THE EFFECT OF MANAGER EDUCATION ON FIRM GROWTH

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Abstract

This paper shows that firm life cycle growth increases strongly with manager education using administrative data on the universe of firms and workers in Portugal. Among firms with college educated managers, the average 40-year old firm employs 12 times as many workers as the average entrant, while among firms with primary-school educated managers that ratio is below two. The same pattern holds when tracking a cohort of firms over time and sorting them by manager education at entry. Consistent with the cross-sectional findings, firms that switch to more educated managers experience a sharp increase in growth relative to comparable firms, and I present evidence that indicates the increase is driven by education itself rather than other manager characteristics correlated with education. Turning to possible mechanisms, I find that the results are stronger for managers with degrees in engineering, science, health and business. More educated managers also increase the use of incentive pay and are more likely to report that their products and services are new and incorporate new technologies. These findings suggest that the effect operates through technology adoption and human resource management. I conclude by calibrating a model of heterogeneous firms to explore the aggregate implications of differences in manager education. Moving from the distribution of manager education in Portugal to that of the U.S. would raise aggregate productivity by about 20 percent, accounting for one third of the gap in output per capita between the two countries.

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I Introduction

A long tradition in economics emphasizes human capital as a driver of productivity (Schultz, 1961; Becker, 1962). Recent evidence suggests that managerial human capital may play a particularly important role. Management practices such as target setting, monitoring and implementing appropriate incentives seem to have large effects on firm productivity,[1] and manager education can account for significant differences in productivity across firms and regions.[2]

The canonical model of management and firm performance is Lucas (1978), and its distinguishing feature is that managerial skill increases the productivity of other workers. The key prediction from this model is that firms with higher skilled managers grow larger, yet evidence on this relationship has been limited. Most of the existing work on management focuses on productivity or profitability in samples of large firms, and little is known about the role of management in determining which firms grow large in the first place.[3] Shedding light on this role requires data on management and firm growth over the life cycle.

Using administrative panel data on the universe of firms and workers in Portugal, this paper shows that firm life cycle growth increases strongly with manager education. The data combine two features that make it uniquely suited for this study. On the worker side, there are detailed occupational codes that identify managers directly.[4] And on the firm side, the

[1]Bloom and Van Reenen (2007, 2010) develop a comprehensive index of management practices and show that it is a strong predictor of productivity. Bloom et al. (2013a) confirm this relationship in an experimental setting. In addition, several studies have found that human resource management practices have important effects on firm productivity, including Ichniowski, Shaw and Prennushi (1997), Lazear (2000), Shearer (2004), Bandiera, Barankay and Rasul (2007).

[2]See (Gennaioli et al., 2013). La Porta and Shleifer (2008, 2014) find that informal firms have less educated managers than their formal counterparts in the World Bank Enterprise Surveys. Other work on the effect of managers on firm performance includes Johnson et al. (1985), Bertrand and Schoar (2003), Pérez-González (2006), Bennedsen, Pérez-González and Wolfenzon (2006), Bennedsen et al. (2007), Goodall, Kahn and Oswald (2011), Kaplan, Klebanov and Sorensen (2012), Bandiera, Prat and Sadun (2013), Becker and Hvide (2013), Mion and Opromolla (2014) and Lazear, Shaw and Stanton (2015).

[3]For example, the average firm in Bloom and Van Reenen (2007) employs around 2000 workers, and in Gennaioli et al. (2013) about 150. Firms with 150 workers are in the top one percent of the firm size distribution in the U.S, according to data for 2011 from the U.S. Census Bureau, accessed here: https://www.sba.gov/advocacy/firm-size-data. There is also an emerging literature that finds that providing management consulting to small firms in developing countries may have a positive effect on firm employment (Bruhn, Karlan and Schoar, 2013; Karlan, Knight and Udry, 2015).

[4]Studies of entrepreneurship using matched employer-employee data have typically relied on proxies such as employment status or presence at entry to identify managers (e.g. Nanda and Sorensen, 2010).

data cover the universe of firms, without any size thresholds, and include the founding year. This enables me to track firms throughout the life cycle. In addition, there is substantial variation in manager education in the data. Starting from a very low base, Portugal has experienced substantial growth in educational attainment since the mid-20th century. In the period I examine, from 1995 to 2009, the median firm's manager had nine years of schooling, and the standard deviation across firms was 4.3.

I start by examining how firm growth varies with manager education in the cross-section. To get a sense of the magnitude of the relationship, consider figure 1a, which displays average firm size by manager education at different firm ages, all measured in 2009. Among firms whose managers have an average of 15 or more years of schooling, the average 40-year old firm employs 12 times as many workers as the average entrant, while among firms whose managers have an average of less than six years of schooling that ratio is below two. The same pattern holds when tracking a cohort of firms over time and sorting them by manager education at entry. These findings are not driven by differences in survival; if anything firms with more educated managers are more likely to survive as well. They do not reflect differences in sector composition, either, and they are not driven by superstars, but by a mass of mid-sized firms. The results are also specific to management. As shown in figure 1b, life cycle growth and non-manager education are largely unrelated.

The cross-sectional evidence could be driven by omitted firm characteristics, and I exploit within-firm variation in manager education to address this concern. Using event studies of manager changes, I find that firm growth increases sharply for firms that switch to college-educated managers relative to firms that switch to managers who completed the 12^{th} grade or less, controlling flexibly for firm characteristics before the change.[5] Extending the analysis to the entire sample, I find that a year of manager education increases firm growth by around 0.3-0.4 percentage points. In a simple simulation, I find that the estimated effect leads to differences in life cycle growth of the same magnitude as those in the cross-sectional evidence.

The key assumption in exploiting within-firm variation is that firms making different changes to manager education would have followed similar growth trends in the absence

[5]These results are consistent with Pérez-González (2006), who finds that firms with family CEO successions underperform firms with non-family CEO successions among U.S. publicly traded firms, and that the effect is entirely driven by family successions where the incoming CEO attended a less selective college.

of these changes. One concern is that more educated managers might sort into firms that experience positive growth shocks. While this type of selection certainly plays a role in management changes at large professionally-managed firms,[6] past research suggests that there are substantial agency costs in employing professional managers, and that the allocation of managerial talent across firms is far from efficient (Burkart, Panunzi and Shleifer, 2003; Caselli and Gennaioli, 2013).[7] In line with this view, the population of firms in Portugal is dominated by small owner-managed firms where selection is less likely to play a role. The median firm in the event studies employs six workers, and at least 68 percent are owner-managed.[8] The idea underlying the event study methodology is that conditional on a rich set of observable firm characteristics – firm size, age, sector and manager and non-manager characteristics – management changes in this sample are likely to be driven by idiosyncratic factors, such as family, social or professional ties to the owner.

I present several pieces of evidence to support this parallel trends assumption. First, pretrends in growth for both groups of firms in the event studies are very similar, with a clear break in the year when the management changes occur. Second, the results hold when restricting the sample to firms that were owner-managed both before and after the manager changes. In these cases, the changes are particularly likely to be driven by family ties. Third, a series of additional event studies reveal that increases in education in other occupation groups – professionals, office workers, service workers and blue-collar workers – have no effect on firm growth. Finally, I restrict the analysis to management changes where at least one of the exiting managers leaves the sample permanently before age 60. Since the data cover the universe of firms, this implies that this manager is likely to have exited the labor force for reasons exogenous to the firm's performance, such as death or

[6]For example, Bandiera et al. (2015) show evidence of matching between firms with stronger incentive policies and more talented and risk-tolerant professional managers in a sample of large Italian firms.

[7]Most firms, particularly in developing countries, are family-owned and managed (La Porta, Lopez-de-Silanes and Shleifer, 1999), even though family successions have a strong negative effect on firm performance (Pérez-González, 2006; Bennedsen et al., 2007). Caselli and Gennaioli (2013) show that this "failure of meritocracy" may have a large impact on aggregate productivity. Bloom et al. (2013a) find that the strongest predictor of firm size among manufacturing firms in India is the number of the owner's male family members.

[8]This number represents a lower bound given by owners who do not receive wage income from the firm. If an owner-manager receives a wage from the firm, then that manager will be reported as an employee rather than as an owner.

disability.[9] I focus on exits before age 60 in order to address concerns with endogenous timing of retirement. In this sample, I find that firms that lose college educated managers experience a decline in both manager education and firm growth relative to firms that lose managers who completed the 12^{th} grade or less.

The results so far indicate that more educated managers increase firm growth, but do not reveal if the cause is education itself. Education could be correlated with factors such as ability or ambition that might affect firm growth. Even if these characteristics were uncorrelated with education in the population, there could be a selection bias: workers with higher schooling could have a better outside option and therefore only choose to become managers when they have higher ability.

I find that the coefficient on manager education is stable when I account for observable measures of ability and experience, namely the manager's age and tenure at the firm, management and industry experience, and the number of prior occupations (Lazear, 2005). In addition, I show that the manager's income in a previous employment spell as a non-manager can be used to account for the manager's ability under plausible assumptions, and I find that the coefficient on manager education after accounting for this measure of ability is very similar to that of the baseline estimate. Put together, these results suggest that the bias from omitted manager characteristics is unlikely to be a significant issue.

Next, I turn to the mechanisms underlying the effect. One view is that more educated managers are more adaptable and adopt new technologies faster (Nelson and Phelps, 1966; Welch, 1970; Schultz, 1975). In line with this view, I find that the effect is stronger for managers with degrees in engineering, science and health. Using data from the Global Entrepreneurship Monitor (GEM), a global survey of entrepreneurs, I also show that more educated managers are more likely to report that their products and services are new and incorporate new technologies, even within narrow sectors. Another view is that higher ability managers are better at coordinating workers and production inputs more broadly (Penrose, 1959; Chandler, 1977; Bloom and Van Reenen, 2007), and that this coordination ability is

[9] This approach parallels research that uses deaths to identify the effects of managers on firm performance (Johnson et al., 1985; Bennedsen, Pérez-González and Wolfenzon, 2006; Bennedsen et al., 2007; Becker and Hvide, 2013). As a simple sanity check, I find that expected deaths alone, given the distribution of age and gender in the population of managers, can account for half of the permanent exits I identify in the data.

enhanced by education. In favor of this second view, I find that the effect is also stronger for managers with degrees in business, and that more educated managers increase the use of incentive pay. The latter finding suggests that, in particular, more educated managers adopt more effective human resource management practices.

I conclude by exploring the aggregate implications of differences in manager education. Hsieh and Klenow (2014) use a standard model of heterogeneous firms to show that observed differences in firm growth between the U.S., Mexico and India can account for significant differences in aggregate productivity. Following their approach, I find that moving from the distribution of manager education in Portugal to that of the U.S. would raise aggregate productivity by about 20 percent, accounting for one third of the gap in output per capita between the two countries.

In addition to the literature on management and firm performance, this paper contributes to several strands in the literature. The literature on firm dynamics offers several theories of differences in firm growth. Besides managerial skill, these papers have for example developed models based on experimentation and learning (Jovanovic, 1982; Ericson and Pakes, 1995; Foster, Haltiwanger and Syverson, 2013), risk aversion (Kihlstrom and Laffont, 1979) and financial constraints (Evans and Jovanovic, 1989; Cooley and Quadrini, 2001; Albuquerque and Hopenhayn, 2004; Clementi and Hopenhayn, 2006). But evidence on the effects of these different factors has been limited.[10] This paper is among the first to find evidence of a firm characteristic that drives large systematic differences in life cycle growth.

In entrepreneurship studies there is a growing emphasis on the fact that growth-oriented entrepreneurs are different from other small business owners in terms of their motivation, ability and ambition (Schoar, 2010; Ardagna and Lusardi, 2010; Hurst and Pugsley, 2011),

[10] Using the same dataset as this paper, Cabral and Mata (2003) show that the firm size distribution is significantly right-skewed at entry and becomes more symmetric as firms age. They find that firms with younger owners start smaller and converge with other firms over time, accounting for this evolution, and they interpret this as evidence of an important role for financial constraints among young firms. Their paper also shows that owner education is positively correlated with firm size, in line with the findings in this paper, although I show below that when the owner and manager are different people it is the manager's education that increases firm growth. Angelini and Generale (2008) find weaker evidence on the effect of financial constraints using more direct measures for a sample of Italian firms. Michelacci and Silva (2007) show that local entrepreneurs grow larger and present evidence that this is due to better access to finance. There is also evidence that access to finance increases firm growth in the context of microenterprises in developing countries (de Mel, McKenzie and Woodruff, 2008; Karlan and Zinman, 2009; Banerjee et al., 2015)

but understanding what distinguishes high growth entrepreneurship is an open challenge. This paper finds that the education of entrepreneurs could be a key distinguishing feature underlying those differences.[11]

Finally, the paper also contributes to the literature on development accounting. It suggests that differences in manager education may be a key driver of differences in firm growth across countries, such as the ones observed by Hsieh and Klenow (2014), and have important effects on aggregate productivity through this channel.[12] In that sense these findings may help reconcile the large aggregate returns to education implicit in regression-based approaches to development accounting (Mankiw, Romer and Weil, 1992) with the smaller individual returns to schooling found in the labor economics literature (e.g. Card, 1999), complementing the findings of Gennaioli et al. (2013).

The rest of the paper is organized as follows. Section II describes the data and analysis sample, and reports summary statistics. Section III presents cross-sectional evidence on the relationship between manager education and firm growth. Section IV reports evidence from management changes. Section V evaluates whether education is itself the cause. Section VI investigates possible mechanisms driving the effect. Section VII explores aggregate implications. And section VIII concludes.

