a robustness check, the analysis is repeated for one- and five-year survival and growth, which does not change the picture much, though one obtains quite precise zeroes for the differences in one-year survival (see appendix A.4). Meanwhile, since a firm's "entry size" in the data at hand already includes whatever growth has occurred before the firm is measured for the first time, I will not estimate the effects on it separately. Hence, to estimate the entrants' contribution, the outcomes used in equation 2 are

1. The number of entering firms (in logarithms[19])

2. The share of a cohort surviving to age three

3. The average size (in logarithms) of three-year-old firms

Theoretically, the product of these three terms should equal the total three-year employment contribution of an entering cohort. To check if this holds, I also estimate the effects on the contribution directly using the (logarithm of) the total employment of three-year old firms.

Note that there are two ways to define the outcomes when it comes to the legal forms of the firms. Firstly, one could only count limited companies, which are the only firms directly affected by the reform

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[17]Note that the pre-reform requirement is in a different currency than the data, which has been converted to euros for the entire period. In the data, the modal value of the share capital in the relevant range is 2 526.3 euros, but there are also many firms coded as having a share capital of exactly 2 500 euros. Hence all firms with a share capital in the range of 2500 to 2530 inclusive are taken to have minimum capital, whereas in the numbers based on 2002-2005 only firms with exactly 8 000 euros are counted.

[18]In fact, it appears that firms were technically required to raise their share capital, but around 50 000 firms, or half of all limited companies at the time, did not adhere to the requirement, and it was eventually dropped in the 2006 reform (Federation of Finnish Enterprises, 2005; 2006).

[19]The number- and size outcomes are transformed to natural logarithms to decrease the noise arising from the fact that there are large differences in scale between the industries and individual firms.

(and usually considered to be more economically significant than the other forms). Indeed, this is the approach that will be taken in all subsequent initial estimations. However, that approach alone misses an important part of the potential effects, namely the substitution between the different legal forms. Thus, after obtaining the estimates when only limited companies are included, the regressions will be repeated with each of the four legal forms included in the outcome measures. In theory, comparing the results from these two parts of the estimation process should give us an idea of the significance of the role of substitution: if for instance the results for number of entrants imply a lot of new firms when considering only limited companies, but significantly less when all legal forms are included, this might be because the new limited companies would have been set up as some other form, absent the reform. In practice, though, the identifying assumption of parallel trends is substantially more plausible when only limited companies are included, so the estimates for all legal forms should be interpreted with caution.

It should also be pointed out that the estimated effects obtained in this fashion are averages over the post-reform period. However, as the phenomenon under study is inherently dynamic, there are potential confounding effects from feedback loops, i.e. the effects might differ depending on the length of the post-period. For instance, competition might be a negative feedback loop in the sense that high entry in one year might discourage entry in the next due to an increasingly saturated marketplace. If this is the case, the further one moves away from the reform, the smaller the difference in entry numbers between industries affected to different extents might get as the initial jumps in levels die down. Thus, the estimated effect might get smaller as years are added to the post-reform period, and not only because the addition of more observations decreases the likelihood of extreme results. Importantly, in this scenario, the effect should get systematically smaller as one moves away from the reform. Hence, as a robustness check, for each outcome of interest, the post-reform period is split into three sub-periods - 2007-2010, 2011-2013 and 2014-2017 - for which equation 2 is estimated separately. It turns out that this is not a substantial concern - the results, presented in appendix A.3, do not systematically change to one direction as one gets farther away from the reform (though they do not stay constant either - in fact it appears that the middle period 2011-2013 often obtains the largest estimates in absolute value, highlighting again the uncertainty related to the magnitude of the effects).

To make a final estimate of the total employment effects of the reform, on would also need to know what effects it has on the firms already in the market. On paper, this can be estimated utilizing the same estimation strategy used here for the entrant outcomes. However, this approach has some inherent problems. Whereas in the case of entrants one can estimate the effects averaged over the entering cohorts, in the case of incumbents there is only one group of firms for which it makes sense to estimate any effects: those that were in existence in the immediate pre-reform period (i.e. in 2005) - any cross section in the post-reform period already includes post-reform entrants, and any previous cross-section might include firms that have already exited before the reform ever takes place.[20] To estimate the effects on incumbents, then, one can take the cross section of firms in existence in 2005 and follow their numbers, average employment and total employment contribution (all in logarithms). Note that since substitution can't play a role here, all legal forms can be straightforwardly included in the estimation.[21]

This estimation, though, is a lot less reliable than in the case of entrants due to the lack of ability to average over cohorts. This leads not only to less observations, but also potential problems with extreme observations. If the years 2005-2006 for instance happen to be exceptionally good or bad for the incumbents of some industries due to some unrelated shocks, followed by regression to mean in the post-reform period, one might get erroneous results. Furthermore, the potential causal chain from the reform to incumbent outcomes is a lot less direct, and as the incumbents are on average significantly larger than entrants, other confounding characteristics may become more of a problem (as even in the

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[20]Strictly speaking, some of the firms existing in 2005 might still exit before the reform. However, the 2006 cross sections would already include post-reform entrants, so the earlier year is preferred.

[21]One might wonder, though, if estimations including only limited companies would yield estimates that make more sense, as seems to be the case for entrant outcomes. However, this does not appear to be the case in practice - while the estimates are omitted here for brevity, I did run regressions to test this.

entrant case the trends are unlikely to be perfectly parallel).

The concerns over the dynamic nature of the effects raised in the context of entrants also apply even more so here. It might be that the increased competition causes a significant difference in the exit rates of the incumbents shortly following the reform, but that the effect fades over time as the passing of time drops all but the most robust incumbents out of the industries regardless of the level of treatment intensity.[22] The same approach of splitting the post-period into three parts is followed to check if these concerns matter in the time frame under study. Again, little pattern emerges, though for the reasons mentioned the results are very uninformative to begin with.

As the results for incumbents are ambiguous, a better approach to evaluating the total effects of the reform might be to directly use the (logarithm of the) total industry employment as the outcome variable. This has the advantage of again being able to average over longer periods both before and after the reform. However, the estimation strategy also runs into trouble here. Namely, the treatment intensity groups clearly exhibit different trends prior to the reform, so that even though one does find results that suggest increasing employment with the treatment intensity, it is again impossible to say without further information what part of it is due to the reform. It is likely that the total employment effects are inflated by job creation coming from incumbents due to unrelated factors.

In total, equation 2 is estimated for 13 outcomes in the main results, eight of which consist of the four entrant outcomes - log number, three-year survival, log mean employment at age three and log total employment of a cohort at age three - evaluated separately with only limited companies and all legal forms included, three are the incumbent outcomes - log number, log mean employment and log total employment - which are only evaluated with all legal forms, and finally two are the log total industry employments with only limited companies and all legal forms. Out of these the entrant outcomes, specifically when only limited companies are included, yield the most interesting results. Due to the difficulties in estimating the effects on the incumbent outcomes or total employment, the overall effect of the reform remains somewhat of a mystery.

Discussion on the treatment intensity groups

As is evident from the above discussion, there are several ways one could present the basic information obtained from this type of estimation. One is the approach adopted by Klapper et al. (2006) where the continuous measure of treatment intensity, or a rank based on it, is used directly in the estimation. The advantage of this approach is that it avoids the arbitrary grouping of industries that is necessary in the approach of Hombert et al. (2017). However, two reasons lead me to favor the latter approach in the main results of this thesis.

Firstly, the interpretation of the coefficients is much more straightforward with the grouping approach. In the case of the single-variable approach, the interpretation of the coefficient is (roughly) either "the change in the outcome when one increases the treatment intensity by one unit" or "the change in outcome when one moves up a rank in the treatment intensity ranking". While this is qualitatively easy to understand, contextualizing such an estimate quantitatively becomes complicated. In contrast, the interpretation of the coefficients with grouping is "the average change in outcome in group X". As it is straightforward to calculate the pre-reform averages for the outcomes in any given group, this allows one to express the size of the estimated effects in intuitive terms with some rough calculations.

Secondly, the single-variable approach only produces a single coefficient estimate, which has a higher likelihood of being consistent with whatever hypotheses are made purely by chance, as interest tends to focus on the sign and significance of that single coefficient. Having at least three groups provides an additional automatic check of the sensibility of the estimates: if the assumptions behind the strategy hold and an effect exists, the estimates should monotonically increase or decrease as one moves from a lower

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[22]Indeed, if we had access to an infinite post-period, one would expect the estimated effect to converge to zero as at some point all incumbents exit, making the difference at that point zero, but years keep being added to the denominator of the average treatment effect.

treatment intensity group to a higher one. Grouping also allows one to directly detect potential non-linearities, whereas the single-variable approach without further investigation fixes a linear interpretation.

With the grouping approach, the next question becomes the number of groups one should use. The simplest grouping would be to split the industries in two, which would give a single coefficient with the interpretation of "the average change in outcome for industries with above median treatment intensity". However, this defeats the second benefit of grouping mentioned in the previous paragraph. Hence, in my view, if one is going to use grouping, one should use at least three groups. Whether to include more than three groups is a more complicated question, and depends on the data at hand. Each additional group obviously decreases the number of observation inside any given group, and hence the within-group precision of any estimation. Furthermore, the between-group variation in the treatment intensity measure declines with the addition of more groups, as the bounds for each group get tighter and the in-group averages get closer together. On the other hand, the confidence in the obtained results rises the more they are robust to increasing the number of groups: the likelihood of observing monotonically increasing or decreasing effects by chance decreases as one increases the number of groups.

Table 1: Distribution of the treatment intensity measure for different groupings

Group	Three groups			Four groups			Five groups		
	Mean	Max	N	Mean	Max	N	Mean	Max	N
1	0.083	0.117	130	0.074	0.108	99	0.066	0.101	79
2	0.138	0.156	131	0.124	0.141	99	0.113	0.125	79
3	0.235	0.514	135	0.154	0.171	99	0.140	0.150	79
4				0.261	0.514	99	0.164	0.187	79
5							0.280	0.514	80

Summary statistics of the distribution with and without the 50 firm minimum threshold

Threshold	Min	p5	p25	Median	p75	p95	Max	Mean	N
Yes	0	0.050	0.108	0.141	0.172	0.331	0.514	0.153	396
No	0	0	0	0.125	0.166	0.330	1	0.133	679

In the main results of this thesis, I choose to use three groups, which maximizes the within-group precision in outcomes and the between-group variation in treatment intensity, while still providing the sensibility check mentioned above. This is in contrast to Hombert et al. (2017), who use four groups in their main results. The reason I opt for three instead of four is illustrated in table 1, which describes the distribution of the treatment intensity measure (the last rows) and the mean and maximum values it takes inside groups using three different groupings: three, four and five. The main message of the table is that the variation in the pre-reform share of minimum capital entrants is quite low for most of the distribution: the difference in the values between the 5th and 75th percentiles is nearly the same as the difference between the 95th and 75th percentiles. This leads to the lower treatment intensity groups in all groupings to have close average treatment intensities while the highest group is always clearly above the others. This means that any effects in the low intensity groups become substantially harder to detect when more groups are added (importantly, though, it also means that even the in the three-group case, the difference between the highest group and the rest is driven by the industries with the very highest intensities). This is borne out in the estimations for the four- and five-group cases, which are presented in appendix A.1: the estimates for the highest group change surprisingly little for any outcomes, while the estimates for the lower groups are not robust to the addition of more groups.

Another important point to notice about the distribution of the treatment intensity is that setting the fifty-company size threshold for industries to be included changes it significantly (see the last two rows of table 1). Recall from the discussion above that the likelihood of extreme observations in the treatment intensity measure is larger for smaller industries. Indeed, in the no-threshold distribution there are a

lot more industries with zero-intensity. On the other hand, the maximum is also cut to half when the threshold is imposed, and the maximum ends up being barely a majority for minimum capital -entrants. This is curious considering the fact that the share of all limited company entrants with minimum capital on average before the reform is as high as 65% (see section 2.5). This suggests that the industries with higher treatment intensities that fall below the threshold may have in total a lot more entrants than those falling below the threshold with lower treatment intensities (especially since the number of limited company entrants for the included industries is the highest in group two in the three-groups case, as seen in table 2). Thus, a lot of the effect in terms of absolute limited companies founded may go undetected (though the average relative per industry effect may even be biased upwards - note that the mean intensity is slightly higher when the threshold is imposed).

Having explored the nature of the variation available for estimation and settled for three treatment intensity groups, a reality check on the characteristics of these groups is in order. After all, there probably are reasons why some industries have a lower share of limited companies entering with minimum capital than others, and, depending on the circumstances, these reasons may very well confound the results obtained. Table 2 presents summary statistics over the 1998-2005 period for different outcomes by the three treatment intensity groups used in the estimation of the effects of the 2006 reform. It's immediately obvious that the groups are not homogeneous in terms of the outcomes before the reform. Of course, this is not a problem for the estimation strategy as long as the development of the outcomes follows similar trends. However, the heterogeneity might reflect underlying factors that make the industries react differently to other shocks than the MCR reform, which would potentially confound the results. A particularly worrying observation is that, while none of the other outcomes increase or decrease monotonically with the treatment intensity group, the mean employment for both entrants (at age three, regardless of whether one counts only limited companies or all legal forms) and incumbents does. In other words, there appears to be a high degree of correlation between the treatment intensity measure and average firm size. Recalling that the 2006 reform was not a reform of the MCR alone, but a larger overhaul of the law on limited companies with a stated purpose of easing the life of small business owners, this suggests that the effects attributed to the lowering of the MCR may be inescapably confounded by other dimension of the reform that also potentially incentivized small firm entry.

Another way in which the groups are heterogeneous is their sectoral composition, as illustrated by figure 12. G1, i.e. the group with the lowest treatment intensity, is also the one with by far the highest share of manufacturing industries, whereas G2 and G3 are more service-industry dominant, while the share of retail and wholesale industries is quite similar across all groups. Intuitively this makes sense: firms in manufacturing industries probably require on average higher initial investments than in the service sector, so that more firms actually need to invest more than 8 000 euros whether it is required or not. This clearly poses a problem for the estimation, though, especially considering the macroeconomic environment of the study period. Namely, the 2008 crisis hit just a few years after the 2006 reform, and it's not hard to imagine that it might have affected entry into manufacturing industries more than service industries: the tightening of financial markets may have made it harder for potential entrepreneurs to get the funding necessary to set up a manufacturing business, while many service companies with relatively small initial costs and hence less need for outside funding would have still been able to enter. Indeed, a visual inspection of the parallel trends assumption for the main results, discussed in more detail in the next section, does suggest that this may explain part of the results. To address this concern, I use three strategies. Firstly, I drop the post-crisis period out of the estimations, leaving only 2007 for the post-reform period. Next, I exclude manufacturing industries from the estimation (keeping the groups otherwise the same). Finally, I make use of the fact that the retail and wholesale sector is similarly represented in all the treatment intensity groups, running the estimations using only those industries. The results from these robustness checks are reported in appendix A.2. Out of the more interesting results, the estimates for the number of entrants are robust to the first two strategies in the sense that they show the same pattern of increasing effects with the treatment intensity, though they do drop substantially in

magnitude. The survival estimates are even quantitatively similar apart from the estimate for the second group in the first strategy. In fact, the survival estimates are not dramatically altered even by the third strategy, though none of the other results survive dropping all industries apart from retail and wholesale. Thus it appears that the question of whether or not the 2008 crisis is responsible for the apparent effects through mechanisms other than the reform is a major one.

Table 2: Summary statistics for various outcomes by treatment intensity group, 1998-2005

Outcome	G	N	Mean	Median	Std. Dev.	Skewness	Kurtosis
Number of entrants (Lim)	1	1026	9.58	5	15.76	4.95	36.81
	2	1046	28.16	11	62.57	5.80	43.94
	3	1032	10.01	6	13.54	3.55	19.56
Number of entrants (All)	1	1026	19.96	11	26.86	3.14	14.99
	2	1046	87.47	28	167.44	4.89	36.14
	3	1032	32.24	15	56.03	5.29	41.69
Share alive at age 3 (Lim)	1	950	0.75	0.79	0.24	-1.09	4.17
	2	1023	0.74	0.75	0.19	-0.95	5.26
	3	991	0.77	0.80	0.22	-1.08	4.38
Share alive at age 3 (All)	1	1000	0.68	0.67	0.20	-0.38	3.53
	2	1040	0.65	0.64	0.14	-0.52	4.22
	3	1021	0.66	0.67	0.18	-0.29	3.81
Mean employment at age 3 (Lim)	1	930	15.18	3.35	44.50	8.82	115.82
	2	1007	9.19	2.96	38.74	16.24	353.72
	3	975	5.64	2.61	13.46	9.08	114.08
Mean employment at age 3 (All)	1	930	10.85	2.37	30.89	8.13	97.80
	2	1007	6.02	1.78	33.35	23.33	635.17
	3	975	3.34	1.63	6.73	7.96	89.67
Cohort employment at age 3 (Lim)	1	930	98.83	19.65	297.71	9.41	132.25
	2	1007	135.66	26.3	419.62	8.08	86.67
	3	975	54.61	15.7	138.38	7.10	72.47
Cohort employment at age 3 (All)	1	930	103.65	23.85	300.33	9.50	135.51
	2	1007	159.54	36.30	448.02	7.69	80.06
	3	975	80.50	24.3	178.01	5.60	49.62
Number of incumbents	1	129	252.15	145	273.01	2.58	10.80
	2	131	987.51	345	1904.78	3.91	19.67
	3	135	342.24	178	540.90	4.69	29.58
Mean employment of incumbents	1	129	11.28	5.56	15.26	3.01	15.68
	2	131	10.17	3.15	44.54	10.42	114.93
	3	135	5.31	3.33	5.57	2.17	7.83
Total employment of incumbents	1	129	2114.41	116.40	2573.44	2.06	7.73
	2	131	4027.47	1272.20	6946.09	3.41	16.67
	3	135	1237.33	675.50	1637.31	3.89	23.78
Total employment (Lim)	1	1026	2018.32	875.55	2600.31	1.95	6.69
	2	1046	3064.56	941.4	5350.13	3.59	18.49
	3	1032	1016.36	550.35	1448.67	4.01	25.70
Total employment (All)	1	1026	2170.76	1069.05	2700.80	1.93	6.44
	2	1046	3779.97	1247.05	6364.84	3.50	17.68
	3	1032	1201.69	673.50	1641.03	4.16	27.64
Total firms (Lim)	1	1027	153.20	92	195.23	3.76	20.83
	2	1046	315.02	151.5	580.87	5.41	39.80
	3	1031	187.75	99	354.46	6.21	51.14
Total firms (All)	1	1027	258.73	157	273.95	2.34	8.59
	2	1046	653.66	305	1146.12	5.36	41.56
	3	1031	623.45	182	1546.47	4.92	28.97

Note: The number of industries in the different treatment intensity groups is 130 for G1, 131 for G2 and 135 for G3.

Figure 12

Sectoral Composition of the Treatment Intensity Groups for the 2006 Reform
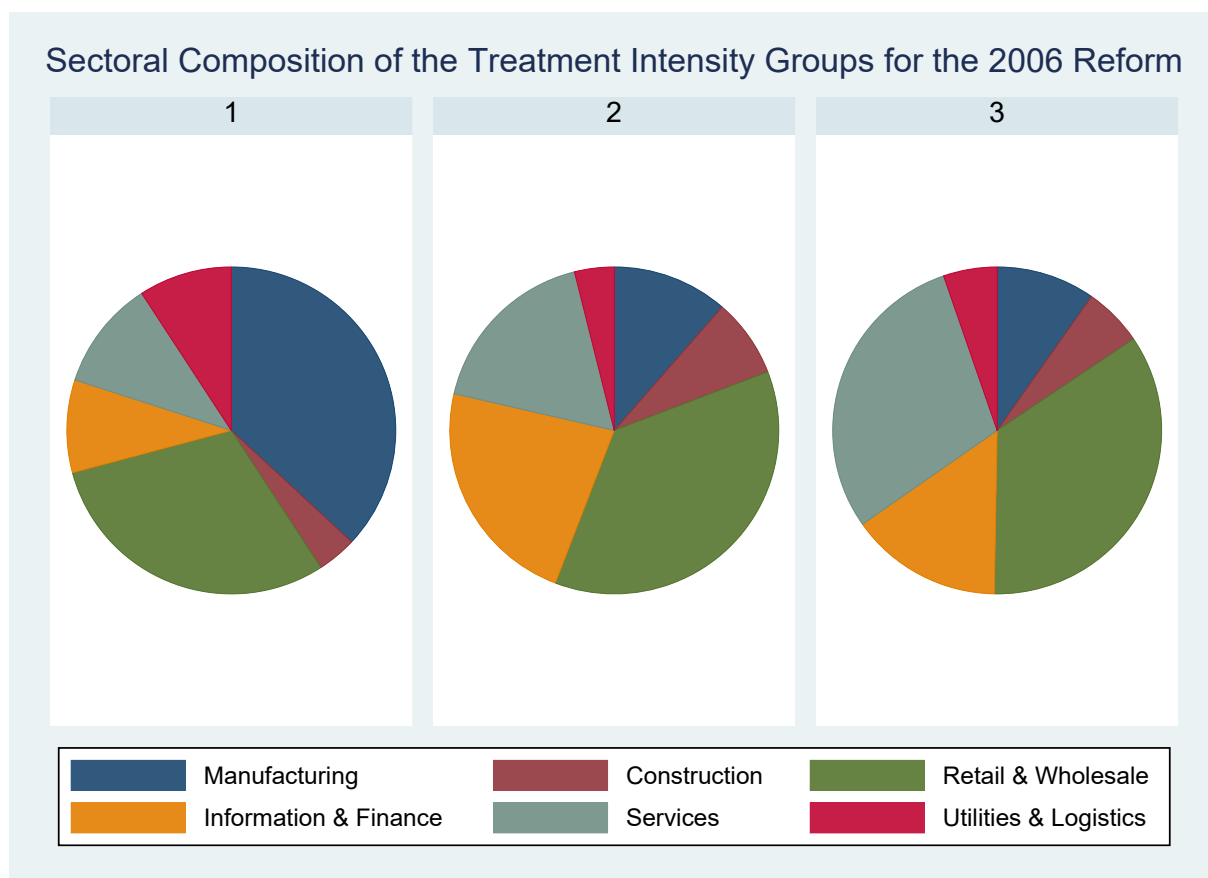
All in all, the heterogeneous sectoral composition of the treatment intensity groups combined with the 2008 crisis as well as the observation of firm size being correlated with the treatment intensity measure together with the larger context of the 2006 reform suggest that the main results are most likely substantially upwards biased. At the same time, though, it is worth keeping in mind that there is almost certainly downward bias coming from the fact that G1 is also likely to be affected by the reform, and the effects are potentially further underestimated because the treatment intensity in the included industries is clearly lower than in the entire set of entrants.

