# How Substitutable Are Workers? Evidence from Worker Deaths

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**Abstract**

The substitutability of workers within a firm, and between incumbent workers and outsiders, matters directly for understanding the sources of labor market frictions and the operation of internal labor markets. To assess the substitutability of workers, I estimate how exogenous exits of workers affect a firm's demand for its remaining incumbent workers. Using matched employer-employee data based on the universe of German social security records, I analyze the effects of 34,000 unexpected deaths of workers and show that such worker exits on average raise the remaining workers' wages and retention probabilities for several years. The findings are difficult to reconcile with frictionless labor markets and perfect substitutability between incumbent workers and outsiders. The average effect masks substantial heterogeneity: positive effects of a worker exit on incumbent worker wages are concentrated among coworkers in the same occupation as the deceased; coworkers in other occupations instead experience wage decreases when a high-skilled worker or manager dies. Coworkers in the same occupation thus appear to be substitutes, while high-skill workers and managers appear to be complements to workers in other occupations. Finally, incumbents' wages respond more and external hiring responds less to a worker death when the external labor market in the deceased's occupation is thin. This suggests that workers are harder to replace when their human capital is more firm-specific.

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# 1 Introduction

How competitive are labor markets? The answer to this question depends on the ease with which the two sides of the market can switch trading partners: workers finding alternative employment suitable for their skills and firms finding perfect substitutes for their current workers. An extensive empirical literature sheds light on the workers' perspective and finds that workers who are displaced from their job suffer persistent earnings losses—consistent with Becker's (1962) idea that human capital has firm-specific components.[1] However, much less is known about the other side of the market: firms' ability to find substitutes for their workers, in particular ones with specific human capital. When a worker leaves a firm, how easily can the firm replace the worker externally or substitute the worker's labor with that of other workers inside the firm?

I offer an empirical answer to this question that is based on a simple idea: the effect of exogenous worker exits on the firm's demand for the labor of the remaining workers identifies the substitutability of workers within and across firm boundaries. I illustrate the underlying intuition in a simple conceptual framework and demonstrate how different assumptions about worker substitutability change the predictions for the sign and magnitude of the effects. The competitive labor market model, which assumes that workers that can be hired externally are perfect substitutes for incumbent workers, predicts that the effect of worker exits on the firm's labor demand for the remaining insiders is zero: in response to any worker exit, the firm can simply hire a suitable new worker so that its demand for the labor of the remaining incumbent workers remains unchanged. In contrast, when outsiders are only imperfect substitutes for insiders—for instance because the firm's production process relies on specific human capital—worker exits can affect the firm's labor demand for its incumbent workers. In models that incorporate such imperfect substitutability, such as Stole and Zwiebel (1996a,b), the sign of the effect identifies the substitutability of the skills of the worker who exited with the skills of the remaining workers: the firm's labor demand increases for substitutes of the worker who exited and, in contrast, falls for workers who are imperfect substitutes of the worker who left the firm.

To test these predictions, I implement a quasi-experimental research design and estimate the causal effect of unexpected worker deaths on hiring and the remaining workers'

[1]See Topel (1991); Gibbons and Katz (1991); Jacobson, LaLonde, and Sullivan (1993); Farber, Hall, and Pencavel (1993); Dustmann and Meghir (2005), and Davis and von Wachter (2011). Additional evidence accords with extensions of Becker's model in Gibbons and Waldman (2004) and Lazear (2009) that can account for occupation, industry, and firm specificity of human capital (see Gibbons and Katz, 1992; Neal, 1995; Parent, 2000; Poletaev and Robinson, 2008; Kambourov and Manovskii, 2009; Gathmann and Schönberg, 2010 and Nedelkoska, Neffke, and Wiederhold, 2015).

wages based on the universe of German Social Security records.[2] In a dynamic difference-in-differences design, I compare roughly 34,000 small firms that experienced the death of a worker in a given year to a comparison group of firms with similar characteristics which did not experience the death of a worker in that year. The sample excludes deaths of workers who experienced a hospitalization or longer sickness spell in the five years before their death. Outcomes in treatment and comparison group firms follow parallel trends in the years prior to the death of a worker in treated firms, suggesting that outcomes in comparison group firms can be used to gauge what would have happened to workers in treatment group firms in the absence of a worker death.

Based on almost 7 million worker-year observations, I show that worker deaths affect the wages of the remaining workers in treated firms. On average, incumbent workers in the treated group experience a highly statistically significant earnings increase of about 0.6% in the year after the death. Over the course of the five years after the death, the average cumulative effect on the earnings of incumbent workers in the treated group is close to 6,000 EUR, corresponding to about 18% of an average deceased worker's annual earnings. Incumbent workers in the treatment group are more likely to stay employed with the same firm. The increased retention probability is due to a shift out of employment at other firms, not a change in the probability of (any) employment. Incumbents in the treated group have a higher probability of an intra-firm switch into an occupation with a higher average wage, suggesting that the increase in earnings is not solely due to a change in hours. Additional evidence also documents that the hours response is limited as the treatment effect on the likelihood of part-time employment is precisely estimated zero.

I leverage the research design to estimate within-firm heterogeneity across occupation and skill groups and find substantial heterogeneity that sheds light on the interdependencies between workers and the sources of frictions in replacing workers. The positive wage effects of worker exits are concentrated among incumbent workers in the same occupation group as the deceased.[3] For deaths of workers in high-skilled occupations, I estimate statistically significant, *negative* effects on the wages of incumbent workers in other occupations. Similarly, I find negative effect on incumbent workers in other occupation for deaths of managers.[4]

[2]The use of deaths as a source of variation builds on previous work in Jones and Olken (2005), Bennedsen, Pérez-González, and Wolfenzon (2006), Azoulay, Wang, and Zivin (2010), Becker and Hvide (2013), Oettl (2012), Isen (2013), Jaravel, Petkova, and Bell (2015), and Fadlon and Nielsen (2015). I discuss the relationship to this line of work in more detail in Section 7.

[3]In my main specifications, I consider workers in the same 1-digit group of the 2010 Classification of Occupations (*Klassifikation der Berufe 2010*) as being in the same occupation group and define workers in other occupations as the complement of that group.

[4]I classify workers as managers if they work in an occupation characterized by managerial, planning and control activities, such as operation and work scheduling, supply management, and quality control and assurance (see Section 3.2 for additional information).

Turning the focus to measures of human capital specificity of the deceased, I find evidence suggesting that longer-tenured workers and workers in specialized occupations are harder to replace by outsiders.[5]

As worker exits affects firms' demand for incumbents, my findings are hard to reconcile with frictionless labor markets and perfect substitutability of outsiders and incumbents. The results are instead consistent a model in which firms face frictions in replacing workers externally, so that worker exits raise the firm's demand for incumbent workers who are substitutes and lowers the demand for incumbents who are complements of the worker who died. In particular, the findings accord with Becker's (1964) conjecture that firms share rents to keep workers with specific human capital from quitting.[6] The finding of positive wage effects on coworkers in the same occupation as the deceased supports this view as workers in the same occupation are arguably closer substitutes than workers in different occupations and thus become more valuable to the firm as a consequence of a coworker exit. The finding of negative wage effects of deaths of workers in high-skilled occupations on incumbents in other occupations suggests imperfect substitutability of high- and low-skilled labor. My findings thereby support a key assumption of models positing that skilled workers raise the productivity of other workers in the same firm (see, e.g., Lucas, 1978, Rosen, 1982, Murphy, Shleifer, and Vishny, 1991, and Gennaioli et al., 2013), and constitute firm-level evidence consistent with studies of how market-wide labor supply shocks, e.g., due to immigration or changes in the college graduation rate, affect the wage structure (see, e.g., Card, 2009, Katz and Murphy, 1992, Goldin and Katz, 2008, and Dustmann, Ludsteck, and Schönberg, 2009).[7]

My interpretation of the empirical results as evidence on the substitutability of workers depends on whether alternative mechanisms can account for my findings. I consider several alternative explanations and evaluate them in light of the evidence: (1) changes in the remaining workers' compensating differential for working at the firm, (2) job assignment purely based on seniority, and (3) pure search frictions without human capital specificity. None of the alternative mechanisms can account for all of the findings. The first alternative explanation, for instance, posits that incumbent worker wages may have gone up as a result of a worker death increasing the compensating differential for working at the firm, e.g., due to decreased utility of interacting with colleagues or increases in the perception of job hazards.

[5] I proxy specialization with a measure used in Bleakley and Lin (2012) who classify occupations as relying on more specific skills when the returns to experience are high, which can be thought of as capturing the importance of occupation-specific capital (see, e.g., Shaw, 1984, Shaw, 1987, and Kambourov and Manovskii, 2009).

[6] While my results provide support for ex post rent sharing, it would in principle still be possible that workers do not earn ex ante rents if labor markets are competitive at the stage when workers enter firms.

[7] Katz and Murphy (1992), for example, find evidence that college- and high-school-educated workers are imperfect substitutes based on changes in the aggregate supply of college graduates.

While such labor supply-driven explanations could explain why wages go up, they would simultaneously predict that workers' probability of staying with the firm goes down. The data, however, reject this explanation as both the probability of staying at the firm and wages go up. Therefore, positive shifts in firms' labor demand dominate negative shocks to incumbent workers' labor supply. Several results are in conflict with the other alternative explanations. For example, the second explanation posits that workers may be perfect substitutes but rise through the ranks purely based on seniority. However, this explanation cannot account for the finding that wage effects of high-skilled worker deaths are negative. In contrast, models in which insiders and outsiders as well as high- and low-skilled workers are imperfect substitutes make this prediction and can also account for the other results of my study.

To shed light on the sources of frictions in replacing workers, I study heterogeneity by external labor market conditions and find that firms in thicker markets for specialized skill change incumbent wages by less and hire more externally in response to a worker death. The investigation is motivated by Marshall's (1890) conjecture that firms and workers in thicker, more agglomerated labor markets face fewer frictions in finding a suitable match and tests Lazear's (2009) idea that the specificity of human capital depends on the thickness of the market.[8] I test the role of market thickness by estimating heterogeneity across labor markets in the agglomeration of workers in the deceased's occupation. Wage effects are smaller in labor markets that are agglomerated in the relevant occupation; consistent with a labor market thickness mechanism, the difference between thick and thin labor markets is larger for occupations with a high degree of specialization. Additional evidence points to the underlying mechanism: firms in thicker labor markets are more likely to hire a new worker externally when a worker in a specialized occupation dies. Taken together, my findings suggest that frictions in replacing workers are larger in thin markets when workers' human capital is more firm-specific.

This paper contributes to several additional strands of the literature. My paper provides direct evidence for the imperfect substitutability of insiders and outsiders, the key assumption of intrafirm bargaining models (Stole and Zwiebel 1996a,b), and thereby resolves an open debate in the literature.[9] By shedding light on the frictions that firms face in replac-

[8] See Marshall (1890): "[A] localized industry gains a great advantage from the fact that it offers a constant market for skill. Employers are apt to resort to any place where they are likely to find a good choice of workers with the special skill which they require; while men seeking employment naturally go to places where there are many employers who need such skill as theirs and where therefore it is likely to find a good market. The owner of an isolated factory, even if he has access to a plentiful supply of general labour, is often put to great shifts for want of some special skilled labour; and a skilled workman, when thrown out of employment in it, has no easy refuge." Lazear (2009) develops a model in which human capital is a combination of general skills and becomes more firm-specific in firms with more idiosyncratic skill requirements compared to the external market.

[9] The canonical intrafirm bargaining model of Stole and Zwiebel (1996a,b) relies crucially on the assumption

ing workers externally, my study adds to a literature—going back to Slichter (1919) and Oi (1962)—that estimates the costs of worker turnover.[10] While this literature aims at gauging firms' expenditure for recruiting, hiring, and training, my research design provides a complementary perspective by providing evidence on how turnover affects firms' labor demand for incumbent workers and by showing that workers are harder to replace when their human capital is firm-specific. In doing so, my research design complements a large literature that assesses how firms' profitability affects wages (see, e.g., Slichter 1950, Dickens and Katz 1987, Blanchflower, Oswald, and Sanfey 1996, Van Reenen 1996, Card, Devicienti, and Maida 2013) as it provides direct evidence for a mechanism—human capital specificity leading to an imperfect substitutability of insiders and outsiders—that gives rise to such rent sharing. Finally, my research design provides new evidence for the importance of internal labor markets (Doeringer and Piore, 1971) by showing how idiosyncratic shocks to firm-specific labor supply—i.e. internal market forces—shape wages.[11]

The rest of the paper is organized as follows. The next section features a simple conceptual framework to illustrate how the effect of worker exits on firms' demand for the remaining incumbent workers identifies the substitutability of workers within and across firms under different modeling assumptions. Section 3 describes the empirical setting and the administrative data used for the analysis. Section 4 outlines the empirical strategy and identification assumptions and describes the exact matching procedure to select the comparison group. Section 5 presents the result of my paper. I assess alternative mechanisms to explain my findings in section 6 and discuss further implications of my finding in section 7. The last section concludes.

that firms face frictions in replacing their workers externally (see applications in trade and macroeconomics in, e.g., Helpman, Itskhoki, and Redding, 2010 and Acemoglu and Hawkins, 2014). Under the converse assumption that firms can hire perfectly substitutable replacement workers in the external labor market, the key result of overemployment in Stole and Zwiebel is overturned (de Fontenay and Gans, 2003). Stole and Zwiebel (2003) themselves note that "empirical work is needed to make a compelling case for one approach over the other". More recently, Elsby and Michaels (2013) assess that the "empirical validity of the Stole and Zwiebel bargaining solution has yet to be assessed".

[10]See overview in Manning (2011).

[11]In an influential contribution, Doeringer and Piore (1971) describe hiring, wage and career dynamics in internal labor markets in which the hiring of new workers is limited to lower-level "ports of entry", higher-level vacancies are filled through internal promotions and wages are "shielded from the direct influences of competitive forces in the external market". For existing tests of internal labor markets see, e.g., Baker, Gibbs, and Holmstrom (1994a,b); Lazear and Oyer (2004b,a), as well as Bertrand (2004) for evidence on the relationship between import competition and the shielding of wages from external labor market conditions. A related literature tests empirically between contract and spot market models of the labor market by estimating the effect of past unemployment on wages (see, e.g., Beaudry and DiNardo, 1991). For overviews, see the surveys in Gibbons and Waldman (1999); Lazear and Oyer (2013); Oyer and Scott (2011) and Waldman (2013).

# 2 Conceptual Framework

This section provides a simple framework to structure my empirical analysis. Building on three benchmark models of the labor market, I illustrate how the effects of worker exits on hiring and the firm's labor demand for incumbent workers identify the substitutability of workers. First, I illustrate the effects of worker exits in the canonical model for wage determination within firms developed by Stole and Zwiebel (1996a,b) in which workers cannot be replaced in the short run; second, in a model in which incumbent workers can be replaced by a pool of outside workers which provides a bridge to the standard model as it nests the competitive labor market as a corner case when the pool of outsiders is large (de Fontenay and Gans, 2003); and, third, in a search and matching framework with heterogeneous labor and wage bargaining following Cahuc, Marque, and Wasmer (2008).[12]

Several robust predictions emerge from the analysis of wage effects in the three models:

1. A nonzero effect of worker exits on firms' demand for the remaining incumbent workers rejects perfect substitutability of incumbent workers and workers that can be hired externally.

2. The sign of this wage effect identifies the substitutability of the incumbent workers.[13] Intuitively, an exit of a hard-to-replace worker raises the firm's demand for the labor of the remaining incumbent workers with substitutable skills. Among workers that are complements, worker exits leads to negative effects on firms' labor demand as the remaining workers' marginal product falls when a hard-to-replace worker with a complementary skill set leaves the firm.

3. The magnitude and duration of wage effects is proportional to the frictions that the firm faces in hiring suitable workers, allowing for a test of the importance of labor market pooling by measuring whether higher labor market thickness (see, e.g., Lazear, 2009) lowers such frictions.

[12] This model is closely related to work in Wolinsky (2000), Elsby and Michaels (2013), Acemoglu and Hawkins (2014), and Hawkins (2015) who develop equilibrium models of multi-worker firms based on the Stole and Zwiebel framework as well as earlier work by Bertola and Caballero (1994) who analyze a Nash bargaining setup with multiple workers bargaining over marginal surplus.

[13] Analogously, in models with homogenous labor, the sign of the wage effect identifies whether the production function features decreasing, constant, or increasing returns to scale.

## 2.1 Incumbent Worker Wage Effects With Homogenous Labor and No Replacement

I illustrate how worker exits affect the remaining incumbent workers' wages in the canonical model for wage determination inside firms by Stole and Zwiebel (1996a,b) who develop a multilateral bargaining setup that generalizes Nash bargaining. A key assumption is that workers cannot be replaced on the external labor market in the short run, for instance, because they have high levels of firm-specific training and human capital, e.g., senior product developers or supervisors. A more realistic interpretation of this assumption is the idea that human capital specificity or turnover costs lead to rents to continuing the employment relationship which is thus characterized by a bilateral monopoly between the firm and each worker.[14] In the Stole and Zwiebel framework, labor contracts are assumed to be nonbinding. This assumption follows a long line of research on holdup and the theory of the firm (see, e.g., Grossman and Hart 1986), which posits that it is costly to write or enforce complete contracts and that contracts can be renegotiated.[15]

I first describe the main features of the Stole and Zwiebel framework and then illustrate wage effects in this setup. In a simple setting with homogenous labor, worker exits raise coworker wages when the firm's production function has decreasing returns to scale and lowers wages when returns to scale are increasing.

Consider a firm negotiating with $N$ identical, specialized workers who cannot be replaced in the short run. Output is produced according to a production function $F(N) : \mathbb{N} \rightarrow \mathbb{R}_+$. The production function does not have to literally capture all of a firm's production, but can be thought of as the output produced by specialized workers on top of a standard production setup with variable factors, e.g., capital or labor for which a fluid market exists. The operator $\Delta$ denotes first differences so that $\Delta F(N) = F(N) - F(N-1)$ captures the increase in output when producing with $N$ rather than $N-1$ workers. The firm's profits are given by $\tilde{\pi}(N) = F(N) - \tilde{w}(N)N$ where $\tilde{w}(N)$ denotes the wage that each worker receives when a total of $N$ workers are employed by the firm.

Wages are determined in pairwise negotiations between the firm and each worker in which

[14] Alternatively, frictions could arise because firms have better information on incumbent workers (see models in Greenwald, 1986 and Waldman, 1984). The evidence is mixed with some studies finding support for such information asymmetry (see, e.g., Gibbons and Katz, 1991, and Kahn, 2013) while others are more consistent with a model in which employer learning about worker ability is public information (Farber and Gibbons 1996; Altonji and Pierret 2001 and Schönberg, 2007). Felli and Harris (1996) provide a model that shows how information about match quality with a given employer can be interpreted as firm-specific human capital.

[15] See Malcomson (1999) for an overview in the context of employment contracts. In Appendix C, I discuss wage renegotiation in a model with partially binding but incomplete contracts which leads to some wage rigidity.

the surplus is split. The setup can be easily extended to situations with asymmetric bargaining power as in section 2.3. When negotiations between a worker and the firm break down, the worker receives an outside wage of $\underline{w}$ and the firm continues the negotiations with the remaining workers. For each pairwise negotiation, the payoffs correspond to the Nash bargaining solution.[16] Labor contracts are assumed to be non-binding in the sense that no long-term contracts can be written.[17] The following analysis focuses on *stable* outcomes which are defined as wage profiles such that neither an individual worker nor the firm can improve their wage or the profit, respectively, by pairwise renegotiation.