II Data

The paper uses data from *Quadros de Pessoal*, a matched employer-employee administrative panel data set collected annually by the Ministry of Employment in Portugal that covers the universe of firms with at least one employee and their workers, including owners and unpaid family workers. The survey combines firm-level information, such as total employment, sales and date of incorporation, with a wide range of worker characteristics. Over the sample period from 1995 to 2008 it contains 36 million worker observations corresponding to 9

[11] Guzman and Stern (2015) use information at the time of registration such as the firm's name or filling for a patent to predict right tail outcomes such as IPOs and acquisitions. Their index has strong predictive power even among highly successful firms, but does not identify the drivers of these differences at registration.

[12] An alternative explanation for the differences in firm growth observed by Hsieh and Klenow (2014) is offered by Akcigit, Alp and Peters (2015), who argue that lack of contract enforcement constrains growth by limiting the extent of delegation of managerial tasks.

million individuals, and 3.8 million firm observations corresponding to 701 thousand firms. This section defines the variables and sample used in the analysis and provides summary statistics.

II.A Variable Definitions

Managers The concept of manager in this paper is that of a top decision maker, what is commonly referred to as top management. Typical top management decisions include resource allocation and high level coordination and evaluation (Chandler, 1977). A key step in the analysis is the identification of managers in the data. Studies of entrepreneurship using large scale employer-employee matched datasets have identified entrepreneurs as workers who report being self-employed or present at the time of incorporation (e.g. Nanda and Sorensen, 2010). These procedures exclude professionally managed firms as well as firms where entrepreneurs choose to become employees of the firm. A unique strength of the data I use is the 6-digit occupational classification system that was introduced in 1995 (CNP 94), which identifies detailed managerial occupations and in particular accounts for managers of small firms. This occupational classification system enables me to consistently define top managers across firms directly based on their function.

I use the occupational data to identify a firm's managers as follows. CNP 94 groups top management positions into two sections according to firm size: section 1.2 Firm Directors and section 1.3 Small Firm Directors and Managers, where small firms are defined as having fewer than 10 workers. For small firms, section 1.3 does not provide additional functional detail. I therefore define small firm managers as all workers reported under section 1.3. For larger firms, section 1.2 classifies managers according to a hierarchy. At the top are General Managers, whose description most closely resembles that of top decision makers. The next category are Operations Managers, who also participate in high level decision making but report to General Managers when they exist. And the last group are Other Managers, who lead narrower functional areas. Other Managers are further sub-divided by functions, such as Administrative, Financial and Sales. I define managers for these larger firms as the workers at the top managerial position that the firm reports: General Managers if they exist, Operations Managers if there are no General Managers, and Other Managers if there are no

General or Operations Managers. The results in the paper are robust to other procedures such as defining all workers under section 1.2 as the firm's managers.

This procedure identifies managers for 63 percent of firm-years and 79 percent of employment among firms with at least two workers.[13] For the firm-years that do not report any workers under sections 1.2 and 1.3, I proceed as follows. If the firm reports any owners among its workers, I define the owners as the managers. This covers an additional 7 percent of firm-years and 3 percent of employment. If the firm does not report any owner-workers, I check whether the firm had any managers in the previous year. If it did, I assign the previous year's managers that are still working at the firm (but are now classified as workers) as managers. I then check whether the firm had any managers in the following year. If it did, I assign the following year's managers that were already working at the firm (but are still classified as workers) as managers. This procedure is meant to correct occupational classification errors, and covers another 4 percent of firm-years and 5 percent of employment. The results are robust to excluding managers identified through these additional procedures. In total, I identify managers for 74 percent of firm-years and 87 percent of employment in the sample.[14]

Other variables The outcome of interest in the paper is the firm's annual growth rate, and I also use data on average manager and non-manager education and age, the firm's age, size, revenue and two-digit sector. In some specifications I also use information on manager employment status and on non-manager income.

Firm growth is defined as the annual percentage change in employment, winsorized at the 99^{th} percentile.[15] In specifications where I account for firm exit, firm growth is defined as -100 percent in the year that a firm exits. All results are robust to winsorizing at the 95^{th} percentile instead. Firm size is defined as the firm's total employment at the beginning of

[13] For single-worker firms the manager would have to be the firm's only worker. In practice, the sample is restricted to firms with two or more workers because I control for non-manager characteristics in most of the empirical analysis below.

[14] The remaining 26 percent are very small firms, with an average of 6.1 workers. Managerial tasks at these firms are presumably minimal and not the primary focus of any of the firm's workers.

[15] An advantage of defining firm growth as a percentage change is that it provides a natural way to account for firm exit. A disadvantage is that the distribution is significantly right-skewed, as compared to the distribution of log growth. I winsorize the data to reduce the influence of outliers on the right tail. All results hold using log growth as well.

the year.

Education is measured as years of schooling completed. The data report the highest level of schooling attained by each worker, where the levels are: no schooling, 4th grade, 6th grade, 9th grade, 12th grade, *bacharelato* and *licenciatura*. The *bacharelato* and *licenciatura* are higher education degrees typically lasting three and five years, respectively.[16] The distinction is similar to that between associate and bachelor's degrees in the U.S.. When a worker reports different levels of educational attainment in different years I take the mode of these reports as that worker's attainment, to reduce measurement error. If there is more than one mode, I take the lowest.

Firm age is constructed using the firm's reported year of incorporation. When a firm reports different years of incorporation over time I take the mode as I do with education. A minor issue with the revenue data is that it corresponds to calendar years, whereas all other variables are measured as of the survey's reference month (October).

II.B Analysis Sample

The data are available for the period from 1985 to 2009, which implies that I can measure firm growth up to 2008. As described above, the occupational classification system used to identify a firm's managers was introduced in 1995. Firm age is also available starting in 1995. I therefore restrict the analysis to the period between 1995 and 2008. In addition, data on worker characteristics is not available in 2001.[17]

Throughout the analysis I use a set of firm-level controls that includes non-manager education and age. This requires restricting the sample to firm-years that have at least one manager and one non-manager,[18]

In addition, the focus of the paper is on private-sector firms. I exclude state-owned firms, defined as those that take the legal form of *Empresa Publica* (state-owned company)

[16] The higher education system changed in 2006 with the EU's Bologna Accords, but these changes are too recent to affect the sample used in this paper.

[17] The data set consists of three databases: a firm-level database (covering firm-level information such as firm age and total employment), an establishment-level database (e.g. location, employment) and a worker-level database (e.g. education, occupation). The worker-level database is not available in 2001.

[18] As well as non-missing controls, although this is not a significant issue. Education and age are available for 99 percent of worker-years; firm age is available for 99 percent of firm-years and firm size is available for all firm-years.

or where the state has an equity stake of at least 50 percent. I also exclude government agencies, which are covered when they employ workers under private sector labor law, and non-profits. A number of large privatizations occurred during the sample period, involving significant mergers, breakups and downsizings. I exclude these firms by also dropping all private firms that were state-owned at any point in time.[19] Altogether, I exclude 2.6 percent of firms with these filters.

The final sample consists of 1.8 million observations and 359 thousand firms. Table 1 presents summary statistics for this sample. Average firm growth conditional on survival is 1.4 percent, with a standard deviation of 31 percent. Two additional facts stand out. First, there is substantial variation in manager education. The median firm's manager has the ninth grade, and the standard deviation across firms is 4.3 years of schooling. Non-manager education has a lower median and displays less variation. Second, the sample is dominated by small firms. Average employment is 14 workers, with a median of five. Even the firm at the 90^{th} percentile employs only 22 workers. Firms employ 1.5 managers on average, and the majority employs just one. This reflects a key advantage of this dataset for the study of firm growth: the data cover the universe of firms, rather than just firms that have already grown beyond a certain threshold.

III Cross-Sectional Evidence

I start by examining the relationship between firm growth and manager education in the cross-section of firms.

Figure 1a shows average firm size by age for different levels of manager education, all measured in 2009. To construct it, I first sort firms by average manager education into five groups – zero to less than six years of manager schooling, six to less than nine, nine to less than twelve, twelve to less than fifteen and fifteen and over. Within these groups, I divide firms into five-year age bins, including a separate bin for entrants and grouping firms over

[19] In some cases the privatized firms were reincorporated and show up as new firms in the data. To identify these cases, I follow the procedure in Braguinsky, Branstetter and Regateiro (2011): I take all entering firms with over 50 employees and identify those where a majority of workers worked at state-owned firms in the previous year. This procedure identifies an additional 49 firms that I exclude.

50 years old into a > 50 bin. For each manager education group, I plot average firm size and average firm age for each age bin, where size is winsorized at the 99^{th} percentile within each age bin. I use a log scale for firm size in order to emphasize differences in growth rates along the life cycle.

The figure shows that firm size increases with age, as is well known, but the magnitude of the relationship depends strongly on manager education. Among firms in the top group, those whose managers have 15 or more years of schooling, the average 15-year old firm is about 2.5 times larger than the average entrant. The average 30 year-old firm is almost five times larger. And the average 40-year old firm is about 12 times larger. Among firms in the bottom group, those whose managers have less than six years of schooling, size differences are much smaller. In fact this ratio remains below two throughout the life cycle. The remaining groups fall in between these two, with the strength of the relationship between firm size and age increasing monotonically with manager education. Figure 1b shows that this relationship is specific to management by repeating the same exercise but sorting firms by non-manager education instead. If anything, firm size is negatively correlated with non-manager education at entry, and the two seem largely unrelated beyond age 10.

Figures 1a and 1b examine a cross section of firms. One explanation for these patterns could be that firms hire more educated managers as they grow. To address this possibility, I repeat the exercise but tracking a cohort of firms over time. I take the oldest cohort of entrants in the sample, the 1995 cohort, sort them by average manager education at entry and track these firms until age 14, in 2009. Figure 2a presents the results. Again, firms with more educated managers grow faster. Among firms in the top group, the average 14-year old firm is 2.4 larger than the average entrant, while for the bottom group the ratio equals 1.4. In both cases the magnitude is consistent with the cross-sectional differences at the same age. Figure 2a tracks only survivors at each age. Figure 3 plots survival rates for the same groups, and shows that firms in the top group are also more likely to survive, while firms in the remaining groups experience very similar survival rates. This suggests that differences in survivor growth are not driven by increased risk taking. Finally, figure 2b sorts the same firms by non-manager education at entry. As in the cross-section, there is a negative correlation between non-manager education and firm size at entry, and firm size across groups seems to converge over time.

Another explanation for these patterns could be that managers with different levels of education sort into very different sectors, and that more educated managers concentrate in sectors where firms grow larger. In figures 4a and 4b I reweight firms in each manager education group so that the distribution of sectors within each group matches the overall distribution across groups. For both the 2009 cross-section and the 1995 cohort there are slightly larger size differences at entry, but the pattern is very similar to that of the unweighted figures, with firms with more educated managers exhibiting substantially faster growth over the life cycle.

These differences are also not driven by superstars. Figure 5a shows the firm size distribution for firms that are at least 30 years old in the top and bottom groups in the 2009 cross-section.[20] Figure 5b does the same for firms from the 1995 cohort in 2009, again using manager education at entry. In both cases the difference in average size between the two groups is driven a mass of firms in the ∼10-250 size range, and not just the right tail.

Consistently with these findings, table 2 shows that the variation in firm size accounted for by manager education increases substantially with firm age. The R^2 from a regression of log firm size on manager education in the 2009 cross-section rises to 20 percent for firms over 40 years old. For non-manager education, it is below 3 percent at all ages.

Overall, the data show that there is a strong relationship between manager education and life cycle growth. In the following analysis I explore this hypothesis in further detail.

IV Evidence From Manager Changes

The cross-sectional relationship between manager education and firm growth could be biased by unobserved firm characteristics. In this section I exploit within-firm variation in manager education to evaluate this concern.

[20] For figures 5a and 5b I use non-winsorized values for firm size.

Event Studies I start by presenting graphical evidence from event studies around manager changes.[21] I define an event as a change in manager education holding the number of managers constant. I further require that manager education be constant for at least one year before and one year after the change. This is meant to identify real changes in management rather than temporary absences in the survey reference month, as well as reduce measurement error in manager education. Let $t = 0$ denote the event year, which means that the management change occurs at some point between $t = 0$ and $t = 1$. I define the event window as the three years before and after the event, that is from $t = -3$ to $t = 2$. If a firm experiences multiple events, I include each event and the corresponding three-year window, regardless of any possible overlap across event windows.

To analyze the impact of manager education on firm growth I define two groups: a treatment group, consisting of firms that switch to college-educated managers (578 events),[22] and a control group, consisting of firms that switch to managers who have on average completed 12 years of schooling or less (2883 events). At each moment in event time, I regress firm growth from t to $t + 1$ on an indicator for treatment and a set of controls: quartics in pre-treatment manager and non non-manager education and age, log firm size and log number of managers, as well as sector, firm age and year fixed effects. All controls are measured at the beginning of year $t = 0$, immediately prior to the manager change. I then plot average growth for treatment and control groups such that at each moment the difference between the two equals the coefficient on treatment and the weighted average of the two groups equals the sample average. The coefficients on treatment in the pretreatment period provide both a placebo test and a visual test of the parallel trends assumption in this approach.

Note that by construction an event requires the firm to be present in the sample between $t = -1$ and $t = 1$. In order to examine growth pretrends, I additionally require that the firm be present in the sample in years $t = -3$ and $t = -2$, which implies that I exclude manager changes before age 3. When a firm exits at $t = 2$ I assign it a growth rate of -100 percent in that year.

[21] The event study methodology used here parallels the one used by Chetty, Friedman and Rockoff (2013) in their study of teacher value-added and student achievement.

[22] College here means the equivalent of a bachelor's degree in Portugal, which typically corresponds to a total of 17 years of schooling.