4.2 Results

With the above discussion in mind, this section moves on to presenting the empirical results obtained by estimating equation (2) for the different outcomes of interest. The main results concern the 2006 lowering of the MCR. The results for the 1997 raising of the requirement are reported separately at the end.

Number of entrants

For the number of entrants, the estimates presented in table 4 show significant positive coefficients for both interaction terms, with the number of entrants rising more in the industries with a larger share of entrants with minimum capital prior to the reform, regardless of whether one defines the outcome as including only limited companies or all legal forms. Intuitively, this is not surprising. What is surprising, though, is the size of the estimated effects.

Table 4: Treatment effects on the number of entrants

Log number of entrants	Regressor	Coefficient	Std. err.	95% Confidence interval	
				Lower bound	Higher bound
Limited only	Post*G2	0.152***	0.031	0.090	0.213
R^2=0.824 N=7 143	Post*G3	0.317***	0.031	0.256	0.379
All legal forms	Post*G2	0.082**	0.028	0.027	0.137
R^2=0.879 N=7 391	Post*G3	0.242***	0.028	0.187	0.297

p < 0.05: "*", <0.01: "**", <0.001: "***"

With the log-linear heterogeneous treatment intensity specification, it's worth carefully pointing out the interpretation of the coefficients. Recall that the reference category is the third of industries with lowest shares of minimum capital entrants (G1). For simplicity, suppose an industry is not in the middle third (G2) of treatment intensity, i.e. $G_i^2 = 0$ in equation (2), and consider what it means when the interaction for the highest third of treatment intensity (G3) and the post-period changes from zero to one. Let Y_{it}^0 correspond to the situation where $G_i^3 = 0$ or $post_t = 0$ or both so that $G_i^3 * post_t = 0$ and Y_{it}^1 to the situation where $G_i^3 * post_t = 1$. With the outcome in logarithms, the deterministic parts under the different situations can then be written as

$$lnY_{it}^0 = \alpha + \lambda_t + \mu_i$$
$$lnY_{it}^1 = \alpha + \lambda_t + \mu_i + \beta_3 \quad (3)$$
$$\implies \Delta lnY_{it} = lnY_{it}^1 - lnY_{it}^0 = \beta_3$$

Hence, the coefficient for the interaction of the indicators for the post-period and G3 is the difference between logarithms in the two situations, i.e. the part of any change attributable to either being in G3 instead of G1 in the post period, or being in the post-period instead of the pre-period when the industry belongs to G3 (with year- and industry fixed effects staying the same). Note that while the former interpretation is explicitly relative to G1, the latter interpretation also relies on the implicit assumption that G1 is unaffected: if the outcome changes in G1 industries as a result of the reform, the estimated coefficient is biased towards zero, so the true change when moving from pre to post when one belongs to G3 is likely larger.

Finally, note that

$$\Delta lnY_{it} = lnY_{it}^1 - lnY_{it}^0 = ln\frac{Y_{it}^1}{Y_{it}^0}$$
$$\implies \frac{Y_{it}^1}{Y_{it}^0} = e^{\beta_3} \quad (4)$$
$$\implies \Delta\%Y_{it} = \frac{Y_{it}^1 - Y_{it}^0}{Y_{it}^0} = e^{\beta_3} - 1$$

so that one gets an approximate[23] percentage change by taking the exponential of the estimated coefficient and subtracting one.[24] Thus - holding the assumption that G1 is unaffected - lowering the MCR causes approximately a 16% rise in the number of entrants in G2 industries, and a 37% increase in G3 industries

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[23]Approximate because any bias arising from the error term is ignored

[24]For small enough β_3, it holds that $\beta_3 \approx e^{\beta_3} - 1$. E.g. $\beta_3 = 0.100 \implies e^{\beta_3} - 1 \approx 0.105$. However, $\beta_3 = 0.300 \implies e^{\beta_3} - 1 \approx 0.350$. The latter is already quite a significant difference considering the scale we're dealing with here. Hence equation (4) will be used to obtain the percentage approximations.

when only limited companies are counted into the outcome. As the average number of limited entrants in the pre-reform period in G2 industries is 28 and in G3 industries 10, and there are 131 G2 industries and 135 G3 industries, this implies the yearly creation of roughly 0.16*28*131 ≈ 587 new limited companies in G2 industries and 0.37*10*135 ≈ 500 in G3 industries. In total then, the estimates imply a yearly average of about 1 087 limited companies created due to the reform. As there were around 94 000 limited companies in the data in 2005, this is a big effect, especially considering that it assumes G1 to be unaffected.

The reliability of the estimates of course critically hinges on whether or not one believes the parallel trends assumption to hold. To visually inspect its plausibility, I first regress the outcome of interest on the interaction of the entry year and the treatment intensity group, controlling for fixed effects of individual industries, and then plot the marginal effects of the individual years (with 1998 serving as the reference year) evaluated separately for each treatment intensity group. With perfectly parallel trends, one would expect to see no differences between the effects of individual years for the different treatment intensity groups in the pre-reform period - the lines in the plot should overlap up to the reform, and disperse after that if there is an effect.
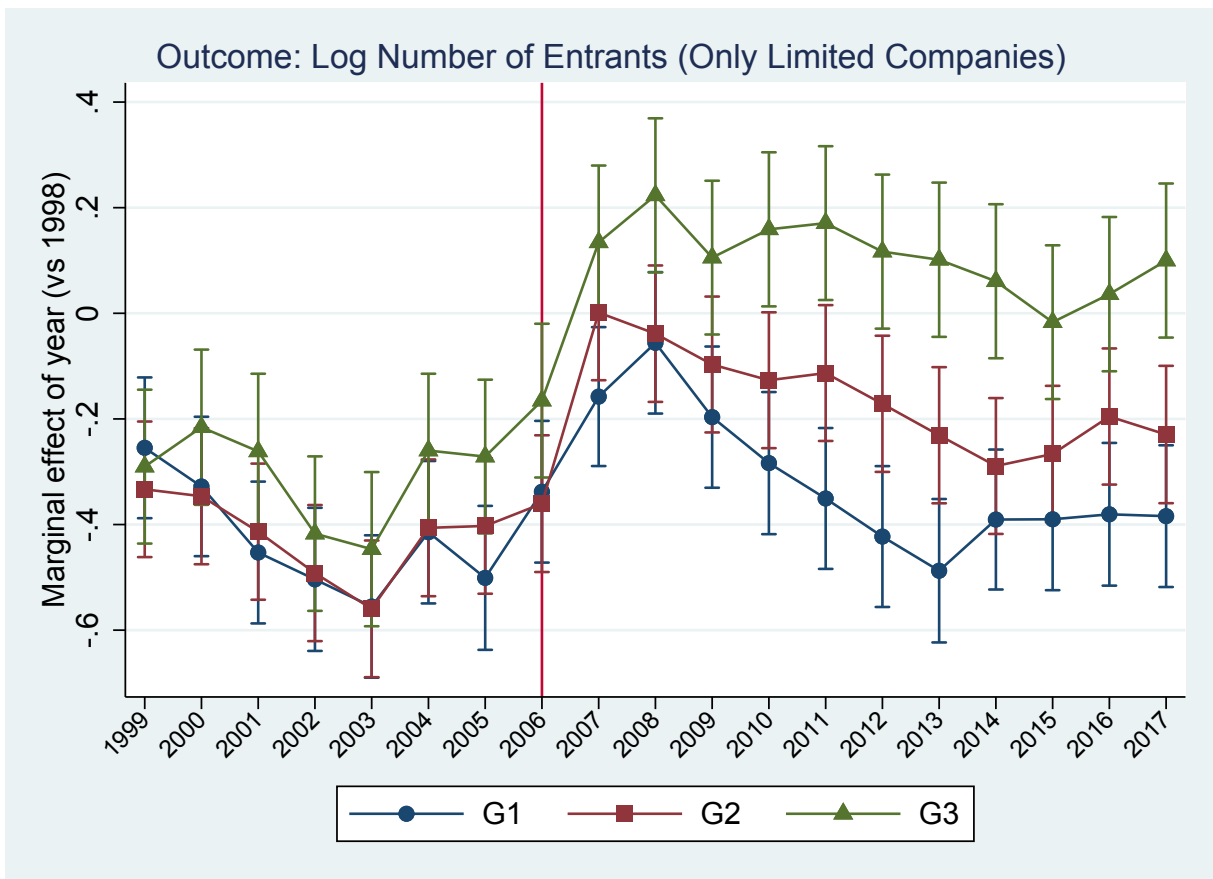
Figure 13a shows the plot for the number of entrants when only limited companies are included in the outcome. One observes that the marginal effects in the different treatment intensity groups in the pre-reform period are quite close together, all having overlapping 95% confidence intervals for the entire period, though the point estimates for G3 are in almost every period slightly higher than for G2 and G1, which overlap almost perfectly. Immediately after the reform, the 2007 marginal effect is the highest so far for all the groups, suggesting some effect from the reform even for G1, though the increase from 2006 is sharper for G2 and G3. After that, the differences start to clearly get wider, for G3 so much so that its confidence interval never again crosses that of G1 and rarely that of G2 after 2008. This pattern of closeness in the pre-reform period and dispersion in the post-reform period in the marginal effects of individual years across the treatment intensity groups is precisely what one would expect to observe if indeed G2 and G3 are more affected than G1. However, the unbiasedness of the estimates is called into question by the timing with which the dispersion happens. Notably, while the increase immediately after the reform is somewhat sharper in G2 and G3, the differences get much larger only after 2008, corresponding of course with the financial crisis.

Recall that the identifying assumption requires that, absent the reform, the development of the outcome would not have been systematically related to the treatment intensity measure. Thus, if we suspect that the crisis plays a role in the dispersion observed after 2008, the critical question becomes whether it happens because the lowering of entry costs has made the entry numbers in G2 and G3 more resilient to the crisis than in the less affected G1, or because of some other differences in the types of industries that comprise the groups. Relatedly, one can ask whether the amount of dispersion that is observed would have happened without the 2008 crisis. This is important for the external validity of the results obtained: even if the reform is the cause in the sense that it makes the entry numbers in G2 and G3 industries more resilient to the crisis than in G3, the resulting estimates may give too rosy a picture of what one can achieve by lowering entry costs in normal times. This would not mean that the results are biased in the sense that they underestimate the creation of firms that would have happened in the counterfactual case of no reform, but it would make it less likely that, for instance, policy makers in other countries could expect quantitatively similar results by implementing the same reform today.
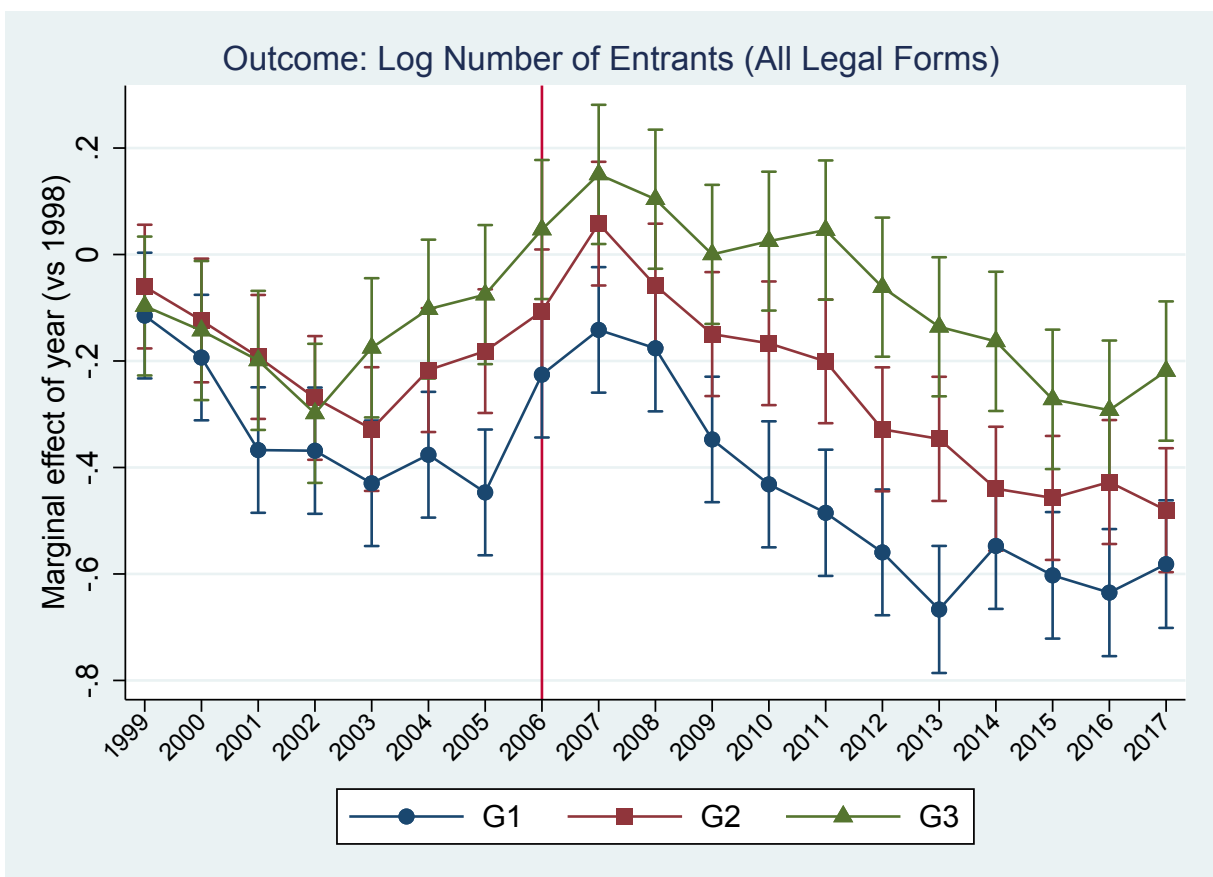
As previously discussed, G1 may have been more affected by the crisis simply due to its large share of manufacturing industries. Dropping the post-crisis years from the estimation or excluding manufacturing industries yield substantially lower estimates (though the pattern of increased effect with rising treatment intensity remains), while the estimates are not at all robust to including only retail and wholesale industries (see appendix A.2). This suggests that much of the "effect" is indeed explained by the 2008 crisis and probably due to characteristics other than the treatment intensity. Still, a significant effect remains with the post-reform period consisting only of 2007, a result that is not surprising in light of figure 13a.

Figure 13

(a)

Outcome: Log Number of Entrants (Only Limited Companies)

(b)

Outcome: Log Number of Entrants (All Legal Forms)

Adding all legal forms to the outcome, the estimates are somewhat different. The increase in G2 industries drops to around 9% and in G3 industries to around 27%. With all legal forms counted, the number of entrants in the pre-reform period is on average 87 in G2 and 32 in G3 industries, which with a similar rough calculation as in the case of limited companies translates to a total of around 1 026 new firms per year in G2 and 1 166 in G3 industries. If there was major substitution between the legal forms, one would expect the absolute number of yearly created new firms attributable to the reform to be smaller when counting all legal forms to the outcome, because the reform should not encourage the entry of new non-limited companies, while some of the new limited companies would have been set up as non-limited companies if the reform didn't happen.

It is surprising then that the estimates together with the sizes of the industries imply a clearly higher number of new entrants when all legal forms are counted. However, the parallel trends assumption is quite suspect when all legal forms are included in the number of entrants, as can be seen from figure 13b. If, as it appears, G2 and G3 are already on an upward trend before the reform while G1 is not, the estimates will overstate the number of new entrants attributable to the reform, so that the role of substitution is hidden.

Another potential confounding factor is if substitution happens to a greater extent in the G1 industries. If that is the case, the effect on them with all legal forms counted will be relatively smaller, increasing the difference between them and the industries in the other groups. Some entrepreneurs may also set up non-limited companies initially, even if they are encouraged by the eased possibility of setting up a limited company later down the line. Finally, and most speculatively, there may be some kind of herding effects at play, where founders of non-limited companies are encouraged to enter industries after an initial rush by non-limited companies, as they perceive these industries to be attractive since so many others are entering them. Whatever the reason, the estimates in the all-legal-forms-case largely fail to satisfy their purpose, leaving one uncertain over whether the increases observed for limited companies are mostly companies that would have been set up using some other legal form in absence of the reform.

Survival of entrants

The survival of entrants can be seen as an aspect of their quality, drawing from the entrepreneurial ability of their managers as discussed in section 3. Recall that the theoretical prediction for the effects of lowering entry costs depends largely on whether the managers themselves are aware of that ability - with informed managers there is a trade-off between the quantity and average quality of managers, while with uninformed managers entry costs are simply barriers to trial and error, so that the average quality should not be affected, though increased competition might still decrease survival. The estimates presented in table 5 indeed show a slight negative effect on the share of a cohort alive at age three with the effect importantly more negative for G3 than G2 industries, though the estimates are quite small.
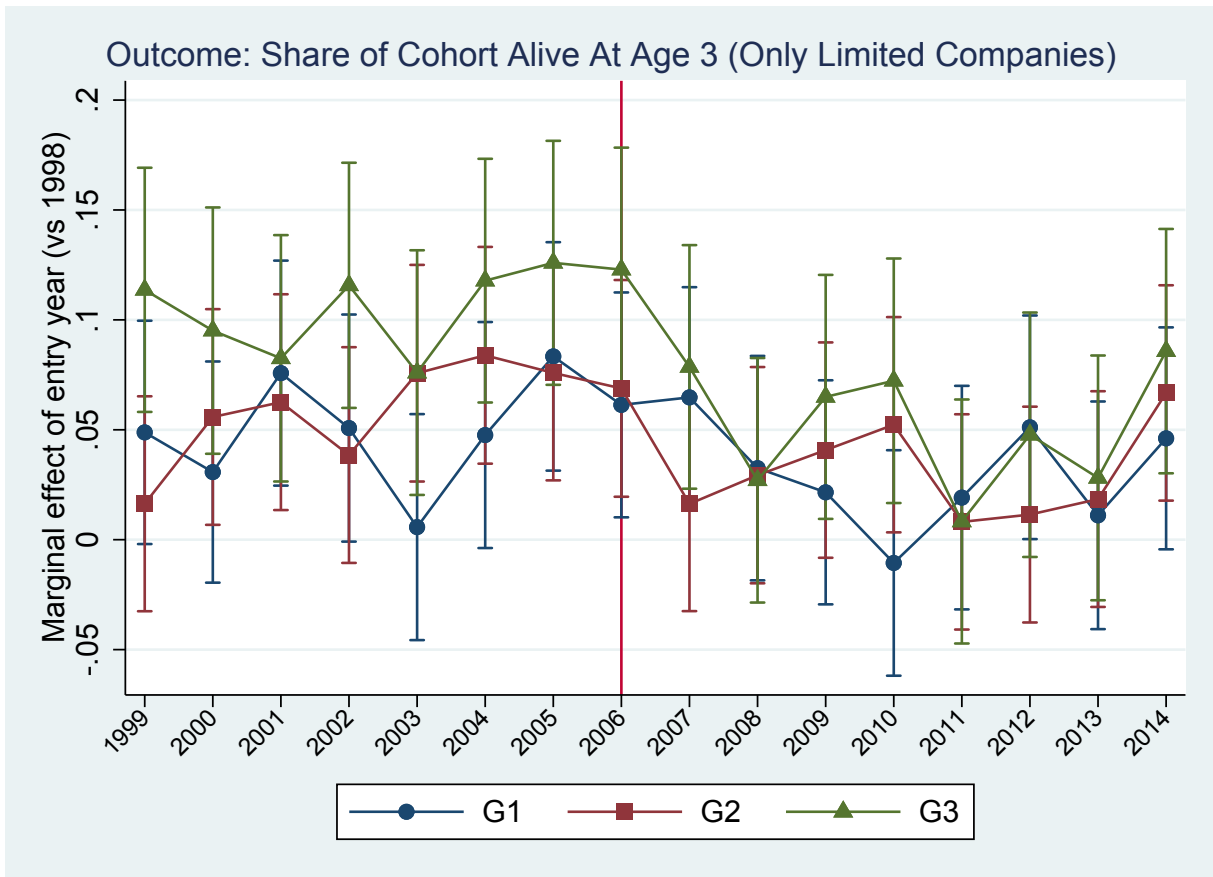
Table 5: Treatment effects on the survival of entrants