Splitting the surplus in the pairwise negotiation requires that the firm's change in profit from retaining a worker equals the worker's wage above her outside wage $\underline{w}$:

$$\underbrace{\tilde{\pi}(N) - \tilde{\pi}(N-1)}_{\text{Firm's surplus}} = \underbrace{\tilde{w}(N) - \underline{w}}_{\text{Worker's surplus}} . \tag{1}$$

In the setup with only one worker, the firm's surplus is $\Delta F(1) - \tilde{w}(1)$, the worker's surplus is $\tilde{w}(1) - \underline{w}$ and the total surplus $\Delta F(1) - \underline{w}$ leading to a wage of:

$$\tilde{w}(1) = \underline{w} + \frac{1}{2}(\Delta F(1) - \underline{w}) = \frac{1}{2}(\Delta F(1) + \underline{w}). \tag{2}$$

This wage will only be feasible if $\Delta F(1) \geq \underline{w}$ as the employee otherwise prefers her outside wage.

In a setup with two workers to be employed by the firm, the firm's outside option when negotiations with one of the workers break down are affected by $\tilde{w}(1)$. This is the key difference to models without multilateral intra-firm bargaining. Specifically, when retaining a second worker the firm's profit will be $\tilde{\pi}(2) = F(2) - \tilde{w}(2)2$; when negotiations with one worker break down the profit will be $\tilde{\pi}(1) = F(1) - \tilde{w}(1)$ so that the splitting rule requires that:

$$\Delta F(2) - \tilde{w}(2) + [\tilde{w}(1) - \tilde{w}(2)] = \tilde{w}(2) - \underline{w}. \tag{3}$$

As a consequence, the wage at the two-worker firm then corresponds to:

$$\tilde{w}(2) = \frac{1}{3}\Delta F(2) + \frac{1}{6}\Delta F(1) + \frac{1}{2}\underline{w}. \tag{4}$$

[16] Stole and Zwiebel prove that this solution corresponds to the subgame-perfect equilibrium of an extensive form game in which the firm negotiates with the workers sequentially. Recently, Brügemann, Gautier, and Menzio (2015) proved that this solution does not correspond to the Shapley value of a corresponding cooperative game and propose an alternative extensive form game between a firm and its workers, labeled Rolodex Game, that does correspond to the Shapley value.

[17] In contrast, when binding long-term contracts can be written, the firm can pay workers their outside wage $\underline{w}$ so that profits correspond to $\pi(N) = F(N) - \underline{w}N$.

Importantly, the wage now not only depends on the marginal product $\Delta F(2)$ but also on the inframarginal change in output $\Delta F(1)$. A simple proof by induction leads to the following, general expression for wages in a firm with $N$ incumbent workers:[18]

$$\tilde{w}(N) = \frac{1}{N(N+1)} \sum_{i=0}^{N} i \Delta F(i) + \frac{1}{2}\underline{w}. \tag{5}$$

Intuitively, the wage corresponds to a weighted average of the marginal products integrated over the size of the firm. Marginal products that are closer to the margin of production receive a higher weight so that, e.g., the marginal product of the $N$th worker has a higher weight than the marginal product of the first worker. Note, though, that all workers are identical and consequently receive identical wages of $\tilde{w}(N)$.

The expression for the wage in (5) can be used to calculate how the wages of the remaining $N-1$ incumbent workers change when a worker exits the firm:

$$\tilde{w}(N-1) - \tilde{w}(N) = \frac{1}{N+1} \left( \sum_{i=0}^{N-1} \frac{2i}{N(N-1)} \Delta F(i) - \Delta F(N) \right). \tag{6}$$

The wage change is proportional to the difference between the marginal product of the $N$th worker, $\Delta F(N)$, and the weighted marginal products of workers 1 through $N-1$.[19] For a single-factor production function with decreasing returns to scale, $F'(N) > 0$, $F''(N) < 0$, the wages of remaining incumbent workers thus rise following the exit of a coworker from the firm. For constant returns to scale production function, the wage effect is zero. If the production function features increasing returns to scale, the wage effect is negative.

## 2.2 Incumbent Worker Wage Effects With Homogenous Labor and Replacement

I now illustrate wage effects in a model featuring a pool of workers on the external labor market from which the firm can hire as in de Fontenay and Gans (2003). The model nests the Stole and Zwiebel model as well as the competitive labor market as corner cases and documents that incumbent worker wage effects are zero in labor markets with a large pool of suitable workers available on the external market. More generally, the magnitude of the wage effects becomes smaller when firms face fewer search frictions.

The setup in the previous section stressed the importance of firm-specific human capital and the irreplaceability of workers in the short run. In contrast, the setup in this section

[18] See equations (2) and (3) in Stole and Zwiebel (1996). Note that this solution is only feasible if $\Delta F(i) \geq \underline{w}$, $\forall i \leq N$.

[19] Note that the weights sum up to 1: $\sum_{i=0}^{N-1} \frac{2i}{N(N-1)} = 1$.

implicitly posits that occupation- or industry-specific human capital may be important but firm-specific human capital is negligible. Suppose, for instance, that when a senior bio-engineer quits, a firm can hire a similar engineer with industry experience without much disruption to the production process but not a worker without any relevant experience.

Following de Fontenay and Gans (2003), there is a pool of $\bar{N}$ workers of which $N \leq \bar{N}$ insiders are employed by the firm. When negotiations with one of the insiders break down, the firm can costlessly hire one of the remaining outsiders. Letting the subscript $\bar{N} - N$ denote the number of outsiders, de Fontenay and Gans (2003) prove that the negotiated wage paid by the firm corresponds to a linear combination of the wage in the setting without replacement, $\tilde{w}(N)$ , and the workers' outside wage $\underline{w}$:

$$\tilde{w}_{\bar{N}-N}(N) = \left(\frac{N}{N+1}\right)^{\bar{N}-N} \tilde{w}(N) + \left(1 - \left(\frac{N}{N+1}\right)^{\bar{N}-N}\right) \underline{w}. \tag{7}$$

This setup nests the competitive labor market case when the number of replacement workers on the outside labor market becomes large so that wages correspond to workers' outside wages and no rents are earned ($\lim_{\bar{N}\to\infty} \tilde{w}_{\bar{N}-N}(N) = \underline{w}$). It also nests the case with irreplaceable workers when no outsiders are available and $\bar{N} = N$.

Based on (7), the wage change for incumbent workers when a worker exits from the firm and outsiders are available ($\bar{N} > N$) corresponds to:

$$\tilde{w}_{\bar{N}-1-N}(N) - \tilde{w}_{\bar{N}-N}(N) = \left(\frac{N}{N+1}\right)^{\bar{N}-N} \frac{1}{N} \left(\tilde{w}(N) - \underline{w}\right). \tag{8}$$

As the worker who exited is replaced by an outsider, employment at the firm stays constant at $N$ but the pool of outsiders is reduced by one. The wage change is proportional to the rents, $\tilde{w}(N) - \underline{w}$, that workers earn above their outside wage and decreases in the number of outsiders that can replace insiders, $\bar{N} - N$.

Based on (8), I can directly test two hypotheses regarding the fluidity of labor markets using my empirical design. First, a non-zero effect of a worker exit on coworker wages rejects the hypothesis that workers' wages equal their outside option, $\tilde{w}(N) = \underline{w}$, and a positive wage change indicates that workers earn a wage above their outside option. Second, a non-zero wage effect of worker exits also rejects the hypothesis that the size of the pool of replacement workers, $\bar{N} - N$, is large as $\lim_{\bar{N}\to\infty} \tilde{w}_{\bar{N}-1-N}(N) - \tilde{w}_{\bar{N}-N}(N) = 0$.

The second hypothesis also delivers a comparative static to test the importance of labor pooling and labor market thickness.[20] Going back to Marshall (1890), economists have

[20] See Lazear (2009) for a definition of labor market thickness: "A market is thick when the worker receives many offers for a given amount of search effort. [...] Empirical proxies of search costs and offer frequencies might include regional population densities and industry and occupation concentration ratios." Similarly, labor market thickness from a firm's perspective can be defined as the frequency of receiving suitable applicants for a given vacancy with similar empirical proxies.

hypothesized that firms benefit from clustering near other firms which employ workers with similar skills so that labor market thickness could act as a strong agglomeration force.[21] Moretti (2011), for instance, argues that "thick labor markets reduce the probability that a firm can't fill a vacancy, following an idiosyncratic shock to the labor supply of an employee", yet also notes that most evidence on the importance of this labor pooling channel is indirect. Based on the research design in this paper, I can directly assess the importance of this labor pooling channel by estimating the incumbent worker wage response to worker exits. If this force matters, firms that are located in local labor markets with an agglomeration of firms relying on similar types of labor will be able to hire replacement workers externally more easily leading to attenuated wage effects on incumbent workers.

### 2.3 Incumbent Worker Wage Effects With Heterogeneous Labor and Search Frictions

While the previous sections considered static models with homogenous labor, I now illustrate employment wage effects in a dynamic search and matching Pissarides (2000) model with intrafirm bargaining and heterogeneous labor following Cahuc, Marque, and Wasmer (2008). Abandoning the assumption of homogenous labor allows for a characterization of wage effects across worker types. Analogous to the predictions of the static model with homogenous labor, the sign of the coworker wage effect of a worker exit identifies the substitutability of different worker types inside the firm with substitutes associated with positive and complements associated with negative wage effects. Similar to the intuition in the previous section, the magnitude of the wage effect is proportional to the search frictions that the firm faces.

Consider a production function $F(N_1, ..., N_n)$ with $n \geq 1$ types of labor, indexed by $i = 1, ..., n$, and let $\mathrm{N} = (N_1, ..., N_n)$ denote the vector of labor inputs. When the representative firm wants to hire a worker of type $i$, it posts a vacancy $V_i$ and incurs a hiring cost of $\gamma_i$. As in standard search models, the matching function $h_i(u_i, V_i)$ is assumed to have constant returns to scale and to be increasing in each argument. Labor market tightness for worker type $i$ is denoted by $\theta_i = V_i/u_i$ and the firm's probability of filling a vacancy for worker type $i$ per unit of time is given by $q_i(\theta_i) = h_i(u_i, V_i)/V_i$. Existing jobs are destroyed at an exogenous destruction rate of $s_i$. The wage of workers of type $i$ is denoted by $w_i(\mathrm{N})$ as it can depend on the vector of labor inputs N.

The firm's hiring decision for each worker type is determined by the solution to the

[21] See, e.g., Helsley and Strange (1990) and Rotemberg and Saloner (2000) for formalizations of labor market pooling as an agglomeration force.

following Bellman equation:

$$\Pi(\mathrm{N}) = \max_{\mathrm{V}} \left(\frac{1}{1+r\,dt}\right) \left\{ \left[ F(\mathrm{N}) - \sum_{j=1}^{n} (w_j(\mathrm{N})N_j - \gamma_j V_j) \right] dt + \Pi(\mathrm{N}^+) \right\}, \tag{9}$$

subject to the law of motion for employment

$$N_i^+ = N_i(1 - s_i\,dt) + V_i q_i\,dt, \forall i \in \{1, .., n\}. \tag{10}$$

Here, V denotes the vector of vacancies for each worker type and $N_i^+$ denotes the employment of worker type $i$ at date $t + dt$. In the steady state, the solution to the firm's problem for hiring workers of type $i$ can be characterized as follows:

$$\underbrace{\frac{F_i(\mathrm{N}) - w_i(\mathrm{N}) - \sum_{j=1}^{n} N_j \frac{\partial w_j(\mathrm{N})}{\partial N_i}}{r + s_i}}_{\text{Marginal Benefit of Employment of Type } i} = \underbrace{\frac{\gamma_i}{q_i}}_{\text{Marginal Cost of Hiring}}. \tag{11}$$

This expression can be rearranged to assess the relationship between the marginal product of workers of type $i$ and labor costs:

$$\underbrace{F_i(\mathrm{N})}_{\text{Marginal Product}} = \underbrace{w_i(\mathrm{N})}_{\text{Wage}} + \underbrace{\frac{\gamma_i(r + s_i)}{q_i}}_{\text{Turnover Costs}} + \underbrace{\sum_{j=1}^{n} N_j \frac{\partial w_j(\mathrm{N})}{\partial N_i}}_{\text{Employment Wage Effect}}. \tag{12}$$

The last term is absent in standard search models without intra-firm bargaining. For constant returns to scale production functions, the employment effect is irrelevant (Cahuc and Wasmer, 2001). For decreasing returns to scale production functions, however, the employment wage effect is negative. This moderates the effect of product demand shocks on wages as firms that increase their employment can lower wages. Previous research designs used calibrations or simulations to gauge the importance of the employment effect. Based on my research design, I can directly estimate the effect of shocks to employment on the wages of the remaining workers and thereby provide an estimate of employment wage effects.

As in Stole and Zwiebel, wages are determined by a Nash bargaining rule:

$$\beta \underbrace{\frac{\partial \Pi(\mathrm{N})}{\partial N_i}}_{\text{Firm's Marginal Profit}} = (1 - \beta) \underbrace{\frac{w_i(N) - rU_i}{r + s_i}}_{\text{Worker's Surplus}}, \tag{13}$$

where $U_i$ denotes the expected value of being unemployed, or the reservation utility, of a worker of type $i$ and $\beta$ denotes worker's bargaining power.[22] Cahuc, Marque, and Wasmer

[22] For ease of exposition, I only discuss the case with constant bargaining power. Cahuc, Marque, and Wasmer (2008) also derive solutions with heterogeneous bargaining weights for each worker type $i$.

(2008) solve the system in (11) and obtain the following expression for the wage of workers of type $i$:

$$w_i(\mathrm{N}) = (1-\beta)rU_i + \int_0^1 z^{\frac{1-\beta}{\beta}} F_i(\mathrm{N}z)\, dz. \tag{14}$$

The wage expression has an intuitive interpretation similar to the Stole and Zwiebel formula in (5). A worker's wage corresponds to the sum of a term proportional to the worker's outside option, $rU_i$, and the worker type's marginal product integrated over the total employment at the firm. The weights, $z^{\frac{1-\beta}{\beta}}$, depend on the worker's bargaining power $\beta$ and are linearly increasing, as in the simple static model in (5), when $\beta = \frac{1}{2}$.

While the employment wage effects in (12) have direct implications for search and matching models and can be identified based on the worker exit research design in this paper, equation (14) can be further used to demonstrate that worker exits identify which worker types are complements or substitutes in production:

$$\frac{\partial w_i(\mathrm{N})}{\partial N_j} = \int_0^1 z^{\frac{1}{\beta}} F_{ij}(\mathrm{N}z)\, dz. \tag{15}$$

Specifically, negative shocks to the labor supply of worker type $j$ *raise* wages of workers of type $i$ when $j$ and $i$ are *substitutes* in production ($F_{ij} < 0$) and *lower* wages for workers of type $i$ when $i$ and $j$ are *complements* in production ($F_{ij} > 0$). In a setup with homogenous labor, the model thus nests the prediction from the static model and predicts coworker wage increases after a worker exit when the production function has decreasing returns to scale. For a Cobb-Douglas production function with two skill groups and complementarities between worker groups and perfect substitution within group, e.g., high-skilled and low-skilled workers, wage effects of a high-skilled worker exit would be positive for other high-skilled workers and negative for low-skilled workers.

In the model described in this section, the firm will respond to a worker exit by posting a vacancy and will, in expectation, converge back to its pre-exit steady state employment level. Therefore, any wage effects will also converge back to zero over time. The speed of convergence is inversely related to the search friction that the firm faces. To see why, consider a discrete time version of the search and matching model and let $q_j(\theta_j)$ now denote the per-period probability of filling a vacancy for worker type $j$. Directly following the worker exit, the wage effect of a $j$-worker exit on $i$-worker wages will be $-\frac{\partial w_i(\mathrm{N})}{\partial N_j}$ as employment of worker type $j$ has changed by $-1$; in the next period, the wage effect will be $-\frac{\partial w_i(\mathrm{N})}{\partial N_j}(1-q_j(\theta_j))$, in expectation, as the vacancy will have been filled with probability $q_j(\theta_j)$. Letting $\Delta N_{jt}$ denote the discrepancy between employment of worker type $j$ in period $t$ and the state employment

level of worker type $j$, the cumulative long-run effect of a $j$-worker exit in $t = 0$ on $i$-worker wages can be characterized as follows:

$$\sum_{t=0}^{\infty} \frac{\partial w_i(\mathrm{N})}{\partial N_j} \Delta N_{jt} = -\sum_{t=0}^{\infty} \frac{\partial w_i(\mathrm{N})}{\partial N_j} (1 - q_j(\theta_j))^t = -\frac{\partial w_i(\mathrm{N})}{\partial N_j} \frac{1}{q_j(\theta_j)}. \tag{16}$$

According to (16), the magnitude of the cumulative long-run effect of a worker exit on wages is proportional to the search friction that the firm faces when hiring workers of type $j$. Lower probabilities $q_j(\theta_j)$ of filling a vacancy lead to larger and longer lasting wage effects.

This result demonstrates that the prediction from the static model with replacement workers in section (2.2) is robust: if firms in thicker labor markets indeed face lower search frictions, the magnitude of wage effects of worker exits will fall with thickness. In addition, this model predicts that longer-run wage effects will be larger in magnitude in tighter labor markets, that is, in labor markets with a high ratio $\theta_j$ of vacancies to unemployed workers.

Equation (16) documents that the the speed of hiring and the speed of wage adjustment are identical in the search model. The intuition is simple: as soon as a vacancy is filled, the newly hired worker becomes a perfect substitute for other workers of the same type in the firm. In contrast to the prediction from this model, there could be a discrepancy between the speed of hiring and the speed at which wage effects revert to zero, for instance if new workers are hired relatively fast but wage effects persist following a worker exit. Such a finding would reject the hypothesis that newly hired workers are perfect substitutes for incumbent workers. Instead, it would be more consistent with a model in which newly hired workers acquire firm-specific human capital (Becker, 1962; Lazear, 2009) and become closer substitutes to incumbent workers over time, in line with the narrative of the internal labor markets literature (Doeringer and Piore, 1971).

# 3 Empirical Setting and Data

## 3.1 Empirical Setting: German Labor Market

To provide context for the following analysis, I briefly highlight several relevant characteristics of the German labor market. My analysis of the effect of worker exits focuses on small firms. These are part of the so-called *Mittelstand*, small and medium-sized firms, which make up a large share of the German labor market. In 2012, firms with less than 250 employees accounted for 99.5% of firms and 61.3% of employment.[23] In comparison, employment in

[23]Source: Eurostat, information for 2012. According to the EU definition, small- and medium-sized enterprises are defined as enterprieses with fewer than 250 employees, with sales not exceeding EUR 50 million and an annual balance sheet not exceeding EUR 43 million.

similar-sized firms in the United States in 2012 accounted for 43.3% of employment.[24] Relative to the OECD average, Germany has a relatively high manufacturing share at 22.6% of GDP (OECD: 15.0%, US: 12.7%).[25]

A key feature of German education system is apprenticeship training offered by firms. As part of an apprenticeship training, workers receive training in occupation- and industry-specific skills at a particular firm and a vocational school.[26] Apprenticeship training programs follow prescribed curricula that lead to a certified qualification in a trade, e.g., as a banking professional, a piano maker, or a mechatronics specialist. Apprenticeships have remained the modal educational qualification in the last decades: in 2004, more than 76% of German workers had completed an apprenticeship training.

In the last decades, the wage setting processes in the German labor market have become increasingly decentralized (Dustmann et al. 2014). Traditionally, collective bargaining agreements (CBA) between employer associations and unions have played a crucial role in the wage setting process, e.g., by providing wage floors in firms covered by an agreement. While employers could always raise wages beyond CBA-levels, opening clauses have become increasingly common which give firms more flexibility to negotiate with their workers directly, in particular to pay below-CBA wages. The prevalence of opening clauses started to increase in the 1990s (Brändle et al., 2011); in 2005, 75% of establishments had an opening clause (Bispinck et al., 2010). The period of decentralization since the 1990s coincided with a dramatic increase in wage inequality and a decline in real wages at the bottom of the wage distribution (see Dustmann et al. 2009, Card et al. 2013, Dustmann et al. 2014).