As a first step, figure 6a shows the effect of treatment on manager education, estimated using the same approach but replacing firm growth with year-end manager education as the outcome variable.[23] Average manager education in the two groups is very similar in the pretreatment period, increases strongly in the event year for the treatment group and remains unchanged in the control group.

Figure 6b shows the effect of treatment on firm growth. In the pretreatment period, there are no differences in trends or levels between the two groups, which supports the validity of the design. In the event year, growth in the treatment group increases sharply, while growth in the control group follows the pretreatment trend, and the difference between the two groups persists in the years after treatment. To provide a measure of the effect I estimate the following equation:

$$\Delta g_j = \alpha_0 + \alpha_1 D_j + \theta \mathbf{X}_j + \delta_t + \varepsilon_j \tag{1}$$

where Δg_j is the change in average growth between the pre and post treatment periods for each event, excluding the event year $t = 0$ since both management teams were present at the firm during that year, and D_j is an indicator for treatment. $\mathbf{X}_j$ is the vector of pretreatment controls described above and δ_t is a year fixed effect. I find that treatment increases firm growth by 4.91 percentage points ($p < 0.01$). Using the same procedure, I find that treatment increases manager education by 7.71 years of schooling. Dividing the two coefficients implies that a year of manager education increases firm growth by 0.64 percentage points.

Figure 7 presents two robustness tests. The top panel restricts the sample to firms that were owner-managed both before and after the change. In these cases, it is likely that the management changes reflect family ties, rather than sorting of better managers into firms that experience positive growth shocks. The bottom panel restricts it to changes where the new manager(s) had already been working at the firm for at least three years. Given that the sample is dominated by small firms, these cases are also more likely to be driven by professional or personal relationships with the previous manager, rather than sorting. In

[23] I drop pretreatment manager education from the set of controls in this first step.

both cases, the pattern is the same as in the main results.

To reinforce the validity of these results, I perform a series of tests by repeating the exercise with changes in average education for occupation groups other than managers. I use the standardized occupational codes in CNP 94 to form four occupation groups besides managers – professionals, office workers, service workers and blue collar workers.[24] I use the same methodology to construct event studies around worker changes in each of these four groups, modifying the set of pretreatment controls accordingly.[25] For professionals I use the same thresholds to define treatment and control groups as for managers: college versus 12th grade or less. For the other groups I adjust them downwards to reflect the level of education in each group. For office and service workers I use 12th grade or more versus 9th grade or less, and for blue collar workers 9th grade or more versus 6th grade or less. Figure 8 presents the results. In all four cases treatment leads to a large and significant increase in average education for the respective occupation group, but there is no effect on firm growth.

Permanent Manager Exits One way to further address concerns with selection is to focus on manager exits that are particularly likely to be unrelated to the firm's performance. Past research has used CEO deaths as a source of exogenous management changes (Johnson et al., 1985; Bennedsen, Pérez-González and Wolfenzon, 2006). While I do not observe deaths, I do observe whether a manager leaves the sample permanently after exiting a firm. Since the data cover the universe of firms and workers in Portugal, this implies that with high probability the manager exited the labor force.

I examine the effect of manager exits where the exiting manager leaves the sample permanently before age 60. These exits are likely to be driven by factors that are exogenous to the firm's performance, such as death or disability.[26] I focus on exits before age 60 to minimize concerns with endogenous retirement.[27] In order to identify true exits, I restrict

[24] Professionals correspond to sections 2 and 3 in CNP94, office workers to section 4, service workers to section 5 and blue collar workers to sections 6 through 9.

[25] For example, for the event study on professionals I split each firm's workers into professionals and non-professionals, and then change the set of pretreatment controls to include average education and age for professionals and non-professionals, instead of for managers and non-managers.

[26] It is also possible that the exiting manager (permanently) became a public servant, self-employed without any employees or wage income, or was simply unemployed for a long period, in which cases they would no longer be covered by the data.

[27] The standard retirement age in Portugal is 65; during the sample period, the fraction of Social Security

the analysis to exits before 2007, so that the the manager must be absent for at least three years within the sample period. As a simple sanity check, I find that just over half of these permanent exits can be accounted for by expected deaths alone, given the distribution of age and gender in the population of managers.[28]

If there are significant frictions in the market for managers, as argued above, then firms that lose a highly educated manager may not easily find an equally educated replacement, while firms that lose a less educated manager may be able to find a more educated replacement. This implies that the exogenous loss of a highly educated manager may on average lead to a drop in manager education relative to the loss of a less educated manager. If these losses are uncorrelated with other changes in firm growth, then firms that lose more educated managers should also experience a relative drop in firm growth. To examine this hypothesis, I again split firms into two groups: a treatment group consisting of firms that lose a college educated manager and a control group consisting of those that lose a manager with 12 years of schooling or less. I examine changes in manager education and firm growth for both groups around the time of the manager's exit using the same methodology as in the event studies above, controlling for quartics in the exiting manager's age and in the log number of managers immediately before the exit. In order to facilitate the comparison, I normalize the difference in manager and education and firm growth between the two groups to zero at time $t-1$.

Figure 9 presents the results. The top panel shows that firms that firms in the treatment group experience a drop in manager education relative to firms in the control group, and that this drop persists in the years after the loss, which is consistent with the presence of frictions. The bottom panel shows that firm growth follows a similar trend for both groups before the manager exits and drops sharply for firms in the treatment group in the years after the change, while growth in the control group increases.

Full Sample Estimates The event studies focus on firms that switch to highly educated managers – managers with a college degree – in order to generate sharp changes in manager

pensioners under age 60 was less than one percent.

[28] I use mortality rates by age and gender from the National Institute of Statistics in Portugal to calculate expected deaths.

education. I now generalize the approach in equation (1) to the entire sample, in order to extend the analysis to different levels of education and obtain more precise estimates. Let g_{jt} denote growth between t and $t+1$ and changes in firm growth be given by

$$\Delta g_{jt} = \alpha_0 + \alpha_1 \Delta s_{jt} + \theta \mathbf{X}_{jt} + \delta_t + \varepsilon_{jt} \tag{2}$$

where $\Delta g_{jt} = g_{jt+1} - g_{jt-1}$ and $\Delta s_{jt} = s_{jt+1} - s_{jt-1}$ is the change in average manager education. As in the event studies, I impose two sample restrictions. First, I condition on the number of managers not changing between t and $t+1$, in order to isolate changes in manager education from changes in the quantity of managers. Second, I condition on $s_{jt-1} = s_{jt}$ and $s_{jt+1} = s_{jt+2}$, so that the manager education is constant for at least a year before and after any change. I also exclude g_{jt} from the comparison, since the effect of manager education is identified from management changes that occur at some point between t and $t+1$. Finally, $\mathbf{X}_{jt}$ is the vector of firm characteristics used in the event studies, measured before any management change, and δ_t is a year fixed effect.

Table 3 presents the results, along with several robustness checks. The top panel reports results that account for exit by assigning a growth rate of -100 percent when a firm exits, while the bottom panel presents results that are conditional on firm survival. Standard errors are clustered at the firm level in all regressions.

Starting with the top panel, column one reports estimates from the baseline specification in equation (2). In this specification, a year of manager education increases annual firm growth by 0.41 percentage points, with a standard error of 0.12. Column two extends the time horizon by comparing average growth three years before and three years after any changes. If a firm exits during the three years after, I use its average growth while present in the sample, including a value of -100 percent in the year that the firm exits. I find that the coefficient remains stable at 0.39 over this longer horizon. Column three uses dummies for different levels of education instead of the linear term in years of schooling. Relative to the omitted category of between zero and and six years of schooling, firm growth increases with each level of education. In line with the event study evidence, college-educated managers have a particularly large effect, more than twice as large as that of managers with high school degrees.

One possible interpretation of these results is that manager education is proxying for lower financial constraints, a prominent alternative explanation for differences in firm growth. If this were the case, then in firms where the owner and manager are different people it should be the owner's education that matters. Column four tests and rejects this hypothesis by adding owner education to the regression. The coefficient on manager education rises slightly to 0.49 relative to the baseline results, and the coefficient on owner education equals -0.04 and is insignificant.

In the baseline results I restrict the sample to years when manager education does not change in the year before and after a management change, in order to identify real changes in management rather than temporary absences and to minimize measurement error in manager education. In column five I remove this restriction and measure manager education as the average of start and year-end values,[29] and the coefficient rises to 0.66.

Finally, one concern with focusing on employment growth as the outcome is that firms might expand inefficiently, without increasing their revenue. Column six shows that this is not the case by using revenue growth as the outcome, also winsorized at the 99^{th} percentile. In fact the coefficient is larger: a year of manager education increases revenue growth by 0.66 percentage points.

All results so far account for firm exit. The bottom panel in Table 3 replicates the specifications in the top panel but conditions on firm survival, and finds very similar results. This implies that the results are primarily driven by survivor growth rather than differential exit, which is consistent with the cross-sectional evidence.

Implications for Life Cycle Growth Does the effect of manager education on annual firm growth estimated from management changes explain the cross-sectional differences in life cycle growth described in section III?

First, in order to account for the large size differences in the data, manager education must be a persistent firm characteristic. It turns out to be extremely persistent, with one and ten year autocorrelations of 0.97 and 0.82, respectively. Non-manager education is also

[29] When a firm exits during the year, I use the starting value only.

strongly autocorrelated, but to a lesser extent, with one and ten year autocorrelations of 0.90 and 0.62.

I perform a simple simulation to evaluate the effect of manager education on life cycle growth. I consider two firms, one whose manager has a college degree (17 years of schooling) and one with a primary-school-educated manager (4 years of schooling). This choice facilitates comparison with the top and bottom groups in figure 1a, which have an average of 16.7 and 4.1 years of manager schooling respectively. Firms in the top group are just 9 percent larger at entry than firms in the bottom group. I take this difference in starting sizes and use the estimated effect of manager education on firm growth conditional on survival to simulate the relative size difference between the two firms at the average firm age in each of the age bins in figure 1a. The coefficient on manager education conditional on survival in table 3 ranges from 0.3 to 0.5. I assume a conservative estimate of 0.3 for the purposes of this simulation, which implies a difference in annual growth rates between the two firms of $(17 - 4) \times 0.3 = 3.9$ percentage points per year. Table 4 shows the results. For firms aged 11 to 15, the bin which corresponds to the average age in the sample, the simulation yields a 1.80 size difference versus 1.81 in the data. For firms aged 31 to 35, the difference rises to 3.88 in the simulation versus 4.00 in the data. And for firms aged 46 to 50, it increases to 7.04 in the simulation versus 7.25 in the data. Across age bins, the simulated size differences are close to the size differences in the cross-sectional data. Overall the simulation shows that the estimated effect of manager education on annual firm growth can lead to substantial differences in size over the life cycle, and that it can explain most of the variation observed in the cross-sectional relationship between manager education and firm life cycle growth.

V Is Education the Cause?

The results using manager changes indicate that more educated managers increase firm growth, but not whether the cause is education itself or unobserved manager characteristics, such as ability, that education could be proxying for. Even if ability were uncorrelated with education in the population, there could be a selection issue: agents with more education may have a better outside option in other occupations and may only choose to become managers if they have high ability.

Answering this question decisively would require random assignment of education and of occupational choice. A more modest goal is to examine whether the results are robust to controlling for other manager characteristics that may affect firm growth. If the coefficient on manager education is stable, then it is less likely to be severely biased. This is the approach taken in this section.

I proceed in two steps. First, I construct observable measures of managerial ability and experience from the data. Second, I develop a strategy to control for omitted ability using the manager's income when working as a non-manager in a previous employment spell as a measure of the manager's outside option.

Observable measures of ability and experience I start by accounting for manager characteristics observable in the data. The manager's age and tenure at the firm are directly reported. I also construct management and industry experience of each manager from their past employment spells.

A measure of ability used in the literature on entrepreneurship is the number of prior occupations (Lazear, 2005). In Lazear's model, entrepreneurs benefit from being "jacks-of-all-trades" who are competent across a range of skills. As a proxy for a diverse skill set, Lazear uses the number of occupations an entrepreneur has had experience with in previous employment spells, and shows that this variable is a strong predictor of the choice to become an entrepreneur. Following the same procedure, I use information about each manager's past employment and the standardized occupational codes in the data to measure each manager's number of prior occupations.

One issue with the measures of management and industry experience, as well as with the number of prior occupations, is that they are censored, since I construct them from past observations in the data, which covers the years from 1995 to 2009. For example, for a manager in the year 2000 I observe at most five years of experience. The coefficients on these measures are therefore likely to be amplified (Rigobon and Stoker, 2007) and caution should be used in their interpretation. The focus here, however, is on how they affect the coefficient on education. In addition, the results are robust to using only the later years in the sample, where the bias from censoring is likely to be less severe.

Outside Option Suppose that the education coefficient is just proxying for the fact that more educated people have a better outside option and only sort into management when they have high ability. Then the manager's income in a prior employment spell before becoming a manager plausibly contains information about that ability.

Formally, let agents be endowed with schooling s, managerial ability a^m and non-managerial ability a^l, and let firm growth be given by

$$g_{jt} = \alpha_0 + \alpha_1 s_{jt} + \alpha_2 a^m_{jt} + \varepsilon_{jt} \tag{3}$$

Let the linear projection of a^m on a^l for the manager of firm j at time t be given by

$$a^m_{jt} = \rho_0 + \rho_1 a^l_{jt} + \eta_{jt} \tag{4}$$

In the background, agents sort into management if their income as managers exceeds their outside option as non-managers. I assume that this outside option is given by the standard Mincerian wage equation

$$\ln w_{jt} = \beta_0 + \beta_1 s_{jt} + \beta_2 a^l_{jt} \tag{5}$$

where $\ln w_{jt}$ is the log of the outside option for the manager of firm j at time t. I omit experience to save on notation but will account for it below. Using (5) and (4), (3) can be rewritten as

$$g_{jt} = \alpha_0' + (\alpha_1 - \frac{\alpha_2 \rho_1 \beta_1}{\beta_2}) s_{jt} + \frac{\alpha_2 \rho_1}{\beta_2} \ln w_{jt} + \alpha_2 \eta_{jt} + \varepsilon_{jt} \tag{6}$$

where $\alpha_0' \equiv \alpha_0 - \frac{\alpha_2 \rho_1 \beta_0}{\beta_2} + \alpha_2 \rho_0$. This expression shows that the outside option $\ln w_{jt}$ can be used as a proxy control for a^m_{jt}.