Share of cohort alive at age 3	Regressor	Coefficient	Std. err.	95% Confidence interval	
				Lower bound	Higher bound
Limited only	Post*G2	-0.008	0.013	-0.033	0.017
R^2=0.178 N=6 016	Post*G3	-0.029*	0.013	-0.054	-0.003
All legal forms	Post*G2	-0.003	0.010	-0.022	0.016
R^2=0.279 N=6 231	Post*G3	-0.031**	0.010	-0.050	-0.012

p < 0.05: "*", <0.01: "**", <0.001: "***"

Figure 14

(a)

Outcome: Share of Cohort Alive At Age 3 (Only Limited Companies)

(b)

Outcome: Share of Cohort Alive At Age 3 (All Legal Forms)
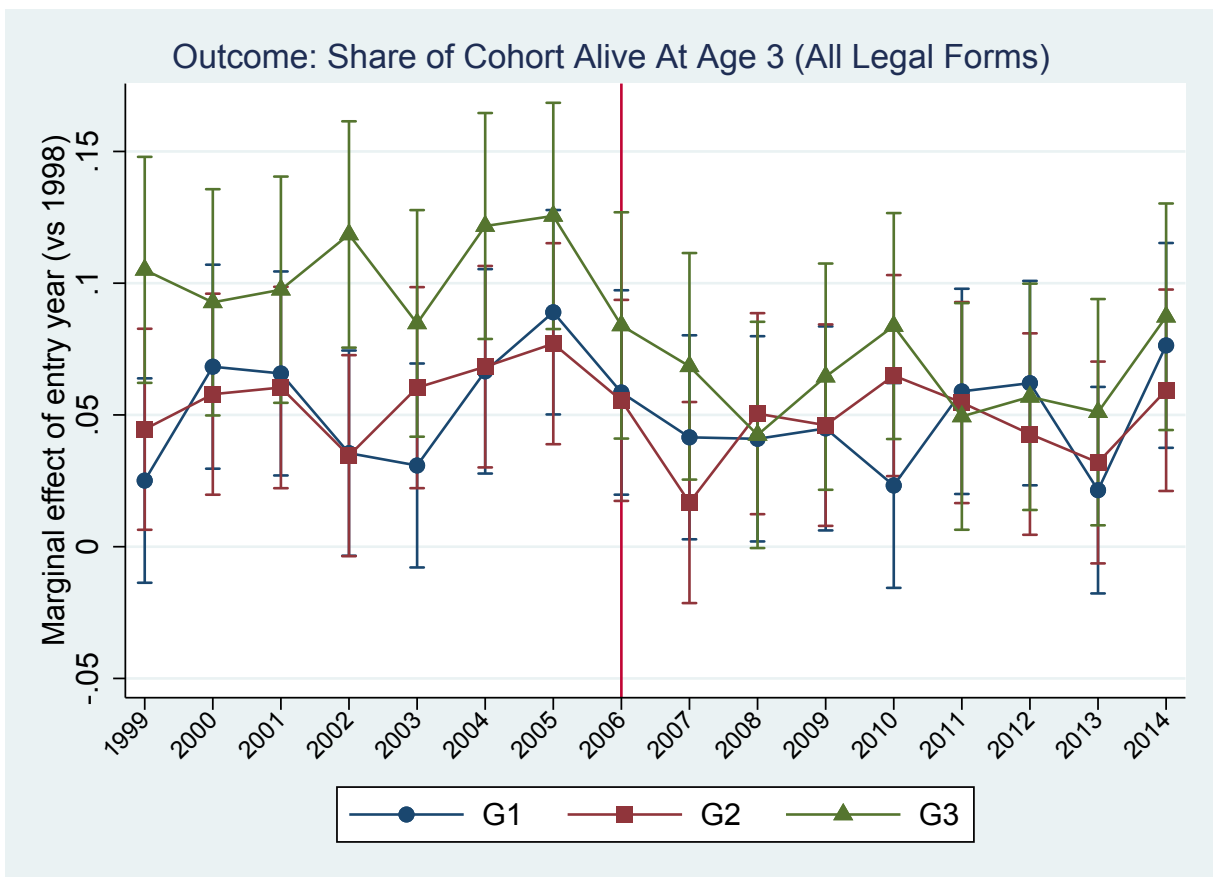
As the outcome here is in percentages rather than logarithms the estimate gives the difference in percentage points. With only limited companies included, then, the share alive at three years drops by 0.8 percentage points more in G2 industries and 2.9 percentage points more in G3 industries than in G1 industries. As the pre-reform average 3-year survival in G2 industries is 0.74 and in G3 industries 0.77, the reform drops the survival in G2 to around 0.73 and in G3 to around 0.74. The relative drop for G2 industries is approximately 1.1% and for G3 industries 3.8%. Once again, it is worth pointing out that these numbers are biased towards zero if there is a change to the same direction but a lesser extent in G1 industries.

With all firms included, the estimate for G2 drops even closer to zero, while the estimate for G3 changes very little. However, the pre-reform averages are clearly lower when all companies are considered, which is not surprising recalling the discussion on figure 9. Hence, while for the near-zero G2 the relative change is still smaller in the all companies -case, for G3 it is in fact larger. This may hint that something about the protection of limited liability is inherently good for the survival of companies: as more founders set up a limited company after the reform, the average survival probabilities among all companies rise relatively more than among only limited companies simply due to the fact that a higher fraction of entrants is now limited companies, which are on average more likely to survive to age three.

A visual inspection of the parallel trends assumption for survival (see figure 14) suggests that the estimates for G3 may in fact be biased towards zero. Regardless of whether one includes only limited companies or all legal forms, the marginal effects of years are consistently higher for G3 than the other groups prior to the reform, but afterward the dispersion actually becomes smaller. If G3 is on an upward trend prior to the reform and G1 isn't, the counterfactual trend that estimation for G3 is based on will be closer to the realized trend than the actual trend would have been, absent the reform.

Growth of surviving entrants

The other aspects of the quality of entrants are their size and post-entry growth. Since these two are to some extent necessarily indistinguishable from each other in the yearly data (size at age 0 already includes whatever growth happened up to the point at which it was measured), I group them together and estimate the effects of the reform on the (logarithm of the) mean size (in terms of employment) of entrants at age three. Note that there are several reasons why one industry might have a higher mean size of three-year old firms than another: the firms might already enter larger or grow faster, but it may also be the result of the smallest or non-growing entrants in an industry exiting faster as the firms that exit before age 3 are not counted in the denominator.

Table 6: Treatment effects on the growth of surviving entrants

Log mean emp. at age 3	Regressor	Coefficient	Std. err.	95% Confidence interval	
				Lower bound	Higher bound
Limited only	Post*G2	0.048	0.061	-0.072	0.168
R^2=0.392 N=5 910	Post*G3	0.029	0.061	-0.091	0.149
All legal forms	Post*G2	0.030	0.056	-0.080	0.140
R^2=0.462 N=6 210	Post*G3	-0.022	0.056	-0.132	0.087

p < 0.05: "*", <0.01: "**", <0.001: "***"

Figure 15

(a)

Outcome: Log Mean Employment At Age 3 (Only Limited Companies)

(b)

Outcome: Log Mean Employment at Age 3 (All Legal Forms)
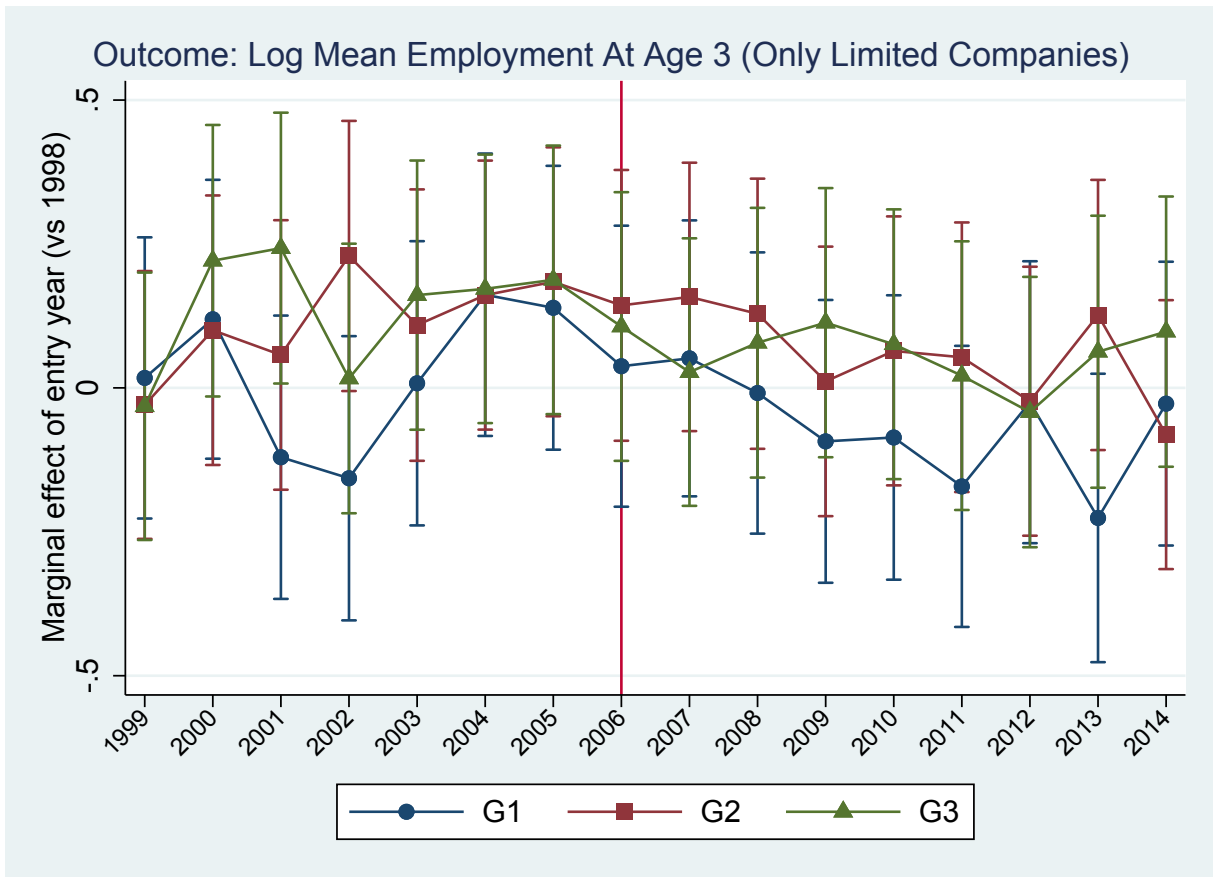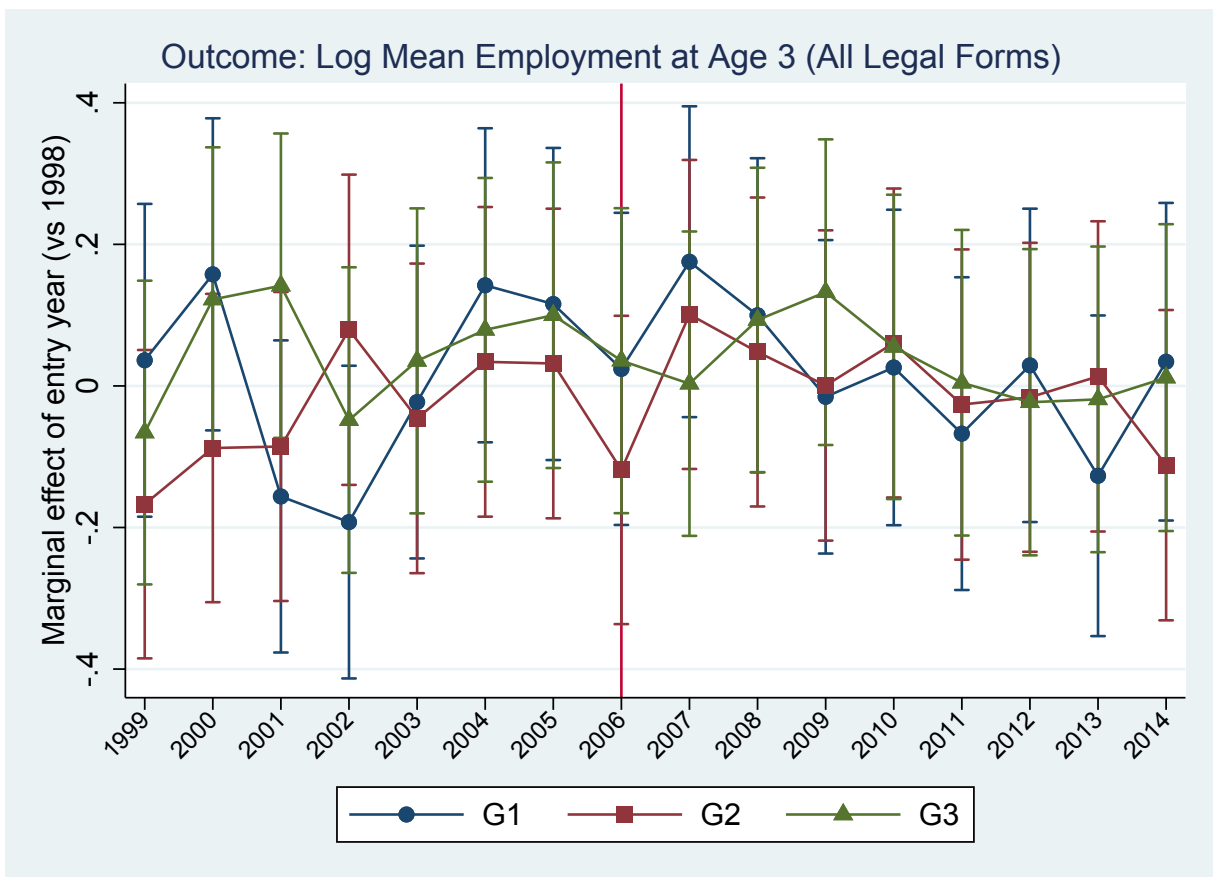
The theoretical predictions for the size and growth of entrants again depend on the extent to which the founders are informed of their abilities. The fact that the estimates in table 6 are not statistically different from zero, and not monotonically increasing or decreasing with the treatment intensity measure, gives credence to the view that entrepreneurs may not be very well informed of their abilities before they enter the market. Note that the estimates on the survival on the other hand were consistent with the view of informed entrepreneurs. One explanation for this discrepancy might be the distinction between subsistence and transformational entrepreneurs: for the former, maximizing survival rather than growth might be the objective, so that in a world of informed subsistence entrepreneurs, one would indeed expect the new entrants to be less able to achieve that objective, while growth would remain unaffected. Another interpretation is that the slight drop in survival is simply driven by increased competition, and once uncompetitive firms are selected out, the growth of the survivors is not affected. However, it is also important to stress that the estimates for growth are very imprecise, so that absence of evidence for an effect does not necessarily translate to evidence of absence.

To get a sense of the scale of the estimated coefficients, note that as the outcome is again in logarithms, the interpretation is similar to the number-of-entrants-case. When only limited companies are included, the average size of entrants at age 3 in the pre-reform period is 9.19 for G2 and 5.64 for G3. Using equation (4), one gets an approximate percentage change of 4.9% for G2 and 2.9% for G3 industries. Thus, according to the estimations, the average firm in G2 employs about $0.049 * 9.19 \approx 0.45$ and in G3 about $0.029 * 5.64 \approx 0.16$ more people at age three due to the reform, again assuming that G1 is not affected and the parallel trends assumption holds. The picture does not change much when all legal forms are included, though now the estimate for G3 turns negative, which is what one would expect for informed entrepreneurs with an objective to grow. However, both estimates are now even closer to zero than in the only-limited-companies-case, and since the pre-reform averages are also smaller when all companies are included, this implies smaller absolute changes in the average employment of entrants at age 3.

Hence the estimated changes in the average employment of firms are quite small. Since the estimates are mostly slightly positive but statistically not different from zero, and the theoretical mechanisms suggest that, if anything, the effect should be negative, one can with some confidence say that the reform appears to have no effect on the growth of surviving entrants. Figure 15 gives some support for the view that there is no effect from the reform. When only limited companies are included, the point estimates for the yearly marginal effects are in most cases the smaller for G1 than G2 and G3 both before and after the reform, but for all years the estimates for all groups are close to zero (though very imprecise) and there is no clear change in pattern when the reform happens. When all legal forms are included, there are even less discernible patterns, as the estimates for G1 are no longer consistently smaller than for the other groups. All in all there seems to be no effect, though the estimates are too imprecise to be certain.

Total contribution of entrants

Having estimated the effects on the number, growth and survival of entrants, one can use them to calculate the effect on the total contribution of entrants - specifically by the definitions used here, the net job creation of entrants by age three. However, one can also directly estimate this from the data by comparing the sum of employment by three-year-old firms that enter before or after the reform. The estimates in 7 are qualitatively and quantitatively similar to those in table 4, which could be expected considering the near-zero effects reported in tables 5 and 6. Figure 16, presenting the visual inspection of parallel trends, also provides no surprises in light of what has already been discussed: the general pattern is similar to the number-of-entrants-case, though with more noise coming from the survival and growth components, and the assumption seems more plausible when only limited companies are included.

Recall from the calculations in the previous sections that, if one only considers limited companies, the estimates imply the yearly creation of roughly 587 new firms in G2 attributable to the reform, with

firms surviving to age with probability 0.73, and growing to an average size of $1.049 * 9.19 \approx 9.64$ if they manage to survive. For G3 industries meanwhile, there are around 500 new firms with a three-year survival probability of 0.74 and an average size of $1.029 * 5.64 \approx 5.80$ upon survival. Hence, the reform would have resulted in around $587 * 0.73 * 9.64 \approx 4130$ more jobs in G2 industries and $500 * 0.74 * 5.80 \approx 2146$ in G3 industries every year from 2010 (the year that the first post-reform cohort turns three) onwards. From the direct estimation, using equation (4), one obtains approximate growth rates in the total employment of three-year-old firms of 17% for G2 and 34% for G3 industries. With pre-reform averages of 135.66 and 54.61 respectively, this then implies around $131 * 0.17 * 135.66 \approx 3021$ new jobs per year in G2 and $135 * 0.34 * 54.61 \approx 2507$ in G3 industries attributable to the reform. Hence the direct estimation implies a lot less jobs created in G2 industries and somewhat more in G3 industries than the indirect calculations.

Since the effects on survival and growth were statistically speaking quite insignificant, one may also want to check what happens if they are just assumed to be zero. In that case, one obtains the number of jobs created simply by multiplying the number of firms created with the original survival probability and size at age three. This sort of calculation would in turn yield $587 * 9.19 * 0.74 \approx 3992$ yearly new jobs in G2 and $500 * 0.77 * 5.64 \approx 2171$ in G3 industries. These then fall in between the direct and indirect calculations, though much closer to the latter, which is not surprising as the changes in the survival and growth were indeed not large.

It then appears that the changes in the number of entrants are the main component driving changes in the total contribution of entrants, as the changes in survival and growth rates are way less striking, and the direct estimation yields numbers in the same ballpark. Recalling the empirical context that most firms are small and exhibit little to no growth, it is likely that the job creation among new firms attributable to the reform is the result of a lot of small business founders setting up companies that provide a job for themselves and perhaps some members of their families, rather than a surge in the number of ambitious startups that end up employing a lot of people. The critical question in terms of total employment effects then becomes whether these new founders were previously employed or unemployed. Another missing piece of the picture is what happens to the employment of the firms that are already in the market when the reform happens. To try to shed some light on these questions, the next step would be to evaluate what happens to the incumbents and total employment in industries with different treatment intensities. In practice, though, it turns out that these estimates add very little information.