## 3.2 Primary Data Source: Social Security Records

I use matched employer-employee data based on the universe of German Social Security records from 1975 until 2011. The data feature detailed information on all workers at an establishment which enables me to measure how worker exits affect both the hiring of new workers as well as the wages of incumbent workers at the establishment. Two additional features of the dataset make it a compelling setting to assess the substitutability of workers. First, wages are reported as part of administrative procedures so that measurement error is low which further increases the reliability and precision of estimates. Second, the dataset is large which allows for a relatively precise estimation of potentially small effects and enables an analysis of wage effects for different types of firms and workers to shed light on the mechanisms

[24]Source: Own calculations based on 2012 employment data from the Longitudinal Business Database 1977-2013, United States Census Bureau.

[25]Source: World Bank National Accounts Data, information for 2012.

[26]See Acemoglu (1997) and Acemoglu and Pischke (1998) for theory and evidence to explain firms' incentives to invest in apprentices' skills that are not completely firm-specific.

driving the results. This is a key difference to many existing tests of internal labor markets which often leverage personnel records from specific firms rather than administrative data. Based on the universe of German Social Security records, the dataset used for my analysis covers about 82% of employment in Germany.[27] The key employment categories that are excluded are civil servants and the self-employed as their employment is not subject to social insurance provided through the Social Security system.

The data stem from the Integrated Employment Biographies (IEB) database of the Institute for Employment Research (IAB). As part of its administrative processes, the German Social Security system collects data from employers on all employees in jobs subject to Social Security taxation. The data that employers mandatorily need to report for each employee include the start and end date of each job, the employee's earnings up to the censoring limit at the maximum taxable earnings level, and data on education levels, apprenticeship status, and occupation as well as basic demographic information like gender, birth date and citizenship. The frequency of reporting is typically once per year and, in addition, whenever a new employment spell starts or ends or the job status changes, e.g., from part-time to full-time employment.

I use data on workers' earnings as the primary outcome variable. The earnings variable reports gross earnings which are reported as daily earnings associated with a specific employment spell. For the analysis, I scale up daily earnings by a factor of 365 to correspond to yearly earnings and deflate all reported earnings to correspond to the 2010 CPI. Measurement error of earnings due to misreporting by employers is likely negligible as earnings are reported as part of existing administrative processes and misreporting is punishable. The data do not contain information on the exact hours worked but do contain information on whether employment is full- or part-time. For full-time workers the reported earnings likely corresponds closely to the wage due to limited variation in working hours so that I will follow the existing literature (see, e.g., Dustmann, Ludsteck, and Schönberg, 2009; Card, Heining, and Kline, 2013) and use the terms earnings and wage interchangeably. In the analysis, I also assess whether hours of work are affected at the part-time/full-time margin. A drawback of the earnings data is that earnings above the Social Security earnings maximum are top-coded.[28] In the sample I work with, 6.0% of earnings observations are censored. As my analysis focuses primarily on within-worker, within-firm variation in wages, imputation procedures based on lagged or current individual or employer-level information would not

[27]Between 1981 and 2011, an average of 81.9% of the German labor force were dependent employees or workers as opposed to civil servants or self-employed (Source: own calculations based on Mikrozensus employment data by the Federal Statistical Office Germany, 2015).

[28]For example, in 2011, the earnings maximum was at 66,000 EUR for West Germany, corresponding to about US$ 88,200 at the time. The average earnings of deceased and incumbent workers in my sample are less than half of that amount at around 30,000 EUR.

add additional information for the analysis. I therefore do not impute earnings above the Social Security earnings maximum and instead set wages to the earnings maximum if they are top-coded. My analysis thus does not capture variation in wages above the earnings maximum.

To assess the interdependencies between workers inside the firm and understand heterogeneity in the effect of worker exits, I leverage detailed data on the deceased workers' and the remaining incumbent workers' occupations. Workers' occupations are reported at the 5-digit level of the 2010 Classification of Occupations (*Klassifikation der Berufe 2010*).[29] Occupations are classified primarily along two dimensions: first, horizontally into occupation groups based on the thematic focus of the work, e.g., production and manufacturing vs. accounting. I use this horizontal classification to identify groups of workers inside a firm who work in jobs with a similar or distinct thematic focus.[30] Second, occupations are classified vertically based on the skill requirements of the occupation. I use this vertical categorization to identify workers in managerial and supervisory roles.[31]

My analysis focuses on wage effects as well as hiring and employment at the establishment level. The Social Security system assigns unique establishment IDs based on ownership, industry, and location at the municipality level.[32] The assignment of establishment IDs implies, for example, that two bakeries operated by the same firm in the same city would be reported as one establishment. In contrast, a bakery and a mill operated by the same firm would be classified as different establishments even when they are located in the same municipality. In all cases, my analysis will be conducted at a within-firm level and all coworkers will be employed by the same firm. The analysis may not capture all employment at a firm in the case of multi-establishment firms. However, for the sample that I consider an estimated 84% of establishments correspond to single-establishment firms.[33]

[29] See Paulus and Matthes (2013) for a detailed overview.

[30] The horizontal classification is based on a worker's 1-digit occupation group.

[31] I classify workers as managers if they work in an occupation requiring "complex specialist activities" (requirement level 3) or "highly complex activities" (requirement level 4). These occupations are characterized by managerial, planning and control activities, such as operation and work scheduling, supply management, and quality control and assurance and typically require a qualification as master craftsperson, graduation from a professional academy, or university studies (see *Klassifikation der Berufe 2010, Band 1: Systematischer und alphabetischer Teil mit Erläuterungen, Bundesagentur für Arbeit*).

[32] The Social Security system issues a new establishment ID after an ownership change and other reorganizations. Hethey-Maier and Schmieder (2013) use a worker flow methodology to document that only about 35 to 40% of new or disappearing establishment IDs in the German Social Security data correspond to actual establishment entries or exits. Due to the uncertainty surrounding the continued operation of an establishment when the establishment ID disappeared, I focus on a balanced panel of establishments with a consistent establishment ID so that the analysis follows a well-defined economic unit that is consistent over time.

[33] Based on a recent record linkage between establishments and firms, about 84% of establishments in the size category considered here correspond to single-establishment firms (Antoni, Laible, and Schild, 2015). In keeping with the literature (see, e.g., Dustmann, Ludsteck, and Schönberg, 2009; Card, Heining, and Kline,

# 4 Empirical Strategy

I implement a dynamic difference-in-differences design in which I compare roughly 34,000 small firms that experienced the death of a worker in a given year to a comparison group of firms—and placebo deceased workers—which have similar lagged characteristics but did not experience the death of a worker in that year. The first part of this section describes the identification of worker deaths in the Social Security data and the sample restriction to deaths that were not preceded by hospitalizations or longer sickness spells. Next, I describe how I select the comparison group for the difference-in-differences design from a sample of firms that did not experience the death of a worker in the relevant year. I then provide summary statistics for the treatment and comparison group. Finally, I describe the estimating equations for the difference-in-differences design and discuss the identification assumptions.

## 4.1 Identifying Unexpected Deaths in Social Security Data

To circumvent the endogeneity of worker exits from a firm, I leverage deaths of workers as a source of variation in a firm's labor supply. I identify deaths based on employer notifications to the Social Security system and restrict the analysis to deaths of workers who are younger than 65 at the time of death and who did not experience a hospitalization or a longer sickness spell in the five years before their death.

The employer needs to notify the Social Security system when an employment spell ends. If an employment spell ends because an employee died, the notification states that the ending of the spell was due to the death of the employee. Death notifications are available from 1980 onwards. I identify deaths in the Social Security data and verify that the death reports are not spurious: for more than 93% of reported deaths, the reported death date corresponds to the latest date for which an employment or unemployment spell is reported in the data. Most of the remaining observations with spells with end dates after the reported death date end within weeks after death, suggesting that in these cases there are some minor inconsistencies in the exact date of reporting. To rule out spurious death notifications, I restrict my analysis to reported deaths with no spell endings more than 30 days after the first reported death date which comprise more than 97% of reported deaths.

I focus on deaths that are arguably premature and unexpected. First, I restrict the sample to deaths of individuals who are younger than 65 at the time of death. Second, I focus on individuals who were employed full time at the time of death. Third, to rule out deaths that were preceded by a debilitating disease, I drop individuals who had a sickness leave in

2013), I will use the terms establishment and firm interchangeably throughout.

the five years before their death.[34] Specifically, the Social Insurance system pays sickness or wage replacement benefits during hospitalizations—of any duration—as well as during sickness leaves of six weeks or more.[35] Receipt of such wage replacement benefits is reported in the data which allows me to restrict the sample to individuals who did not experience a hospitalization or longer sickness leave before their death.[36] So while the cause of death is not reported in the data, the additional restrictions lead to the exclusion of deaths that are caused by slow-moving, debilitating diseases, such as many cancers, but do include deaths that occur relatively unexpected, such as deaths due to accidents or strokes.

## 4.2 Matched Sampling Procedure to Select Comparison Group

A key challenge is to find an appropriate comparison group for firms that experience the death of an employee. One option would be to use firms that experience a worker death at an earlier or later point in time as a comparison group conditional on firm fixed effects. However, such specifications will be biased if the death leads to a change in the trend in the outcome of interest (Azoulay, Wang, and Zivin, 2010). To circumvent this problem, I use a matched sampling procedure—similar to the approach in Azoulay et al. (2010)—to identify a comparison group of placebo deceased worker-firm pairs in which the worker did not die but that have lagged characteristics similar to the ones of treated worker-firm pairs in which the worker died.

**Time Notation.** I let $t$ denote calendar years, $d$ event years, and $k = t - d$ the year relative to an event. For a given year $t$, I measure outcomes on July 1 of that year.[37] A death is defined to occur in event year $d$ if it occurs between July 1 of $d$ and June 30 of $d + 1$ so that a death occurs between $k = 0$ and $k = 1$.

**Treatment Group.** For each event year $d$ from 1980 to 2007, I identify the set of worker deaths in $d$ for whom the restrictions described 4.1 are met.[38] For each worker who died in $d$ and for their employer at the time of death, I record a rich set of baseline characteristics in $d - 4$, i.e. four years before death.

**Pool for Comparison Group.** For each event year $d$, the comparison group is sampled

[34]This restriction leads me to drop 42% of employer-reported deaths.

[35]Shorter sickness leaves are mandatorily covered by employers.

[36]The data do not distinguish between the different kinds of wage replacement benefits ("Entgeltersatzleistungen") which also include maternity benefits. As I exclude individuals who received any kind of wage replacement benefits, the restriction will also exclude some individuals who received maternity benefits in the five years before death.

[37]Employment is reported at a daily frequency. As noted in 3.2, earnings are reported as an annual average for the typical employee.

[38]The time period chosen ranges from 1980 to 2007 as death notifications are reported from 1980 onwards and as I require a sufficiently long post-death period exists as employment and wage data are available until 2011.

from the set of worker-firm pairs in firms which did not experience the death of an employee in $d$. Analogous to the procedure for the treatment group, I record baseline characteristics in $d-4$ for this comparison group pool.

**Matched Sampling to Select Comparison Group.** I implement a matched sampling procedure *separately for each event year d*. For each deceased worker-firm pair in the treatment group, I select a worker-firm pair from the comparison group pool with similar lagged characteristics. This approach is motivated by Rosenbaum and Rubin (1985) and Imbens and Rubin (2015) (chapter 15) which describe how matched sampling can be used to find a comparison group of similar size and with similar observed characteristics as the treatment group and follows the precedent in the literature (Azoulay et al. 2010). In each event year $d$, I select placebo deceased worker-firm pairs from the comparison group pool to exactly match the following characteristics of actual deceased worker-firm pairs in the treatment group:

- Worker characteristics: age in years, gender, education group[39], deciles of earnings in $d-4$
- Firm characteristics: number of employees in $d-4$, deciles of average earnings at the firm in $d-4$

These variables are chosen to create a comparison group with similar observed characteristics as the treatment group, in particular age and gender as deceased workers in the sample are on average 7.4 years older and more likely to be male than workers in the pool for the comparison group (86% vs. 62% men).[40] An exact match is found for 95.81% of worker-firm pairs in the treatment group. When multiple potential matches for a deceased worker-firm pair are available, I select the unit from the comparison group pool with the closest propensity score calculated based on a rich set of worker- and firm-level covariates.[41]

The matched sampling procedure implies that the comparison between the treatment and the comparison group is between coworkers and establishments of actual and placebo

[39] I categorize workers into three education groups: workers with no apprenticeship training (low), workers with an apprenticeship training (medium), and workers with further formal education (high). Further formal education refers to workers with a qualification for university studies (*Abitur*) or a university-level education.

[40] I have also verified that I obtain similar results when using a different matching approach, e.g., purely based on propensity scores. Due to the precedence in the literature (Azoulay, Wang, and Zivin, 2010) and recent arguments for the use of exact matching procedures (Iacus, King, and Porro, 2011; King and Nielsen, 2015), I implement an exact matching approach. I obtain similar results when matching on a richer set of covariates. However, due to the curse of dimensionality the number of successful matches falls when increasing the number of characteristics for matching.

[41] The propensity score is calculated based on a linear probability model that includes linearly the average wage at the establishment and the individual wage of the worker, tenure and occupation experience, dummies for the number of full-time workers at the establishment and the age of the establishment, as well as fixed effects for industry (3 digit) and occupation (5 digit) in addition to the variables used for the exact matching. All characteristics are measured in $d-4$. In each event year, a firm is sampled at most once from the comparison group pool but firms can be sampled multiple times across years.

deceased workers with the same year of birth and the same age at—actual or placebo—death and, moreover, the same gender and earnings. Importantly, I do not match on trends—only on lagged covariates in $d-4$—so that the pre-trends themselves can be used to evaluate the plausibility of the common trends assumption.

**Sample Restrictions.** In both the treatment and the comparison group, I restrict the sample to employers with between 3 and 30 full-time employees four years before death which comprise about 30.5% of employment subject to Social Security in Germany.[42] There are two key reasons for focusing on smaller establishments. First, in larger establishments worker exits due to death occur more frequently due to the law of large numbers. Second, as outlined in Section 2, the effect of a worker death on average coworker wages decreases mechanically with firm size so that it will be hard to detect in larger firms.[43] I drop establishments that are part of the government or the social insurance system, churches and other non-profits and keep establishments in the service, manufacturing and agricultural sector.[44] Finally, I exclude firms with multiple worker deaths in a given year to rule out deaths due to larger disasters that may have independent effects on outcomes. In both the treatment and the comparison group, I require that the—actual or placebo—deceased was employed full-time in $d$ and in $d-4$, thereby restricting the sample to individuals with high labor force attachment.

## 4.3 Summary Statistics

This section provides summary statistics for workers and firms in the treatment and comparison group. The difference-in-differences design that I implement permits differences in average levels of outcome variables between the treatment and comparison group and instead relies on a common trend assumption (see 4.4). However, the summary statistics present information to assess to what extent the matched sampling created a balanced comparison group for the difference-in-differences design and provide context for the interpretation of treatment effects.

**Characteristics of Actual and Placebo Deceased Workers.** Columns (1) and (2) of Table 1 report summary statistics for the 33,855 actual and the same number of placebo deceased workers in the treatment and comparison group, respectively. The average deceased worked is 47 years old and overwhelmingly male (86%) with 10.6 years of education, corresponding approximately to an apprenticeship training—the most common educational

[42]A cutoff of 30 employees is a common legal threshold to distinguish small employers from larger ones (see, e.g., Act on the Compensation of Employer Expenditures (*Aufwendungsausgleichsgesetz*)).

[43]In Appendix Table D.1, I document that the treatment effect of a worker exit on the remaining incumbent workers' wages decreases with establishment size.

[44]Specifically, I drop all establishments with an industry code larger than 870 in the 1973 edition of the German Classification of Economic Activities.

credential in Germany. In the year before death, $k = -1$, actual and placebo deceased workers earned a wage corresponding to an annual salary of EUR 31,458 in the treatment and EUR 31,536 in the comparison group. The difference between the treatment and comparison group is not statistically significant ($p = 0.41$) and the similarity between actual and placebo deceased workers is not a mechanical effect of the matched sampling as the matching relied on variables in $k = -4$.[45] Both groups of workers have an average tenure of 9.5 years at the firm in $k = -1$.

**Characteristics of Incumbent Workers in Treatment and Comparison Group.** In order to gauge the effects of worker exits on firms' labor demand for the remaining workers, I define a sample of incumbent workers as the set of full-time coworkers of the deceased in event year $d$.[46] Columns (3) and (4) of Table 1 report summary statistics for these incumbent workers who are slightly younger than the actual and placebo deceased workers with an average age of 39 and are more likely to be female (26%). Incumbent workers have average earnings in $k = -1$ of about EUR 28,000 (EUR 27,788 in the treatment, EUR 27,856 in the comparison group), an average level of education of 10.9 years, and have about 7 years of tenure with the establishment.

**Characteristics of Firms in Treatment and Comparison Group.** I report summary statistics for the firms in the treatment and comparison group in Table 2 in period $k = -1$. The average establishment in the treatment group has 14.44 employees (14.50 in the comparison group), of which about 15% are new employees in $k = -1$, and has been observed in the data for about 14.8 years. About 3% of firms are in the primary sector (agriculture, mining), 50% in the secondary sector (manufacturing), and 47% in the tertiary sector (services). Since I do not match exactly on industry, occupation of the deceased, and the location of the firm, a potential concern could be that there is substantial imbalance in these dimensions. I adress this concern by regressing treatment status on industry fixed effects (3 digit), fixed effects for the occupation of the deceased (5 digit), and labor market region fixed effects (50 regions based on Kropp and Schwengler 2011) and find that these variables are jointly insignificant in predicting treatment status in my sample ($p = 0.336$).

[45] In $k = -4$, the year of matching, actual and placebo deceased workers earned average wages corresponding to an annual salary of EUR 31,475 in the treatment and EUR 31,476 in the comparison group. The wages of actual and placebo deceased workers thus developed parallelly and stagnated from $k = -4$ to $k = -1$.

[46] Similar to the sample restriction for the actual and placebo deceased workers, I restrict this sample to incumbent workers younger than 65 in $k = -1$. Incumbent workers remain in the sample regardless of whether they remain at the firm in subsequent periods. In case of non-employment in a given year, I set their earnings to zero.

## 4.4 Estimating Equations and Identification

I implement a dynamic difference-in-differences design to estimate how shocks to firm-specific labor supply due to worker deaths affect hiring and employment as well incumbent worker wages and retention rates. Two advantages of this approach are that (1) the research design allows for a direct, graphical assessment of treatment effects over time, and that (2) outcome variables can be observed for both the treatment and the comparison group in the pre-period so that the common trend assumption can be evaluated directly. Here, I describe the econometric framework and discuss and test the identification assumptions.

**Estimating Equations for Firm-Level Outcomes.** I estimate the effect of a worker death on hiring and employment based on the following dynamic difference-in-differences framework:

$$\begin{aligned} y_{jk} &= \alpha + \gamma_j + \sum_{k=-3}^{5} \beta_k \times 1(\text{period}_k) \\ &\quad + \sum_{k=-3}^{5} \beta_k^{Treated} \times 1(\text{period}_k) \times \text{Treated}_j + \epsilon_{jk}. \end{aligned} \tag{17}$$

where $y_{jk}$ denotes the outcome $y$ for firm $j$ in year $k = t - d$ relative to the worker death occurring in year $d$. The model includes firm fixed effects, $\gamma_j$, and leads and lags around event time, $1(\text{period}_k)$.[47] $\text{Treated}_j$ is an indicator function for treatment status. The coefficients of interest, $\beta_k^{Treated}$, capture the effect of an actual worker death in year $k = t - d$ in the treated group and it is normalized to zero for $k = -1$. I define the *short-run treatment effect* as the effect in the first post-death year, i.e. $\beta_1^{Treated}$, and a *long-run treatment effect* as the average of treatment effects in the five-year post-period, i.e. $\frac{1}{5}\sum_{k=1}^{5} \beta_k^{Treated}$. I cluster standard errors at the firm level. While treatment varies at the finer firm by year relative to death level, clustering at the firm level addresses potential concerns of serial correlation of outcomes across periods raised in Bertrand, Duflo, and Mullainathan (2004).[48]

The model allows for average differences between the treatment and the comparison group as they are absorbed by the firm fixed effects, $\gamma_j$, so I do not assume that the treatment and comparison group would have the same average outcomes in the absence of treatment.