This addresses the bias from omitted a^m_{jt} while introducing another bias, since $\ln w_{jt}$ is partly determined by schooling s_{jt}. But unlike the original bias the new bias is plausibly negative. It is captured by the $\frac{\alpha_2 \rho_1 \beta_1}{\beta_2}$ term in the coefficient on s_{jt}. α_2 and β_2 are the effects of a^m and a^l on firm growth and wages, respectively, which are positive by definition. ρ_1 will be positive if the two abilities are positively correlated, as can reasonably be expected. A test of this condition is that the coefficient on $\ln w_i$ in (6) should be positive. Finally, β_1 is the labor market return to schooling in equation (5), which a large literature has shown is also positive (e.g. Card, 1999). Intuitively, conditional on $\ln w_{jt}$, higher s_{jt} implies lower a^l_{jt}, which in turn should imply lower a^m_{jt} and therefore lower g_{jt}. It follows that the coefficient on schooling in this specification represents a lower bound on the true coefficient α_1. In addition, the bias term is equal to the coefficient on $\ln w_{jt}$ multiplied by β_1. Since β_1 is a parameter that has been well identified in the literature, I can draw on reasonable estimates of β_1 to recover the bias and obtain an estimate of the true coefficient α_1.

The key assumption in this approach is that $corr(s_{jt}, \eta_{jt}) = 0$, meaning that there are no components of ability that increase firm growth and are uncorrelated with the manager's outside option but are correlated with schooling. While this might not hold exactly, it seems reasonable to argue that any such residual components of ability should not lead to first order biases. For example, it is likely that important unobserved characteristics like talent, ambition or family background affect both firm growth and the outside option. Moreover, since the outside option represents the manager's best alternative occupation it is likely that the mix of abilities required in that particular occupation is not too distant from the mix required in the managerial position that the agent chose.

In order to estimate equation (6), data on the managers' outside option is required. For this purpose I use a sample of switchers – people who worked as non-managers before becoming managers within the sample period. In this sample, which comprises just under one third of the full sample, I observe a manager's income when working as a non-manager in a prior employment spell, and I take the manager's last observed non-managerial income, residualized on year and experience dummies, as the manager's outside option. The results are robust to using the average of all previous observations of non-managerial income, rather than just the last one.

One concern with this procedure could be measurement error. I do not observe the manager's actual outside option at year t, and instead proxy for it with earnings at a previous job. This might attenuate the coefficient on $\ln w_{jt}$ and amplify the coefficient on s_{jt}. But measurement error would attenuate the bias correction for the schooling coefficient as well.

As long as measurement error is not correlated with schooling, the bias-corrected estimate of α_1 would be minimally affected.[30]

Results Accounting for these measures of managerial ability and experience using management changes, as in section IV, would require data on these characteristics both before and after the management change, which reduces the sample dramatically in the case of the outside option. I therefore account for them in a cross-sectional specification. I compare firms within narrow cells defined by the firm's age, sector, municipality and year of observation, that is by estimating regressions of the following form

$$g_{jt} = \alpha_0 + \alpha_1 s_{jt} + \gamma y_{jt} + \theta X_{jt} + \lambda_{akrt} + \varepsilon_{jt} \tag{8}$$

where y_{jt} represents the additional manager characteristic, X_{jt} is a vector of firm-level controls including quartics in non-manager education and age, log firm size and log number of managers, and λ_{akrt} is a firm age $\times$ sector $\times$ municipality $\times$ year fixed effect. The focus in this approach is on how the coefficient on manager education changes when additional manager characteristics are added to the regression, rather than on its level, and therefore bias from omitted firm characteristics should be less of a concern. In any case, I find that the coefficient on manager education in this specification is similar to the coefficient estimated

[30] Any effect would depend on the interaction of measurement error and ability bias in a regression of the outside option in equation (5) on the manager's education. Formally, let the noisy measure of the outside option be given by $\ln w^*_{jt} = \ln w_{jt} + u_{jt}$, where u_{jt} represents classical measurement error. Then the probability limit of the coefficient on $\ln w^*_{jt}$ in (6) would equal $\frac{\alpha_2\rho_1}{\beta_2}\frac{\sigma_e^2}{\sigma_e^2+\sigma_u^2}$, where σ_e^2 is the variance of the residual from a regression of $\ln w_{jt}$ on the remaining covariates in (6). The probability limit of the coefficient on manager schooling s_{jt} would equal $\alpha_1 - \frac{\alpha_2\rho_1\beta_1}{\beta_2} + \beta_1^*\frac{\alpha_2\rho_1}{\beta_2}(1-\frac{\sigma_e^2}{\sigma_e^2+\sigma_u^2})$, where β_1^* is the coefficient from a regression of $\ln w_{jt}$ on s_{jt}. β_1^* in turn would equal $\beta_1 + \beta_2\gamma$, where γ is the coefficient from a regression of a^l_{jt} on s_{jt}. Applying the bias correction to the coefficient on s_{jt} would therefore lead to a consistent estimate of

$$\alpha_1 - \frac{\alpha_2\rho_1\beta_1}{\beta_2} + (\beta_1+\beta_2\gamma)\frac{\alpha_2\rho_1}{\beta_2}(1-\frac{\sigma_e^2}{\sigma_e^2+\sigma_u^2}) + \beta_1\frac{\alpha_2\rho_1}{\beta_2}\frac{\sigma_e^2}{\sigma_e^2+\sigma_u^2} = \alpha_1 + \beta_2\gamma\frac{\alpha_2\rho_1}{\beta_2}(1-\frac{\sigma_e^2}{\sigma_e^2+\sigma_u^2}) \tag{7}$$

The literature on returns to schooling has found the ability bias term $\beta_2\gamma$ to be small, on the order of 10 percent of β_1 (Card, 1999), which implies that the bias term on the right-hand side of (7) will be minimal even if measurement error in the outside option is severe. To give with a quantitative example, suppose that measurement error is such that $\frac{\sigma_e^2}{\sigma_e^2+\sigma_u^2} = 0.5$, which implies that the coefficient on the outside option in (6) is attenuated by 50 percent. Assuming a return to schooling of $\beta_1 = 7\%$ and using the coefficient on $\ln w^*_{jt}$ from column two in table 5, the bias on α_1 would equal $7\% * 10\% * 0.94 = 0.007$. This compares with an estimate for α_1 of 0.49 in column two of table 5.

from management changes.

Table 5 presents the findings. All regressions use a common sample where information on all manager characteristics is available, and standard errors are clustered at the firm level. The top panel presents results accounting for firm exit and the bottom panel conditions on survival.

Starting with the top panel, column one presents the baseline effect of manager education accounting for firm exit, which equals 0.51, and the following columns add other manager characteristics one by one. Column two adds the manager's age. Age is negatively correlated with growth, with a coefficient of -0.08 per year, suggesting that younger managers may be more ambitious, or perhaps that those who become managers at a younger age are especially talented. When age is added, the education coefficient drops by about 10 percent to 0.46. This indicates that education by itself is partly proxying for youth, but the bias is small. Column three adds management experience, which as expected has a positive effect on growth. The coefficient on education, however, remains unchanged at 0.51. Column four adds industry experience, which also has a positive effect on growth, and the education coefficient rises slightly to 0.53. Industry experience seems to matter more than management experience, although as cautioned above these regressors are censored and their coefficients are likely to be biased. Column five adds the number of prior occupations, which is also a strong predictor of growth in line with Lazear (2005), although this measure is also censored. The coefficient on education drops marginally to 0.49. Column six adds the manager's tenure at the firm, which is negatively correlated with growth, and again the coefficient on education is only minimally affected, at 0.50.

Column seven adds the log of the outside option. First, as expected, the coefficient on the outside option is positive and significant, which validates the assumption of a positive ρ_1. Second, the coefficient on manager schooling falls to 0.45 percentage points and also remains significant. This represents a lower bound on the true effect in this sample. Third, the bias-corrected coefficient α_1 equals 0.53 percentage points. As detailed above, this is obtained by adding the coefficient on the log outside option, multiplied by an estimate for the return to schooling in non-management, β_1, to the coefficient on manager education. I assume an estimate of 7 percent for the returns to schooling parameter β_1, but the results are not very sensitive to this choice. Assuming a value of 10 percent, in the upper end of the typical range in the literature, increases α_1 to 0.55 percentage points.

Finally, columns eight and nine add all measures of managerial ability and experience simultaneously, first excluding the outside option (column eight) and then including it (column nine). Age, industry experience and the number of prior occupations remain significant predictors of growth, while management experience and tenure become insignificant. Most importantly, the coefficient on education is still largely unaffected, equaling 0.47 in column eight, and 0.49 after the bias-correction in column nine.

The bottom panel presents results conditional on survival and reports very similar results. One difference is that conditional on survival age has a larger negative coefficient, which equals 0.15 per year in column two, and this lowers the education coefficient by about 20 percent relative to column one (versus 10 percent when accounting for exit). This suggests that younger managers may pursue riskier strategies, which lead to higher growth when successful.

Put together these results show that the education coefficient is remarkably stable when accounting for various other measures of managerial ability and experience. In addition, the coefficients in this cross-sectional specification are estimated quite precisely. This suggests that bias from omitted manager characteristics in the baseline estimates is unlikely to be a significant issue.

VI Evidence on Mechanisms

What do more educated managers do differently? This section offers evidence on possible mechanisms for the effect of manager education on firm growth. I focus on two hypotheses.

First, more educated managers may be more adaptable and adopt new technologies faster. This is an idea that originated in the early literature on human capital (Nelson and Phelps, 1966; Welch, 1970; Schultz, 1975). In line with this view, Huffman (1974) showed that more educated farmers in the U.S. adapt faster to changes in the optimum amount of fertilizer.

Second, higher ability managers may be better at coordinating other workers, or production inputs more broadly, and this ability might be enhanced by education. Better coordination in turn increases the manager's span of control and enables the firm to grow larger. This is also an old idea, going back to the work of Penrose (1959) and Chandler (1977). Bloom and Van Reenen (2007) and Bloom et al. (2013a) show that management practices such as target setting, monitoring and human resource management have a positive effect on firm productivity and profitability. Bloom et al. (2013b) find that these management practices are positively related with the share of managers with a college degree.

These two views are of course not mutually exclusive, and I find evidence that is consistent with both. I examine each one in turn.

VI.A Adaptability

If the effect operates through higher adaptability, then among college-educated managers those with more technical degrees should have a larger effect, since they are presumably better equipped to adopt new technologies. The data provide information on field of study for college graduates, and I use this information to classify college degrees into the following fields: humanities (including social science), business, science, engineering, health and other, where other includes degrees where the field is unknown. I then replace manager years of schooling with the share of managers with degrees in each field in the management changes specification in equation (2), the omitted category being the share of managers without a college degree. Columns one and two of table 6 report the findings accounting for exit and conditional on survival, respectively. In column one, the coefficients on the share of managers with degrees in science, engineering and health are the highest, ranging from 5.3 to 8.0 percentage points per year, followed by the share of managers with business degrees, with 3.75. The coefficient on the share of managers with degrees in humanities and social science is much smaller, at 1.2. Column two shows the same pattern. The stronger effect from science, engineering and health is consistent with the technology adoption hypothesis. The fact that business is also stronger is consistent with the coordination hypothesis, which I discuss below.

The evidence from college degrees is indirect, and I do not observe measures of technology adoption in the Quadros de Pessoal data. The Global Entrepreneurship Monitor (GEM), a survey of actual and potential entrepreneurs in over 50 countries, offers more direct evidence on the relationship between education and technology. The survey asks detailed questions on entrepreneurial activity and collects basic demographics, including education. The microdata are publicly available at http://www.gemconsortium.org.

The survey asks two questions from managers of existing businesses. First, "Do all, some, or none of your potential customers consider this product or service new and unfamiliar?". And second, "Have the technologies or procedures required for this product or service been available for less than a year, or between one to five years, or longer than five years?". I use data from the latest survey wave available, the 2011 GEM, and examine how the answers are related to the manager's education. The results are reported in table 7. All regressions include country fixed effects, and manager education is measured in years of schooling.

For the first question, I define the answer as 100 if the manager reports that at least some customers consider the product or service new, and 0 otherwise. Column one shows that managers are 0.9 percentage points more likely to give a positive answer per year of schooling. For the second question I define the answer as 100 if the manager reports that the technology has been around for five years or less, and 0 otherwise. As shown in column two, managers are 0.4 percentage points per year of schooling more likely to report using a new technology. Columns three and four add four-digit sector dummies. Both coefficients fall but remain positive and significant. Even within narrow sectors, more educated managers are more likely to report selling new products or services and using new technologies.

VI.B Coordination

One piece of evidence in favor of the coordination hypothesis was already presented above: the effect of manager education on growth is higher for managers with degrees in business than in humanities, social science and other non-technical fields. This is of course consistent with interpretations other than better coordination – managers with business degrees could be better at marketing, sales or strategy, for example.