Table 7: Treatment effects on the employment contribution of entering cohorts

Log total cohort emp. at age 3	Regressor	Coefficient	Std. err.	95% Confidence interval	
				Lower bound	Higher bound
Limited only	Post*G2	0.159*	0.073	0.015	0.303
R^2=0.601 N=5 910	Post*G3	0.290***	0.073	0.146	0.434
All legal forms	Post*G2	0.089	0.067	-0.041	0.220
R^2=0.646 N=6 210	Post*G3	0.168*	0.066	0.038	0.297

$p < 0.05$: "*", <0.01: "**", <0.001: "***"

Figure 16

(a)

Outcome: Log Total Cohort Employment at Age 3 (Only Limited Companies)

(b)

Outcome: Log Total Cohort Employment at Age 3 (All Legal Forms)
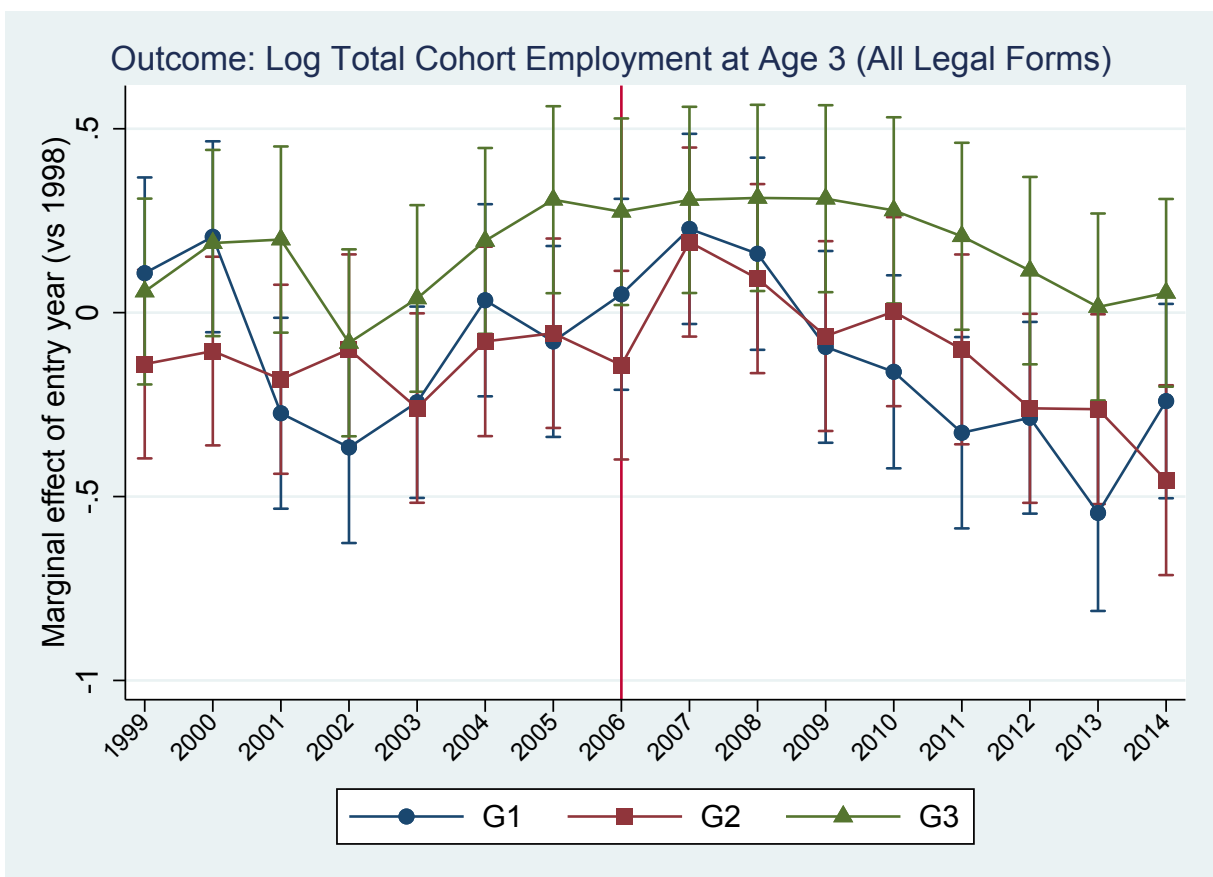
Effects on incumbents

When it comes to the incumbents, the theoretical discussion predicts quite unambiguously that lowering entry barriers should decrease their survival probability, while their growth and total employment contribution could go either way depending on the relative strengths of the underlying mechanisms. To estimate the effects on incumbents, I take the cross section of firms that is in existence in the immediate pre-reform period, i.e. in 2005. Note that since substitution should play no role as no new incumbents can be set up, all estimates include all legal forms.

I first estimate the difference in the (logarithm of) the number of incumbents. Note that by definition this number can only decrease over time as no new incumbents can be set up. What one would expect based on the theory is that the number would decrease more in G2 industries than in G1, and even more in G3. The first part of that prediction is indeed borne out by the estimates in table 8. However, while the effect on G3 industries also seems to be negative (though insignificant), it is less so than for G2 industries. This suggests that something else than the reform is driving the estimates.

The average size of the incumbents, meanwhile, does follow a monotonic pattern from one treatment intensity group to another. Interestingly, the effect appears to be positive, suggesting that incumbents in industries with more new competition grow faster than those in industries with less of an increase in new entrants. Note that the "growth" of the average here is not necessarily the result of growth of individual firms: it could just mean that less successful incumbents are screened out by the new competition, as would happen in the Hopenhayn-framework if the selection effect dominated over the price effect. However, this interpretation is somewhat at odds with the observation from the number of industries that selection does not seem to happen systematically more in industries with higher treatment intensity. The estimates for the sum of incumbent sizes, i.e. their total employment contribution, also reveal no systematic pattern with respect to treatment intensity, with G2 being apparently negatively affected and G3 significantly positively affected.

All in all, the estimates in table 8 do not tell us much about the effects the MCR reform has on firms already in the market. This is likely due to the factors already mentioned: having to do the estimation utilizing only one cohort of firms leads both to less observations and the lack of ability to control for year-effects, and the fact that incumbents are on average larger combined with the less direct causal chain may make confounding factors more of a problem than they were for entrants, hiding any potential signal under layers of noise. Although the pattern of the estimates for the size of incumbents does make sense in light of the Hopenhayn-framework, considering the context of the estimates for the number and total contribution, one should probably not take them too seriously either.

Visual inspection of the parallel trends assumption, presented in figures 17-19, supports the view that the incumbent-estimates should be given very little weight. Note that he pre-reform period now only includes 2005 and 2006, so that there is only one point at which the difference in marginal year effects can be inspected before the reform happens (2005 being now the reference year).For the number of incumbents, one observes that there is very little difference between the marginal effects of 2006, followed by a steadily increasing dispersion in the post-period. This is precisely what one would expect if the reform had an effect on the incumbents via yearly increasing competition, were it not for the fact that G2 is the group consistently below the others, which suggests something else than the effects of the reform is at play. For the average size of incumbents on the other hand, one observes an almost monotonic ordering in the post-period - though G1 catches up to G2 in 2015 - but G3 seems to be a bit above the others already in 2006. Finally, for the total employment of incumbents, one again unsurprisingly observes something that shares elements from the two factors that make it up. In sum, for the incumbent outcomes, the 2006 marginal effects are close enough to each other to suggest that parallel trends may hold, but after the reform the marginal effects are not systematically monotonically increasing or decreasing with the treatment intensity. Hence, making causal interpretations in the case of the estimates obtained for the incumbents is highly suspect.

Table 8: Treatment effects on incumbent outcomes

Outcome	Regressor	Coefficient	Std. err.	95% Confidence interval	
				Lower bound	Higher bound
Log number of incumbents	Post*G2	-0.063***	0.011	-0.086	-0.041
R^2=0.989 N=5 148	Post*G3	-0.019	0.011	-0.041	0.003
Log mean employment of incumbents	Post*G2	0.044*	0.020	0.004	0.084
R^2=0.964 N=5 148	Post*G3	0.108***	0.020	0.069	0.148
Log total employment of incumbents	Post*G2	-0.019	0.022	-0.063	0.242
R^2=0.973 N=5 148	Post*G3	0.089***	0.022	0.046	0.133

$p < 0.05$: "*", <0.01: "**", <0.001: "***"

Figure 17

Figure 18

Outcome: Log Mean Employment of Incumbents

Figure 19

Outcome: Log Total Employment of Incumbents

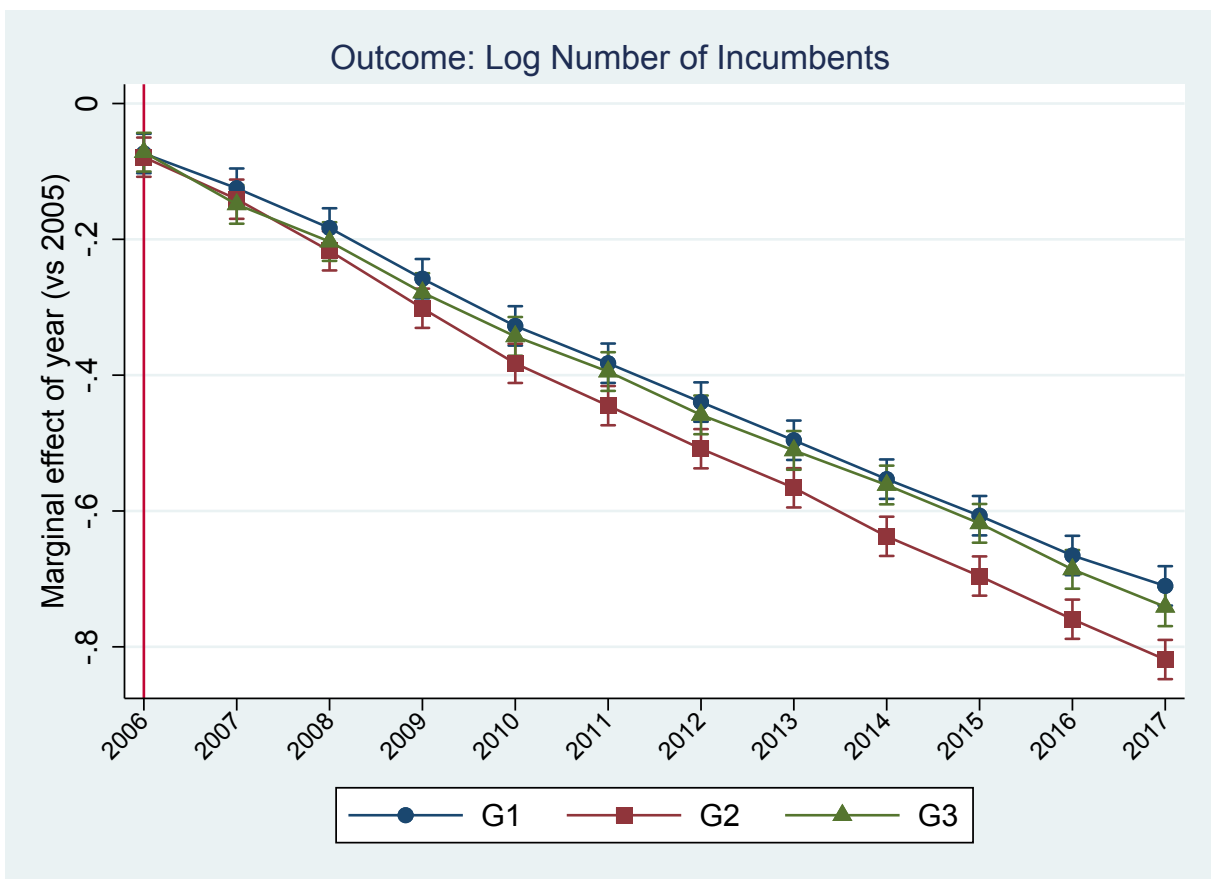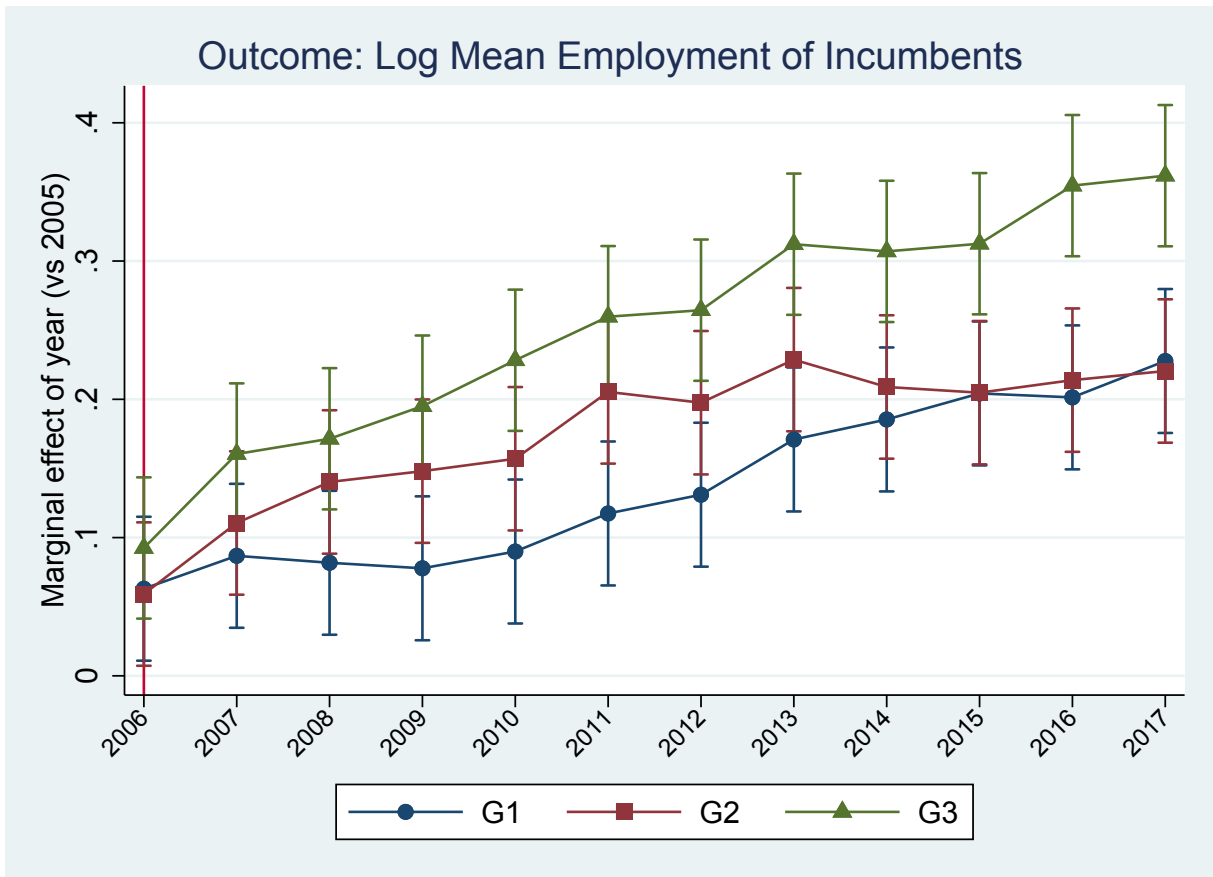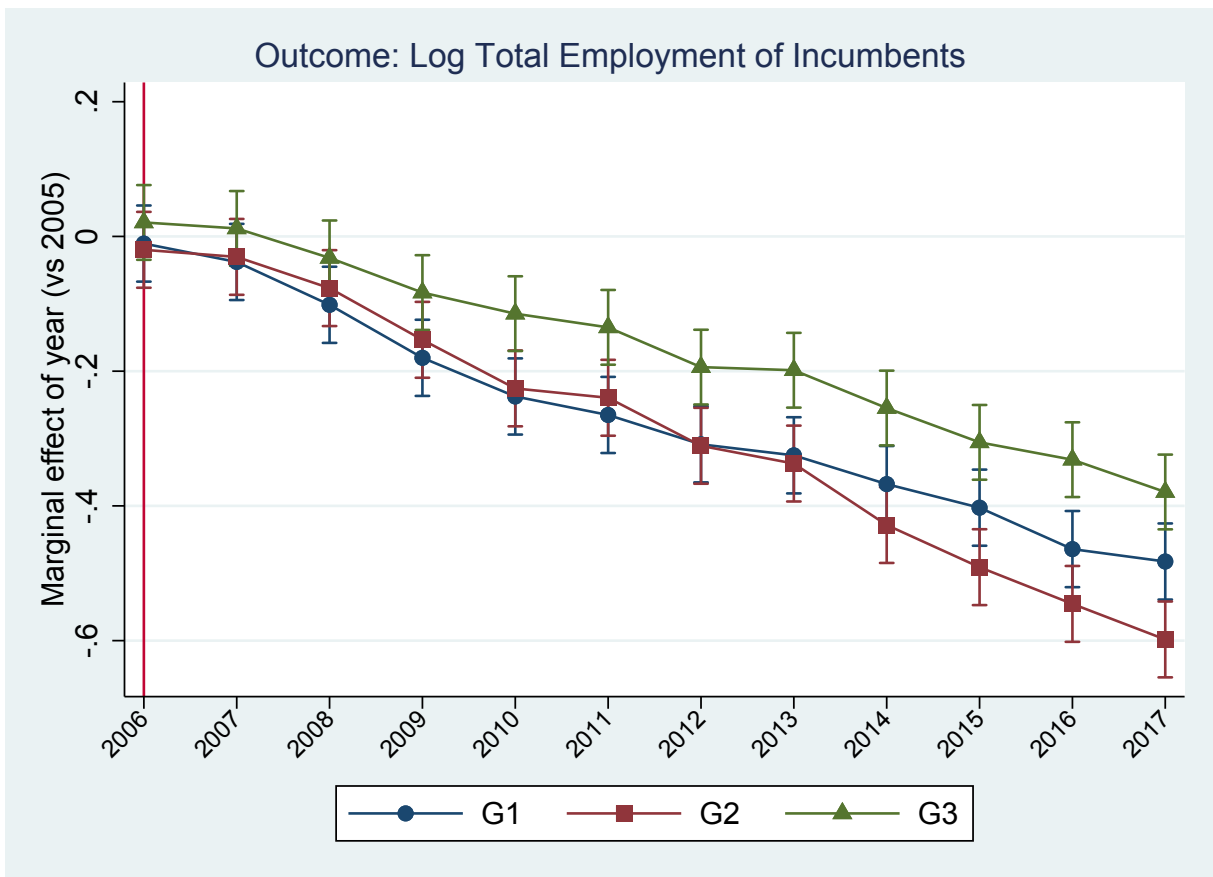**Total employment effects**

While the entrant estimates suggest a substantially positive result from the reform, the ambiguous results for incumbents leave the total effect inconclusive. However, one can also try to estimate the total employment effects directly using the (logarithm of the) total employment of an industry as the outcome variable. Doing so yields the estimates presented in table 9, which suggest an overwhelmingly positive overall employment effect that makes sense in terms of the pattern of effects increasing with the treatment intensity.

Inspecting figure 20 makes it clear, though, that the parallel trends assumption is very suspect for the direct estimation of total employment effects. While dispersion again increases post-reform, there is quite a clear gap opening up between G1 and the other groups already in the pre-reform period. Also, while G2 and G3 do overlap to a plausible extent with each other in the pre-period, even differences between them might be biased as their difference gets consistently smaller in the pre-period with G3 overtaking G2 in 2006 and never falling below it again. The story is the same regardless of whether one includes only limited companies or all legal forms in the outcome.

Thus, it seems that whether one tries to estimate the total employment effects of the 2006 MCR reform directly or indirectly, the results are quite unreliable. Parallel trends seems like a plausible assumption for the individual components of the contribution of entrants, as well as the direct estimation of the total contribution of entrants, but the results obtained for incumbents are likely to reflect some other forces than the reform. Meanwhile, total employment in G2 and G3 industries also appears to be on an upward trend already before the reform. Hence, the best approximation for the total employment effects of the reform may be just the total contribution of entrants. It is very well possible that the increased competition from the entrants is in the end insignificant for the incumbents, and that their performance is driven by some unrelated factors that also lead the total employment trends of the different industries to diverge already before the reform.