[47] Formally, I consider firms sampled in different event years as different firms, leading to a finer set of fixed effects. For example, if firm $A$ is sampled in event year $d = 1985$ and in event year $d = 1991$, the model includes separate fixed effects for $A_{1985}$ and $A_{1991}$ which are finer and subsume a fixed effect for $A$ only.

[48] As a robustness check, I also estimate specifications with standard errors clustered at the match level (based on the matched sampling procedure) and standard errors clustered at the firm level treating a firm sampled in different event years as one firm. Both alternative procedures lead to almost identical standard errors.

Rather, the variation I leverage for identification occurs within the same firm, comparing outcomes relative to $k = -1$, and within the same time $k$ relative to the actual or placebo worker death, comparing treated firms to firms in the comparison group.

**Identification Assumption and Potential Threats to Identification.** The key assumption for identification is that worker deaths are exogenous conditional on the covariates included in the model. This implies that firms in the treatment and the comparison group would have followed parallel trends in $k > 0$ if, counterfactually, no worker death had occurred in the treatment group. Since firms are observed in periods before the actual or placebo worker death occurs, the plausibility of this assumption can be tested by assessing whether outcomes followed parallel trends in the treatment and comparison group in the pre-period.

Potential threats to identification would be the existence of contemporaneous shocks that affect outcomes and also the timing of deaths in the treated group. Given that the estimated effects on coworker wages are positive on average, a potential threat to identification that could drive results arises if deaths of workers reflect additional stress from an uptick in firm performance that results in higher wages. Alternatively, the positive estimates could be downward-biased if deaths occur as a consequence of negative shocks to the firm. However, when pre-trends are parallel, such shocks would have to be sudden in onset but, at the same time, large enough to be associated with worker deaths. This, in turn, makes some potential threats to identification less compelling: coronary heart disease, for instance, develops over a long time span and is caused by long-term rather than short-term stress levels (Kivimäki et al., 2006).[49]

In addition to analyzing pre-trends, I implement a further test to gauge the importance of these potential challenges to identification that documents that firms in the treatment group do not have a higher propensity of experiencing a worker death in future periods, $k > 0$, relative to the comparison group. Unobserved shocks that are sudden in onset could be hard to detect in the pre-period but could affect mortality and outcomes in future periods, thereby leading to a bias in the estimate of the treatment effect. If that were the case, one would expect to see an increased propensity of firms in the treatment group to experience worker deaths in $k > 0$. I test this hypothesis by regressing an indicator for whether a firm experienced a worker death in a given future period, $k > 0$, on treatment status. Table 3 reveals that firms in the treatment and comparison group have an identical probability of about 1.2% of experiencing a worker death in a given future period as the indicator for treatment status is statistically insignificant, small and even slightly negative at -0.007%. As

[49] In meta-analysis of the effects of work stress on coronary heart disease, Kivimäki et al. (2006) summarize the short- and long-term effects of work-related stress on coronary heart disease (CHD) as follows: "All studies with null findings assessed job strain at one point in time only. As CHD develops over a long time span, long-term rather than short-term levels of job strain are assumed to have an impact on CHD incidence."

firms in the treatment group do not have a higher propensity to experience future worker deaths it appears that the worker deaths under study are indeed idiosyncratic shocks to the labor supply of firms in the treatment group.

**Estimating Equations for Incumbent Worker Outcomes.** The estimating equation in (17) above describe specifications to estimate treatment effects on firm-level outcomes such as employment and hiring. To analyze treatment effects on outcomes for incumbent workers, for instance, wages, I estimate very similar difference-in-differences specifications on the sample of incumbent workers defined as the set of full-time coworkers of the deceased in event year $d$ (see summary statistics in Section 4.3 and Table 1). Individuals remain in the incumbent worker sample if they were coworkers of the deceased in $d$ regardless of whether they remain at the same firm in subsequent years as the probability of retainment could itself be affected by a worker death.

I use the following difference-in-differences framework to estimate treatment effects on incumbent workers:

$$\begin{aligned} y_{ijk} &= \alpha + \gamma_{ij} + \sum_{k=-3}^{5} \beta_k \times 1(\text{period}_k) \\ &\quad + \sum_{k=-3}^{5} \beta_k^{Treated} \times 1(\text{period}_k) \times \text{Treated}_{ij} + \epsilon_{ijk}. \end{aligned} \tag{18}$$

where $y_{ijk}$ denotes the outcome $y$ for incumbent worker $i$ at firm $j$ in year $k = t - d$ relative to the worker death occurring in year $d$. The model includes incumbent worker-firm effects which absorb unobserved heterogeneity across incumbent workers. As before, the model includes leads and lags around event time, $1(\text{period}_k)$ and the coefficients of interest are the $\beta_k^{Treated}$. The model is estimated as a weighted regression in which each incumbent-worker observation is weighted by the inverse of the total number of incumbent workers at a firm in $d$ so that all worker deaths have equal weight and treatment effects can be readily compared between (17) and (18). As before, standard errors are clustered at the firm level. Short-run and average long-run treatment effects are also defined analogously as $\beta_1^{Treated}$ and $\frac{1}{5}\sum_{k=1}^{5} \beta_k^{Treated}$, respectively. Finally, the identification assumption also remains the same and requires that worker deaths are exogenous conditional on the covariates included in the model.

**Heterogeneity of Treatment Effects.** In order to assess heterogeneity in the treatment effects, I estimate variations of the econometric models in (17) and (18) that include interactions between the post-period treatment effects, i.e. the interaction of $1(\text{period}_k)$ and treatment status, and some covariates, e.g., the skill level of the deceased worker. Whenever

such interaction terms are included, the model also include a set of interactions of the baseline period effects, $1(\text{period}_k)$, with the relevant covariate.

# 5 Results

My main results show that worker deaths lead to increases in the wages and retention rates of the remaining incumbent workers by about 0.6% in the short run and the positive effects persist for several years. The average effects shroud substantial heterogeneity: positive effects are concentrated among incumbent workers in the same occupation group as the deceased. For deaths of high-skilled workers and managers, I estimate *negative* effects on the wages of workers in other occupation groups. Finally, I document that firms in thicker markets for skill hire more externally and change wages of incumbents by less in response to a worker exit.

Taken together, my results therefore show that firms face frictions in replacing workers externally as idiosyncratic shocks to the firm's labor supply affect the firm's labor demand for the remaining workers. Based on the pattern of effects inside the firm, coworkers in the same occupation appear to be substitutes, while high-skilled workers and managers appear to be complements to workers in other occupation groups. Finally, the heterogeneity in the effect by labor market thickness suggests that replacement frictions arise when worker's human capital is firm-specific.

## 5.1 Effects of Worker Exits on Firm Employment and Hiring

To set the stage for the main analysis, the estimation of effects on incumbent wages, I first document that worker deaths constitute a shock to a firm's labor supply and affect employment and hiring. Following a worker death, employment in treated firms is temporarily lowered. Hiring rises sharply and some hiring occurs in occupations other than the one of the deceased, thus providing first evidence consistent with the notion that workers that can be hired externally may not be perfect substitutes for insiders.

Figure 1 shows that worker deaths are a shock to the firm's labor supply. I show the effect on the probability of employment of the actual and placebo deceased worker at treatment and comparison group firms in red. The trend in the pre-period is flat; there is a sharp drop after the death of the worker in the treatment group between $k = 0$ and $k = 1$. If there were no turnover of placebo deceased workers in the comparison group, the drop would equal $-1$. If there was so much turnover that no worker remained with the same firm for more than a year, the drop would equal 0 as all placebo deceased workers in the comparison group would

have left the firm after a year. In the data, the drop is closer to -1 at -0.865 (se 0.0027) in the first post-death period and is equal to -0.564 after five years. Stated differently, the death of a worker is a sharp shock to a firm's labor supply that decreases in magnitude over time as workers that do not die have a positive probability of leaving the firm over time.

The blue series in Figure 1 documents that the shock to the labor supply of an individual worker due to death affects employment at the firm in the short-run. Employment drops by -0.294 (se 0.034) in the first period after death. The gap is substantially smaller and indistinguishable from zero in the subsequent periods. If workers were immediately replaced externally, the effect in the first period would equal zero as firms could hire a replacement worker instantaneously.

Figure 2 shows that hiring of new workers rises sharply following a worker death but the magnitude of the effect on hiring is substantially smaller than a one-for-one external replacement. In the first post-death period, $k = 1$, firms hire on average 0.417 (se 0.026) new workers and an additional 0.240 and 0.090 workers in the subsequent two periods. Figure 3 decomposes the hiring effect into two components: the hiring of workers who work in the same 5-digit occupation as the deceased and hiring of workers in other occupations. About a quarter of the hiring response to worker exits is due to hiring in other occupations. This finding is consistent with the notion that firms do not always hire perfect substitutes to replace workers.[50]

To give an overview of the employment effects, I decompose the labor supply shock due to a worker death into three effects: hiring, retention, and a residual employment effect. Figure 4 shows these three effects in the short run, $k = 1$, relative to the magnitude of the labor supply shock. As shown in Figure 1, the direct labor supply shock due to a worker death equals 0.865 in $k = 1$. Relative to this magnitude, the effect on the firm's hiring of new worker equals 48.2% and the employment effect equals 33.9%, and the effect on the retention of incumbent workers is 17.9%. As worker exits affect the retention of incumbents, the next section will analyze the effects on incumbents wage and employment outcomes more closely.

[50] A potential concern is that an effect on hiring of workers in other occupations could be a purely spurious finding due to misreporting of workers' occupations. However, part of the hiring response to a worker death is the hiring of apprentices (long-run effect: 0.025, $p < 0.01$). While the magnitude of the effect is small, it documents that in some cases, firms respond to a worker exit due to death by hiring a new worker in the ultimate "port of entry" (Doeringer and Piore 1966), i.e. as an apprentice.

## 5.2 How Do Worker Exits Affect Incumbent Worker Wages and Employment Outcomes?

This section presents the average effects of worker exits due to unexpected death on incumbents and shows that such exits raise incumbent worker wages. The finding of nonzero wage effects is hard to reconcile with completely fluid labor markets and perfect substitutability of outsiders and incumbent workers and implies that firms face frictions in replacing worker externally. Interpreted through the lens of the intrafirm bargaining models in Section 2, the positive effects suggest that coworkers are, on average, closer substitutes than workers that can be hired externally.

Figure 5 documents the dynamics of the treatment effect on the earnings of incumbents.[51] The upper panel uses individual incumbent workers' labor earnings as the outcome variable and documents a statistically significant increase of 174.47 EUR (SE 37.6 EUR) in the first post-death period, $k = 1$. Compared to incumbent workers' average yearly earnings of 27,856 EUR in $k = -1$, this corresponds to an increase of about 0.6%. Wages of incumbent workers in the treatment group stay elevated for several years and remain statistically significant as long as the fourth post-death period, $k = 4$.

The lower panel of Figure 5 provides a similar picture based on a specification which uses the sum of earnings of all of the deceased worker's coworkers as the outcome variable. The sum of coworker earnings increases by 1,791.14 EUR (SE 406.74 EUR) in the year following a worker death. The treatment effect then gradually decreases over time and remains statistically significant for the first three post-death periods. The total effect on the sum of coworker earnings in the first five post-death years is 5,660 EUR so that the increase in incumbent worker earnings corresponds to about 18% of the deceased worker's average annual earnings (31,500 EUR in $k = -1$).

For both outcome variables, the pre-trends leading up to the worker death are small and statistically indistinguishable from zero which suggests that the outcomes in the comparison group can be used to gauge what would have happened in the treatment group had the worker death not occurred. As wages are reported as a yearly average for a typical worker, the outcomes in period $k = 0$ could be affected by a worker death which occurs between July 1 of $k = 0$ and June 30 of $k = 1$ (see Section 4.2). Indeed, the treatment effects are statistically significant and positive in period $k = 0$ for both outcome variables. However, the nonzero effect in $k = 0$ is not a violation of the parallel trends assumption as the positive effect in $k = 0$ is entirely driven by worker deaths that occur in the same calendar year as

[51]Incumbent workers are defined as full-time coworkers of the deceased or placebo deceased in the year before death and remain in the incumbent worker group regardless of whether they stay at the same firm or not. All estimates are also reported in Table D.2 in the Appendix.

wage measurement in $k = 0$ and is not affected by deaths that occur in the first half of the subsequent calendar year. In Figure 6, I show incumbent wage effects in $k = 0$ and split the analysis by the calendar time quarter of death of the deceased worker. The results clearly document that the positive treatment effects in $k = 0$ are driven by deaths that occur in the third and fourth quarter of the same calendar year. In contrast, deaths that occur in the first two quarters of $k = 1$ are associated with substantially smaller and statistically insignificant wage effects in $k = 0$. The fact that deaths in the first quarters of the following calendar year do not have a statistically detectable effect on incumbent worker wages in the previous calendar year supports the parallel trends assumption and suggests that the worker deaths under study are unexpected even at a relatively short horizon.

**Magnitude:** I offer three empirical benchmarks to gauge the magnitude of the average effects on incumbent wages. From the workers' perspective, one benchmark for the wage effects is the standard deviation of wages. In the period $k = -1$ before a worker death, the standard deviation of wages of incumbent workers in the sample is 13,600 EUR so that the average increase of 174.47 EUR in the treatment group roughly corresponds to 1.3% of a standard deviation. A second benchmark from the workers' perspective is a comparison with firm effects (see, e.g., Abowd, Kramarz, and Margolis, 1999). Recent research by Card, Heining, and Kline (2013) has documented the increasing importance of firm heterogeneity in contributing to the rise in inequality in Germany. Compared to the standard deviation of 0.189 of firm effects in Card, Heining, and Kline (2013), the short-run treatment effect of 0.6% in my sample corresponds to roughly 3% of a standard deviation.[52]

Third, to provide a benchmark of the magnitude from the firm's perspective, I compare the treatment effect on the sum of incumbent worker wages to estimates of standard turnover costs. The comparison is motivated by theory: in a modified search and matching model that relaxes the standard assumption of single-worker firms or constant returns to scale, wage effects due to changes in employment enter firms' labor demand completely analogous to turnover costs (see Cahuc, Marque, and Wasmer, 2008, and Section 2.3). In absolute terms, the estimate of wage effects of a worker exit of 5,660 EUR—over a five year horizon—is of the same order of magnitude as estimates of turnover costs in Villena-Roldan (2012) who estimates that firms spend about US$4,200 per worker on recruiting based on data from the 1997 National Employer Survey (NES). In relative terms, the estimated magnitude of 17% of the deceased worker's average annual earnings is comparable in magnitude to the estimates

[52]Note that the outcome variable in Card et al. (2013) is the logarithm of real wages. The standard deviation of 0.189 that I report is the average of four estimates of 0.159, 0.172, 0.194, and 0.230 that Card et al. (2013) estimate for four time periods from 1985 to 2009. Note that my comparison of this standard deviation to the treatment effect in my sample is rough and approximate as I use wage levels rather than log wages as outcome variable and transform the treatment effect by dividing it by the comparison group mean to arrive at a relative effect of 0.6%.

in Boushey and Glynn (2012) who report a median estimate of turnover costs of 21 percent of an employee's annual salary based on a survey of 27 case studies.[53] The fact that the wage effects are of the same order of magnitude as consensus estimates of turnover costs—which are thought to be the source of frictions that firms face in replacing workers in standard search and matching models—documents that the wage effects estimated here indicate the presence of a quantitatively important frictions that firms face in replacing workers.

**Additional Incumbent Worker Outcomes:** In Figure 8, I document treatment effects on several employment outcomes which—in combination with the effects on wages—imply that worker exits lead to positive, firm-specific labor demand shocks for incumbent workers.[54] Turnover of incumbent workers in treated firms is lower: each incumbent worker has, on average, about a 0.5 percentage points higher probability of remaining employed at the same firm. Incumbents in the treatment group are, however, not more likely to be employed at all as the long-run effect on full-time employment is zero. In sum, worker deaths lead to, on average, positive firm-specific labor demand shocks for incumbent workers who, as a consequence, are more likely to remain employed at the same firm and less likely to take up employment with other firms. The treatment effect on the probability of part-time employment is a precisely estimated zero. Even though the data do not contain fine-grained measures of working hours, the absence of an effect on part-time work status suggests that the intensive margin hours response may be limited.

Further evidence on the treatment effect on the probability of a promotion also suggests that factors other than only changes in working hours underlie the positive treatment effects on earnings as workers in the treatment group have a higher probability of being internally promoted (see Figure 8). To obtain a proxy for internal promotions, I first calculate average wages at the 5 digit occupation level by drawing on a 10% sample of individuals from the IEB and regress individual's log wage on occupation dummies and individual fixed effects. I use the estimated occupation effects to classify changes of occupation as a promotion when a worker changes into an occupation with a higher average salary. Specifically, the outcome variable "Promotion" is equal to 1 when a worker is employed at the same firm as in $k = -1$ and works in an occupation with a higher average wage as the occupation in $k = -1$. While the treatment effects are positive and small in absolute magnitude at 0.08% and 0.12% in the short- and long-run, respectively, the baseline probability of workers in the comparison group group being promoted is also very small at 0.8%.[55] The probability of an internal promotion of an incumbent worker in the treatment group is therefore 10 percent higher than in the

[53] See also Manning (2011), Table 2, for an overview of estimates of hiring costs.

[54] See also Appendix Table 4.

[55] The baseline magnitudes are relatively small as they cannot reflect promotions within an occupation which richer, firm-level personnel records may report.

comparison group.

Figure 7 shows that the positive shift in the wage distribution in the treatment relative to the comparison group is due to higher fractions of individuals receiving positive nominal earnings increases. The outcome variable in Figure 7 is a binary measure of whether an incumbent worker has experienced a nominal earnings change of more than $X\%$ from period $k = -1$ to period $k = 1$, i.e. $1((y_1 - y_{-1})/y_{-1} \cdot 100 > X)$ where the subscript of $y$ denotes the period $k$ relative to a worker death. Each point estimate is based on a separate regression of this outcome variable on treatment status. The largest shift of nominal earnings changes occurs at $X$ =10% increase with a treatment effect implying that the fraction of incumbent workers who experience a 10% increase in their nominal earnings from period $k = -1$ to period $k = 1$ is 1.1 percentage points higher in the treatment group.

**Implications:** The results presented in this section—in particular the positive wage and retention effects—are hard to reconcile with frictionless labor markets and perfect substitutability of insiders and outsiders. Interpreted through the lens of the models in Section 2, the results reported here imply that workers inside the firm are, on average, closer substitutes to one another than workers that can be hired in the external labor market. When a worker exits from the firm, the firm raises the wages of its incumbent workers to keep them from accepting jobs at other firms.

### 5.3 Heterogeneity of Incumbent Wage Effects: The Role of Occupations and Skills

Having established that worker exits affect wages of incumbent workers, I next assess heterogeneity in the effect across across occupational boundaries and skill levels. Positive wage effects are concentrated among incumbent workers in the same occupation group as the deceased. In contrast, deaths of high-skilled workers and managers have negative effects on the wages of workers in other occupations, suggesting that these workers are complements to other workers inside the firm.

In a first step, I estimate the effect on the wages of incumbent workers in the same occupation group as the deceased and on incumbents in other occupation groups.[56] Figure 9 shows that the effects of a worker death on incumbent workers are statistically significant and positive at 239.91 EUR in the short run and 171.86 in the long run (see also columns (1) and (2) of Table 5). In contrast, the average effect on workers in other occupation groups is about 75% smaller and statistically not significant. The results support the premise that the

[56] I classify workers as being in the same or in other occupation groups based on their 1-digit occupation in the year before death. The 1-digit occupation groups classify occupations based on the broad thematic focus of the work, e.g., production and manufacturing vs. accounting.

research design can identify the within-firm substitutability of workers inside the firm to the extent that occupation is a natural measure of similarity and substitutability of workers.