A natural area to examine for further evidence is human resource management.[31] More educated managers may implement more effective incentive systems, and in particular may increase the use of incentive pay, which past research has found has a positive effect on

[31] See Bloom and Van Reenen (2011) and Lazear and Oyer (2012) for surveys of this field.

productivity (Lazear, 2000; Shearer, 2004). I test whether firms that switch to more educated managers increase the use of incentive pay. The Quadros de Pessoal data splits compensation into a base component and a variable component. The variable component includes profit sharing and bonuses for attendance and performance. On average, firms in the sample pay variable compensation to 13 percent of their workers. I estimate equation (2) using changes in the fraction of employees with incentive pay as the outcome. In line with this view, column three of table 6 shows that a year of manager education increases this fraction by 0.12 percentage points.

VII Aggregate Implications

In this last section I turn to the implications of the paper's findings for aggregate productivity. In a recent paper, Hsieh and Klenow (2014) find that firms in the U.S. exhibit much stronger employment growth than firms in Mexico and India, and show that when interpreted through the lens of a standard model of heterogeneous firms these differences in firm growth imply large differences in aggregate productivity.

The logic of their exercise is simple. In models of heterogeneous firms, the marginal revenue productivity of workers must decline with firm size, otherwise the most productive firm would employ all workers. This decline might be driven by downward sloping demand curves as in Melitz (2003) and in the model used by Hsieh and Klenow (2014), or by decreasing returns to scale in production as in Lucas (1978). Whatever drives the decline, more productive firms employ more workers in equilibrium, so that the marginal revenue productivity of workers is equalized across firms (absent any frictions). Faster employment growth therefore implies faster productivity growth.

I follow the same approach in order to evaluate the implications for aggregate productivity of differences in firm growth caused by differences in manager education. In order to discipline the exercise, I use the same model and parameter assumptions as Hsieh and Klenow (2014).[32]

[32]To be precise, I use the model from section IV in their paper, which in turn draws from Hsieh and Klenow (2009). In section V, Hsieh and Klenow (2014) extend their framework to incorporate endogenous entry, as well as endogenous productivity growth as a function of firm-level distortions to revenue and to capital and labor costs. Here I hold entry fixed and do not account for such distortions.

I provide here a brief summary of the model, please see their paper for additional details. Aggregate output is given by a CES aggregate of the output of individual firms

$$Y = \left[\sum_a \sum_{j=1}^{J_a} Y_{a,j}^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1}} \tag{9}$$

where $Y_{a,j}$ is the output of firm j at age a, J_a is the number of firms of age a and $\sigma > 1$ is the elasticity of substitution between varieties. Firms are monopolistic competitors choosing labor and capital to maximize profits given by

$$\pi_{a,j} = P_{a,j}Y_{a,j} - wL_{a,j} - rK_{a,j} \tag{10}$$

where $P_{a,j}$ is the price of the firm's output, $L_{a,j}$ and $K_{a,j}$ are its employment and capital stock, and w and r the wage and cost of capital. Firm output takes the following Cobb-Douglas form

$$Y_{a,j} = A_{a,j} K_{a,j}^{\alpha} L_{a,j}^{1-\alpha} \tag{11}$$

where $A_{a,j}$ is the firm's productivity. As Hsieh and Klenow (2014) show, in equilibrium firm employment is increasing in firm productivity

$$L_{a,j} \propto A_{a,j}^{\sigma-1} \tag{12}$$

and aggregate productivity is given by

$$TFP = \left[\sum_a \sum_{j=1}^{J_a} A_{a,j}^{\sigma-1} \right]^{\left(\frac{1}{\sigma-1}\right)} \tag{13}$$

Equations (12) and (13) are the key elements of the model for my calibration exercise. Equation (12) translates firm employment into productivity, and equation (13) combines firm-level productivities into aggregate TFP. Following Hsieh and Klenow (2014), I assume $\sigma = 3$.[33]

[33] As in Hsieh and Klenow (2014), the results are sensitive to the choice of σ. This is the key parameter that governs how fast the price of a firm's product declines as the firm expands, and therefore how much its marginal revenue productivity declines. A larger σ implies varieties are more substitutable, and that

I use the model to calculate the effect on aggregate productivity of moving from the distribution of manager education in Portugal to that of the U.S. To measure the two distributions consistently I use data from the 2011 GEM, the global survey of entrepreneurs described in section VI. The GEM reports the education of managers in five levels, labeled "None", "Some secondary", "Secondary degree", "Post-secondary" and "Graduate experience". Table 8 shows the distribution of reported levels for both countries. I assign these levels 6, 9, 12, 16 and 18 years of schooling, respectively.[34] This implies average manager years of schooling of 10.5 for Portugal, which compares with a value of 10.1 in 2009 in the Quadros de Pessoal data, and 14.2 in the U.S.

I use the effect of manager education on firm growth estimated in the microdata from section IV to construct employment and survival histories for firms with each of the five levels of manager education reported in the GEM. I start by regressing initial log firm employment on manager education and the same set of controls used in the growth regressions. I use the resulting coefficient on manager education, which equals 0.033, to calculate initial firm sizes for each level of manager education, relative to the average size and manager education of entrants in the sample. The estimated effect of manager education on annual firm growth conditional on survival ranges between 0.3 and 0.5 in table 3, and I pick a conservative value of 0.3 for this exercise. I use it to calculate annual firm growth rates at every firm age up to 100 years old, relative to average growth and manager education in the sample at the corresponding firm age. This enables me to calculate firm employment at every age for each manager education level, conditional on survival. Finally, I regress firm survival on manager education and the set of controls used in the growth regressions. The effect is positive but

marginal productivity declines less as the firm expands. This in turn implies that the same difference in observed firm size implies a smaller difference in $A_{a,j}$, the firm's physical productivity. The opposite holds if σ is lower. For example $\sigma = 5$ would lower the effect on aggregate productivity reported below to 10 percent, whereas $\sigma = 2$ would increase it to 46 percent.

[34] "None" presumably includes any education below the secondary level, namely primary school. Consistent with this interpretation, it comprises 23 percent of managers in Portugal in the GEM data, which is similar to the fraction of managers with six years of schooling or less in the Quadros de Pessoal data (in the U.S. it comprises only 3 percent). I therefore assign it 6 years of schooling, which is also consistent with UNESCO's definition for ISCED level 1 in both countries. I define "Some secondary" as 9 years of schooling, in line with the definition for ISCED level 2 in both countries. "Secondary degree" corresponds unambiguously to 12 years of schooling in both countries. I define "Post-secondary" as a first higher education degree. In Portugal this most commonly refers 3 or 5-year degrees. In the U.S. it most often corresponds to a 4-year bachelor's degree. I assign a value of 16 years to both cases. "Graduate experience" presumably includes master's, professional degrees and PhDs. I assume a conservative average duration of 2 years for these degrees.

small and statistically insignificant (0.039 percentage points per year of schooling). I use this point estimate to construct survival probabilities at each age for each manager education level, using the same procedure as for firm growth.

Combined with the distribution of manager education from the GEM,[35] this procedure yields a distribution of firm employment. I then use equation (12) to translate each firm's employment into its (relative) productivity, and equation (13) to aggregate all firms and calculate aggregate productivity. Doing this for both countries, I find that moving from the distribution of manager education in Portugal to that of the U.S. would increase aggregate productivity by 21 percent. Allowing for endogenous capital accumulation in response to the higher productivity level, the elasticity of output per capita with respect to TFP would equal $\frac{1}{1-\alpha}$. Assuming $\alpha = 1/3$, output per capita would therefore rise by 33 percent ($1.21^{3/2} = 1.33$), or about one third of the difference in output between the two countries.

There is one important reason to believe that these estimates are conservative, which is that they ignore the effect of changes in aggregate manager education on firm entry. There is increasing evidence that firms in more developed countries are on average larger (Bloom, Sadun and Van Reenen, 2012; Bento and Restuccia, 2014), and in particular that a left tail of small, unproductive firms tends to disappear with development. It is possible that as manager education increases and firms become more competitive, less productive potential entrepreneurs find it more profitable to become workers in more productive firms instead, and that this also has a positive effect on aggregate productivity. Exploring this possibility is left for future research.

VIII Conclusion

This paper shows that life cycle firm growth increases strongly with manager education, in line with the key prediction in Lucas (1978), the canonical model of managerial skill and firm performance. I find a consistent pattern exploiting both cross-sectional and within-firm variation in manager education. The coefficient on education is stable when accounting for

[35] I assume the distribution of entrants equals the distribution of manager education in the GEM, which covers all firms and not just entrants. This matters little for the results though, since I use the same procedure for both countries and because the effect of manager education on survival is small.

other measures of managerial ability and experience, which suggests that the effect is driven by education itself, rather than other manager characteristics correlated with education.

These findings point to an important role for managerial human capital in understanding firm growth, and suggest three important implications. First, the role of education in accounting for income differences across countries may be understated in the development accounting literature (e.g. Caselli, 2005). In the last section of the paper I show that when interpreted through the lens of a standard model of heterogeneous firms, such as the one in Hsieh and Klenow (2014), observed differences in manager education can account for substantial differences in aggregate productivity. In my calibration I hold entry fixed, but it is likely that increases in manager education also dissuade less productive firms from entering in the first place. Accounting for such selection effects could strengthen these findings.

Second, as argued by Murphy, Shleifer and Vishny (1991) and Schoar (2010), attracting people with high human capital intro entrepreneurship seems to be key to fostering the kind of entrepreneurship that leads to economic growth. In fact, if more educated managers adopt new technologies faster as my findings suggest, there could be crucial spillover effects on the technology of other firms as well, as in Murphy, Shleifer and Vishny (1991)'s model, which would amplify the effects of educated managers even further. Such spillover effects are an important topic for future research. In addition, accounting for school quality and for levels of schooling above college, namely masters and PhDs, would be valuable contributions to this line of research.

Third, Caselli and Gennaioli (2013) show that an inefficient allocation of managerial talent across firms may hurt aggregate productivity. Facilitating the allocation of highly educated managers to firms with strong growth prospects, for example through improvements in contract enforcement or the development of financial markets, could also have important implications for growth.

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Table 1: Summary Statistics

	Mean	SD	P10	P50	P90
Firm Growth (%)	1.37	31.27	-33.33	0.00	33.33
Number of Workers	13.82	84.58	2.00	5.00	22.00
Number of Managers	1.52	1.67	1.00	1.00	2.00
Manager Education	8.69	4.33	4.00	9.00	17.00
Non-Manager Education	7.56	3.03	4.00	6.96	12.00
Manager Age	44.92	10.32	32.00	44.50	59.00
Non-Manager Age	35.74	8.50	25.25	35.00	46.94
Firm Age	12.39	12.35	2.00	9.00	27.00

Notes: This table presents summary statistics for the main variables used in the analysis. Firm growth is the annual growth rate in employment, conditional on survival, winsorized at the 99^{th} percentile. Manager education is the average years schooling of managers at the start of the year. Non-manager education, manager and non-manager age are defined analogously. The number of workers includes all firm workers regardless of employment status, including unpaid workers. Managers are defined in section II of the text. Firm age is based on the firm's reported year of incorporation.

Table 2: Firm Size Variation Accounted for by Manager and Non-Manager Education

	R^2 From Univariate Regressions of Log Firm Size		
Firm Age Bin	Manager Education	Non-Manager Education	Observations
0	0.000	0.015	5113
1 to 5	0.001	0.007	39559
6 to 10	0.007	0.000	42095
11 to 15	0.013	0.000	24997
16 to 20	0.037	0.002	20003
21 to 25	0.063	0.003	12895
26 to 30	0.082	0.003	7899
31 to 35	0.121	0.007	4758
36 to 40	0.137	0.002	2527
41 to 45	0.203	0.026	2081
46 to 50	0.175	0.020	1069
51 and over	0.217	0.016	3074

Notes: This table reports the R^2 from unvariate regressions of log firm size on manager education in the 2009 cross-section, by firm age bins (second column) and the R^2 from the corresponding regressions of log firm size on non-manager education (third column). The fourth column reports the number of observations in the regressions in each age bin.

Table 3: Effect of Manager Education on Firm Growth

	Growth = -100% if Firm Exits					
	Baseline	3-year Δ	By level	Owner Ed.	Avg. Ed.	Revenue
	(1)	(2)	(3)	(4)	(5)	(6)
Manager Education	0.409***	0.387***		0.498***	0.656***	0.664***
	(0.120)	(0.099)		(0.171)	(0.070)	(0.247)
Owner Education				-0.044		
				(0.080)		
Relative to [0,6):						
[6,9)			2.199*			
			(1.170)			
[9,12)			2.624**			
			(1.291)			
[12,15)			2.733**			
			(1.365)			
15+			6.466***			
			(1.558)			
Observations	659457	659457	659457	480515	755032	489542
Number of Firms	206215	206215	206215	166058	221345	177900

	Conditional on Survival					
	(1)	(2)	(3)	(4)	(5)	(6)
Manager Education	0.446***	0.302***		0.497***	0.499***	0.910***
	(0.111)	(0.082)		(0.155)	(0.065)	(0.267)
Owner Education				0.053		
				(0.072)		
Relative to [0,6):						
[6,9)			2.576**			
			(1.040)			
[9,12)			3.225***			
			(1.169)			
[12,15)			3.005**			
			(1.225)			
15+			6.577***			
			(1.444)			
Observations	620929	620929	620929	452328	713402	454349
Number of Firms	192406	192406	192406	155020	207219	157594

Notes: This table reports regressions of changes in annual firm growth, measured by employment, on changes in average manager years of schooling. The top panel presents results accounting for exit by assigning a growth rate of -100 percent when a firm exits, while the bottom panel accounts conditions on firm survival. All specifications include controls for quartics in average non-manager education, average manager and non-manager age, log firm size and log number of managers before management changes, as well as firm age, sector and year fixed effects. Errors are clustered at the firm level. The sample is restricted to firm-years where manager education does not change between the start and end of the year, to minimize measurement error, except in Column (5) which uses the average of start and year-end values. In Column (6) the dependent variable is revenue growth.