Table 9: Treatment effects on total employment

Log total employment	Regressor	Coefficient	Std. err.	95% Confidence interval	
				Lower bound	Higher bound
Limited only	Post*G2	0.112***	0.022	0.069	0.155
R^2=0.936 N=7 453	Post*G3	0.306***	0.022	0.263	0.348
All legal forms	Post*G2	0.107***	0.019	0.069	0.145
R^2=0.939 N=7 467	Post*G3	0.274***	0.019	0.236	0.312

p < 0.05: "*", <0.01: "**", <0.001: "***"

Figure 20

(a)

Outcome: Log Total Industry Employment (Only Limited Companies)

(b)

Outcome: Log Total Industry Employment (All Legal Forms)
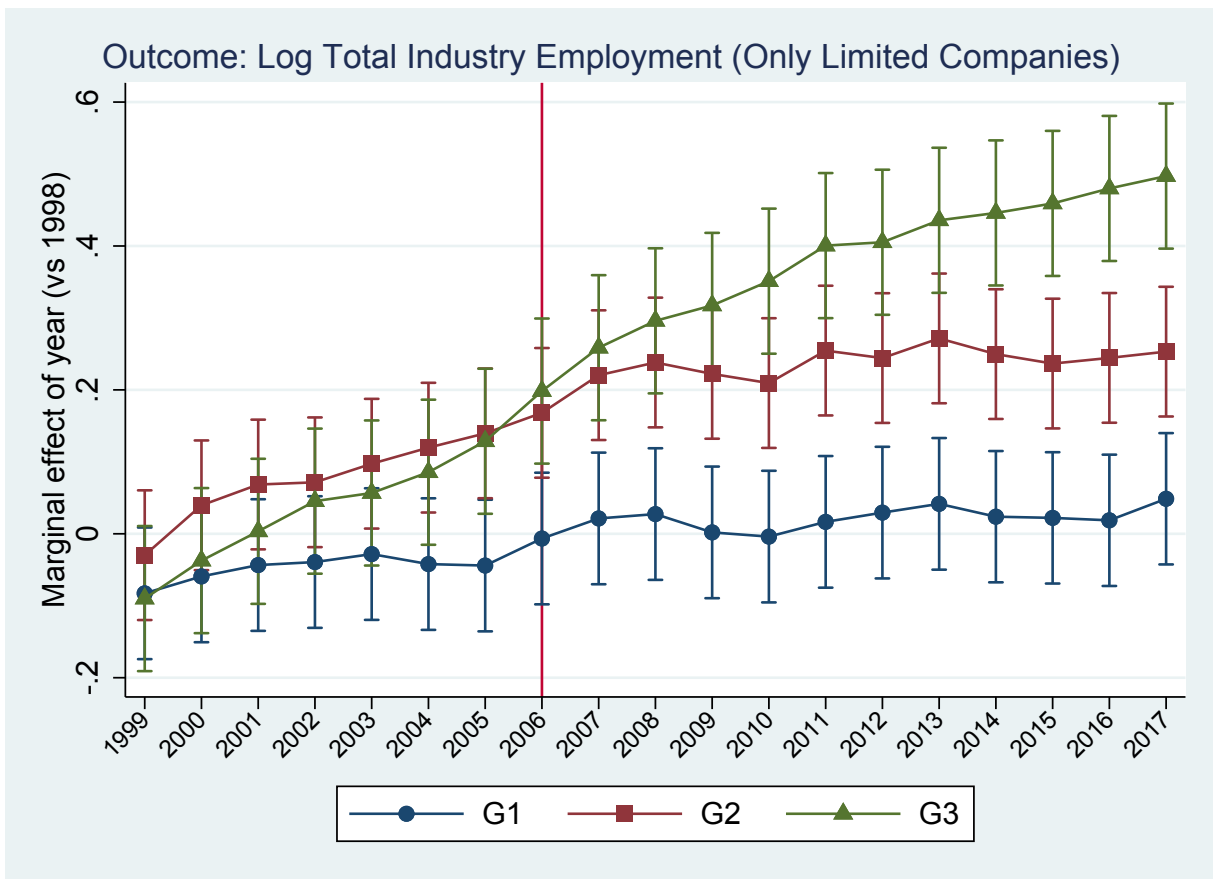
The 1997 reform

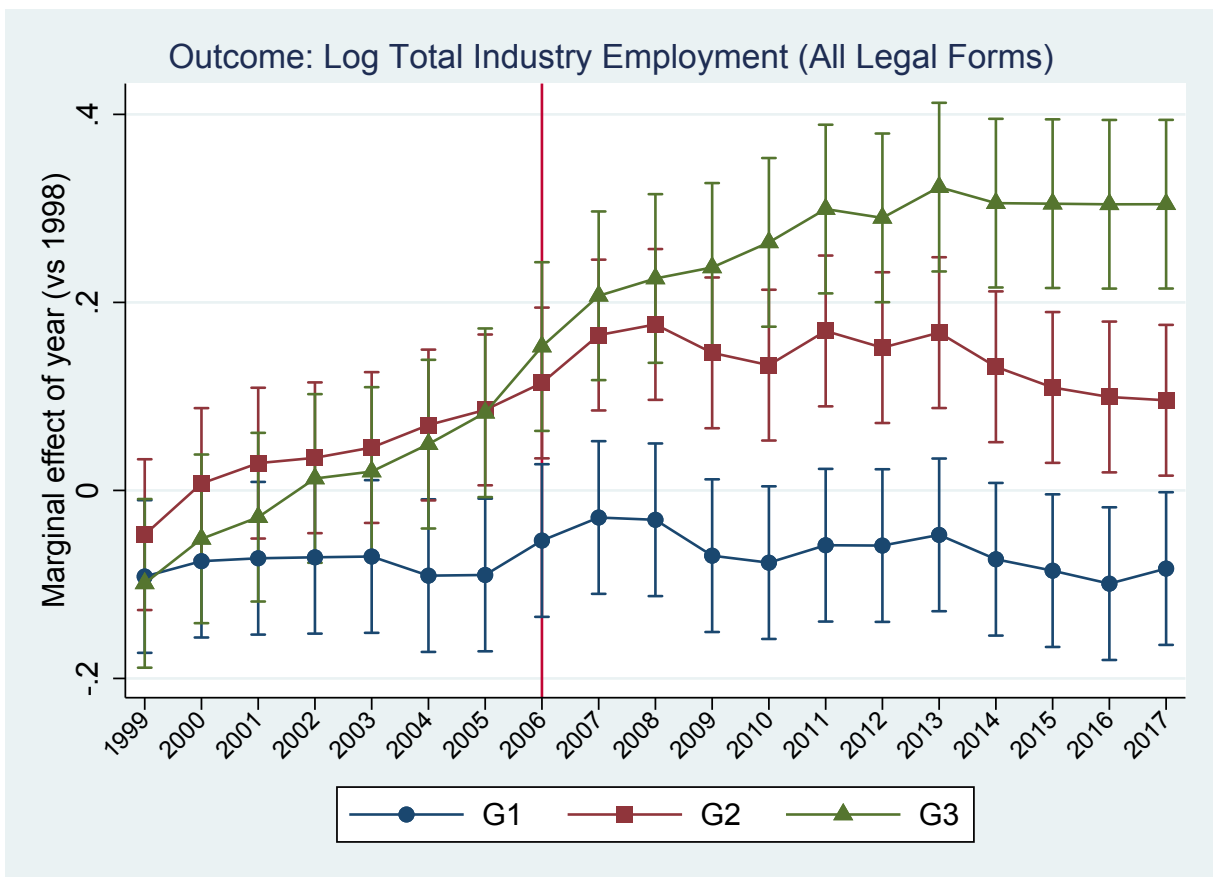If the 2006 lowering of the MCR was behind the estimated effects obtained in the previous section, one would expect to find qualitatively opposite results when estimating the effects of the 1997 raising of the same requirement. Inspecting table 10 reveals that this not quite the case. For the number of entrants with only limited companies included, it is encouraging that the estimated coefficients for both interactions are negative, but what is less encouraging is that the estimate for G3 is less negative than for G2, and neither is statistically different from zero at the 95% confidence level. When all legal forms are added to the picture, the coefficients turn positive, and quite significantly so for G3. As for the rest of the estimates, there are only two other coefficients that are statistically different from zero, and with this many outcomes it is entirely possible to obtain some "false positives"[25] even when dealing with setups closer to the ideal of randomized experiments. Hence the overall message of table 10 seems to be that the 1997 reform had no effect on the dynamics of firms.

Table 10: Estimates for the 1997 reform

Outcome	Included	N	R^2	Coefficient (Std. err.)	
				Post*G2	Post*G3
Log number of entrants	Limited only	1 917	0.833	-0.088 (0.047)	-0.010 (0.047)
	All legal forms	1 917	0.889	0.012 (0.044)	0.259*** (0.044)
Share of cohort alive at age 3	Limited only	1 917	0.480	0.000 (0.013)	0.005 (0.013)
	All legal forms	1 917	0.551	-0.011 (0.011)	-0.001 (0.011)
Log mean employment at age 3	Limited only	1 962	0.477	0.011 (0.081)	-0.053 (0.081)
	All legal forms	1 963	0.525	-0.033 (0.075)	-0.172* (0.075)
Log total cohort employment at age 3	Limited only	1 962	0.636	-0.112 (0.096)	-0.057 (0.096)
	All legal forms	1 963	0.679	-0.091 (0.086)	0.068 (0.086)
Log number of incumbents	All legal forms	1 405	0.971	-0.065* (0.028)	-0.022 (0.028)
Log mean employment of incumbents	All legal forms	1 405	0.924	-0.028 (0.040)	0.048 (0.040)
Log total employment of incumbents	All legal forms	1 405	0.936	-0.092 (0.048)	0.026 (0.048)
Log total employment	Limited only	1 917	0.902	-0.074 (0.042)	0.021 (0.042)
	All legal forms	1 919	0.878	-0.029 (0.046)	0.050 (0.046)

p $<$ 0.05: "*", $<$0.01: "**", $<$0.001: "***"

There are several reasons why the estimates for the 1997 reform might remain insignificant, while the estimates for the 2006 reform appear to show effects for some outcomes. Firstly, since the treatment intensity measure in this case is constructed in an indirect matter, it may include much more measurement error than the original intensity measure used in the case of the 2006 reform. This can lead to downward bias in the estimates: if a lot of industries are for instance erroneously placed into G3 when they would actually belong to G1 and vice versa, any potential effect will be diminished in the estimates as G1 reacts stronger and G3 weaker than they in reality should.

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[25]In the sense of not being statistical zeros, though both are actually negative in this case.

Another potential reason has to do with the inherent complex nature of markets, where causes condition other causes and the same cause in one circumstance has a different effect than in another. One such multi-causal mechanism that might be at play was already discussed in the context of figure 13a, where we observed that a lot of the dispersion in entry numbers after the 2006 reform happened only after 2008 and hence the financial crisis. The crisis seems to have hit G1 the hardest, but one can't conclusively say whether this was because the reform had made the other groups more resilient, or because of some unrelated factors (though the heterogeneous characteristics of the treatment intensity groups strongly suggests that the latter interpretation is likely). In the case of the 1997 reform, it's possible that, for instance, the other costs of setting up a business back then were so high that the increase of the MCR - though in the same ballpark nominally as the decrease in 2006 - was relatively not large enough to discourage people who already were committed to setting up businesses. This could be the case for example if we believe that the spread of information technology among households after the turn of millennia has made it less costly to set up a business. One could also speculate about a psychological explanation: an increase in cost may not defer potentially overconfident entrepreneurs as much as a decrease in cost encourages experimentation.

Finally, there are two possibilities that call into question the validity of the interpretations made here. For one, it might of course be that the 2006 estimates are entirely erroneous. As mentioned, if 2008 hit G1 the most because of some other factors than the reform, the estimates in table 4 may be highly inflated, even if a qualitatively an effect might exist regardless. The fact that G1 clearly has a higher share of manufacturing industries while G2 and G3 are more dominated by the service sector does make this very much a possibility. However, parallel trends for the number of entrants seem to hold quite well prior to the reform, suggesting that the groups react at least to smaller shocks similarly when the MCR is high. Furthermore, even if one ignores the post-crisis years or excludes manufacturing companies, some effect remains.

The other possibility is that the estimates reflect some other dimensions of the 2006 reform. Recall that the 1997 and 2006 MCR reforms were both parts of larger reforms of the law on limited companies. Of course, the treatment intensity measure used here is designed to specifically pick up differences in the importance of the MCR, but judging by the characteristics of the different groups constructed using it, it is not the only thing systematically different between the groups. Out of the characteristics that were considered, only the average size of entrants at age 3 and the average size of incumbents monotonically increase with the treatment intensity - G3 has the smallest firms and G1 the largest (though in the case of incumbents the difference between G2 and G3 is not large). However, since the 2006 reform was indeed statedly pro small business, it is also a very real possibility that some other dimension of the reform is responsible for at least part of the effects estimated in the previous section.

Inspecting the parallel trends figures (presented for the 1997 reform in appendix C) suggests that, at least for the number of entrants when only limited companies are included, measurement error does seem to play a role. Indeed, there is more dispersion between the groups before the reform than after it, but after 1997 the marginal effects are consistently lower for all groups, hinting that an effect may be present but the different treatment intensities are not sufficiently captured by the indirect measure. Similarly, survival appears to increase for all groups after the reform. Meanwhile, in contrast to the 2006 reform, the assumption seems to fail clearly for entrant growth and total contribution, with G3 experiencing consistently higher year-effects than the other groups. Finally, the incumbent outcomes exhibit similar problems as in the 2006, and the total employment outcome also fails the parallel trends "test", though the manner in which the latter fails is somewhat different than in the 2006 case: the dispersion between the groups stays quite constant for the entire period, except for G2 which drops suddenly two years after the reform. All in all, the figures reinforce the need to access the direct treatment intensity measure for the pre-1997 period to conclude how much of the apparent contradiction between the results for the 1997 and 2006 reforms can be explained by measurement error. The necessary data, while not currently readily available, probably does exist. This is hopefully something that future research can address.

5 Lessons Learned and Open Questions

As of July 1st, 2019, the minimum capital requirement is a thing of the past in Finland, at least for now. In light of the evidence presented in this thesis, one might expect to see a significant increase in limited incorporation following this latest reform, and indeed, according to YLE news, a thousand new limited companies - roughly the same amount that the main estimates here imply as the yearly increase following the 2006 reform - were founded in just 11 days following the removal (Muilu, 2019). This might be interpreted as evidence of a non-linear effect from removing the barrier entirely as opposed to just lowering it, though in nominal terms the drop from 2 500 euros to zero was actually smaller than the decrease from 8 000 to 2 5000 euros. However, one should keep in mind that the estimates in this thesis may not have reflected such an increase even if it happened in 2006 as well, for several reasons.

Firstly, the Statistics Finland data used imposes a relatively low, but still substantial, threshold for firms to be included in the panel, namely that they are employers or cross a minimum turnover limit of around 8 000 - 10 000 euros (the minimum is different every year). This means that very small companies - for instance companies set up for purposes of running a part-time business on the side of a main source of income - may not be counted. While firms of this size are too small to have much economic impact alone, it is possible that their mass is still significant enough to not be ignored. This is due to the highly skewed nature of the firm size distribution.

The concept of the "long tail" popularized by Anderson (2006) in the context of niche offerings provides some useful insights here. His argument in a nutshell is that in many markets there are a few hits - products or services that are widely known and have a big impact on their own - but that the wide variety of niches behind the hits with small but dedicated followings put together is worth at least as much (both economically and especially culturally speaking). The connection to micro business is obvious - consider for instance a neighborhood thrift store run as a hobby by a single person with low turnover and no intentions of growing. It's measurable economic impact alone might be practically non-existent, but the non-pecuniary benefits derived from it by the owner and even their customers may in fact be significant. Furthermore, it might be, as Axtell (2001) finds using data covering the entire set of tax-paying U.S. firms, that firm size follows a power law distribution, which would imply that the tail of the distribution censored by the thresholds in the data is very long indeed, and the combined contribution of these firms even in monetary terms becomes something worth considering. Without access to more comprehensive data, the impact of this tail remains a mystery.

Secondly, the industry size threshold adopted in order to avoid measurement error in treatment intensity means that neither the estimates or the illustrative calculations based on them are based on the entire set of industries. In the case of the latter, this leads mechanistically to lower numbers. Furthermore, the average pre-reform share of limited company entrants with minimum capital is substantially lower in the included industries than in the set of all firms in Finland, suggesting that a lot of affected firms do indeed go unnoticed due to excluding small industries. However, because of the measurement error stemming from industry smallness, it is hard to say which of the excluded industries are truly more affected and which are not.

Finally, as stressed throughout the previous section, no industries are probably entirely unaffected by the reform, implying almost certain downward bias in the estimates resulting from the effect on the "control group". What this means is that the proper interpretation of the estimates is relative to the least affected third of industries. Note that the illustrative calculations that yield the thousand firms a year -figure assume no effect on the control group, so they should be taken as rough approximations of the absolute effect only if one believes that the least affected industries are affected to a negligible extent.

Still, it might also be that the removal of the MCR does in reality have very different effects on some margins than the lowering. This ties into the fact that, while the purpose of this thesis has been to use the MCR as a case study of the relationship between entry barriers and firm dynamics, a full cost-benefit analysis of reforms in the regulation would have to take into account other potential consequences.

For instance, the tax administration has raised concerns that the removal of the MCR may make life easier for those operating on the illicit side of the economy, for example by making the setup of front companies easier (Muilu, 2019). Indeed, in the government proposal for the 2019 removal (Parliament Of Finland, 2018), it is mentioned that the out of all the stakeholders that were asked to comment on the proposal, only the black market investigation unit of the tax administration ("harmaan talouden selvitysyksikkö" in Finnish) explicitly objected it. Another unintended consequence of easing limited incorporation is that employees may find it more tempting to formally become "independent contractors" selling services to their employer in order to seek lower tax rates, and the employer may have an incentive to encourage this due to a lighter regulative burden in "sub-contracting" versus employing individuals. One might expect that, if these phenomena were a major problem, the estimates for the 2006 reform would reflect that as fronts for illicit operators and employees masquerading as contractors are likely to employ less people, and the former also probably survive for shorter periods of time. However, it might also be that even the modest MCR of 2 500 euros was sufficient to deter such behaviour to a large extent, and the removal will open the floodgates. In this light, the above-mentioned surge of a thousand new limited companies in the eleven days following the removal appears less encouraging.

Thus, it should be emphasized that one needs to be cautious in interpreting the evidence presented here as endorsement of the removal of barriers such as the MCR. What I do believe the evidence manages to establish are two things:

1. The 2006 lowering of the MCR did cause an increase in the number of limited company entrants.

2. Quantifying the increase and its broader impact is highly uncertain, perhaps inherently, but at least with the data and methodology used in this study.

Note that the first point is specific to the 2006 reform. Though the lack of important data for the 1997-period likely plays a role in the fact that no evidence is found of any effects for the 1997 reform, that fact still leaves a big question mark over the external validity of the results when it comes to implementing MCR reforms. Macroeconomic conditions may amplify or diminish the effects of such reforms in many ways.

For instance, the model of Clementi and Palazzo (2016) suggests that entry itself acts a feedback mechanism: negative shocks lead to less entry which makes the recovery from the shocks more sluggish. The authors argue that this is a major reason for the slow recovery from the 2008 crisis, and it can also help explain the large estimated effects of the 2006 reform even in absence of bias arising from the fact that the industries with the lowest treatment intensities are disproportionately in the manufacturing sector. To see why, note that the positive estimates do not necessarily imply an absolute increase in the number of entrants, simply that the number of entrants was higher than it would have been without the reform. Thus, if the reform did not happen, the 2008 crisis might have diminished entry numbers even more, especially in the industries where the MCR was a relatively more important obstacle, and those diminished entry numbers would have persisted longer due to them propagating the negative shock.

Practically speaking, it might be that the entry numbers in manufacturing industries suffered more as a result of the 2008 crisis because the lowering of the MCR did not effectively lower the entry costs of firms in those industries as they needed high initial investments in any case. By contrast, entry numbers in service industries suffered less because the firms in those industries had less entry costs, but they would also have suffered more if the high MCR was still in place as then the firms would have needed to find the funding for that initial capital.

Another mechanism that might explain the lack of effect in 1997 is that the other costs of setting up a business back then might have been so high that the higher MCR carried less weight. It's easy to imagine, for example, that the spread of the personal computer had made running a small business significantly more cost effective by 2006 compared to 1997, so that the 8 000 euro investment had become unnecessary for many businesses.

In addition to being 2006-specific, the first point also explicitly only mentions the MCR. This simply means that extrapolating the results to other kinds of entry barriers is questionable. Recall for instance that van Stel et al. (2007) find that the MCR is the only entry barrier they study that has an effect on entrepreneurship rates, suggesting at least that different entry barriers can't be straightforwardly equated. Finally, it is specific to limited companies, reflecting the fact that the estimates when all legal forms are included appear to be less reliable. What this means is that one can't conclude whether the new limited companies are adding to economic activity in the sense that no businesses would have been set up in absence of the reform, or if the businesses would have been set up using some other legal form. Even if the latter is the case, though, one might interpret the increase in limited incorporation as a success, as limited companies seem to perform better on average.

The second point is the result of many factors. Starting already with the measurement of firms dynamics, one has to necessarily make arbitrary decisions to present information in a quantified form. Next, the universal nature of the MCR reforms makes it difficult to control for the other factors that might be influencing firm dynamics at any time. The heterogeneous differences-in-differences strategy adopted here provides in my view a relatively convincing approach to test the predictions arising from the theoretical discussion, but has some drawbacks when it comes to estimating the actual magnitude of the effects. Firstly, the effect on the control group remains unknown, and the rest of the effects are downward biased if it is not zero. Secondly, the measure of treatment intensity is rarely likely to be random, implying that there is a reason why one unit of observation has a lower measure of intensity than another, and that reason can be potentially confounded with other factors that might affect the outcome of interest. In this specific case the treatment intensity measure is correlated with firm smallness and the probability of an industry being in the manufacturing section, both of which may directly reflect in the outcomes - most notably the 2006 reform might have had other dimension that helped small business entry, and the 2008 crisis might have hit entry numbers in manufacturing worse than other industries. Finally, in this case at least, as the treatment intensity measure is essentially a sample moment, it suffers definitionally from decreasing accuracy as the size of the industry for which it is calculated decreases.

Even if one were to ignore the aforementioned uncertainty and take the large main estimates of this thesis at face value, the lowering of entry barriers, at least the MCR, seems insufficient to deal with the observed falling entry rates and economic contribution of young firms that has served as motivation for this and many other a study. This point is illustrated concisely by figure 4, where one observes that despite the apparent effects on the relative popularity of different legal forms near the 1997 and 2006 reforms, the overall trend of entry rates is falling for all firms, reflecting the trends observed in many other countries as well (Decker et. al. 2014; 2016, Calvino et al., 2015). These trends likely reflect other systematic trends, and require more systematic solutions. Of course, it might also be that these trends can not or do not need to be overturned - further research is needed, and indeed currently actively pursued, on their sources and implications.