Next, I analyze heterogeneity in the effect by the skill level of the deceased worker based on three measures of skill. A core assumption of models of human capital inside firms (see, e.g., Lucas, 1978, Rosen, 1982) is that high-skilled workers or managers are complements to other workers inside the firm. Insofar as worker deaths identify the substitutability or complementarity of workers, these models predict negative effects of deaths of high-skilled workers and managers on other workers. I analyze effect heterogeneity for three measures of worker skill and find evidence for complementarities of skilled workers along all three dimensions: (1) the skill intensity of the deceased worker's occupation, (2) the education level of the deceased worker, and (3) the managerial status of the deceased worker.

First, I analyze heterogeneity based on the skill intensity of the deceased's occupation and find negative effects of worker deaths in high-skilled occupations (see panel (A) of Figure 10).[57] The reason for focusing on the skill intensity of the occupation level rather than on education levels directly is that the modal education level is an apprenticeship training and such apprenticeship programs differ widely in the level of skills of the targeted occupations. To measure the skill level of an occupation I calculate the average years of education at the 5-digit level based on a 20% sample of IEB biographies. I then classify occupations as low-skilled when the average years of education are below the 20th percentile, as medium-skilled for occupations between the 20th and 80th percentile, and as high-skilled for occupations above the 80th percentile of average years of education in the sample of deceased workers. As panel (A) of Figure 10 reveals, deaths of workers in high-skilled occupations lead to statistically significant negative effects on the wages of incumbents in short run (point estimate -301.78 EUR, se 134.05 EUR). In the longer run, the point estimates remain negative but are not statistically significant. For deaths in low-skilled occupations, the wage effects on workers in other occupations are close to zero.

As a second skill measure, I focus on education levels directly and find a similar pattern of effects (see panel (B) of Figure 10).[58] I categorize deceased workers' education levels as low, medium, or high based on whether they have no apprenticeship training (low), an apprenticeship training (medium), or further formal education (high).[59] Since the overwhelming majority of workers in my sample have an apprenticeship training, the effects of deaths of workers in the low- and high-education group are imprecisely estimated. The point estimate for the effects of worker deaths in the high education group on workers in other occupations is large and negative at -447.42 EUR but only marginally signicant ($p < 0.1$), providing

[57]See also Table 5 for additional information.

[58]See also Table 6for additional estimation results by education levels.

[59]Further formal education refers to a university entrance exam (*Abitur*) or a college degree.

evidence suggestive of complementarities.

As a third dimension of skill, I explore heterogeneity in the deceased worker's managerial status and find that deaths of managers are associated with negative effects on the wages of incumbent workers in other occupation groups (see panel (C) of Figure 10 and Table 7). I proxy manager status of the deceased worker based on their occupation. Deceased workers are classified as managers if they worked in an occupation characterized by managerial, planning and control activities, such as operation and work scheduling, supply management, and quality control and assurance.[60] Based on this distinction, I find that deaths of workers in non-manager occupations are associated with positive effects on incumbent wages. In contrast, the effect of manager deaths on incumbents in other occupations is negative and large (short-run effect: -338.31 EUR, se 149.12).

**Implications:** Both the findings of positive average effects and the finding of negative effects of manager and high-skilled worker deaths are consistent with the bargaining models in Section 2 as they allow for positive and negative wage effects depending on the degree of substitutability of different worker types in the firm's production function. The canonical model for the role of human capital in firms (see, e.g., Lucas, 1978) posits a two-factor production function with decreasing returns of low-skilled labor and complementarities between high- and low-skilled labor:

$$Y = h \cdot F(L),$$

where $Y$ is the firm's output, $h$ the manager's or high-skilled worker's human capital, and $F(L)$ a concave, increasing function of the number of low-skilled workers (see applications in organizational economics and growth in, e.g., Rosen, 1982, Murphy, Shleifer, and Vishny, 1991, and Gennaioli et al., 2013). While low-skilled workers are substitutes in this production function, high-skilled workers' human capital raises the productivity of other workers inside the firm. Importantly, output is more sensitive to managerial human capital than to lower-skilled workers' human capital.

The empirical findings from this section are consistent with the predictions from the Lucas (1978) model as it predicts positive wage effects of lower-skilled work deaths on incumbent workers in similar occupations and negative wage effects of manager or high-skilled worker deaths on the wages of incumbents in other occupations. My results suggest a directed complementarity as high-skilled worker deaths lower wages of other workers but lower-skilled worker deaths do not have a symmetric, negative effect on the wages of workers in other

[60]Specifically, I define occupations that requires "complex specialist activities" (requirement level 3) or "highly complex activities" (requirement level 4) based on the 2010 Classification of Occupations as managerial occupations. See *Klassifikation der Berufe 2010, Band 1: Systematischer und alphabetischer Teil mit Erläuterungen, Bundesagentur für Arbeit.*

occupations. The Lucas (1978) model's feature that output is sensitive to managerial human capital but less so to lower-skilled worker's human capital would predict just that. Taken together, the results from this section support imperfect substitutability of high-skilled workers with workers in other occupations and provide micro-evidence consistent with how more aggregate changes in the supply of skilled workers affect the wage structure (see, e.g., Katz and Murphy, 1992, and Dustmann, Ludsteck, and Schönberg, 2009).

## 5.4 Heterogeneity by Worker-Level Measures of Human Capital Specificity

I investigate treatment effect heterogeneity by tenure of the deceased worker and by the specialization of the deceased worker's occupation. Tenure is a natural measure of specific human capital: first, in models of on-the-job training, human capital specificity increases with tenure, e.g., due to on-the-job training or learning by doing (Becker, 1962). Second, in search and matching models (Jovanovic, 1979a,b), worker-firm matches last longer for workers who have a high firm-specific productivity or match quality. Consistent with both theoretical considerations, an extensive literature documents that longer-tenured workers command higher wages and experience larger earnings losses in case of displacement (see, e.g., Topel, 1991).[61]

Columns (1) and (2) of Table 8 present treatment effects separately by tenure of the deceased worker and document larger point estimates of the wage effects for deaths of longer-tenured workers. Short, medium, and long tenure indicate tenure between one and five years, five to ten years, and more than ten years, respectively. The effects for long-tenured workers are 50 to 100% larger than the wage effects for shorter-tenured workers. While the point estimates are not estimated precisely enough to reject equality of the coefficients, the pattern of results is consistent with the hypothesis that workers become harder to replace externally with tenure.

In a next step, I assess treatment effect heterogeneity based on a measure of specialization at the occupation level. To proxy specialization, I rely on a measure used in Bleakley and Lin (2012) who classify occupations as relying on more specific skills when the returns to experience are high. Intuitively, this proxy can be interpreted as measuring the importance of occupation-specific capital (see, e.g., Shaw, 1984, Shaw, 1987, and Kambourov and Manovskii, 2009). Using a different sample of IEB records, I calculate returns to experience based on Mincer equation estimated separately for each 5-digit occupation. I then use

[61]See also recent evidence in Shaw and Lazear (2008) who document that worker *productivity* increases with tenure.

the estimated occupation-specific returns to experience to classify occupations into three categories: occupations with returns to experience below the 20th percentile are classified as low-specialization occupations, occupations with returns to experience between the 20th and 80th percentile are classified as medium-specialization, and occupations above the 80th percentile of returns to experience as high specialization occupations.

Columns (3) through (6) of Table 8 show treatment effects on incumbent worker wages by occupational specialization of the deceased worker. The baseline effects of specialization appear to be non-monotonic as the largest effects are found in the medium-specialization group. However, as in the case of heterogeneity by occupational skill and education levels (see Tables 5 and 6), the average effects shroud heterogeneity in the effect on coworkers in the same occupation as the deceased and on coworkers in other occupations. In columns (5) and (6), I document that treatment effects on incumbent worker wages rise in magnitude with the specialization of the deceased worker's occupation and that deaths of workers in highly specialized occupations lead to negative effects on the wages of incumbents in other occupations.

## 5.5 Do Thick Markets Make Workers More Replaceable?

The results in the previous subsection suggest that human capital specificity may lead to imperfect substitutability of incumbents and outsiders. In this section, I investigate this mechanism further and assess whether incumbents and outsiders are more substitutable in thick labor markets with an agglomeration of workers with the relevant skills. I find that incumbents' wages respond less and external hiring responds more to a worker death when the external labor market in the deceased's occupation is thick, lending further support to the hypothesis that workers are harder to replace when their human capital is more firm-specific.

The investigation builds on and tests Lazear's (2009) idea that the specificity of human capital depends on the thickness of the market. If human capital were either completely general or completely specific to a firm, external market thickness—e.g., an agglomeration of workers with relevant skills in the external labor market—would not reduce hiring frictions as newly hired workers would not have specific human capital. However, if human capital is thought of as a combination of general skills and is more specific the more idiosyncratic a firm's preferred skill combination (Lazear, 2009), firms located near other firms that rely on similar types of labor may be able to replace insiders with specialized skills more easily. Stated differently, in Lazear (2009) the human capital of workers in a firm that relies on occupation- or industry-specific skills is more firm-specific if the external market for those skills is thin.

By testing whether incumbent wages are more responsive to worker deaths in thin labor

markets, my research design also sheds light on a particular labor pooling channel. Going back to Marshall (1890), economists have hypothesized that firms benefit from clustering near other firms which employ workers with similar skills so that labor market thickness could act as a force of agglomeration.[62] Moretti (2011), for instance, describes a potential benefit of labor market thickness for firms noting that "thick labor markets reduce the probability that a firm can't fill a vacancy, following an idiosyncratic shock to the labor supply of an employee" and points out that "this argument applies particularly to workers with specialized skills". As my research design analyzes the effects of idiosyncratic shocks to workers' labor supply, I can directly assess the importance of this particular labor pooling channel.

I operationalize this test by assessing heterogeneity in the effect of worker deaths by measures of labor market thickness and occupational specialization. To proxy labor market thickness, I measure the agglomeration of workers in the deceased's occupation in the local labor market. To delineate local labor markets, I focus on commuting zones, which are defined as clusters of districts characterized by a large commuter flow within and a small commuter flow across zone boundaries. Figure (11) shows the 50 German labor market regions based on the categorization in Kropp and Schwengler (2011) that I follow.[63] I measure thickness at the 5-digit occupation × commuting zone level as the share of employment in the relevant occupation in the commuting zone relative to the overall share of employment in that occupation.[64] I then classify 5-digit occupation × commuting zone cells as a thin or thick labor market based on a median split. As an intuitive example, the labor market for mechanical engineers in Munich will be described as thick based on this measure if Munich has a high share of mechanical engineers relative to the overall share of mechanical engineers in the German labor market. Importantly, the empirical exercise that I implement relies on observational variation in labor market thickness so the results cannot be interpreted as causal estimates of the effect of labor market thickness. The goal is instead to assess to what extent the cross-sectional patterns predicted by models of labor market thickness hold up empirically.

[62] See, e.g., Helsley and Strange (1990) and Rotemberg and Saloner (2000) for formalizations of labor market pooling as an agglomeration force. Ellison, Glaeser, and Kerr (2010) provide evidence on the role of labor market pooling relative to other Marshallian agglomeration forces. A large literature assesses from the workers' perspective whether measures of match quality are higher in denser or thicker labor markets (see, e.g., Wheeler, 2008, Bleakley and Lin, 2012, Geel, Mure, and Backes-Gellner, 2011, and Harmon, 2013). My research design complements this line of work by shedding light on whether thicker labor markets allow firms to substitute more flexibly between incumbents and outsiders.

[63] An advantage of the Kropp and Schwengler (2011) categorization is that the classification into labor market regions is relatively stable over time.

[64] Formally, I calculate labor market thickness for 5-digit occupation $o$ in labor market (commuting zone) $l$ in year $d$ as $T_{old} = \frac{\sum_{o' \in \mathcal{O}} \frac{e_{old}}{e_{o'ld}}}{\sum_{o' \in \mathcal{O}} \frac{e_o}{e_{o'}}}$, where $e_{old}$ denotes employment in occupation $o$ in labor market $l$ in year $d$ and $e_o$ denotes total employment in occupation $o$ averaged over the sample period.

In thick labor markets, incumbent wages respond less to a worker death and the differential is particularly pronounced for specialized occupations. Figure 12 shows wage effects of worker deaths on incumbents in the same occupation group as the deceased, i.e. a group of workers that appear to be substitutes, by labor market thickness in the occupation of the deceased.[65] For the sample of all worker deaths, the point estimate for the wage effect is twice as large in thin compared to thick labor markets; the difference is marginally statistically significant ($p = 0.12$). If this difference in estimates is indeed mediated through an effect of labor market thickness on firms' ease of finding suitable workers in the external labor market, one would expect this difference to be more pronounced for workers with specialized skills (Moretti, 2011). To test this prediction, I focus on a sample of deaths of workers in occupations with an above-median return to occupational experience (see also the analysis in 5.4). The analysis reveals substantially larger differences between thin and thick labor markets with point estimates for the short-run wage effect of 487 EUR in thin and 161 EUR in thick labor markets, respectively; the difference in the effect between thick and thin labor markets is statistically significant ($p = 0.02$).

I find qualitatively similar patterns when using different measures of labor market thickness to estimate heterogeneity in the treatment.[66] Two other measures of thickness that I consider are employment density and the 3-digit industry agglomeration at the commuting zone level (defined analogously to the occupation-based agglomeration measure). For both of these measures, I find larger estimates of wage effects on incumbents in the same occupation in thin compared to thick labor markets. The differences in point estimates between thick and thin labor markets measured based on these two measures tend to be slightly smaller in magnitude than the difference by measures of thickness based on agglomeration of worker in the same occupation. One interpretation of this finding is that agglomeration in an occupation may be a better proxy for local labor market thickness. However, since the analysis relied on observational variation in thickness, the results do not consist definite evidence favoring one thickness measure over another.

To shed further light on the relevance of labor market thickness, I assess differences in the treatment effect on hiring across labor markets and find that firms in thick labor markets hire more externally in response to a worker death in specialized occupations. Figure 13 shows the treatment effect of a worker death on the number of new workers in the firm in $k = 1$. For the sample of all worker deaths, the point estimate is minimally larger in thick markets and the difference is statistically indistinguishable from zero. For deaths of workers in specialized occupations, I find a substantial differential between thick and thin

[65] See also Panel (A) of Table D.4.

[66] See panels (B) and (C) of Table D.4. In Table D.4, I also report differences by the local unemployment rate which I discuss in Section 6.

labor markets with approximately 50% more external hiring in thick markets ($p < 0.01$).

**Implications:** The findings that incumbents' wages respond less and external hiring in specialized occupations responds more to a worker death in thick labor markets for the deceased's occupation suggest that workers are more replaceable in thick labor market and that the substitutability of incumbent workers and outsiders decreases with human capital specificity.[67] My findings thus favor Lazear's (2009) view of firm-specific human capital—according to which the firm-specificity of workers' human capital decreases with labor market thickness—over a model with a dichotomous distinction between firm-specific and general human capital, which would not predict attenuated wage effects in thicker markets. From the perspective of the urban and agglomeration literature, the results presented here provide evidence that labor pooling reduces hiring frictions in specialized occupations.

# 6 Alternative Mechanisms

My results are in line with a model in which human capital specificity generates replacement frictions and worker deaths affect the firm's labor demand for incumbents. In the following, I investigate to what extent my results could be rationalized through alternative explanations.

First, I consider whether changes in the incumbent workers' amenity value of working at the firm could explain my findings. Prima facie, the positive wage effect could be driven by increases in incumbent workers' compensating differential (Rosen, 1974; Thaler and Rosen, 1976) of working at the firm: for instance, the perception of job hazards could have increased as a consequence of a death or the amenity value of working at the firm and interacting with coworkers is lower after having lost a colleague. Stated differently, deaths could be negative shocks to coworkers' firm-specific labor supply. Such labor supply-driven explanations could explain why wages increase on average in the treated group. However, they would also predict that workers' probability of staying with the firm decreases. The data, in contrast, reject this explanation as both the probability of staying at the firm and wages go up on average. The results therefore imply that shifts in firms' labor demand are indeed the driving force underlying the effects that I estimate.

A second class of alternative explanations builds on the idea that workers may rise through the ranks of the firm based on seniority, independent of their substitutability with outsiders.

[67] One could also interpret the finding that wage effects are attenuated in thicker markets as suggesting that workers in thick markets specialize more so incumbents in the same occupation are more likely to be complements rather than substitutes in thick markets (in line with this mechanism, Garicano and Hubbard (2007) and Garicano and Hubbard (2009) find that lawyers in thick labor markets specialize more). Taken together with the result that firms hire more in thick markets when a worker in a specialized occupation dies, the evidence presented here still suggests that workers can be replaced more easily in thick markets.

Such a mechanism could arise purely as a consequence of institutional rules or could be the result of an incentive structure set by the firm to solve an agency problem. Examples of such incentive structures include upward-sloping wage profiles (Lazear, 1979) that incentivize workers to put forth efforts earlier in the career to reap later rewards as well models of job assignment and promotions based on seniority which induce workers to invest in specific human capital (Carmichael, 1983, and Prendergast, 1993).[68] Such models of wages tied to seniority and job titles are consistent with the finding of positive effects on wages and retention rates insofar as worker deaths increase the remaining workers' seniority. However, my additional findings provide ancillary evidence that would not be predicted by such models. Models of incentive contracts designed to induce worker effort, in contrast to models based on human capital specificity, do not predict that effects are attenuated in thicker markets where firms have access to a larger pool of suitable workers on the external market. In addition, neither contracts to incentivize effort nor ones designed to induce specific investments—or other models in which wages rise monotonically with seniority—can account for the finding that wage effects are negative for workers in other occupations in the cases of deaths of highly skilled workers and managers. In contrast, a simple model with replacement frictions due to human capital specificity and imperfect substitutability of high- and low-skilled workers is consistent with all of the findings.

Finally, I explore whether the source of frictions that firms face in replacing insiders—which I attribute primarily to human capital specificity—could be a consequence of standard search costs. As recruiting is costly and it takes time to fill a vacancy, standard search frictions could lead to a temporary effect of worker exits on firms' labor demand for remaining incumbent workers. However, several pieces of evidence reject that a mechanism based on search frictions drives the positive labor demand effects on incumbents. Importantly, models of pure search frictions imply that wage effects vanish as soon as a new worker can be hired and employment is back to trend. In the data, I find no evidence for long-term effects of worker deaths on employment but estimate wage and retention effects that persist for several years.[69] My findings are therefore inconsistent with a model in which newly hired workers immediately become insiders upon hiring. Instead, they are consistent with a model in which it takes time for newly hired workers accumulate specific human capital and become

[68]Similarly, Kuhn (1988) shows that wages rising with seniority can also arise in a bilateral monopoly setup between a union with members who possess firm-specific skills and a firm with a production function with decreasing returns in homogenous labor (see Buhai, Portela, Teulings, and van Vuuren, 2014, for evidence). Gibbons and Waldman (1999), Oyer and Scott (2011), and Lazear and Oyer (2013) provide surveys of related literature.

[69]The absence of a longer-term effect on employment is consistent with evidence from the literature documenting that the mean duration of filling a vacancy is short, e.g., Davis et al. (2014) estimate a vacancy duration of 76 calendar days in Germany. See also evidence in Davis, Faberman, and Haltiwanger (2013) for the US.

substitutes to their longer-tenured coworkers, who—in a slight twist on Polanyi's paradox—may know more than they can or *are willing to* tell.[70] In an additional contrast to a model in which wage effects are purely driven by search frictions, I find no evidence that wage effects are attenuated in labor markets with a high number of unemployed jobseekers.[71] Taken together with my results on the role of labor market thickness and human capital specificity, the results therefore corroborate Marshall's (1890) conjecture that "the owner of an isolated factory, *even if he has access to a plentiful supply of general labour*, is often put to great shifts for want of some special skilled labour" [emphasis added].