Table 4: Relative Firm Size: College vs Primary-School Management

Firm Age Bin	Simulation	Data	Percent Explained
1 to 5	1.22	1.26	97
6 to 10	1.48	1.59	93
11 to 15	1.80	1.81	99
16 to 20	2.20	2.57	86
21 to 25	2.63	2.72	97
26 to 30	3.24	2.88	113
31 to 35	3.88	4.00	97
36 to 40	4.74	5.76	82
41 to 45	5.71	7.89	72
46 to 50	7.04	7.25	97

Notes: This table compares simulated firm sizes using the estimated effect of manager education on firm growth to observed differences in firm size. The effect of manager education on annual growth is set at 0.3 percentage points per year of schooling. I simulate firm size over the life cycle for two firms, one with college-educated managers (17 years of schooling) and another with primary-school-educated managers (4 years of schooling). These two firms are chosen for comparison with the top and bottom groups of manager education in figure 1a, which have an average of 16.7 and 4.1 years of manager schooling respectively. The simulation column presents the ratio of simulated sizes (college/primary-school) in different age bins. The age used for the simulation in each bin is the average age for firms in the top and bottom groups in figure 1a in that age bin. The data column presents the same ratio in the data, comparing the top and bottom groups in figure 1a.

Table 5: Accounting for Other Manager Characteristics

	Growth = -100% if Firm Exits								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Education	0.51***	0.46***	0.51***	0.53***	0.49***	0.50***	0.45***	0.47***	0.39***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Age		-0.08***						-0.09***	-0.11***
		(0.02)						(0.02)	(0.02)
Management Experience			0.26**					-0.15	-0.14
			(0.11)					(0.13)	(0.13)
Industry Experience				0.75***				0.74***	0.75***
				(0.07)				(0.07)	(0.07)
Prior Occupations					0.73***			0.43***	0.31*
					(0.14)			(0.17)	(0.17)
Tenure at Firm						-0.09***		-0.01	-0.01
						(0.03)		(0.03)	(0.03)
Log Outside Option							1.07***		1.30***
							(0.22)		(0.22)
Implied α_1							0.53***		0.49***
($\beta_1 = 7\%$)							0.04		0.04
Observations	476432	476432	476432	476432	476432	476432	476432	476432	476432
Number of Firms	132085	132085	132085	132085	132085	132085	132085	132085	132085
	Conditional on Survival								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Education	0.39***	0.31***	0.39***	0.40***	0.38***	0.37***	0.33***	0.30***	0.20***
	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
Age		-0.15***						-0.15***	-0.18***
		(0.01)						(0.01)	(0.01)
Management Experience			0.09					-0.07	-0.05
			(0.10)					(0.11)	(0.11)
Industry Experience				0.30***				0.27***	0.28***
				(0.06)				(0.06)	(0.06)
Prior Occupations					0.63***			0.54***	0.39***
					(0.13)			(0.15)	(0.15)
Tenure at Firm						-0.13***		-0.03	-0.03
						(0.02)		(0.02)	(0.02)
Log Outside Option							1.28***		1.71***
							(0.19)		(0.19)
Implied α_1							0.42***		0.32***
($\beta_1 = 7\%$)							0.03		0.04
Observations	448053	448053	448053	448053	448053	448053	448053	448053	448053
Number of Firms	121778	121778	121778	121778	121778	121778	121778	121778	121778

Notes: This table presents regressions of annual firm growth on average manager years of schooling and other manager characteristics. The manager's outside option is measured by log wages in a previous job as a non-manager. The implied α_1 is the bias-corrected coefficient on manager education, assuming a labor market return to schooling of 7%. It is obtained by adding the coefficient on the outside option multiplied by the return to schooling to the coefficient on manager education. See main text for details. All regressions include firm age $\times$ sector $\times$ municipality $\times$ year fixed effects. Standard errors are clustered at the firm level.

Table 6: Evidence on Mechanisms

	(1) Firm Growth	(2) Firm Growth	(3) Incentive Pay
Manager Education			0.121*
			(0.069)
College Degree by Field			
Humanities	1.167	0.949	
	(1.695)	(1.616)	
Business	3.750**	3.731**	
	(1.628)	(1.510)	
Science	7.681**	5.397*	
	(2.990)	(2.850)	
Engineering	5.301***	4.439***	
	(1.673)	(1.549)	
Health	7.979***	6.601***	
	(1.718)	(1.577)	
Other	3.203*	3.279**	
	(1.759)	(1.658)	
Observations	662175	623448	618180
Number of Firms	207030	193132	194206
Accounting for Exit	Y		
Conditional on Survival		Y	

Notes: This table presents evidence on mechanisms driving the effect of changes in manager education on changes in firm growth. The first and second columns replace manager years of schooling in column (1) of table 3 with the share of college educated managers by field of study. The omitted category is the share of managers without a college degree. Column three examines the effect of changes in manager education on changes in the fraction of workers that receive incentive pay. All regressions include the set of firm-level controls from table 3. Standard errors are clustered at the firm level.

Table 7: Technology Adoption: Evidence from the Global Entrepreneurship Monitor

	(1)	(2)	(3)	(4)
	Product	Technology	Product	Technology
Manager Education	0.902***	0.400***	0.555***	0.254***
	(0.090)	(0.084)	(0.098)	(0.094)
Observations	17140	16301	16809	15995
Country Fixed Effects	Y	Y	Y	Y
Sector Fixed Effects			Y	Y

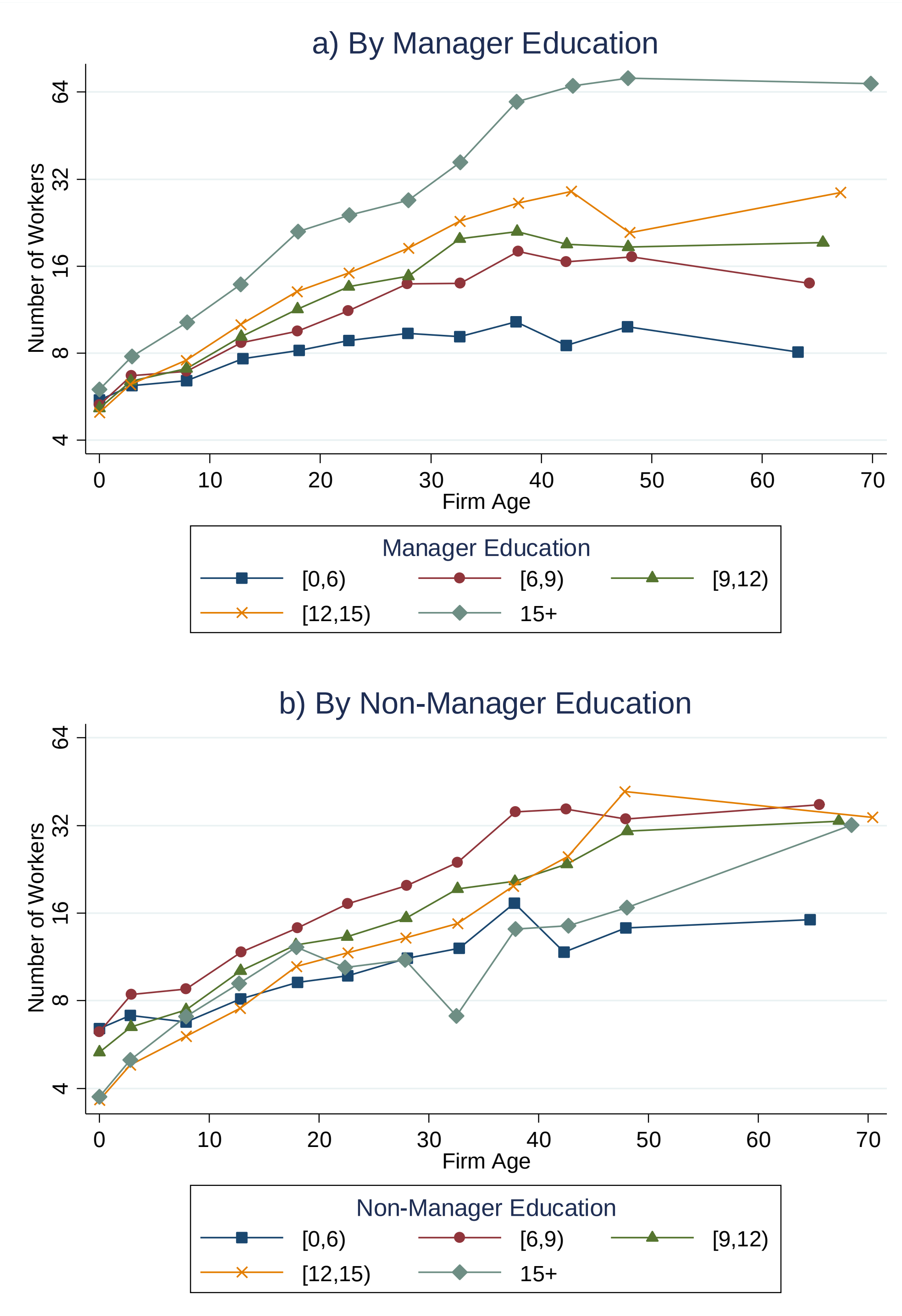
Notes: This table presents regressions of technology adoption on manager education using data from the Global Entrepreneurship Monitor (GEM) 2011 survey. Manager education is measured as years of schooling. The outcome variable in columns one and three equals 100 if the manager reports that at least some of the firm's customers consider its products or services new and unfamiliar, and 0 otherwise. The outcome variable in columns two and four equals 100 if the manager reports that the technologies or procedures required for the firm's products only became available in the last five years, and 0 otherwise. All regressions include country fixed effects, and columns three and four include 4-digit sector fixed effects.

Table 8: Distribution of Manager Education in the GEM

Level	Portugal	United States
None	23%	3%
Some secondary	45%	11%
Secondary degree	2%	27%
Post-secondary	30%	43%
Graduate experience	0%	17%

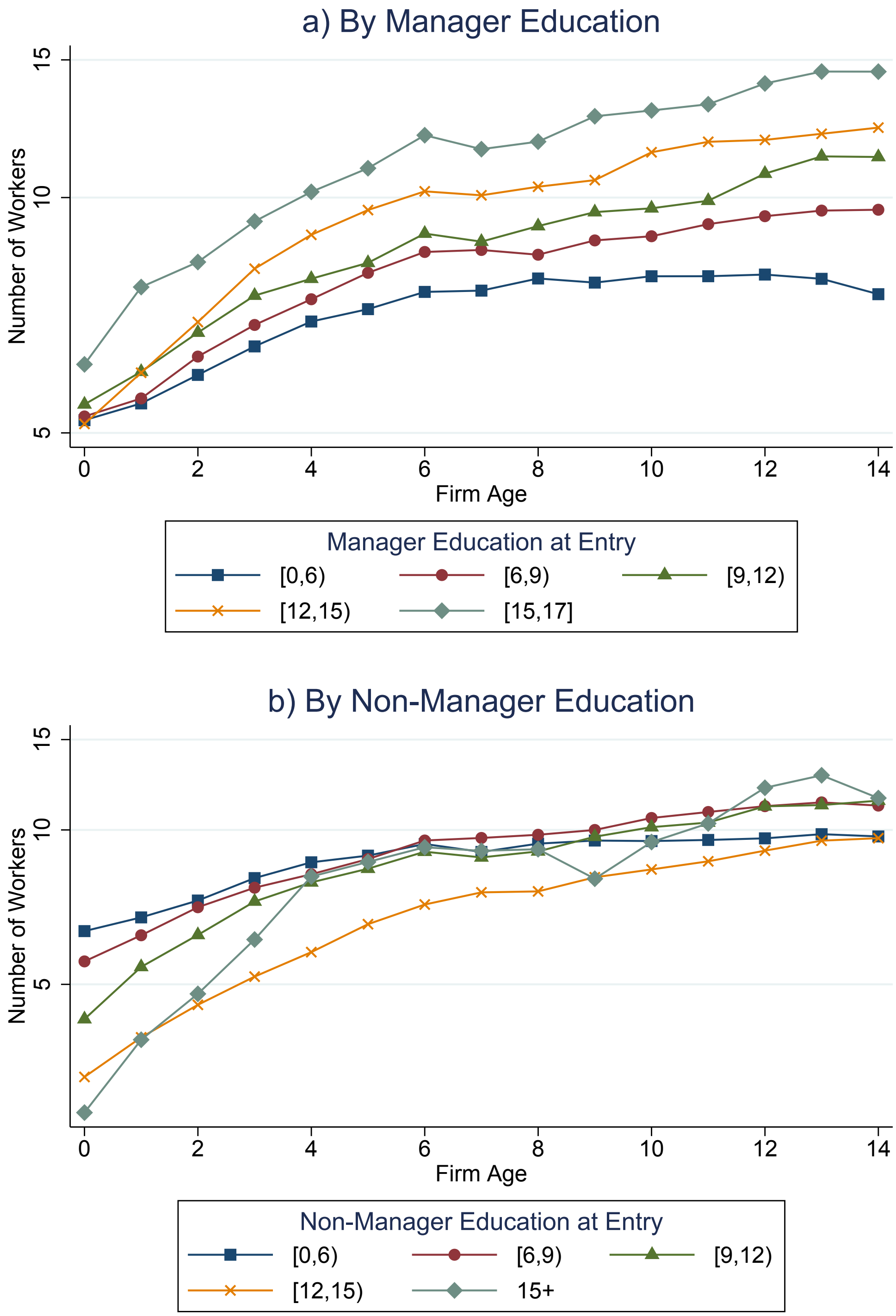
Notes: This table presents the distribution of manager education in Portugal and in the U.S. according to the five levels reported in the Global Entrepreneurship Monitor (GEM)