6   Conclusion

This thesis has aimed to evaluate the effect of the minimum capital requirement (MCR for short) on Finnish firm dynamics, making use of two reforms that altered its level: an increase in 1997 and a decrease in 2006. Taking into consideration the descriptive evidence and quantitative estimates as well as the plausibility of the assumptions the latter builds on, I am confident in concluding that the number of limited companies founded per year has been significantly affected by the level of the MCR, though considerable uncertainty remains over the exact magnitude of the effect.

The estimates presented here, if interpreted as absolute effects, imply that roughly a 1 000 limited companies would have been founded per year between 2007 and 2017 because of the reform, with the average survival of entrants dropping a few percentage points and the average employment growth remaining the same. However, the absolute interpretation relies on the unrealistic assumption that the third of industries with the lowest pre-reform share of entrants with minimum capital would be entirely unaffected by the reform. It is far more likely that even these industries, which serve as the control group in the identifying strategy utilized here, are also affected to some extent, implying that the estimates for the more affected industries are biased towards zero and the true effect is even larger. On the other hand, this is only true if the identifying assumption of parallel trends between the treatment intensity groups holds, which is plausible to some extent but far from certain. In particular, the group with the highest treatment intensities may have a slightly faster growing trend than the others. Furthermore, the size of the differences between the groups in the post-reform period grows increasingly wide after the 2008 crisis, raising the question of whether this increase in differences is due to the reform or some other systematic heterogeneity between the industries comprising the treatment intensity groups. Finally, any effects are only detectable for the 2006 reform, which is suspicious considering the size of the effect, though data limitations do make the 1997 estimates less reliable if the identifying assumption holds. Despite this, however, descriptive evidence, especially on the entry rates of different legal forms, presents patterns that are unlikely to be coincidences with no effects behind them.

Though the evidence in sum suggests that lowering entry barriers, at least in the form of the MCR, has a net positive effect on the total employment contribution of entrants, the overall effect remains a question. While I fail to find evidence that incumbent firms in the more affected industries would be significantly negatively affected, and do in fact estimate significant positive effects directly on the total employment of more affected industries, it is evident that the identifying assumption fails in both of these cases. Hence it appears that a different strategy is required to conclusively determine whether the net employment impact of the 2006 reform was positive. With that said, if one takes the estimated effects for entrants at face value, it seems unlikely that the effects on incumbents could be negative enough to diminish the net employment effects to zero or below.

It should be noted, though, that, while beyond the scope of this paper, the lowering of entry barriers may have other potential negative effects, such as decreases in consumer and debtor protections. This is especially relevant in the context of the recent 2019 removal of the MCR in Finland. According to YLE news, a thousand new limited companies were founded in just 11 days following the removal, and the tax administration has raised concerns over the potential of easing the founding of cover firms for illicit operations (Muilu, 2019). Also, while the results in this thesis can provide some idea of the employment effects the removal might have in the long run, it should be noted that their external validity depends on a lot of factors, not the least of which in this case is that, the removal of a barrier might have very different implications all things considered than simply lowering one.

Finally, a deeper question is the effectiveness of lowering entry barriers in fighting the perceived crisis of falling entry rates and contribution of young firms. As pointed out, this trend is shared by a lot of developed nations, and might hence have systematic causes beyond entry barriers. This possibility in the Finnish case is supported for instance by the fact that the overall trend in the entry rate is decreasing for all legal forms, even if the reforms in the MCR may have caused changes around the trend.

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A Robustness Checks

A.1 Treatment intensity groupings

Outcome	Regressor	Only limited companies			All legal forms		
		Three groups	Four groups	Five groups	Three groups	Four groups	Five groups
	Post*G2	0.152***	-0.028	-0.189***	0.082**	-0.064*	-0.166***
Log number		(0.031)	(0.036)	(0.041)	(0.028)	(0.032)	(0.036)
of entrants	Post*G3	0.317***	0.162***	0.114**	0.242***	0.099**	0.050
		(0.031)	(0.036)	(0.040)	(0.028)	(0.032)	(0.036)
	Post*G4		0.324***	0.091*		0.245***	0.031
			(0.036)	(0.040)		(0.032)	(0.036)
	Post*G5			0.283***			0.233***
				(0.041)			(0.036)
	Post*G2	-0.008	-0.007	-0.017	-0.003	-0.003	-0.008
Share of cohort		(0.013)	(0.015)	(0.017)	(0.010)	(0.011)	(0.013)
alive at age 3	Post*G3	-0.029*	-0.012	-0.020	-0.031**	-0.011	-0.008
		(0.013)	(0.015)	(0.017)	(0.010)	(0.011)	(0.013)
	Post*G4		-0.034*	-0.021		-0.032**	-0.026*
			(0.15)	(0.017)		(0.011)	(0.013)
	Post*G5			-0.045**			-0.036**
				(0.017)			(0.013)
Log mean	Post*G2	0.048	0.081	-0.040	0.030	0.018	-0.091
employment		(0.061)	(0.071)	(0.080)	(0.056)	(0.065)	(0.073)
at age 3	Post*G3	0.029	0.082	0.035	-0.022	0.026	-0.017
		(0.061)	(0.070)	(0.080)	(0.956)	(0.065)	(0.072)
	Post*G4		0.043	0.020		-0.029	-0.041
			(0.070)	(0.079)		(0.065)	(0.072)
	Post*G5			0.048			-0.048
				(0.079)			(0.072)
Log total cohort	Post*G2	0.159*	0.035	-0.196*	0.089	-0.064	-0.261**
employment		(0.073)	(0.085)	(0.096)	(0.067)	(0.077)	(0.086)
at age 3	Post*G3	0.290***	0.220**	0.138	0.168*	0.115	0.034
		(0.073)	(0.084)	(0.095)	(0.066)	(0.076)	(0.085)
	Post*G4		0.319***	0.098		0.173*	-0.027
			(0.085)	(0.095)		(0.076)	(0.086)
	Post*G5			0.296**			0.142
				(0.095)			(0.085)

Outcome	Regressor	Coefficient (Std. err.)					
		Only limited companies			All legal forms		
		Three groups	Four groups	Five groups	Three groups	Four groups	Five groups
Log number of incumbents	Post*G2				-0.063***	-0.074***	-0.105***
					(0.011)	(0.013)	(0.015)
	Post*G3				-0.019	-0.051***	-0.074***
					(0.011)	(0.013)	(0.015)
	Post*G4					-0.036**	-0.038**
						(0.013)	(0.015)
	Post*G5						-0.060***
							(0.015)
Log mean employment of incumbents	Post*G2				0.044*	0.038	0.025
					(0.020)	(0.023)	(0.026)
	Post*G3				0.108***	0.066**	0.035
					(0.020)	(0.023)	(0.026)
	Post*G4					0.101***	0.120***
						(0.023)	(0.026)
	Post*G5						0.101***
							(0.026)
Log total employment of incumbents	Post*G2				-0.019	-0.036	-0.080**
					(0.022)	(0.026)	(0.029)
	Post*G3				0.089***	0.015	-0.039
					(0.022)	(0.026)	(0.029)
	Post*G4					0.063*	0.082**
						(0.026)	(0.029)
	Post*G5						0.041
							(0.028)
Log total employment	Post*G2	0.112***	-0.004	-0.106***	0.107***	0.013	-0.072**
		(0.022)	(0.025)	(0.028)	(0.019)	(0.022)	(0.025)
	Post*G3	0.306***	0.177***	0.090**	0.274***	0.153***	0.099***
		(0.022)	(0.025)	(0.028)	(0.019)	(0.022)	(0.025)
	Post*G4		0.283***	0.169***		0.267***	0.145***
			(0.025)	(0.028)		(0.023)	(0.025)
	Post*G5			0.293***			0.285***
				(0.028)			(0.025)

$p < 0.05$: "*", <0.01: "**", <0.001: "***"

A.2 The 2008 crisis

Outcome	Regressor	Coefficient (Std. err.) from specification number			
	(Included)	0	1	2	3
	Post*G2	0.152***	0.141*	0.071	-0.069
Log number	(Lim.)	(0.031)	(0.061)	(0.036)	(0.056)
of entrants	Post*G3	0.317***	0.176**	0.237***	0.039
	(Lim.)	(0.031)	(0.061)	(0.036)	(0.057)
	Post*G2	0.082**	0.082	-0.024	-0.074
	(All)	(0.028)	(0.052)	(0.032)	(0.049)
	Post*G3	0.242***	0.135**	0.115***	0.060
	(All)	(0.028)	(0.052)	(0.032)	(0.047)
	Post*G2	-0.008	-0.058*	-0.006	-0.014
Share of cohort	(Lim.)	(0.013)	(0.027)	(0.014)	(0.023)
alive at age 3	Post*G3	-0.029*	-0.038	-0.025	-0.044
	(Lim.)	(0.013)	(0.027)	(0.014)	(0.024)
	Post*G2	-0.003	-0.027	-0.008	-0.007
	(All)	(0.010)	(0.020)	(0.011)	(0.018)
	Post*G3	-0.031**	-0.020	-0.031**	-0.028
	(All)	(0.010)	(0.020)	(0.011)	(0.018)
	Post*G2	0.048	0.026	-0.091	-0.137
Log mean	(Lim.)	(0.061)	(0.130)	(0.066)	(0.102)
employment	Post*G3	0.029	-0.128	-0.087	-0.178
at age 3	(Lim.)	(0.061)	(0.129)	(0.066)	(0.103)
	Post*G2	0.030	-0.034	-0.054	-0.135
	(All)	(0.056)	(0.119)	(0.060)	(0.092)
	Post*G3	-0.022	-0.210	-0.099	-0.225*
	(All)	(0.956)	(0.118)	(0.060)	(0.092)
	Post*G2	0.159*	0.070	-0.099	-0.304*
Log total cohort	(Lim.)	(0.073)	(0.153)	(0.080)	(0.123)
employment	Post*G3	0.290***	-0.034	0.081	-0.164
at age 3	(Lim.)	(0.073)	(0.152)	(0.080)	(0.124)
	Post*G2	0.089	0.001	-0.119	-0.265*
	(All)	(0.067)	(0.138)	(0.072)	(0.110)
	Post*G3	0.168*	-0.112	-0.033	-0.203
	(All)	(0.066)	(0.137)	(0.071)	(0.110)
Log number	Post*G2	-0.063***	-0.014	-0.075***	-0.062***
of incumbents	(All)	(0.011)	(0.014)	(0.014)	(0.014)
	Post*G3	-0.019	-0.025	-0.027	-0.031*
	(All)	(0.011)	(0.014)	(0.014)	(0.014)
Log mean	Post*G2	0.044*	0.026	0.018	0.084**
employment	(All)	(0.020)	(0.020)	(0.023)	(0.033)
of incumbents	Post*G3	0.108***	0.059**	0.075**	0.136***
	(All)	(0.020)	(0.020)	(0.023)	(0.033)
Log total	Post*G2	-0.019	0.012	-0.057*	0.023
employment	(All)	(0.022)	(0.019)	(0.023)	(0.036)
of incumbents	Post*G3	0.089***	0.034	0.048	0.105**
	(All)	(0.022)	(0.019)	(0.026)	(0.037)

Outcome	Regressor	Coefficient (Std. err.) from specification number			
	(Included)	0	1	2	3
	Post*G2	0.112***	0.092*	-0.029	-0.027
Log total	(Lim.)	(0.022)	(0.039)	(0.025)	(0.031)
employment	Post*G3	0.306***	0.173***	0.162***	0.060
	(Lim.)	(0.022)	(0.038)	(0.025)	(0.032)
	Post*G2	0.107***	0.094**	-0.009	0.001
	(All)	(0.019)	(0.035)	(0.022)	(0.026)
	Post*G3	0.274***	0.168***	0.155***	0.093***
	(All)	(0.019)	(0.034)	(0.022)	(0.027)

$p < 0.05$: "*", <0.01: "**", <0.001: "***"

Descriptions of the specifications by number (0 is the original estimation):

1. The post-crisis years are excluded (the post-reform period consists only of the year 2007)

2. Manufacturing is excluded

3. Only retail and wholesale are included

A.3 Different study periods

Outcome	Regressor (Included)	Coefficient (standard error) from specification number						
		0	1	2 a	2 b	2 c	3	4
Log number of entrants	Post*G2 (Lim.)	0.152*** (0.031)	0.133*** (0.038)	0.093* (0.038)	0.233*** (0.044)	0.135** (0.044)	0.142*** (0.034)	0.168*** (0.034)
	Post*G3 (Lim.)	0.317*** (0.031)	0.285*** (0.038)	0.213*** (0.038)	0.438*** (0.044)	0.323*** (0.044)	0.284*** (0.034)	0.346*** (0.035)
	Post*G2 (All)	0.082** (0.028)	0.042 (0.033)	0.077* (0.033)	0.161*** (0.040)	0.023 (0.039)	0.099** (0.030)	0.114*** (0.031)
	Post*G3 (All)	0.242*** (0.028)	0.154*** (0.033)	0.188*** (0.033)	0.367*** (0.039)	0.201*** (0.039)	0.246*** (0.030)	0.300*** (0.031)
Share of cohort alive at age 3	Post*G2 (Lim.)	-0.008 (0.013)	-0.021 (0.016)	-0.002 (0.015)	-0.023 (0.012)	0.012 (0.023)	-0.013 (0.014)	-0.007 (0.014)
	Post*G3 (Lim.)	-0.029* (0.013)	-0.041** (0.016)	-0.017 (0.015)	-0.050** (0.018)	-0.011 (0.027)	-0.031* (0.014)	-0.027 (0.014)
	Post*G2 (All)	-0.003 (0.010)	-0.005 (0.012)	0.004 (0.012)	-0.007 (0.013)	-0.021 (0.020)	-0.002 (0.010)	-0.006 (0.011)
	Post*G3 (All)	-0.031** (0.010)	-0.041*** (0.012)	-0.020 (0.012)	-0.042** (0.013)	-0.038 (0.020)	0.032** (0.010)	-0.006 (0.011)
Log mean employment at age 3	Post*G2 (Lim.)	0.048 (0.061)	-0.006 (0.075)	0.049 (0.075)	0.118 (0.083)	-0.117 (0.137)	0.042 (0.066)	0.031 (0.066)
	Post*G3 (Lim.)	0.029 (0.061)	0.032 (0.074)	0.007 (0.075)	0.056 (0.833)	0.047 (0.137)	0.001 (0.065)	0.009 (0.066)
	Post*G2 (All)	0.030 (0.056)	-0.030 (0.066)	0.024 (0.068)	0.090 (0.077)	-0.096 (0.125)	0.028 (0.060)	0.014 (0.061)
	Post*G3 (All)	-0.022 (0.956)	-0.023 (0.068)	-0.035 (0.067)	0.008 (0.076)	-0.056 (0.125)	-0.035 (0.059)	-0.044 (0.060)
Log total cohort employment at age 3	Post*G2 (Lim.)	0.159* (0.073)	0.069 (0.089)	0.109 (0.089)	0.327** (0.100)	-0.117 (0.162)	0.153 (0.078)	0.157* (0.079)
	Post*G3 (Lim.)	0.290*** (0.073)	0.234** (0.089)	0.173 (0.089)	0.427*** (0.100)	0.388* (0.163)	0.233** (0.078)	0.312*** (0.079)
	Post*G2 (All)	0.089 (0.067)	0.008 (0.080)	0.060 (0.080)	0.221* (0.091)	-0.164 (0.145)	0.096 (0.070)	0.089 (0.072)
	Post*G3 (All)	0.168* (0.066)	0.074 (0.079)	0.077 (0.079)	0.311*** (0.090)	0.113 (0.145)	0.143* (0.070)	0.197** (0.071)

Outcome	Regressor (Included)	Coefficient (standard error) from specification number						
		0	1	2			3	4
				a	b	c		
Log number of incumbents	Post*G2 (All)	-0.063*** (0.011)		-0.034** (0.013)	-0.064*** (0.013)	-0.091*** (0.014)	-0.044*** (0.012)	
	Post*G3 (All)	-0.019 (0.011)		-0.021 (0.013)	-0.016 (0.013)	-0.019 (0.014)	-0.019 (0.011)	
Log mean employment of incumbents	Post*G2 (All)	0.044* (0.020)		0.057** (0.018)	0.073** (0.023)	0.009 (0.026)	0.064*** (0.018)	
	Post*G3 (All)	0.108*** (0.020)		0.090*** (0.018)	0.124*** (0.023)	0.115*** (0.026)	0.101*** (0.018)	
Log total employment of incumbents	Post*G2 (All)	-0.019 (0.022)		0.022 (0.020)	0.008 (0.026)	-0.082** (0.029)	0.020 (0.020)	
	Post*G3 (All)	0.089*** (0.022)		0.069*** (0.020)	0.108*** (0.025)	0.096*** (0.028)	0.082*** (0.020)	
Log total employment	Post*G2 (Lim.)	0.112*** (0.022)	0.073** (0.022)	0.104*** (0.025)	0.121*** (0.031)	0.111*** (0.032)	0.110*** (0.023)	0.135*** (0.024)
	Post*G3 (Lim.)	0.306*** (0.022)	0.256*** (0.022)	0.229*** (0.025)	0.319*** (0.031)	0.375*** (0.032)	0.258*** (0.023)	0.334*** (0.025)
	Post*G2 (All)	0.107*** (0.019)	0.067*** (0.018)	0.107*** (0.023)	0.118*** (0.027)	0.095*** (0.028)	0.112*** (0.021)	0.130*** (0.022)
	Post*G3 (All)	0.274*** (0.019)	0.222*** (0.017)	0.217*** (0.023)	0.291*** (0.027)	0.320*** (0.028)	0.240*** (0.021)	0.304*** (0.022)

$p < 0.05$: "*", <0.01: "**", <0.001: "***"

Descriptions of the specifications by number (0 is the original estimation):

1. The years 1998-2001, for which share capital data isn't directly available, are dropped (note that this doesn't affect the incumbent outcomes)

2. The post-period is split to three to see if dynamic feedback loops such as competition diminish or amplify the estimated effects over time

   (a) The post-period is 2007-2010

   (b) The post-period is 2011-2013

   (c) The post-period is 2014-2017 (note that for entrant survival, growth and total contribution numbers this only covers the 2014 entrants)

3. The years 2013-2017, which are warned to not be comparable with the previous years by Statistics Finland, are dropped

4. The years 2004-2005 are dropped. The cohorts that enter in those years are three years old only after the reform. Hence their performance measured at that point might be affected by the competition coming from the first cohorts of post-reform entrants, unlike other pre-reform entrants (2003 entrants are at age three in 2006, so there may already be some post-reform entrants present when their performance is measured. However, the reform goes into effect at then end of the year, and

it probably takes the first entrants a while to get their business off the ground, so the increase in competition should not be too significant). Note that this mechanism does not concern incumbents. It should also not affect the number of entrants or total employment, as they are measured every year, but the estimates are included for comparison.

A.4 Other

| Outcome | Regressor (Included) | Coefficient (standard error) from specification number | | | | |
		0	1	2	3 a	b
Log number of entrants	Post*G2 (Lim.)	0.152*** (0.031)	0.237*** (0.037)	-0.086** (0.026)		
	Post*G3 (Lim.)	0.317*** (0.031)	0.328*** (0.038)	0.106*** (0.030)		
	Post*G2 (All)	0.082** (0.028)	0.138*** (0.033)	-0.179*** (0.027)		
	Post*G3 (All)	0.242*** (0.028)	0.252*** (0.034)	-0.002 (0.027)		
Share of cohort alive at age 3	Post*G2 (Lim.)	-0.008 (0.013)	-0.014 (0.015)	-0.002 (0.014)	-0.000 (0.009)	-0.006 (0.015)
	Post*G3 (Lim.)	-0.029* (0.013)	-0.011 (0.015)	-0.017 (0.015)	-0.008 (0.009)	-0.032* (0.015)
	Post*G2 (All)	-0.003 (0.010)	0.012 (0.012)	0.014 (0.013)	0.003 (0.007)	-0.006 (0.011)
	Post*G3 (All)	-0.031** (0.010)	-0.006 (0.012)	0.004 (0.013)	0.000 (0.007)	-0.036** (0.011)
Log mean employment at age 3	Post*G2 (Lim.)	0.048 (0.061)	0.036 (0.057)	0.156* (0.071)	0.084 (0.057)	0.105 (0.069)
	Post*G3 (Lim.)	0.029 (0.061)	0.034 (0.057)	0.186** (0.072)	0.086 (0.056)	0.076 (0.068)
	Post*G2 (All)	0.030 (0.056)	-0.042 (0.052)	0.085 (0.066)	0.068 (0.052)	0.089 (0.062)
	Post*G3 (All)	-0.022 (0.956)	-0.048 (0.052)	0.065 (0.066)	0.034 (0.051)	0.022 (0.062)

Outcome	Regressor (Included)	Coefficient (standard error) from specification number				
		0	1	2	3 a	3 b
Log total cohort employment at age 3	Post*G2 (Lim.)	0.159* (0.073)	0.223** (0.071)	0.138 (0.082)	0.239*** (0.069)	0.204* (0.082)
	Post*G3 (Lim.)	0.290*** (0.073)	0.298*** (0.071)	0.321*** (0.083)	0.392*** (0.068)	0.300*** (0.082)
	Post*G2 (All)	0.089 (0.067)	0.069 (0.064)	-0.039 (0.075)	0.144* (0.061)	0.141 (0.073)
	Post*G3 (All)	0.168* (0.066)	0.163* (0.064)	0.090 (0.076)	0.261*** (0.061)	0.172* (0.073)
Log number of incumbents	Post*G2 (All)	-0.063*** (0.011)	0.017 (0.018)	-0.104*** (0.013)		
	Post*G3 (All)	-0.019 (0.011)	0.026 (0.018)	-0.056*** (0.013)		
Log mean employment of incumbents	Post*G2 (All)	0.044* (0.020)	-0.004 (0.020)	0.085** (0.031)		
	Post*G3 (All)	0.108*** (0.020)	0.011 (0.020)	0.150*** (0.030)		
Log total employment of incumbents	Post*G2 (All)	-0.019 (0.022)		-0.021 (0.034)		
	Post*G3 (All)	0.089*** (0.022)		0.093** (0.034)		
Log total employment	Post*G2 (Lim.)	0.112*** (0.022)		0.080** (0.026)		
	Post*G3 (Lim.)	0.306*** (0.022)		0.304*** (0.025)		
	Post*G2 (All)	0.107*** (0.019)		0.049 (0.025)		
	Post*G3 (All)	0.274*** (0.019)		0.279*** (0.025)		

p < 0.05: "*", <0.01: "**", <0.001: "***"

Descriptions of the specifications by number (0 is the original estimation):

1. Stricter definition of entry: a firm is only considered an entrant if the establishment-level definition agrees and the disagreement with the legal entry date is zero or one. Note that this does not affect the total employment effects. As for incumbents, all firms that appear in the data after the reform but are not considered to be entrants by the stricter definition are counted into incumbents. However, there are no observations for the employment of these firms before they appear. This is not an issue for estimating the number (as one can simply add the late-appearing incumbents in all previous years' totals) or average size of incumbents (disregarding worries of biasdness), but

renders the direct estimation of incumbent contribution pointless, as the total industry employments attributable to incumbents are inflated by these late-appearing incumbents.