# 7 Discussion

My paper speaks to several other strands of the literature that investigate how wages are set when human capital has specific components. Becker (1964) hypothesized that workers may not get any return on specific human capital as investments in specific human capital do not raise their outside option at other firms, but also noted that firms may increase wages of workers with specific skills to keep turnover low (see also Parsons, 1972, and Hashimoto, 1981).[72] My results document that workers reap some of the benefits of specific human capital and that, moreover, firms indeed respond to changes in the scarcity of specific human capital by paying higher wages to workers with similar skills when another worker dies. The results can also be interpreted through models in which workers and firms write contracts to protect specific human capital investments and can renegotiate the contract when the surplus from continued employment changes (see, e.g., MacLeod and Malcomson 1993a,b and Appendix C). In particular, the combined results that, on average, both wages and retention probabilities increase as a consequence of a worker death are consistent with such models which allow for renegotiation of wages when workers have a credible outside option that they prefer over the contracted wage. My results suggest that the surplus from continued employment of the remaining incumbent workers has, on average, increased after a worker death so that workers—who otherwise would have left the firm for outside employment—can

[70]The term Polanyi's paradox has recently been coined by Autor (2014) to describe philosopher Michael Polanyi's (1967) aphorism that "we can know more than we can tell". In the setting that I study, incumbent workers may not have an incentive to share tacit knowledge and skills with newly hired workers if doing so means that the newly hired workers become substitutes for incumbents. See also Lindbeck and Snower (1986) who explore the macroeconomic consequences of insider-outsider models of the labor market in which insiders have incentives not to cooperate with newly hired outsiders.

[71]See Panel D in Table D.4.

[72]A related literature investigates firms' and workers' incentives to invest in general human capital and finds that, contrary to the results in Becker (1964), firms may bear some of the costs of investment in workers' general human capital when there are frictions in the labor market (see, e.g., Acemoglu, 1997, and Acemoglu and Pischke, 1998).

renegotiate their wages.

In addition, my results relate to work by Manning (2003) who advocates a view of the labor market in which firms have monopsony power over workers. My results are consistent with a key assumption of such monopsony models, namely that the elasticity of labor supply to the firm is less than infinity, as my results reject a model in which firms can simply raise wages by an infinitesimally small amount to hire a suitable new worker externally. However, my results are harder to reconcile with the second key assumption of monopsony models that distinguishes such models from matching models (see, e.g., Mortensen and Pissarides, 1999). Specifically, monopsony models assume that firms set wages unilaterally and wages are set in advance (see p. 14-15 in Manning, 2003), while matching models assume that wages are determined through ex post bargaining after a worker and a firm have met. My results are therefore harder to reconcile with a strict interpretation of the monopsony perspective and instead lend support to matching models of the labor market with ex post bargaining and renegotiation of wages.

The empirical strategy in my paper relates to and builds on previous work that has used unexpected deaths as a source of variation. Most work along this vein has focused on deaths of exceptionally skilled individuals such as CEOs (Bennedsen, Pérez-González, and Wolfenzon, 2006), superstar scientists (Azoulay, Wang, and Zivin, 2010), or inventors (Jaravel, Petkova, and Bell, 2015) and documents negative effects on outcomes such as firm performance, collaborator productivity, and co-inventor productivity and earnings.[73] The negative average effects on productivity in Azoulay, Wang, and Zivin (2010) and on productivity and wages in Jaravel, Petkova, and Bell (2015) imply complementarities among collaborators and co-inventors—or increasing returns to the size or quality of the network—and are consistent with the negative point estimates that I find for deaths of highly-educated workers in my study. Finally, Isen (2013) aims to measure the marginal revenue product of workers by estimating the effect of worker deaths on firms' revenue and labor costs and presents evidence suggesting that workers' wages are lower than their marginal revenue product as revenue drops by more than labor costs in response to a death. In contrast to my study, Isen (2013) does not focus on the substitutability of a firm's workers with outsiders or the substitutability among incumbents.

At a broader level, my paper contributes to a literature that levies quasi-experimental variation in group composition to identify competition and spillover effects. Waldinger (2012) and Borjas and Doran (2014), for instance, investigate spillover effects between researchers in academia. Hayes, Oyer, and Schaefer (2006) find evidence for complementarities between

[73]See also recent work on the effects of entrepreneurs (Becker and Hvide, 2013) and scientists (Oettl, 2012) as well as Fadlon and Nielsen (2015) who analyze the effect of the death of a spouse on labor supply.

members of top management teams. Mas and Moretti (2009) and Cornelissen, Dustmann, and Schönberg (2013) leverage variation in the composition of teams of workers and find evidence of positive peer effects on productivity and wages, respectively. In the context of high-skilled immigration, Doran, Gelber, and Isen (2014) find that firms who win an H-1B visa in a lottery and hire an H-1B worker moderately reduce the employment of other workers at the firm. Lazear, Shaw, and Stanton (forthcoming) estimate large supervisor effects on worker productivity in a setting of technology-based services workers.

# 8 Conclusion

Analyzing shocks to firm-specific labor supply due to unexpected deaths of workers, I showed that firms face frictions in replacing workers externally as such worker deaths affect firms' labor demand for the remaining workers and documented that the effects on labor demand are larger when workers' human capital is more firm-specific. I argued that my findings can be interpreted through a simple model in which human capital specificity leads to imperfect substitutability of insiders and outsiders and provided evidence at odds with alternative explanations for my findings. My research design allowed me to shed light on the within-firm substitutability of workers which uncovered findings consistent with substitutability of workers in the same occupation as well as complementarities between high-skilled workers and workers in other occupations.

While my empirical analysis considered the effects of worker exits due to death, it seems plausible that my findings can be used to understand the effects of separations more generally, e.g., quits due to other reasons, such as the poaching of a worker by another firm. Clearly, my estimates—taken at face value—cannot be directly extrapolated to these other settings as circumstances and samples differ. However, one may reasonably expect that the same economic mechanisms that I identified will operate in other settings as well. Conceptually, my analysis therefore contributes to our understanding of the factors that make workers hard to replace on the external market. By showing that firms hire more externally and raise incumbent workers' wages by less in thick markets, my empirical analysis offers new insights into the black box of matching frictions in the labor market and suggests that human capital specificity lowers the fluidity of labor markets.

By documenting that firms face frictions in hiring workers with suitable human capital, my paper provides evidence lending support to models in which the supply of skilled workers affects firms' technology adoption due to matching frictions (see Acemoglu, 1996, 1997).[74] For

[74] Acemoglu (1996) develops a model with costly search and a strategic complementarity of investments in human and physical capital. See Chander and Thangavelu (2004) for an extension to a setup with general

firms considering whether to invest in a new technology that is complementary to specific skills, having access to a pool of appropriately skilled workers is vital. Such a capital-skill complementarity in combination with replacement frictions could therefore generate a pecuniary externality and social increasing returns to skills so that firms may only invest in a new technology when the pool of skilled workers is large enough. My paper provides evidence supporting two key assumptions underlying such models by showing that firms face frictions in replacing workers and that these frictions appear greater when human capital is specific. As a natural next step, more research is needed to examine how exogenous changes in the supply of workers with specific skills affect the adoption of new technologies or organizational structures by firms.

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and specific human capital. See, e.g., Griliches (1969) and Goldin and Katz (1996) for evidence on the complementarity between capital and skills.

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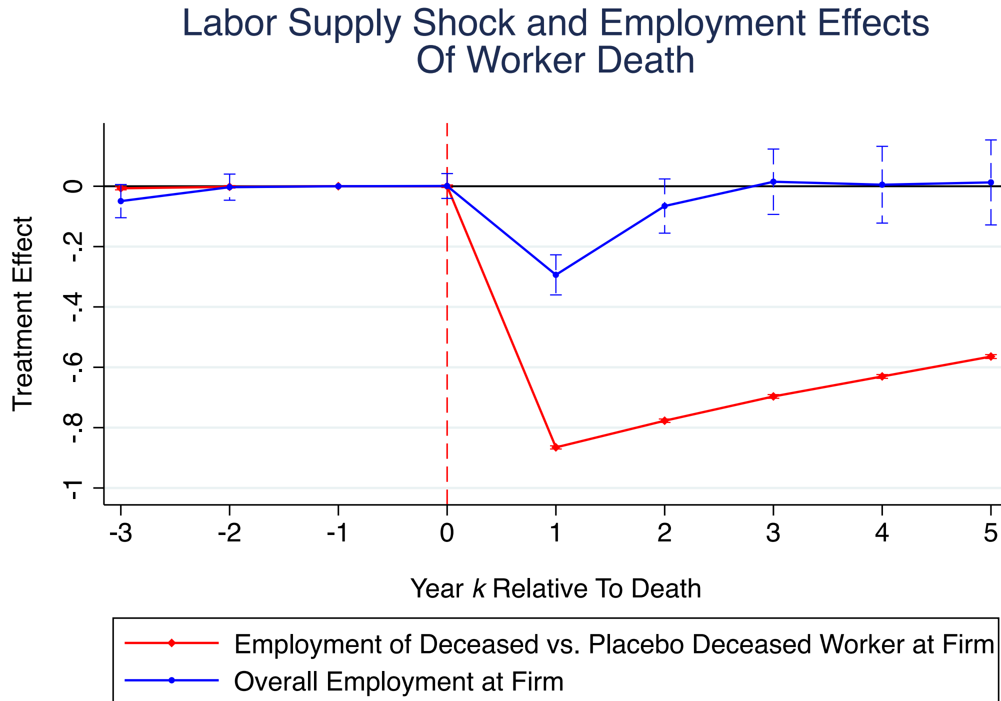
# Appendix

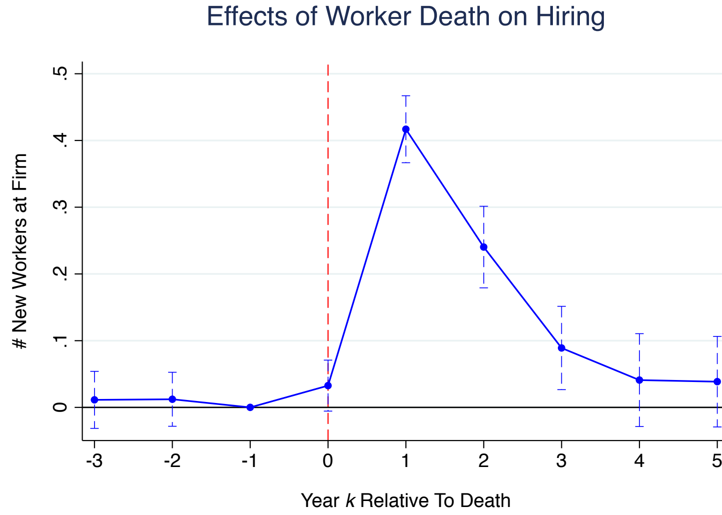
## A Figures

Figure 1: Labor Supply Shocks Due to Worker Deaths and Employment Effects



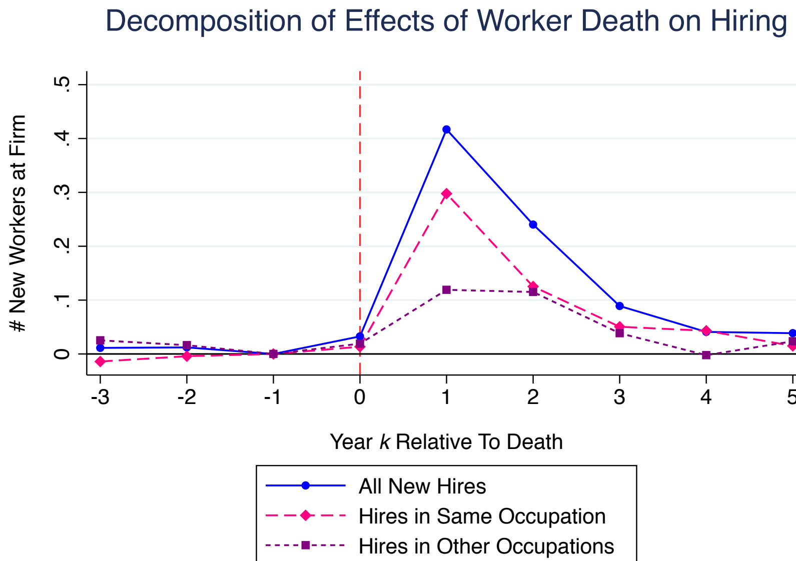

Note: The figure shows regression coefficients and associated confidence intervals for the difference between treatment and comparison group in a given year $k$ relative to the death of a worker in the treated firms, i.e. the $\beta_k^{Treated}$ from the difference-in-differences model in (17). The coefficient in $k = -1$ is normalized to zero. The outcome variable in the specification "Employment of Deceased vs. Placebo Deceased Worker at Firm" is an indicator variable that is equal to 1 if the deceased or placebo deceased is employed at the firm under study. If there were no turnover of placebo deceased workers in the comparison group, the treatment effect in year $k = -1$ would be -1. Due to turnover of placebo deceased workers, the drop is smaller than 1 in magnitude. The second outcome variable measures the overall employment at a firm. The comparison group mean for employment in $k = -1$ is 14.5. The dashed vertical lines denote 95% confidence intervals based on standard errors are clustered at the firm level.

Figure 2: Effects of Worker Deaths on Hiring



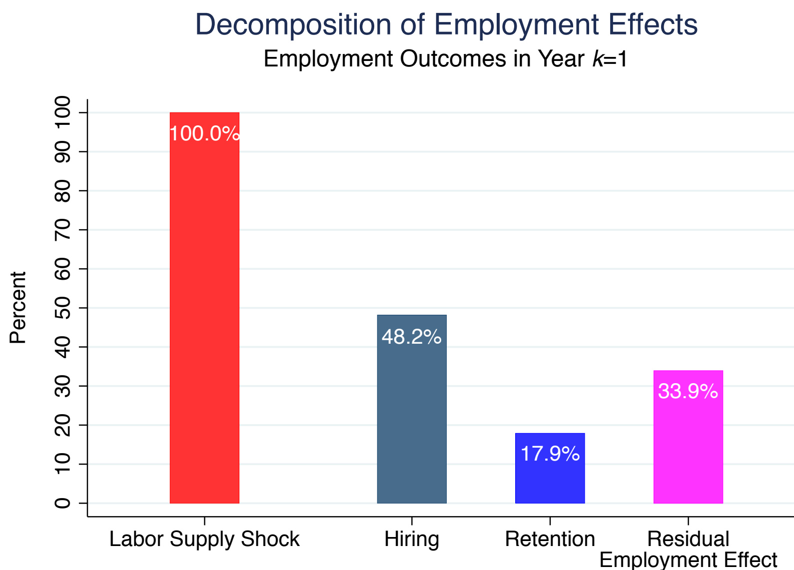

Note: The figure shows regression coefficients and associated confidence intervals for the difference between treated and comparison group firms in a given year $k$ relative to the death of a worker in the treated firms, i.e. the $\beta_k^{Treated}$ from the difference-in-differences model in (17). The coefficient in $k = -1$ is normalized to zero. The outcome variable is the number of new workers at the firm. The comparison group mean of the number of new workers in $k = -1$ is 2.2. The dashed vertical lines denote 95% confidence intervals based on standard errors are clustered at the firm level.

Figure 3: Decomposition of Effects of Worker Death on Hiring



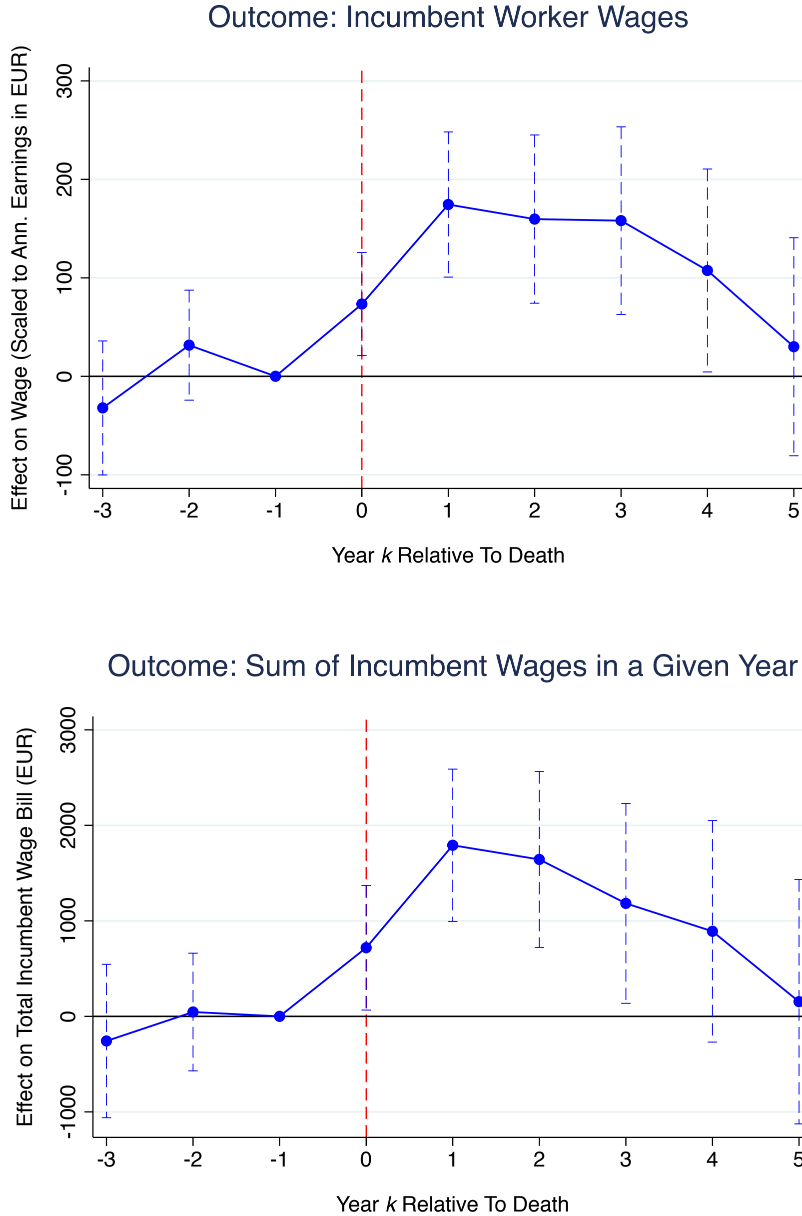

Note: The figure shows the treatment effect on hiring of new workers and decomposes the effect on total hiring (All New Hires) into hiring in the same 5-digit occupation as the deceased worker (Hires in Same Occupation) and hiring of workers into other occupations (Hires in Other Occupations). The treatment effect is normalized to zero in $k = -1$.

Figure 4: Decomposition of Employment Effects of Labor Supply Shock Due to Worker Death



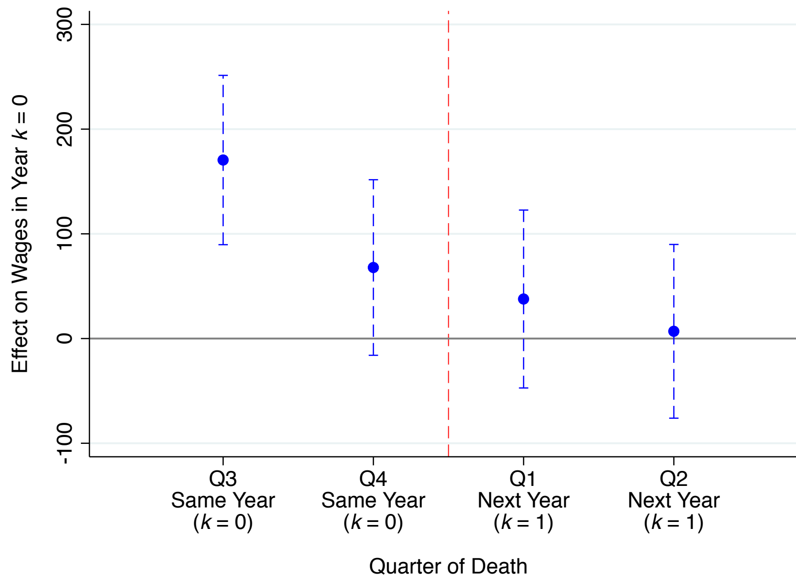

Note: The figure decomposes the labor supply shock in period $k = 1$ due to a worker death into hiring, retention, and a residual effect of the worker death on employment at the firm (accounting identity: Labor Supply Shock = Hiring + Retention + Residual Employment Effect). The labor supply shock is defined as the coefficient of the treatment effect on the outcome variable that indicates whether the deceased or placebo deceased is employed at the firm under study (see Figure 1). Hiring is the number of new workers at the firm in $k = 1$ (see Figure 2). Retention refers to the number of additional workers retained.