Figure 1: Firm Size by Age, 2009 Cross-Section



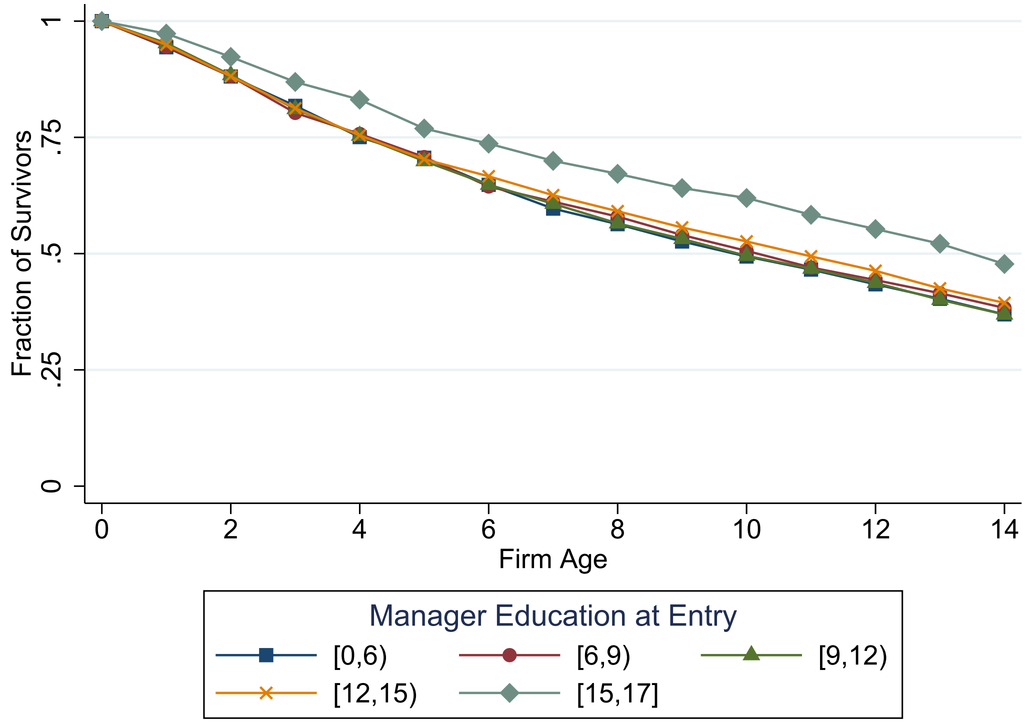
Notes: These figures plot average firm size (number of workers) by firm age bins in the 2009 cross section. The first bin includes entrants and the last includes firms over 50. The remaining firms are grouped into 5-year bins. Firm size is winsorized at the 99^{th} percentile within each age bin, The top graph sorts firms into five groups by average manager years of schooling, while the bottom graph sorts firms by average non-manager education instead.

Figure 2: Firm Size by Age, 1995 Cohort



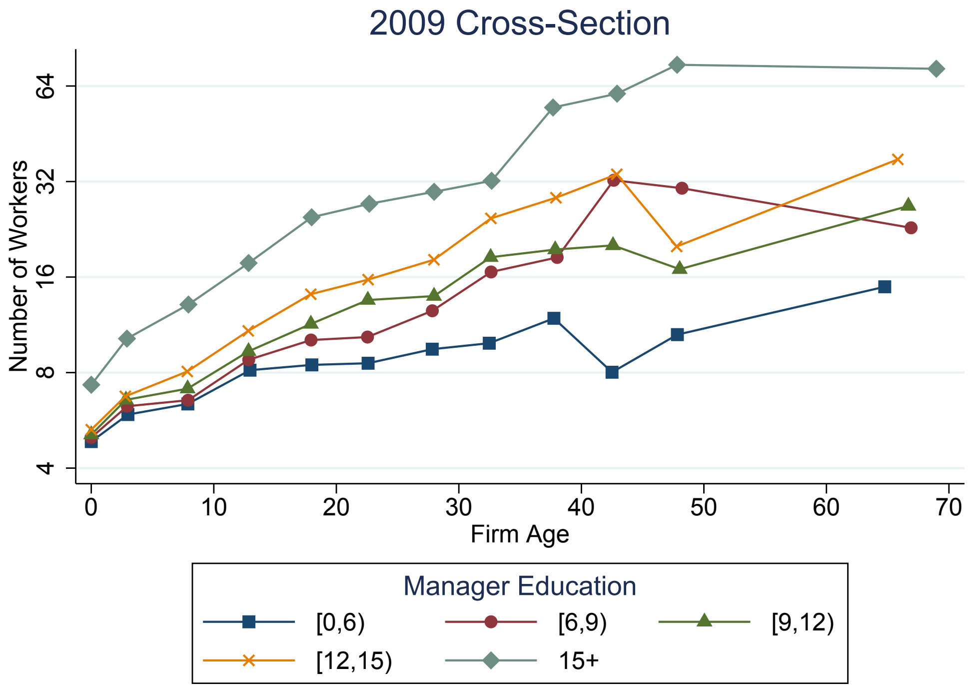
Notes: These figures plot average firm size (number of workers) by firm age for the 1995 cohort. Firm size is winsorized at the 99^{th} percentile within each age bin. The top graph sorts firms into five groups by average manager years of schooling at entry, while the bottom graph sorts firms by average non-manager education at entry instead.

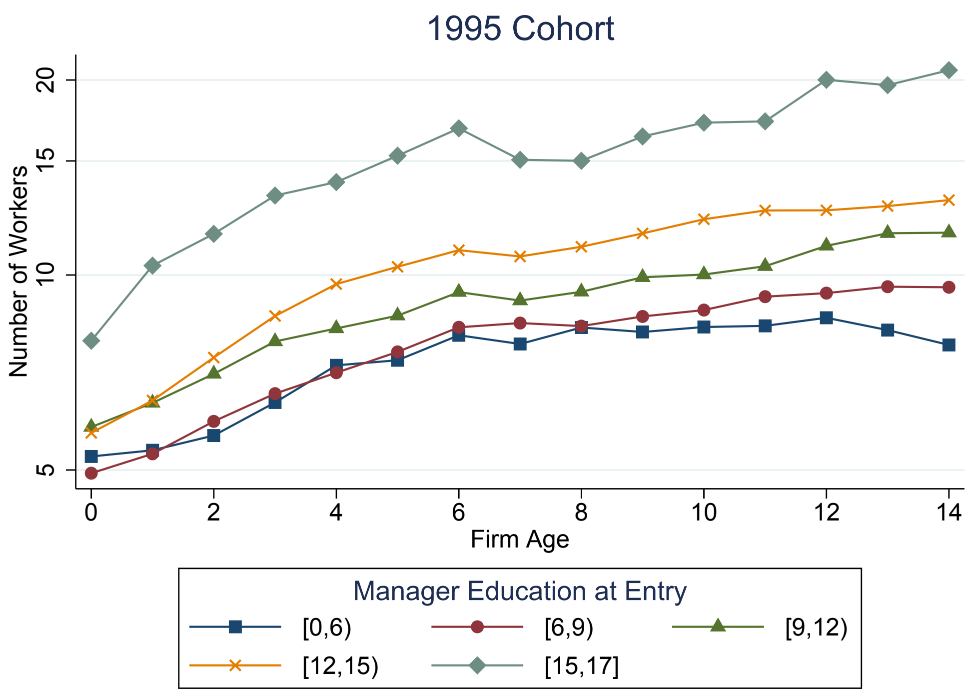
Figure 3: Firm Survival, 1995 Cohort



Notes: This figure plots survival rates for the 1995 cohort. Firms are sorted into five groups by average manager years of schooling.

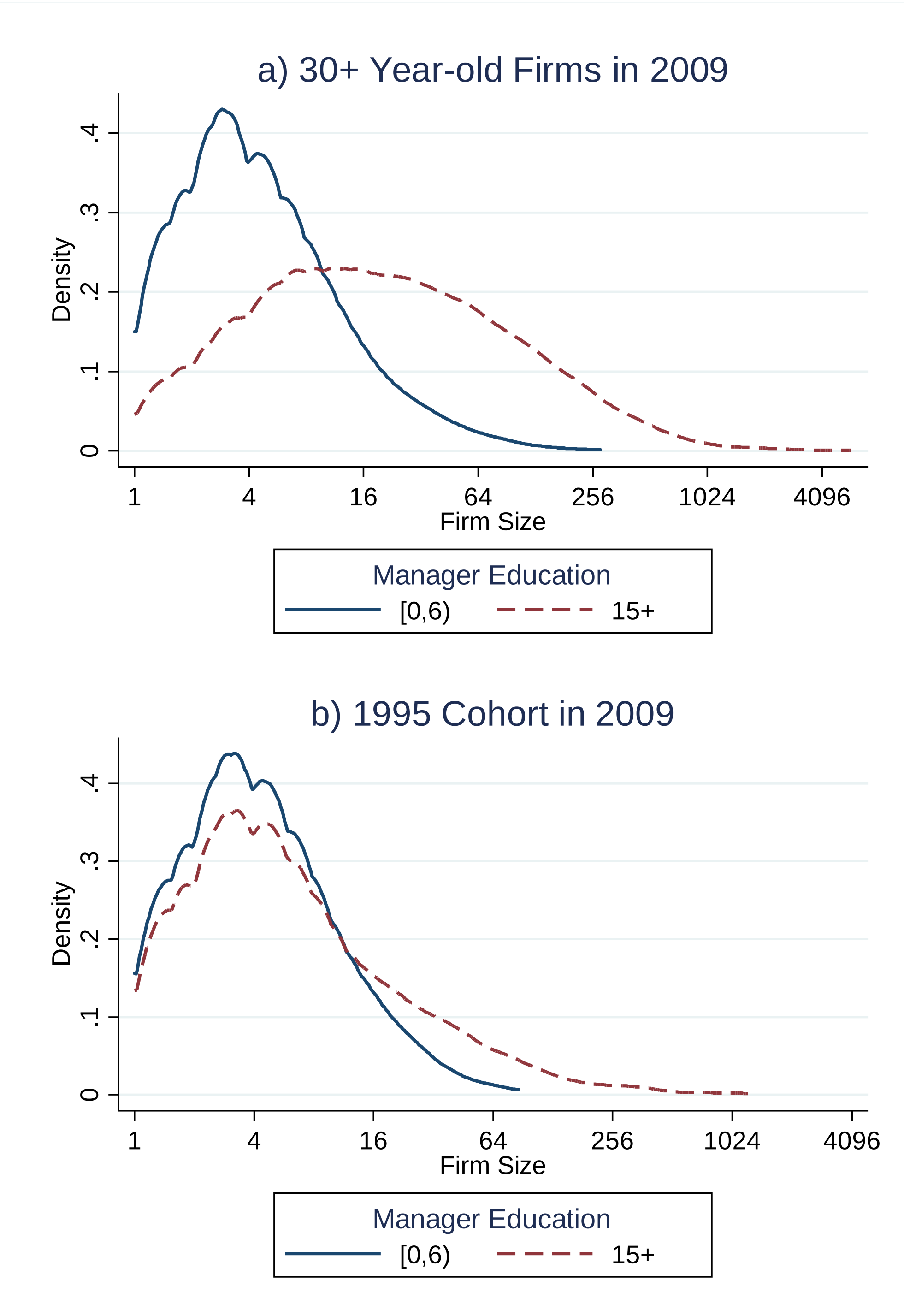
Figure 4: Firm Size by Age, Reweighted by Sector





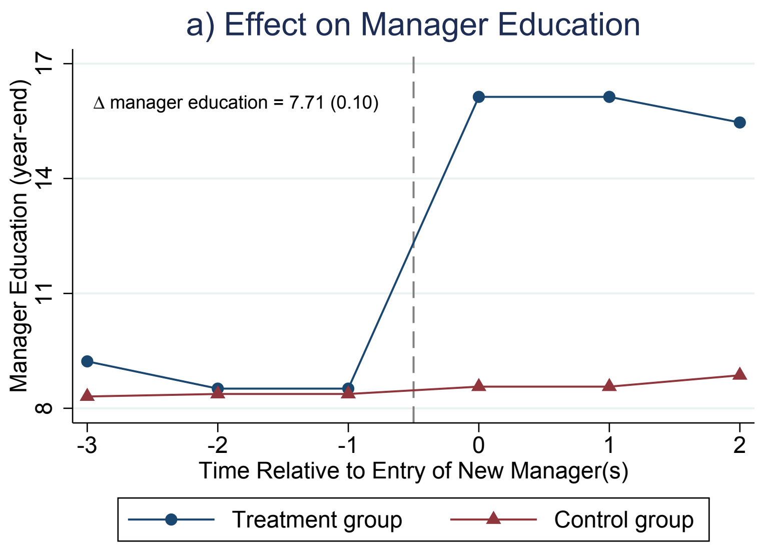
Notes: These figures plot average firm size (number of workers) by firm age bins in the 2009 cross section and the 1995 cohort. Firm size is winsorized at the 99^{th} percentile within each age bin. Firms are sorted into five groups by average manager years of schooling, and within each group they are reweighted so that the distribution of sectors within each group matches the overall distribution across groups.