2. There is no size threshold for an industry to be included in the estimation (the original estimation required the included industries to have at least an average of 50 firms in the 2002-2005 and 2007-2017 periods).

3. Survival, growth and total contribution of entrants are evaluated at different ages

   (a) At age 1
   (b) At age 5

B Sectors in Terms of the 2008 Standard Industrial Classification

In figures 11 and 12, some of the industry categories (at the broadest level, denoted as letters A-U) from the standard industrial classification of 2008 used by Statistics Finland (Statistics Finland, 2008) are grouped together under a sector title to make the figures less cluttered. The below table lists all of the standard industrial classification one-letter categories that are included under each title. Primary production (A and B) and real estate (L) industries are excluded for the reasons mentioned in the data description in section 2.1. In addition, public administration, defense and compulsory social security (O), activities of households as employers (T) and activities of extraterritorial organizations and bodies (U) are excluded due to lack of observations. Firms with unknown industries (X) are also dropped out of consideration.

| Sector title used in the text	One-letter industries included (Letter)
Manufacturing	Manufacturing (C)
Wholesale & Retail	Wholesale and retail trade; repair of motor vehicles and motorcycles (G)
Construction	Construction (F)
Utilities & Logistics	Electricity, gas, steam and air conditioning supply (D)
	Water supply, sewerage, waste management and remediation activities (E)
	Transportation and storage (H)
Services	Human health and social work activities (Q)
	Accommodation and food services (I)
	Arts, entertainment and recreation (R)
	Other service activities (S)
Information & Finance	Information and communication (J)
	Professional, scientific and technical activites (M)
	Financial and insurance activites (K)
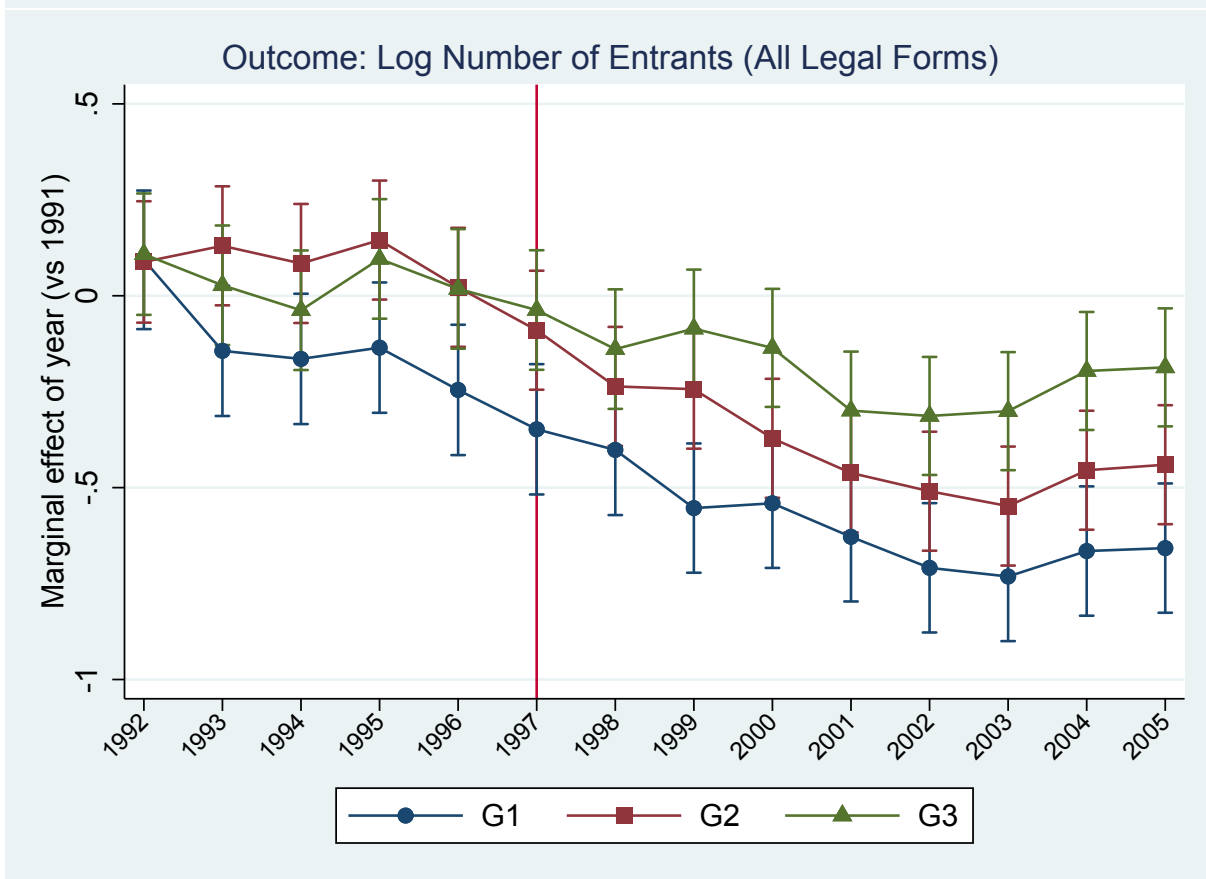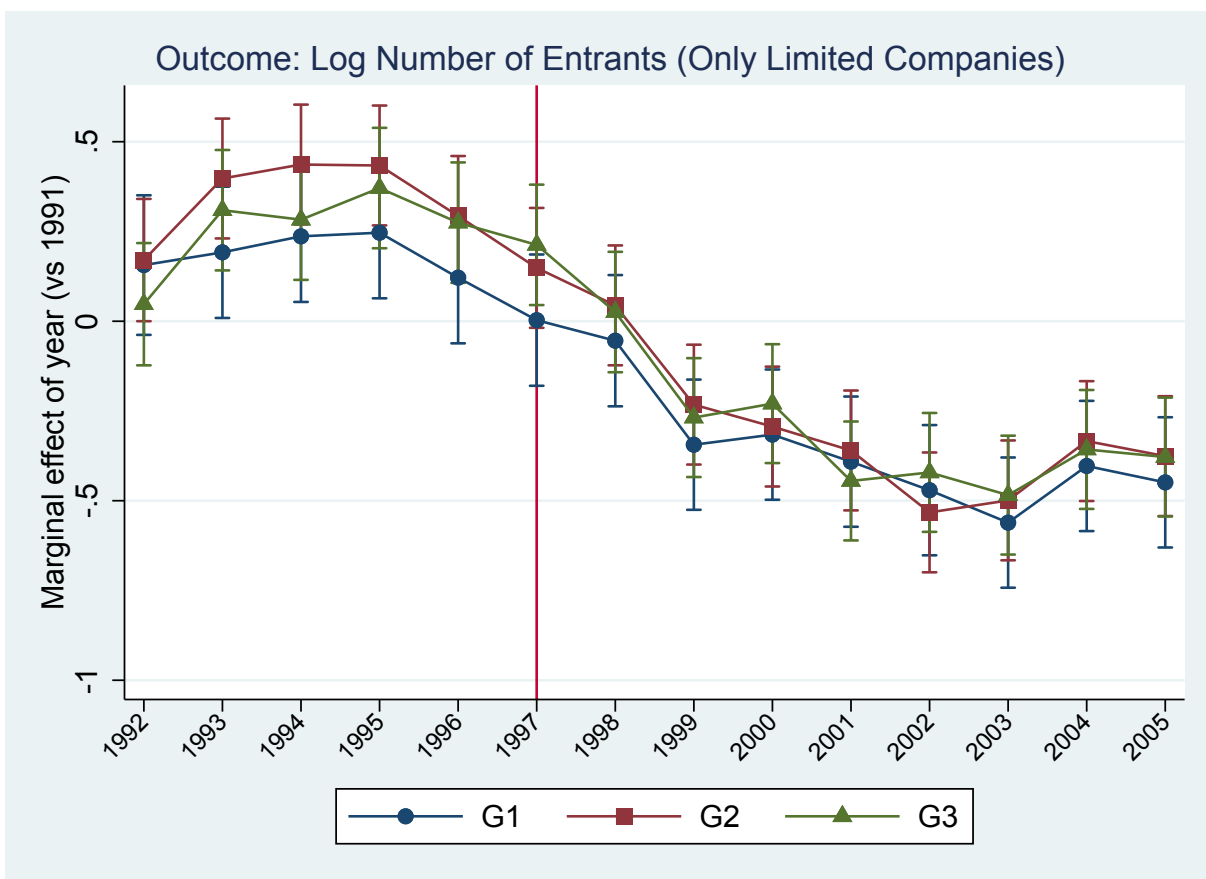
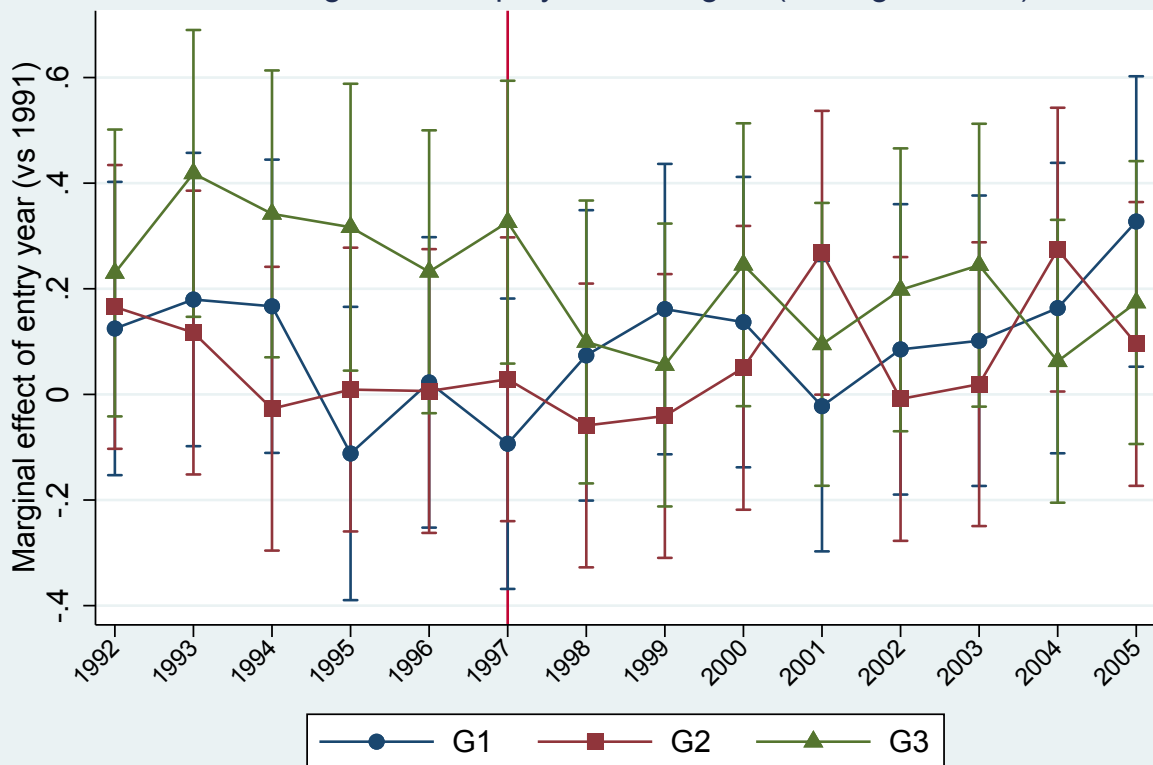Outcome: Share of Cohort Alive at Age 3 (Only Limited Companies)
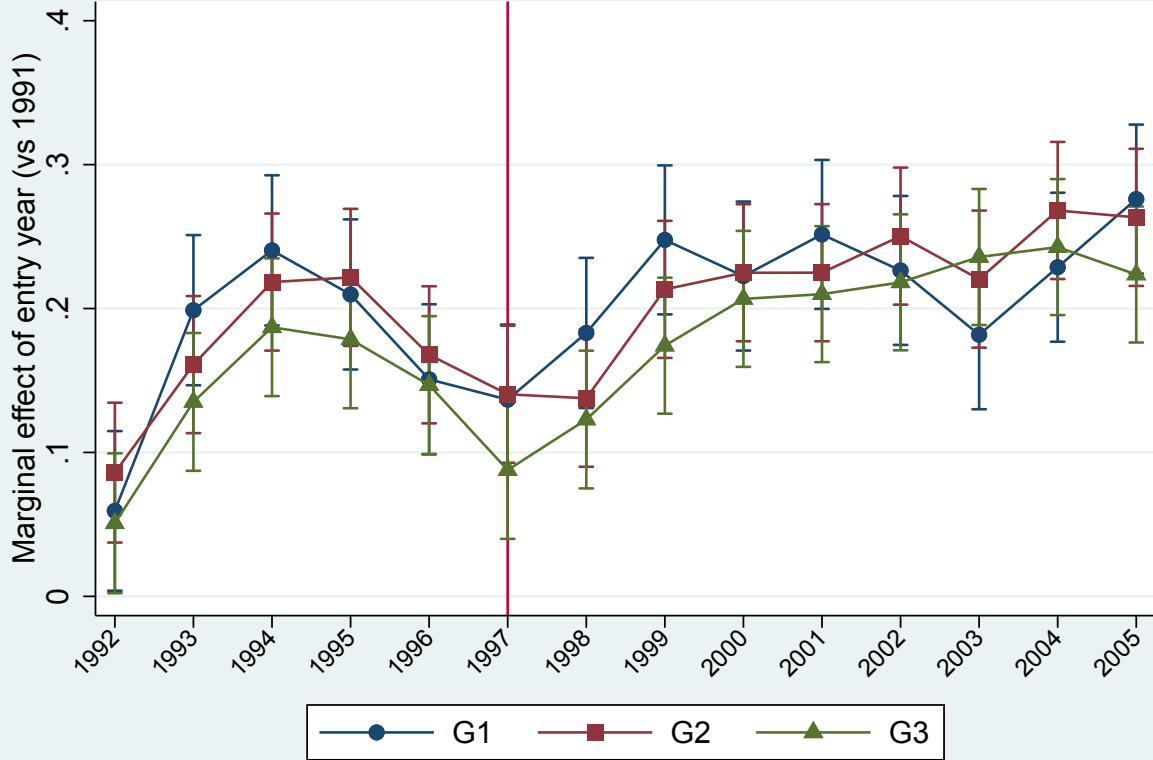
Outcome: Share of Cohort Alive at Age 3 (All Legal Forms)
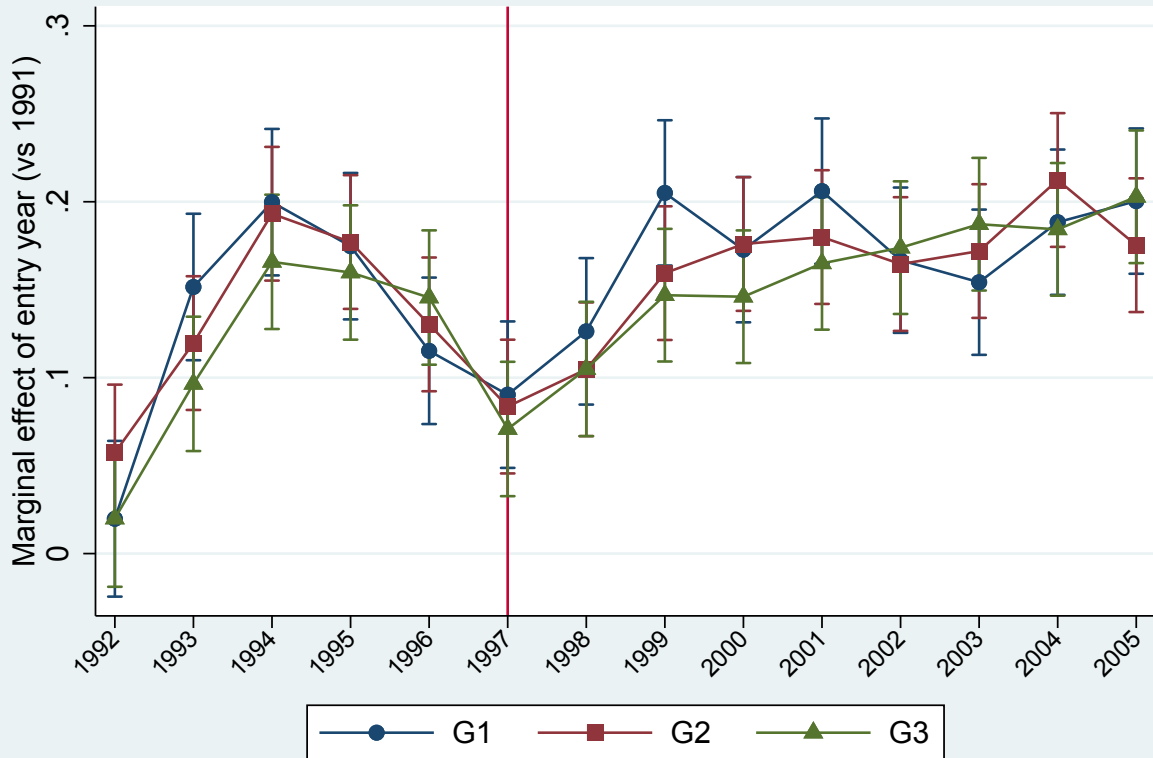
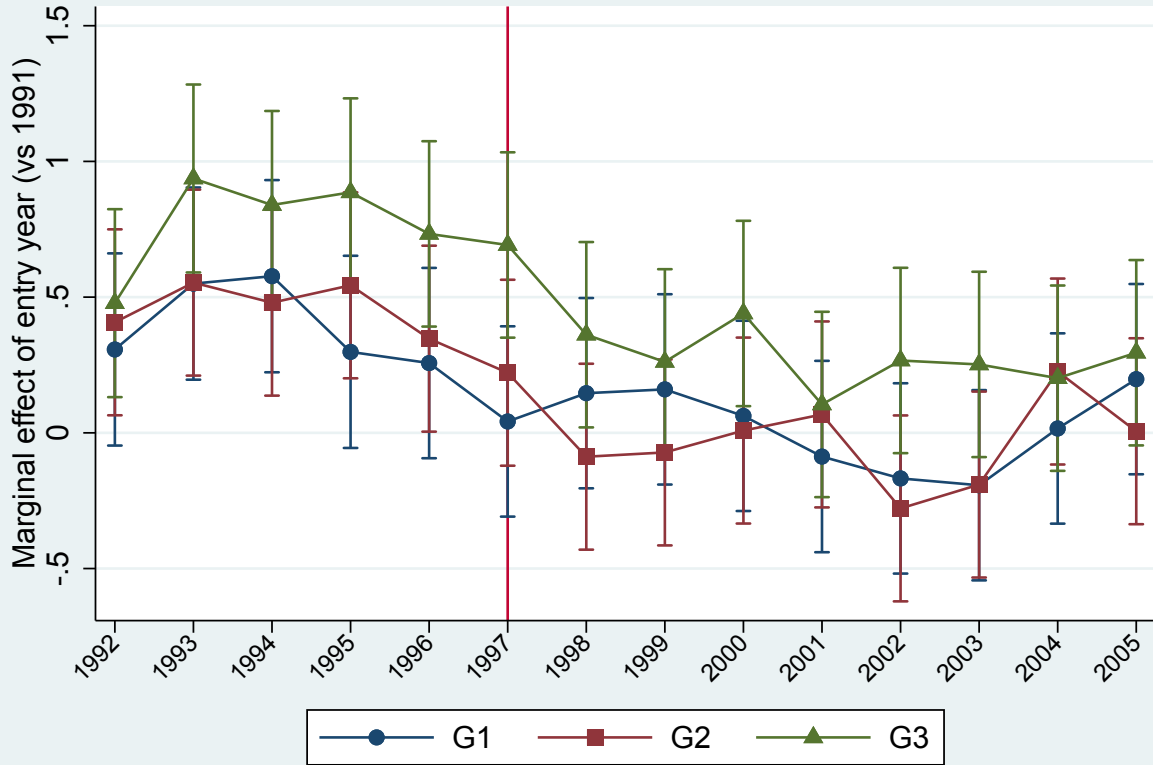Outcome: Log Mean Employment at Age 3 (Only Limited Companies)
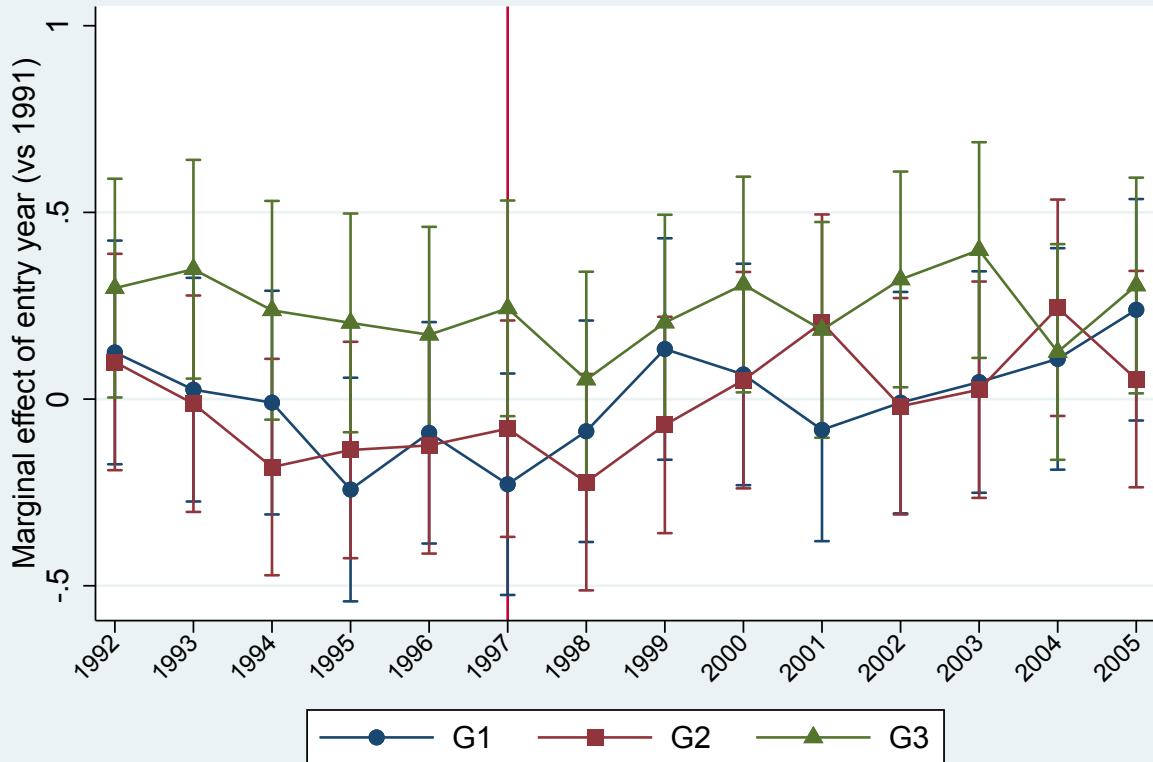
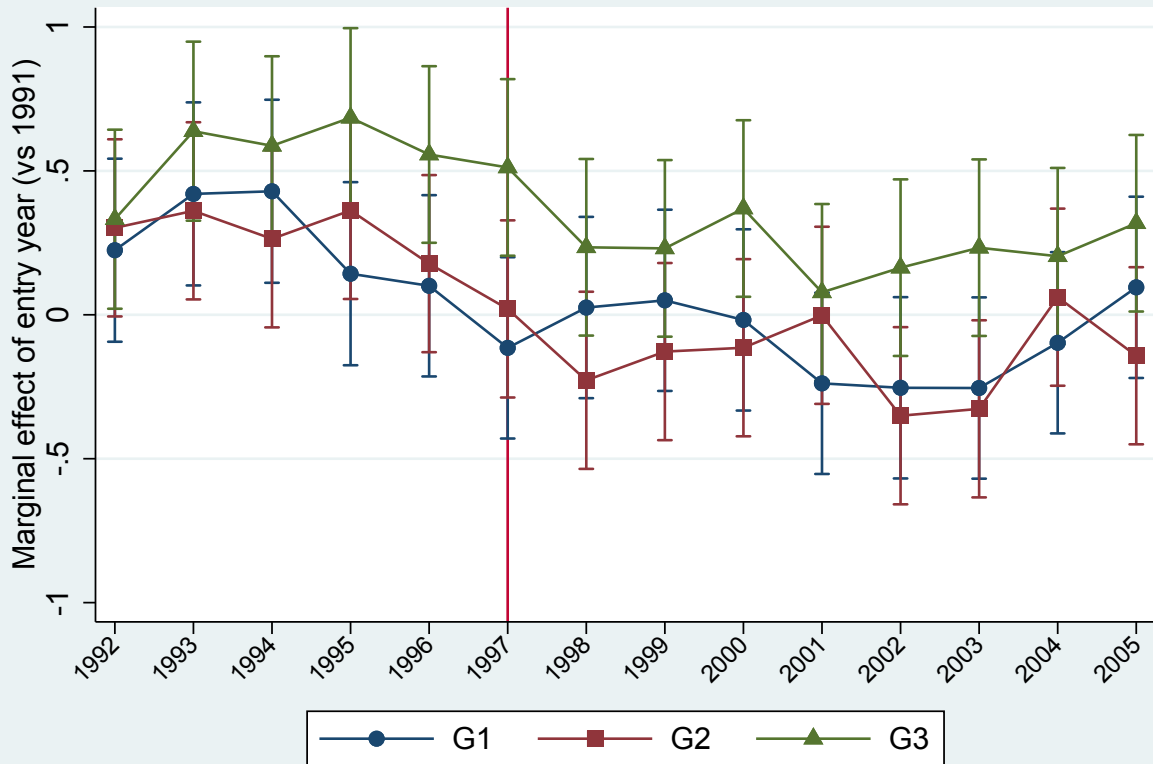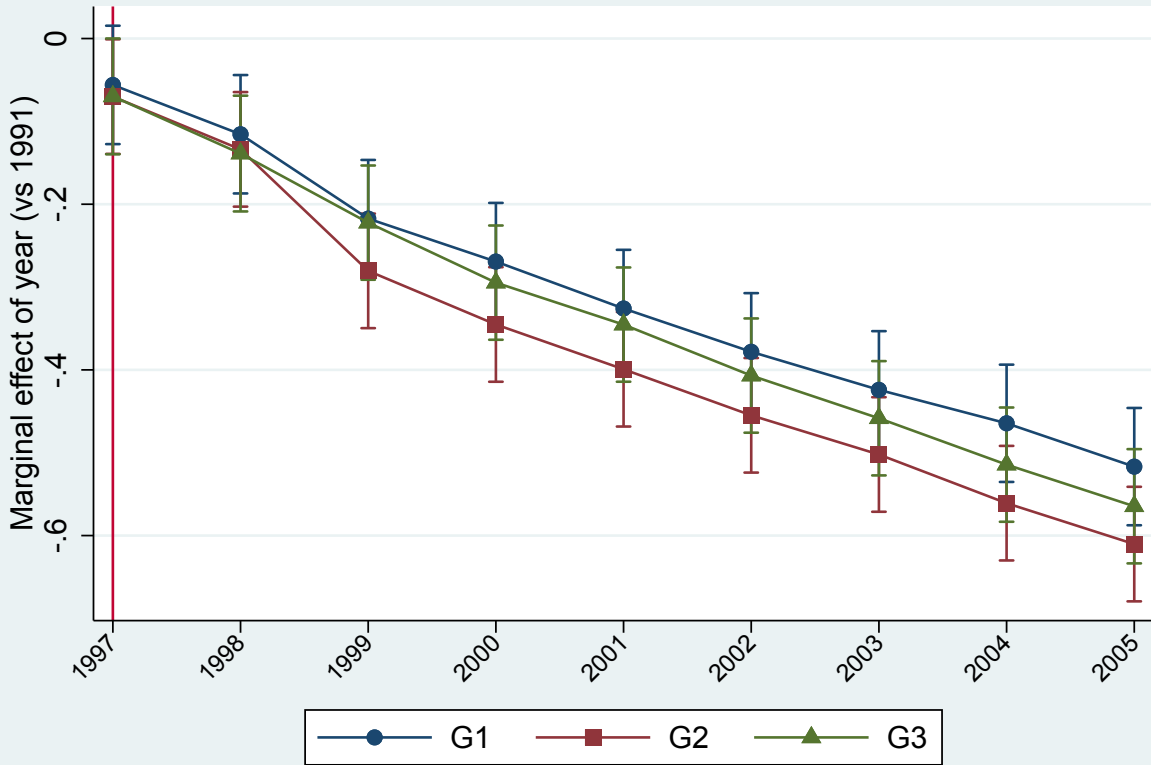Outcome: Log Mean Employment at Age 3 (All Legal Forms)