Figure 5: Effect of Worker Deaths on Incumbent Worker Wages



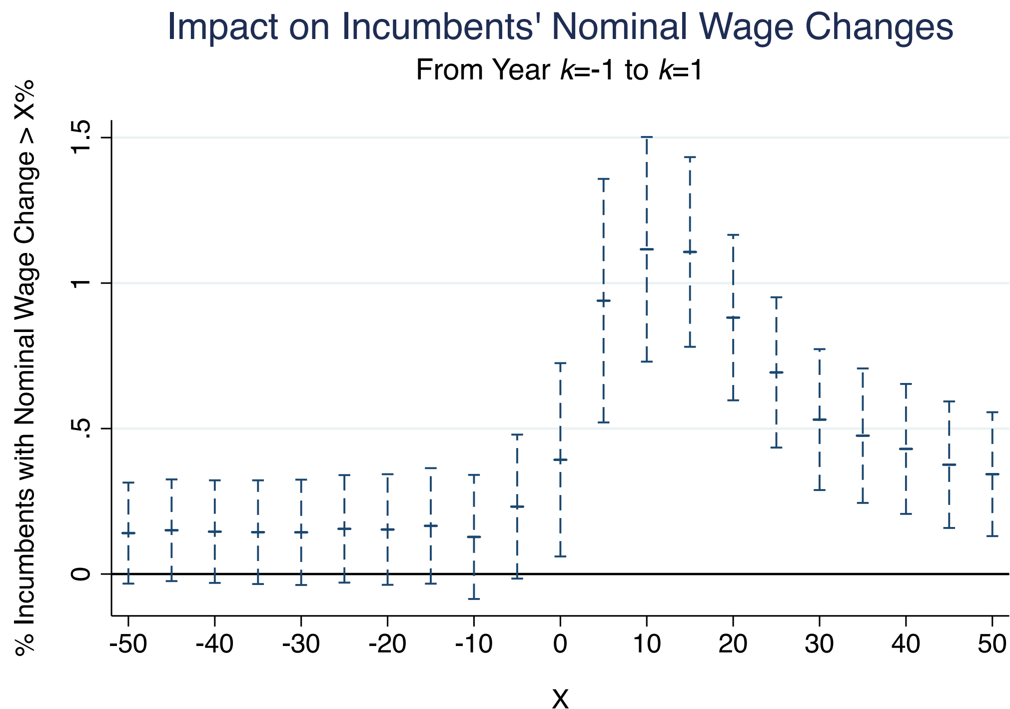

Note: The two panels display regression coefficients and associated 95% confidence intervals for the difference between incumbent worker in the treated and comparison group, i.e. the $\beta_k^{Treated}$ from equation (18). The coefficients in $k = -1$ are normalized to zero. In the first panel, the outcome variable is the wage of an incumbent worker (scaled to correspond to yearly earnings, CPI 2010). Incumbent workers are defined as full-time coworkers of the deceased or placebo deceased in the year before death. The comparison group mean of incumbent worker wages in year k $= -1$ is EUR 27,856 (SD 13,631) so that the EUR 174.47 increase in $k = 1$ corresponds to a 0.6% average wage increase. In the second panel, the outcome variable are the total earnings of the set of incumbent workers, i.e. the sum of the outcome variable in the first panel over all incumbent workers in a given year relative to death $k$. The solid vertical lines denote 95% confidence intervals based on standard errors clustered at the firm level. See Appendix Table D.2 for additional information.

Figure 6: Effects in on Incumbent Worker Wages in Year $k = 0$ By Quarter of Death



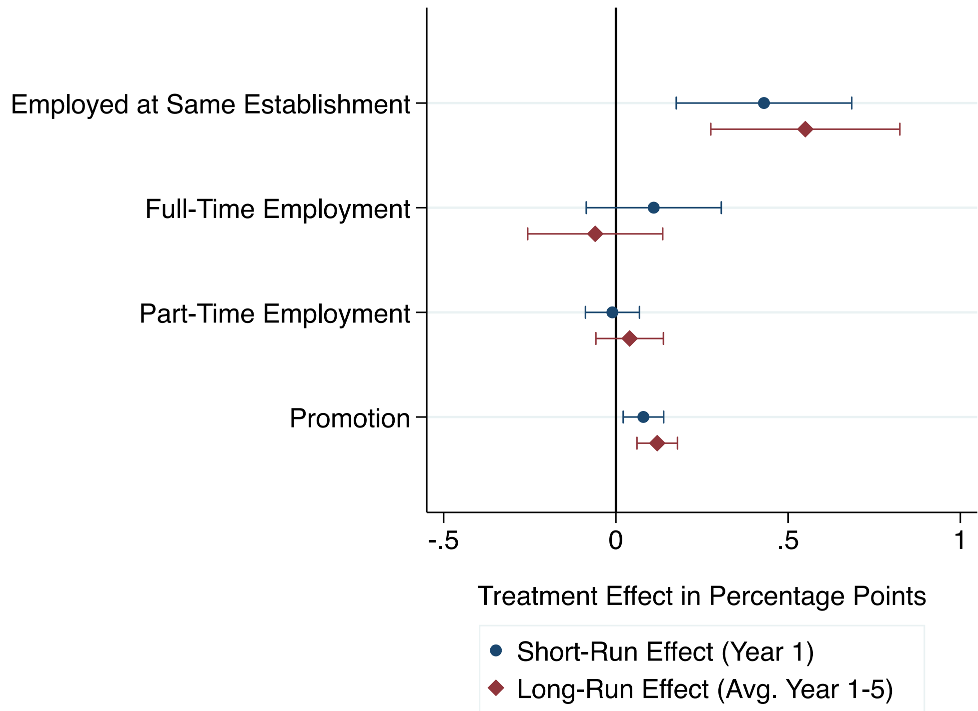

Note: The figure results of a difference-in-differences regression of wages in year k=0 on treatment status interacted with dummies for the quarter of death of the deceased worker in the treated group. The positive and statistically significant coefficients for wage effects in year 0 of deaths that occur in Q3 (July, August, and September) document that the positive wage effects in year $k = 0$ (see, e.g., Figure 5) are driven by deaths that occur in the same calendar year, as wages for most workers correspond to average wages calculated over a calendar year horizon so that deaths in, e.g., August will have an effect on average wages in that year. The figure also demonstrates that deaths in the first quarter of the following calendar year do not have a statistically detectable effect on incumbent worker wages in the previous calendar year. Vertical lines denote 95% confidence intervals. See also Table D.3.

Figure 7: Treatment Effect on Distribution of Incumbents' Nominal Wage Changes



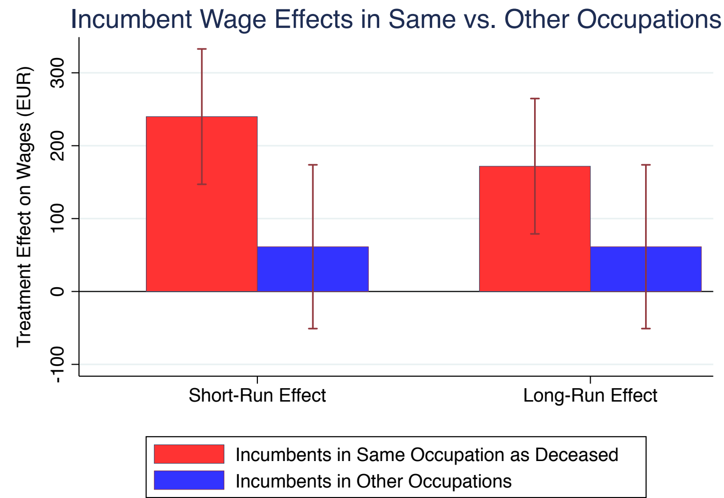

Note: The figure shows the treatment effect on outcomes variables that measure whether a worker has experienced a real wage change of more than $X\%$. Each point estimate is based on a separate regression of $1((y_1 - y_{-1})/y_{-1} \cdot 100 > X)$ on an indicator for treatment status, where the subscript of $y$ denotes the period $k$ relative to a worker death. As an example to illustrate the interpretation of these point estimates, a treatment effect of 1.1 for $X = 10\%$ implies that the fraction of coworkers who experience a 10% increase in their earnings from year $k$ =-1 to year $k$−1 is 1.1 percentage points higher in the treatment group. The lower and upper end of the vertical bars denote the 95% confidence interval for the treatment effect based on standard errors clustered at the firm level.

Figure 8: Treatment Effect on Incumbent Worker Employment Outcomes



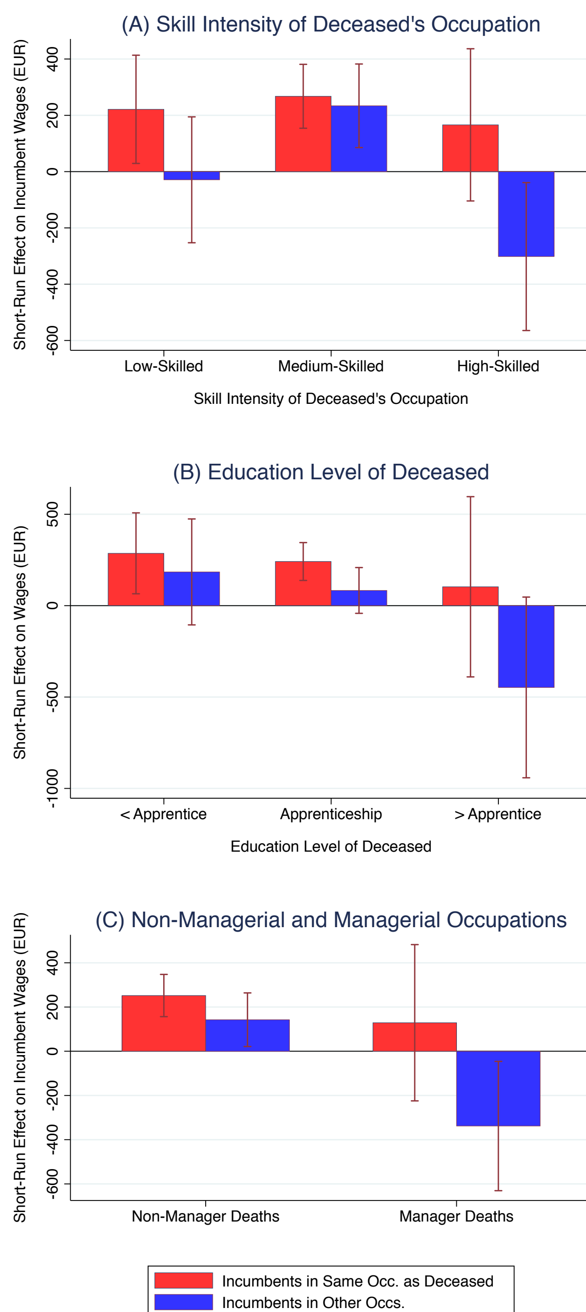

Note: The figure displays treatment effects on several employment outcomes of incumbent workers. The midpoints of each interval denote the point estimate of the treatment effect; the range of the interval corresponds to the 95% confidence interval of the treatment effect based on standard errors clustered at the firm level. Short-run effects refer to the treatment effects in year $k = 1$ post-death; long-run effects refer to the average treatment effects in years $k = 1$ through $k = 5$. Employed at the same establishment is an outcome variable that is equal to one when an incumbent worker is still employed at the same firm as in year $k = -1$. Full- and part-time employment are outcome variables that indicate the respective employment status independent of the establishment at which the individual is employed. Promotion is an outcome variable that is equal to 1 when an individual is employed at the same firm in an occupation with a higher average wage as the occupation he or she worked in in year $k = -1$. To calculate average wages at the 5 digit occupation level, I draw a 10% sample of individuals from the IEB and regress individual's log wage on occupation dummies and individual fixed effects. I use the estimated occupation effects to measure promotions. See Table 4 for additional information.

Figure 9: Incumbent Wage Effects in Same vs. Other Occupations



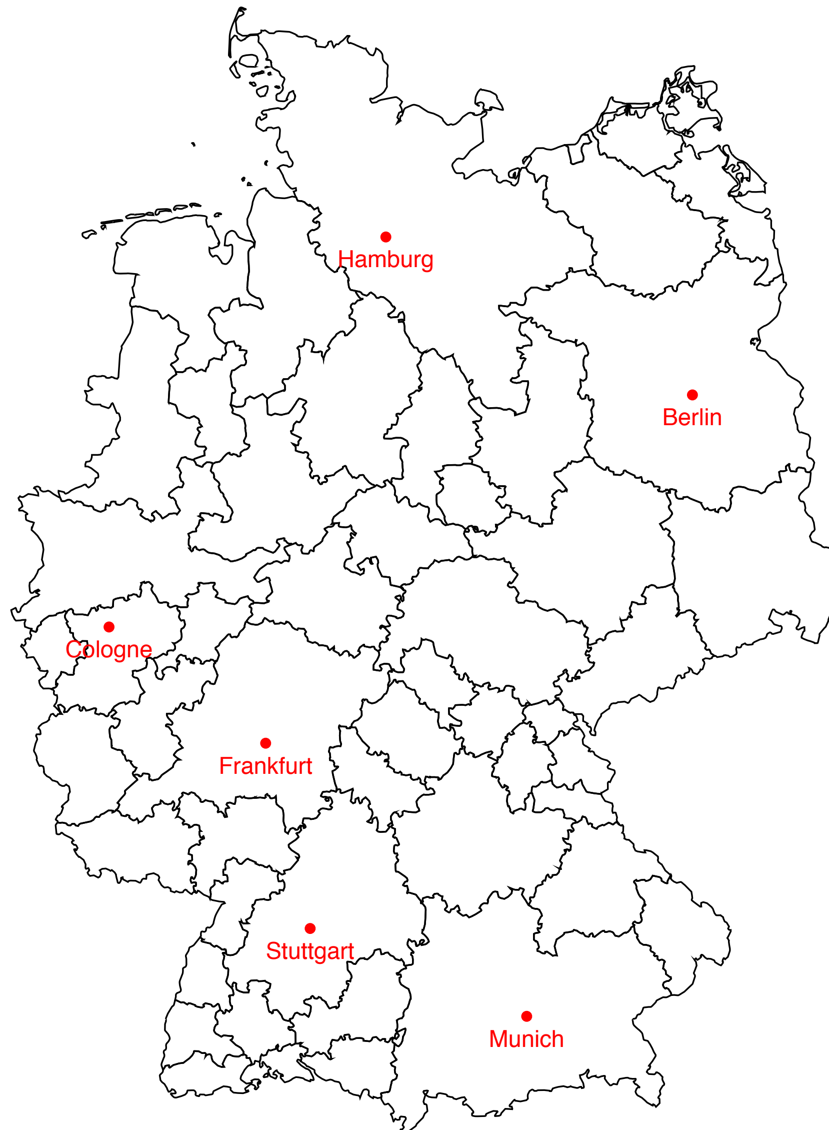

Note: The figure displays treatment effects of worker exits on the wages of incumbents in the same 1-digit occupation group as the deceased and on incumbents in other 1-digit occupation groups. 1-digit occupation groups stratify occupations horizontally based on the thematic focus of the work, e.g., production and manufacturing vs. accounting. Short-run effects refer to the treatment effects in year $k = 1$ post-death; long-run effects refer to the average treatment effects in years $k = 1$ through $k = 5$. The vertical lines indicate 95% confidence intervals based on standard errors clustered at the firm level. See Table 5 for additional information.

Figure 10: Incumbent Wage Effects by Skill Level of Deceased



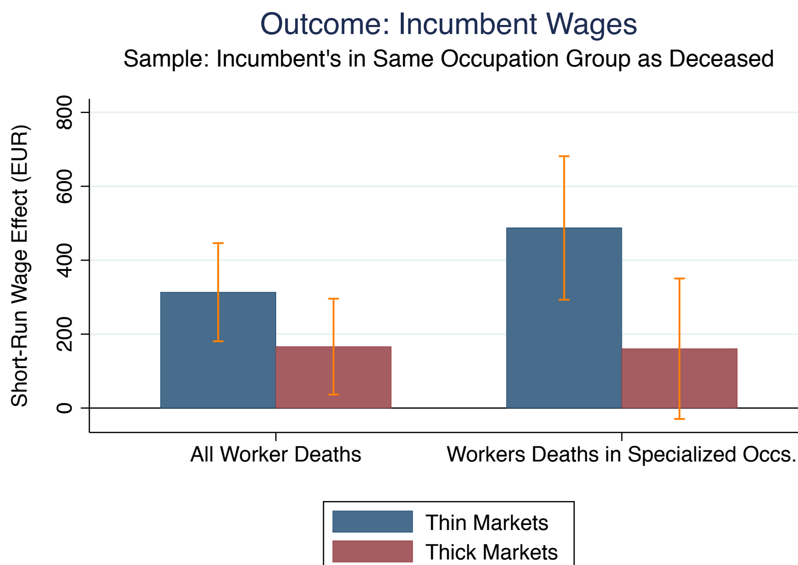

Note: The three figure display short-run treatment effects of worker exits on the wages of incumbents in the same 1-digit occupation group as the deceased and on incumbents in other 1-digit occupation groups for different measures of the skill level of the deceased worker. 1-digit occupation groups stratify occupations horizontally based on the thematic focus of the work, e.g., production and manufacturing vs. accounting. In panel (A), I show heterogeneity by the skill intensity of the 5-digit occupation of the deceased measured by the average years of education of workers in the occupation. Low-, medium-, and high-skilled occupations are defined as occupations below the 20th percentile, between the 20th and 80th percentile, and above the 80th percentile of average years of education, respectively. In panel (B), I show heterogeneity by the education level of the deceased and classify workers into three groups depending on whether they have no apprenticeship training, an apprenticeship training, or further formal education. In panel (C), I show heterogeneity by the managerial status of the deceased's occupation as proxied by occupations requiring "complex specialist activities" (requirement level 3) or "highly complex activities" (requirement level 4) based on the 2010 Classification of Occupations. In all panels, the vertical lines indicate 95% confidence intervals based on standard errors clustered at the firm level. See Tables 5, 6, and 7 for additional information.

Figure 11: Labor Market Regions in Germany




Note: The map shows German labor market regions developed in Kropp and Schwengler (2011) based on commuter flows between municipalities from 1993 to 2008. There are 50 labor market regions that are characterized by a high share of commuting within and a low share of commuting across region boundaries. A key advantage of the Kropp and Schwengler (2011) classification approach is that the classification into labor market regions is relatively stable over time. For orientation, I show the location of the six largest German cities. The map is based on geographic data from the Federal Agency for Cartography and Geodesy (© GeoBasis-DE / BKG 2011).

Figure 12: Heterogeneity of Wage Effects by External Labor Market Thickness




Note: The figure shows short-run treatment effects of worker exits on the wages of incumbents in $k = 1$ by measures of external labor market thickness. The sample is restricted to incumbents in the same 1-digit group as the deceased. Thickness is measured at the 5-digit occupation$\times$ commuting zone level as the share of employment in the relevant occupation in the commuting zone relative to the overall share of employment in that occupation and 5-digit occupation$\times$ commuting zone cells are characterized as thick or thin based on a median split (see Figure 11 for an overview of labor market regions). Occupations are classified as specialized if they have an above-median return to occupational experience (see Table 8 for more details). The orange vertical lines denotes 95% confidence intervals.