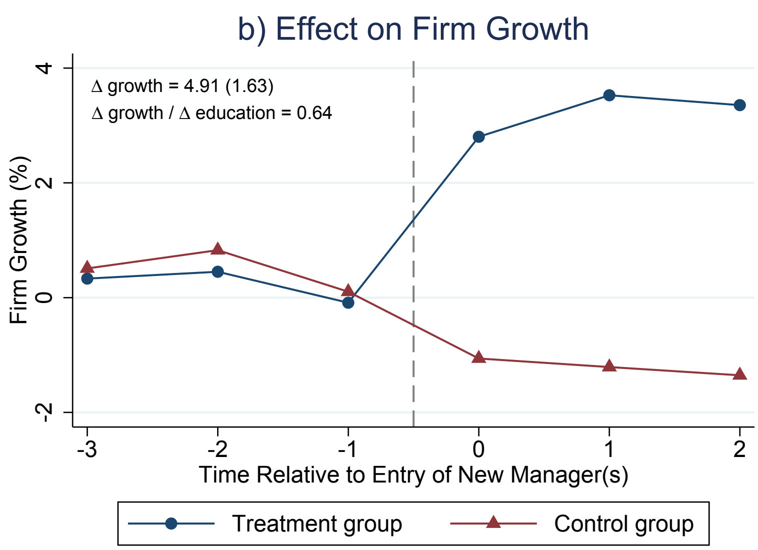
Figure 5: Firm Size Distribution



Notes: These figures plot kernel density estimates of the firm size distribution for firms with different levels of average manager education. The solid line is the distribution for firms with managers that have between 0 and 6 years of schooling, and the dashed line is the distribution for firms with managers with 15 or more years of schooling. The top figure plots these two distributions for firms over 30 years old in the 2009 cross-section, with manager education measured contemporaneously (i.e. in 2009). The bottom one plots them for firms from the 1995 cohort in 2009, with manager education measured at entry.

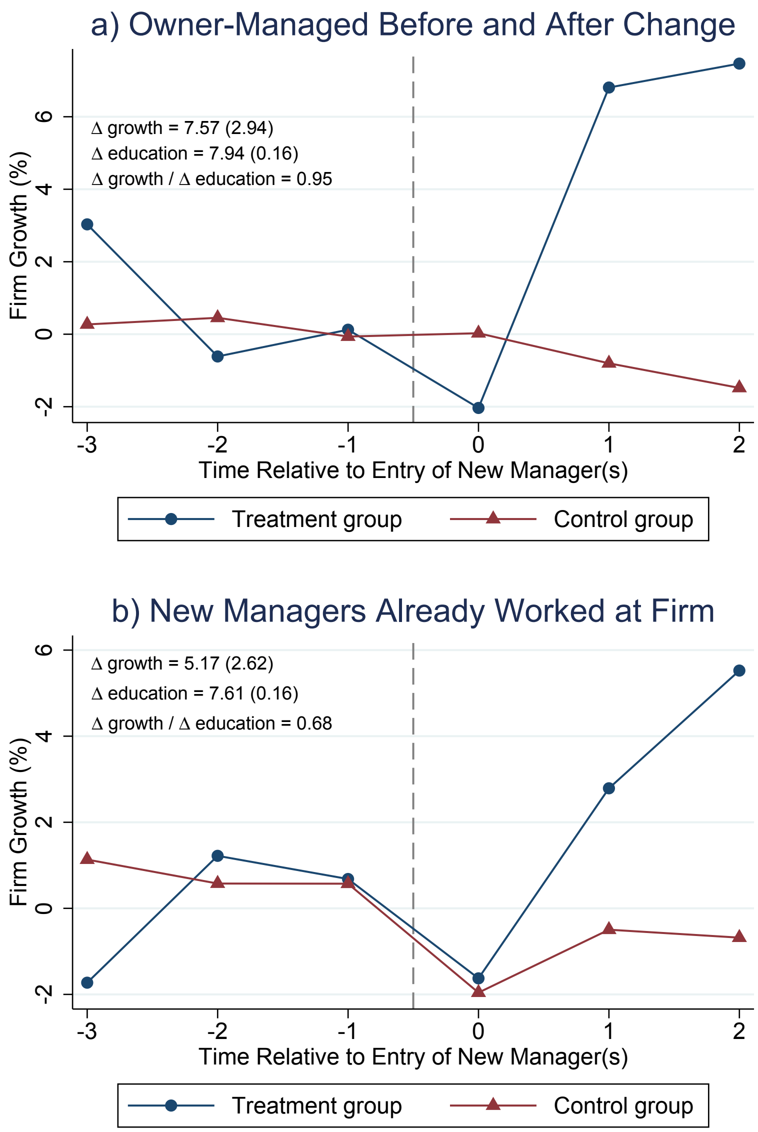
Figure 6: Event Studies of Manager Changes





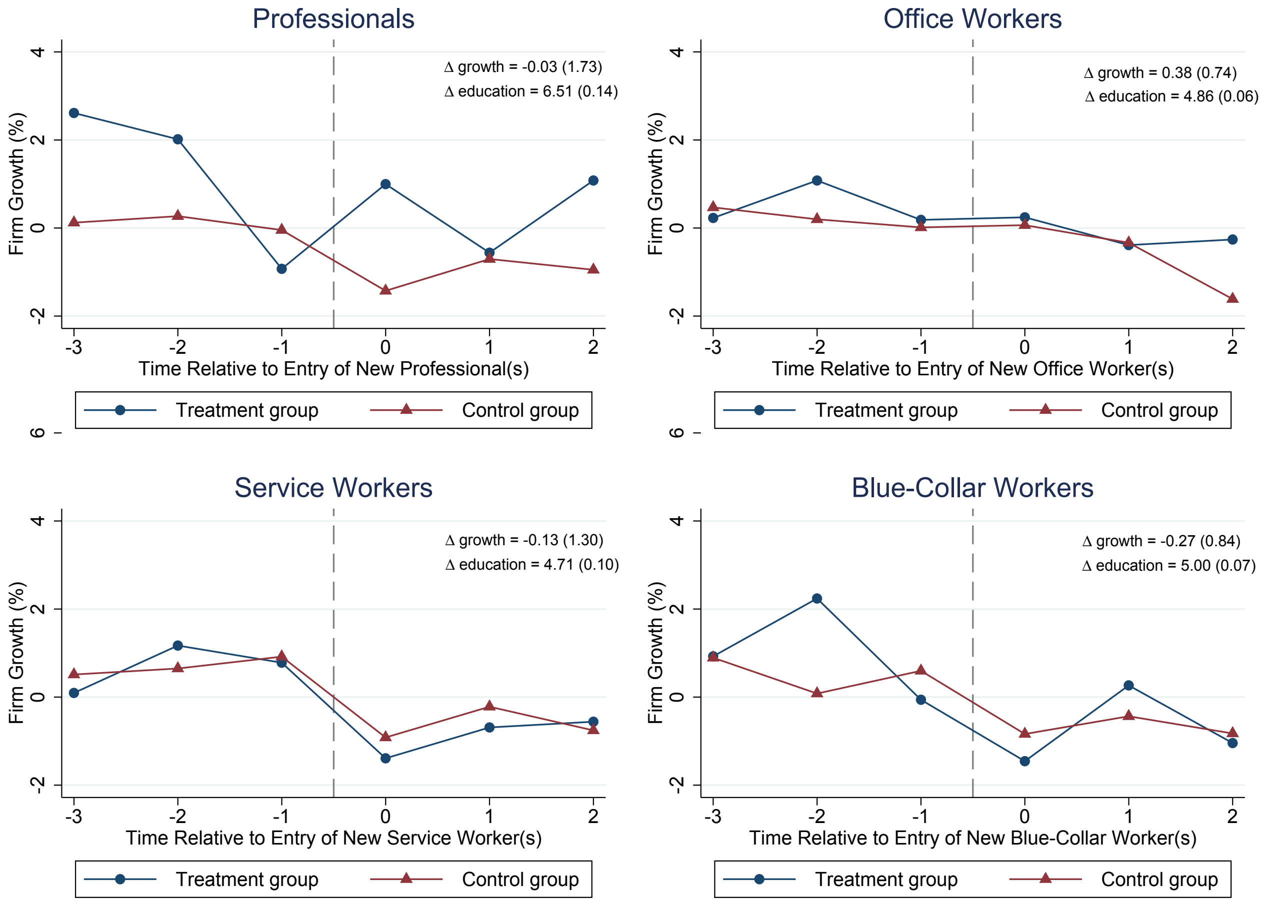
Notes: These figures plot event studies of manager changes. An event is defined as a change in manager education holding the number of managers constant. t=0 denotes the event year and the event window is defined as the three years before and after the event. I further require that manager education be constant for at least one year before and one year after the change, and that the firm be present in the sample in years $t = -3$ and $t = -2$. When a firm exits at $t = 2$ I assign it a growth rate of -100 percent in that year. If a firm experiences multiple events, I include each event and the corresponding three-year window, regardless of any possible overlap across event windows. The treatment group is defined as firms that hire college-educated managers and the control group as firms that hire managers with an average of 12 years of schooling or less. The top graph plots the effect of treatment on manager education, and is constructed by regressing manager education at each moment in event time on an indicator for treatment and quartics for pretreatment manager age, non-manager education and age, log firm size and log number of managers, as well as sector, year and firm age fixed effects. The bottom graph plots the effect on firm growth, and is constructed analogously, adding a quartic in manager education to the set of pretreatment controls. All controls are measured at the end of year t=-1. In both graphs, I plot the average outcome for treatment and control groups such that at each moment the difference between the two equals the coefficient on treatment and the weighted average of the two groups equals the sample average. I also report the coefficient on treatment from regressions of the change in manager education (top graph) and the change in firm growth (bottom graph) on an indicator for treatment and the set of pretreatment controls (standard errors in parenthesis). The changes are measured by averaging the pre and post treatment outcomes, excluding t=0, and taking the difference.

Figure 7: Event Studies of Manager Changes - Robustness



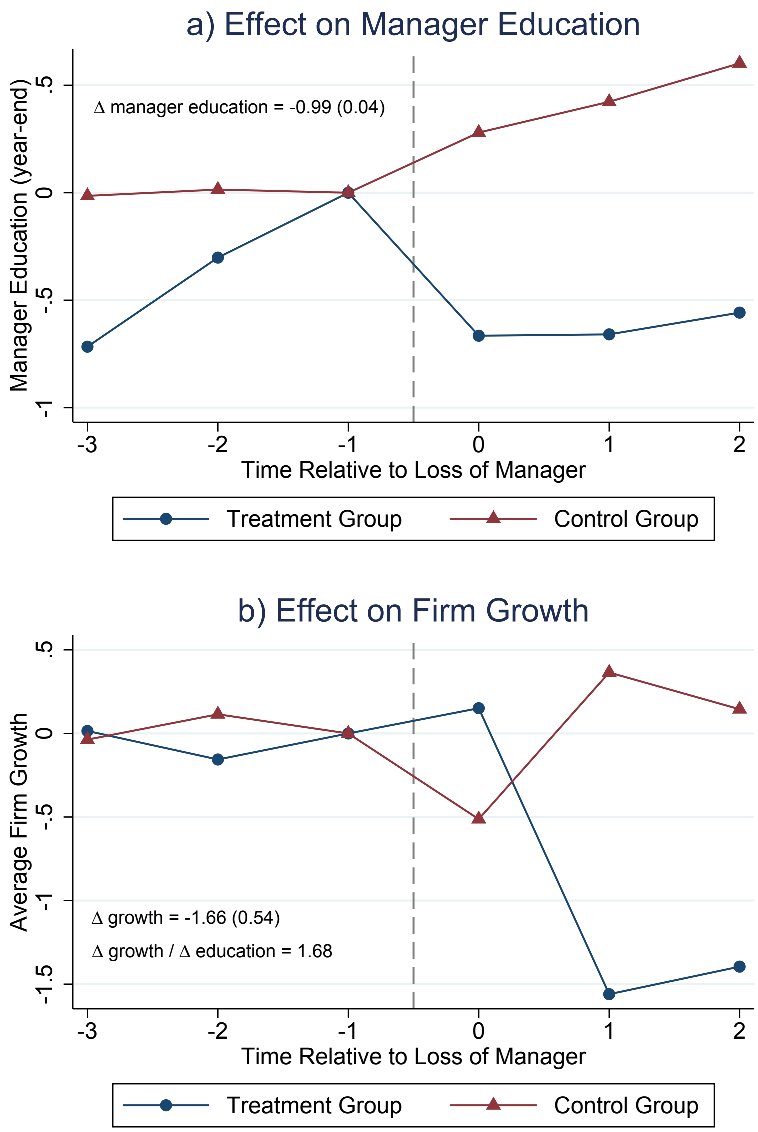
Notes: These figure plots event studies of manager changes, implementing the same design as in figure 6. The top panel restricts the sample to firms that were owner-managed both before and after the manager changes. The bottom panel restricts it to manager changes where the new managers had already been working at the firm for at least three years.

Figure 8: Event Studies of Non-Manager Changes



Notes: These figures plot event studies of non-manager changes. I split non-managers into four occupation groups using the standardized occupational codes in CNP 94 – professionals, office workers, service workers and blue-collar workers. Each graph plots the effect of treatment on firm growth from event studies for the corresponding group, replicating the methodology used for managers (see notes to figure 6) and adapting the set of pretreatment controls accordingly (e.g. in the graph for professionals, manager and non-manager education and age are replaced with professional and non-professional education and age). For professionals I use the same thresholds to define treatment and control groups as for managers: college vs. 12th grade or less. For the other groups I adjust them downwards to reflect the level of education in each group. For office and service workers I use 12th grade or more vs. 9th grade or less, and for blue collar workers 9th grade or more vs. 6th grade or less.

Figure 9: Event Studies of Permanent Manager Exits



Notes: These figures plot event studies of permanent manager exits, using the methodology described in figure 6. The treatment group is defined as firms that lose a college-educated manager and the control group as firms that lose a manager with 12 years of schooling or less.