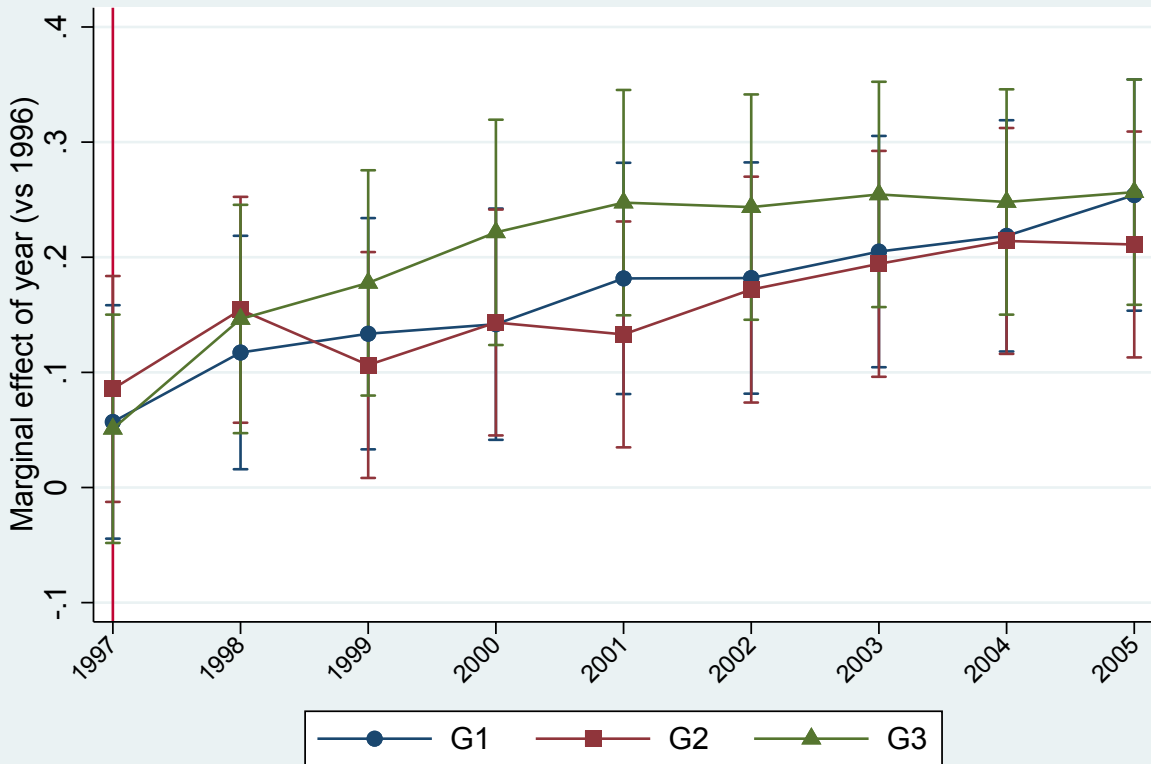Outcome: Log Total Cohort Employment at Age 3 (Only Limited Companies)

Outcome: Log Total Cohort Employment at Age 3 (All Legal Forms)

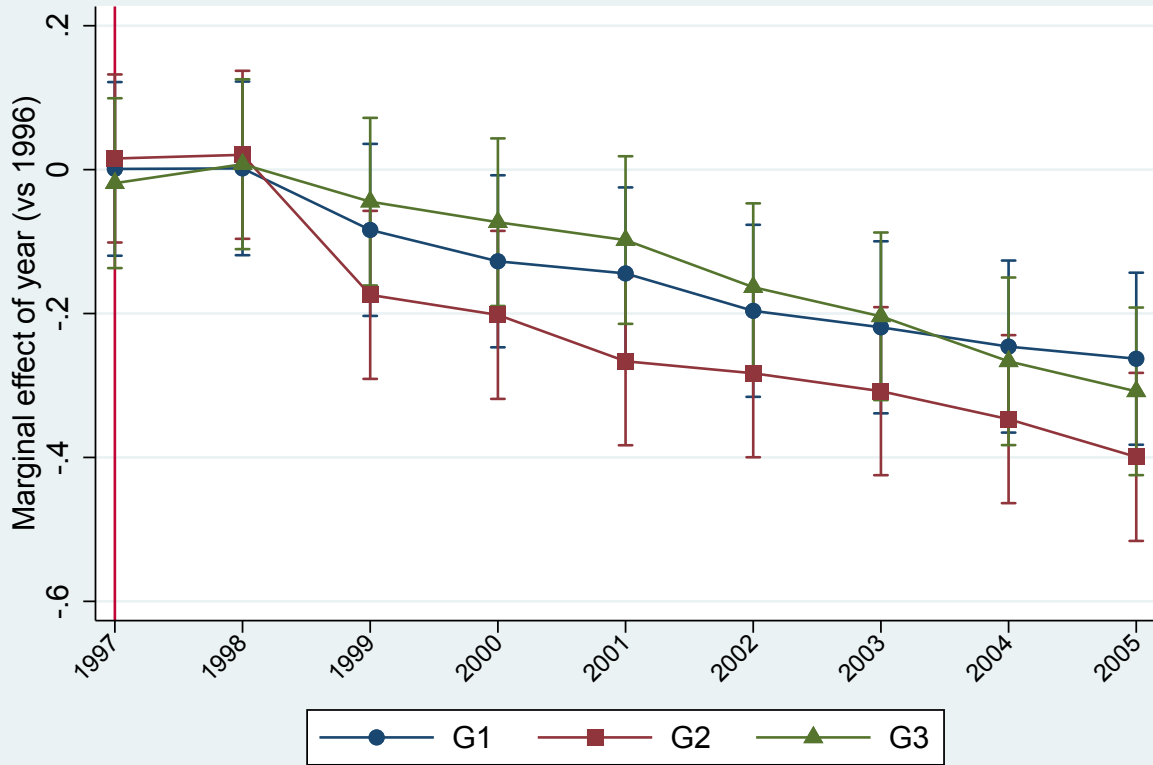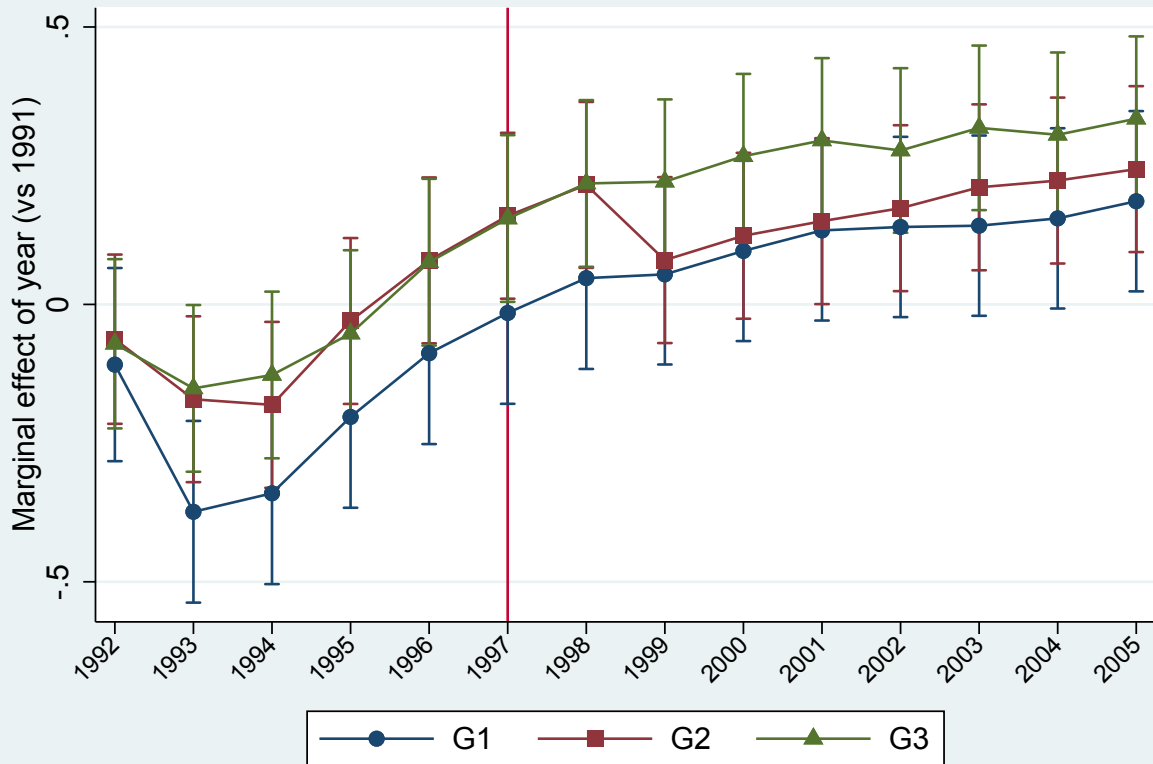Outcome: Log Number of Incumbents
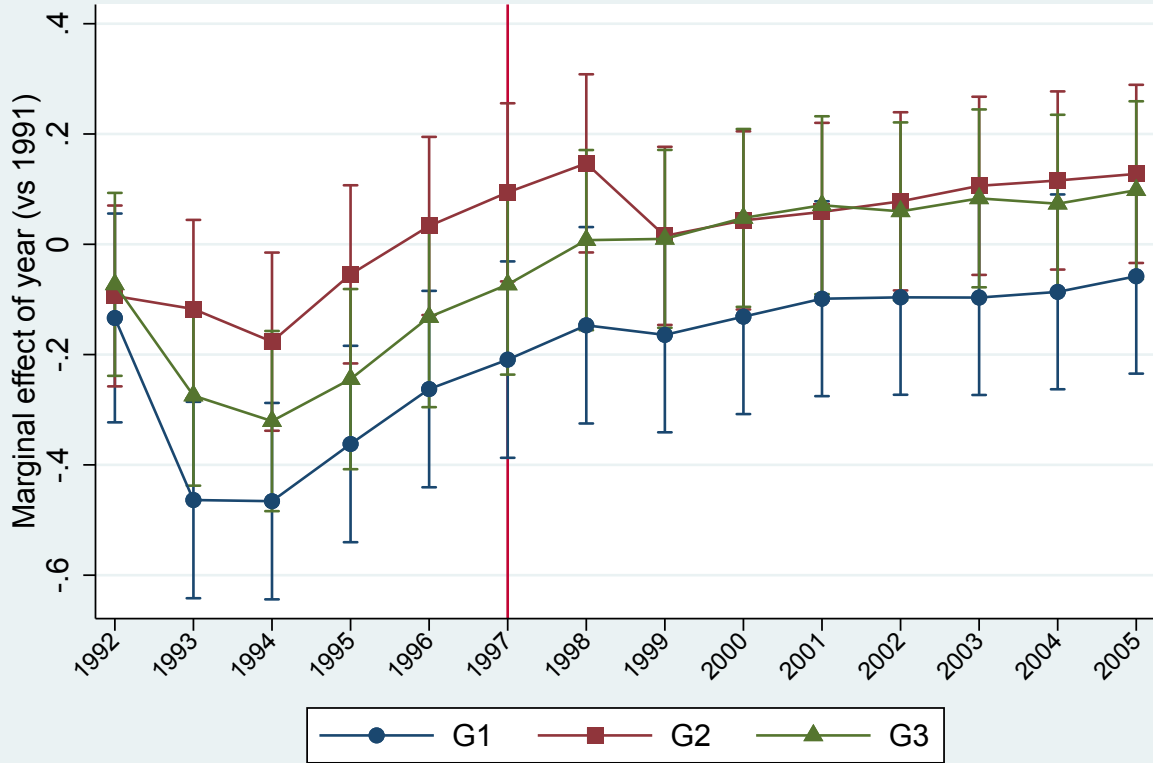
Outcome: Log Mean Employment of Incumbents

Outcome: Log Total Employment of Incumbents

Outcome: Log Total Industry Employment (Only Limited Companies)

Outcome: Log Total Industry Employment (All Legal Forms)

Sectoral Composition of the Treatment Intensity Groups for the 1997 Reform