Figure 13: Heterogeneity of Hiring by External Labor Market Thickness

Note: The figure shows short-run treatment effects of worker exits on the number of new workers hired in $k = 1$ by measures of external labor market thickness. Thickness is measured at the 5-digit occupation× commuting zone level as the share of employment in the relevant occupation in the commuting zone relative to the overall share of employment in that occupation and 5-digit occupation× commuting zone cells are characterized as thick or thin based on a median split (see Figure 11 for an overview of labor market regions). Occupations are classified as specialized if they have an above-median return to occupational experience (see Table 8 for more details). The orange vertical lines denotes 95% confidence intervals.

# B Tables

Table 1: Individual-Level Summary Statistics

| | Actual and Placebo Deceased Workers | | Incumbent Workers | |
|---|---|---|---|---|
| | Treatment Group | Comparison Group | Treatment Group | Comparison Group |
| Age | 47.22 | 47.22 | 39.44 | 39.33 |
| | (9.90) | (9.90) | (11.30) | (11.29) |
| Female | 0.14 | 0.14 | 0.26 | 0.26 |
| | (0.35) | (0.35) | (0.44) | (0.44) |
| Earnings (EUR, 2010 CPI) | 31,458 | 31,536 | 27,788 | 27,856 |
| | (12,313) | (12,451) | (13,651) | (13,631) |
| Years of Education | 10.6 | 10.6 | 10.9 | 10.9 |
| | (1.5) | (1.5) | (1.9) | (1.9) |
| Tenure | 9.52 | 9.53 | 7.04 | 7.06 |
| | (6.15) | (6.14) | (5.48) | (5.47) |
| $N$ | 33,855 | 33,855 | 380,001 | 380,665 |

Note: The first two columns show summary statistics for the actual and placebo deceased worker in the treatment and comparison group. The second two columns show summary statistics for the sample of incumbent workers, i.e. full-time coworkers of the actual or placebo deceased in the year before the actual or placebo death. Standard deviations are reported in parentheses. All variables are measured in $k = -1$, the year before the actual or placebo death. For the incumbent worker sample, observations are weighted inversely by the number of incumbent workers at a firm. Earnings are real annual earnings in EUR (2010 CPI). Years of education are calculated as follows: 9 years for individuals with no degree, 10.5 years for individuals with only an apprenticeship training, 13 years for individuals with a general qualification for university entrance (*Abitur*), 14.5 years for individuals with *Abitur* and an apprenticeship training, 16 years for individuals with a degree from a technical college or a university of applied sciences, and 18 years for individuals with a university degree. Tenure measures the years of employment at the establishment.

Table 2: Firm-Level Summary Statistics

| | Treatment Group | Comparison Group |
|---|---|---|
| Total Number of Employees | 14.44 | 14.50 |
| | (7.38) | (7.40) |
| Number of New Workers | 2.27 | 2.23 |
| | (2.40) | (2.41) |
| Number Part-Time Workers | 1.19 | 1.20 |
| | (2.24) | (2.25) |
| Number Apprentices | 0.83 | 0.86 |
| | (1.51) | (1.52) |
| Firm Age | 14.77 | 14.79 |
| | (6.77) | (6.77) |
| Primary Sector | 0.029 | 0.029 |
| | (0.167) | (0.169) |
| Secondary Sector (Manufacturing) | 0.500 | 0.494 |
| | (0.500) | (0.500) |
| Tertiary Sector (Service) | 0.472 | 0.477 |
| | (0.499) | (0.499) |
| $N$ | 33,855 | 33,855 |

Note: Standard deviations are reported in parentheses. All variables are measured in $k = -1$, the year before the actual or placebo death. Number of new workers refers to the number of workers who were employed at the establishment in $k = -1$ but not before. Firm age refers to the number of years the establishment ID has been observed in the data. The sectors are classified based on the 1973 classification of economic activities (*Klassifikation der Wirtschaftszweige* 1973).

Table 3: Robustness Test: Probability of Future Deaths by Treatment Status

| Outcome: Indicator for Worker Death | |
|---|---|
| Treatment | -0.000071 |
| | (0.00023) |
| Constant | 0.01203*** |
| | (0.00017) |
| No. of Observations | 1,097,018 |
| No. of Clusters | 67,710 |

Note: The table reports the results of a regression of an indicator variable that is equal to 1 if a firm experienced a worker death in a given year on treatment status for the sample of years after the actual or placebo death. The magnitude of the point estimates implies that firms in the comparison group face a 1.2% probability of a worker death in a given year and that this probability is on average 0.0071% lower in the treatment group. Standard errors are clustered at the firm level. Levels of significance: * 10%, ** 5%, and *** 1% level.

Table 4: Treatment Effect on Incumbent Worker Employment Outcomes

| | Short-Run Effect | Long-Run Effect |
|---|---|---|
| Outcome: Employed at Same Establishment | | |
| Treated | 0.0043*** | 0.0055*** |
| | (0.0013) | (0.0014) |
| Comparison Group Mean in $k = 1$: 0.825 | | |
| Outcome: Full-Time Employment | | |
| Treated | 0.0011 | -0.0006 |
| | (0.001) | (0.001) |
| Comparison Group Mean in $k = 1$: 0.894 | | |
| Outcome: Part-Time Employment | | |
| Treated | -0.0001 | 0.0004 |
| | (0.0004) | (0.0005) |
| Comparison Group Mean in $k = 1$: 0.0121 | | |
| Outcome: Promotion | | |
| Treated | 0.0008** | 0.0012*** |
| | (0.0003) | (0.0003) |
| Comparison Group Mean in $k = 1$: 0.0084 | | |
| No. of Incumbent Workers | 760,666 | 760,666 |
| No. of Clusters | 67,710 | 67,710 |

Note: The table displays treatment effects on several employment outcomes based on difference-in-differences regressions. Treated refers to the Post × Treated coefficient. Short-run effects refer to the diff-in-diff effects using year $k = 1$ post-death as the post period; long-run effects refer to the specifications using years 1 through 5 post-death as the post period. Employed at the same establishment is an outcome variable that is equal to one when an incumbent worker is still employed at the same establishment as in year $k = -1$. Full- and part-time employment are outcome variables that indicate the respective employment status independent of the establishment at which the individual is employed. Promotion is an outcome variable that is equal to 1 when an individual is employed at the same establishment in an occupation with an higher average wage as the occupation he or she worked in in year $k = -1$. To calculate average wages at the 5 digit occupation level, I draw a 10% sample of individuals from the IEB and regress individual's log wage on occupation dummies and individual fixed effects. I use the estimated occupation effects to measure promotions. Standard errors are based on 67,710 clusters at the firm level. Observations are weighted inversely by the number of incumbent workers at the firm of the deceased. Levels of significance: * 10%, ** 5%, and *** 1% level.

# C Appendix to Conceptual Framework

## Wage Effects With Renegotiation Under Mutual Consent

The analysis in Section 2.1 assumed that contracts are completely nonbinding for either party. Here, I illustrate how the conclusions from this section need to be amended when contracts can only be renegotiated under mutual consent of both the firm and the worker (see MacLeod and Malcomson 1993a,b). The main consequence of relaxing the assumption that contracts are nonbinding is that renegotiation under mutual consent introduces some wage rigidity so that the wage response will be muted in some cases compared to the benchmark with nonbinding contracts.

This intuition can be illustrated in a simple two-period model as in MacLeod and Malcomson (1993b). In the first period, the firm makes a wage offer to a worker in the competitive labor market. The firm and the worker can then make specific investments that raise the worker's productivity in the second period. Potential rents from continuing the employment relationship can arise either because investments are specific (Becker, 1962) or, if investments are general, because of turnover costs and other costs of switching trading partners. At the beginning of the second period, after the worker's productivity and utility of staying with the firm have been realized, the worker can accept an offer from the outside labor market or renegotiate the wage with her firm. The previously contracted wage can only be changed if both the firm and the worker agree. MacLeod and Malcomson (1993b) prove that the following three cases arise as equilibrium of the renegotiation game:

1. Efficient separation: If the rents from continuing the employment relationship are negative, for instance, because of a negative shock to the worker's specific productivity, the firm and the worker separate and receive their outside option.

2. Continued employment with no renegotiation: If the rents from continuing the relationship are nonnegative and both the firm and the worker prefer continuation of employment at the contracted wage to their outside option, the employment relationship will continue under the contracted wage. To see why, suppose that the worker wanted to renegotiate the wage. Then the firm could refuse to renegotiate and would anticipate that the worker would still accept employment under the contracted wage to her outside option.

3. Continued employment with renegotiation: This case arises when the rents from continuing the relationship are nonnegative but one party prefers the outside option to continued employment under the contracted wage. Formally, MacLeod and Malcomson (1993b) distinguish between a specified outside option and no employment - this

## Table 5: Wage Effects and Skill Intensity of Deceased Worker's Occupation

| Outcome: Incumbent Worker Wages | Short-Run Effect | Long-Run Effect | Short-Run Effect | Long-Run Effect | Short-Run Effect | Long-Run Effect |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treated × Same Occupation | 239.91*** | 171.86*** | | | | |
| | (47.37) | (51.66) | | | | |
| Treated × Other Occupations | 61.42 | 47.56 | | | | |
| | (57.32) | (62.95) | | | | |
| Treated × Low Skilled Occupation | | | 118.74 | 85.07 | | |
| | | | (76.48) | (83.29) | | |
| Treated × Medium Skilled Occupation | | | 256.68*** | 182.80*** | | |
| | | | (47.32) | (52.01) | | |
| Treated × High Skilled Occupation | | | -45.47 | -17.64 | | |
| | | | (100.00) | (111.28) | | |
| Treated × Low Skilled Occupation × Same Occupation | | | | | 221.31** | 143.35 |
| | | | | | (98.08) | (105.57) |
| Treated × Low Skilled Occupation × Other Occupations | | | | | -29.08 | 2.12 |
| | | | | | (114.10) | (123.95) |
| Treated × Medium Skilled Occupation × Same Occupation | | | | | 267.60*** | 197.66** |
| | | | | | (57.93) | (63.01) |
| Treated × Medium Skilled Occupation × Other Occupations | | | | | 233.92** | 151.33* |
| | | | | | (75.72) | (83.27) |
| Treated × High Skilled Occupation × Same Occupation | | | | | 166.15 | 119.22 |
| | | | | | (137.96) | (153.29) |
| Treated × High Skilled Occupation × Other Occupations | | | | | -301.78** | -181.73 |
| | | | | | (134.05) | (148.44) |
| No. of Observations | 6,845,994 | 6,845,994 | 6,845,994 | 6,845,994 | 6,845,994 | 6,845,994 |
| No. of Incumbent Workers | 760,666 | 760,666 | 760,666 | 760,666 | 760,666 | 760,666 |
| No. of Clusters | 67,710 | 67,710 | 67,710 | 67,710 | 67,710 | 67,710 |

Note: The table shows heterogeneity of the treatment based on the difference-in-differences framework in 18. Short-run effects refer to the treatment effects in year $k = 1$ post-death; long-run effects refer to the average treatment effects in years $k = 1$ through $k = 5$. Covariates that are included as interactions with treatment status are also included as baseline effects, i.e. as an interaction of the baseline period effect $1(\text{period}_k)$ with the covariate. Same Occupation and Other Occupation are dummy variables indicating whether an incumbent worker was in the same 1-digit occupation group as the deceased or in a different one in the year before a worker death. Low-, medium-, and high-skilled occupations are indicators for the skill intensity of the deceased's 5-digit occupation as measured by the average years of education of workers in the occupation. . Low-, medium-, and high-skilled occupations are defined as occupations below the 20th percentile, between the 20th and 80th percentile, and above the 80th percentile of average years of education, respectively. Observations are weighted inversely by the number of incumbent workers at the firm of the deceased. Standard errors are clustered at the firm level. Levels of significance: * 10%, ** 5%, and *** 1% level.

Table 6: Wage Effects and Education Level of Deceased Worker

| Outcome: Incumbent Worker Wages | | | | | | |
|---|---|---|---|---|---|---|
| | Short-Run Effect | Long-Run Effect | Short-Run Effect | Long-Run Effect | Short-Run Effect | Long-Run Effect |
| Treated × Same Occupation | 239.91*** | 171.86*** | | | | |
| | (47.37) | (51.66) | | | | |
| Treated × Other Occupations | 61.42 | 47.56 | | | | |
| | (57.32) | (62.95) | | | | |
| Treated × Low Education | | | 249.42** | 119.45 | | |
| | | | (92.39) | (101.07) | | |
| Treated × Medium Education | | | 184.03*** | 142.75** | | |
| | | | (41.92) | (46.19) | | |
| Treated × High Education | | | -131.33 | -73.48 | | |
| | | | (184.87) | (201.98) | | |
| Treated × Low Education × Same Occupation | | | | | 286.18** | 187.75 |
| | | | | | (112.93) | (124.16) |
| Treated × Low Education × Other Occupations | | | | | 184.43 | -1.90 |
| | | | | | (147.85) | (158.14) |
| Treated × Medium Education × Same Occupation | | | | | 241.40*** | 174.92** |
| | | | | | (52.79) | (57.50) |
| Treated × Medium Education × Other Occupations | | | | | 83.06 | 84.74 |
| | | | | | (63.88) | (70.56) |
| Treated × High Education × Same Occupation | | | | | 103.16 | 74.15 |
| | | | | | (251.50) | (274.00) |
| Treated × High Education × Other Occupations | | | | | -447.42* | -278.07 |
| | | | | | (252.18) | (275.45) |
| No. of Observations | 6,845,994 | 6,845,994 | 6,845,994 | 6,845,994 | 6,845,994 | 6,845,994 |
| No. of Incumbent Workers | 760,666 | 760,666 | 760,666 | 760,666 | 760,666 | 760,666 |
| No. of Clusters | 67,710 | 67,710 | 67,710 | 67,710 | 67,710 | 67,710 |

Note: The table shows heterogeneity of the treatment based on the difference-in-differences framework in 18. Short-run effects refer to the treatment effects in year $k = 1$ post-death; long-run effects refer to the average treatment effects in years $k = 1$ through $k = 5$. Covariates that are included as interactions with treatment status are also included as baseline effects, i.e. as an interaction of the baseline period effect $1(\text{period}_k)$ with the covariate. Same Occupation and Other Occupation are dummy variables indicating whether an incumbent worker was in the same 1-digit occupation group as the deceased or in a different one in the year before a worker death. Low, medium, and high education indicate the education level of the deceased worker: low education - less than apprenticeship training, medium education - apprenticeship training, and high education - formal education beyond apprenticeship training. Observations are weighted inversely by the number of incumbent workers at the firm of the deceased. Standard errors are clustered at the firm level. Levels of significance: * 10%, ** 5%, and *** 1% level.

## Table 7: Wage Effects and Manager Status of Deceased Worker

| Outcome: Incumbent Worker Wages | Short-Run Effect (1) | Long-Run Effect (2) | Short-Run Effect (3) | Long-Run Effect (4) | Short-Run Effect (5) | Long-Run Effect (6) |
|---|---|---|---|---|---|---|
| Treated × Same Occupation | 239.91*** | 171.86*** | | | | |
| | (47.37) | (51.66) | | | | |
| Treated × Other Occupations | 61.42 | 47.56 | | | | |
| | (57.32) | (62.95) | | | | |
| Treated × Deceased Non-Manager | | | 214.29*** | 155.38*** | | |
| | | | (39.35) | (43.28) | | |
| Treated × Deceased Manager | | | -108.82 | -78.57 | | |
| | | | (120.54) | (133.16) | | |
| Treated × Deceased Non-Manager × Same Occupation | | | | | 251.95*** | 178.58*** |
| | | | | | (48.71) | (53.06) |
| Treated × Deceased Non-Manager × Other Occupations | | | | | 142.59** | 110.11 |
| | | | | | (61.86) | (67.93) |
| Treated × Deceased Manager × Same Occupation | | | | | 128.95 | 109.80 |
| | | | | | (180.28) | (198.45) |
| Treated × Deceased Manager × Other Occupations | | | | | -338.31** | -262.42 |
| | | | | | (149.12) | (164.23) |
| No. of Observations | 6,845,994 | 6,845,994 | 6,845,994 | 6,845,994 | 6,845,994 | 6,845,994 |
| No. of Incumbent Workers | 760,666 | 760,666 | 760,666 | 760,666 | 760,666 | 760,666 |
| No. of Clusters | 67,710 | 67,710 | 67,710 | 67,710 | 67,710 | 67,710 |

Note: The table shows heterogeneity of the treatment based on the difference-in-differences framework in 18. Short-run effects refer to the treatment effects in year $k = 1$ post-death; long-run effects refer to the average treatment effects in years $k = 1$ through $k = 5$. Covariates that are included as interactions with treatment status are also included as baseline effects, i.e. as an interaction of the baseline period effect $1(\text{period}_k)$ with the covariate. Same Occupation and Other Occupation are dummy variables indicating whether an incumbent worker was in the same 1-digit occupation group as the deceased or in a different one in the year before a worker death. I measure the managerial status of the deceased's occupation as proxied by occupations requiring "complex specialist activities" (requirement level 3) or "highly complex activities" (requirement level 4) based on the 2010 Classification of Occupations. These occupations are characterized by managerial, planning and control activities, such as operation and work scheduling, supply management, and quality control and assurance and typically require a qualification as master craftsperson, graduation from a professional academy, or university studies (see *Klassifikation der Berufe 2010, Band 1: Systematischer und alphabetischer Teil mit Erläuterungen, Bundesagentur für Arbeit*). Observations are weighted inversely by the number of incumbent workers at the firm of the deceased. Standard errors are clustered at the firm level. Levels of significance: * 10%, ** 5%, and *** 1% level.

## Table 8: Wage Effects by Tenure and Occupational Specialization of Deceased Worker

| Outcome: Incumbent Worker Wages | Short-Run Effect | Long-Run Effect | Short-Run Effect | Long-Run Effect | Short-Run Effect | Long-Run Effect |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treated × Short Tenure of Deceased Worker | 115.33 | 81.52 | | | | |
| | (104.59) | (112.50) | | | | |
| Treated × Medium Tenure of Deceased Worker | 140.10** | 66.03 | | | | |
| | (61.46) | (66.89) | | | | |
| Treated × Long Tenure of Deceased Worker | 178.57** | 161.22** | | | | |
| | (56.96) | (63.93) | | | | |
| Treated × Low Specialization Occupations | | | 82.24 | 132.79 | | |
| | | | (87.38) | (96.07) | | |
| Treated × Medium Specialization Occupations | | | 216.61*** | 120.13** | | |
| | | | (47.07) | (51.92) | | |
| Treated × High Specialization Occupations | | | 122.13 | 140.99 | | |
| | | | (88.87) | (96.75) | | |
| Treated × Low Specialization Occupations × Same Occupation | | | | | 161.79 | 162.20 |
| | | | | | (115.00) | (125.59) |
| Treated × Low Specialization Occupations × Other Occupations | | | | | -26.81 | 89.88 |
| | | | | | (124.03) | (135.70) |
| Treated × Medium Specialization Occupations × Same Occupation | | | | | 220.32*** | 138.70** |
| | | | | | (57.42) | (62.88) |
| Treated × Medium Specialization Occupations × Other Occupations | | | | | 209.12** | 81.79 |
| | | | | | (75.43) | (83.17) |
| Treated × High Specialization Occupations × Same Occupation | | | | | 427.11*** | 333.38** |
| | | | | | (121.60) | (129.45) |
| Treated × High Specialization Occupations × Other Occupations | | | | | -244.26** | -92.97 |
| | | | | | (124.61) | (136.09) |
| No. of Observations | 6,845,994 | 6,845,994 | 6,845,994 | 6,845,994 | 6,845,994 | 6,845,994 |
| No. of Coworkers | 760,666 | 760,666 | 760,666 | 760,666 | 760,666 | 760,666 |
| No. of Clusters | 67,710 | 67,710 | 67,710 | 67,710 | 67,710 | 67,710 |

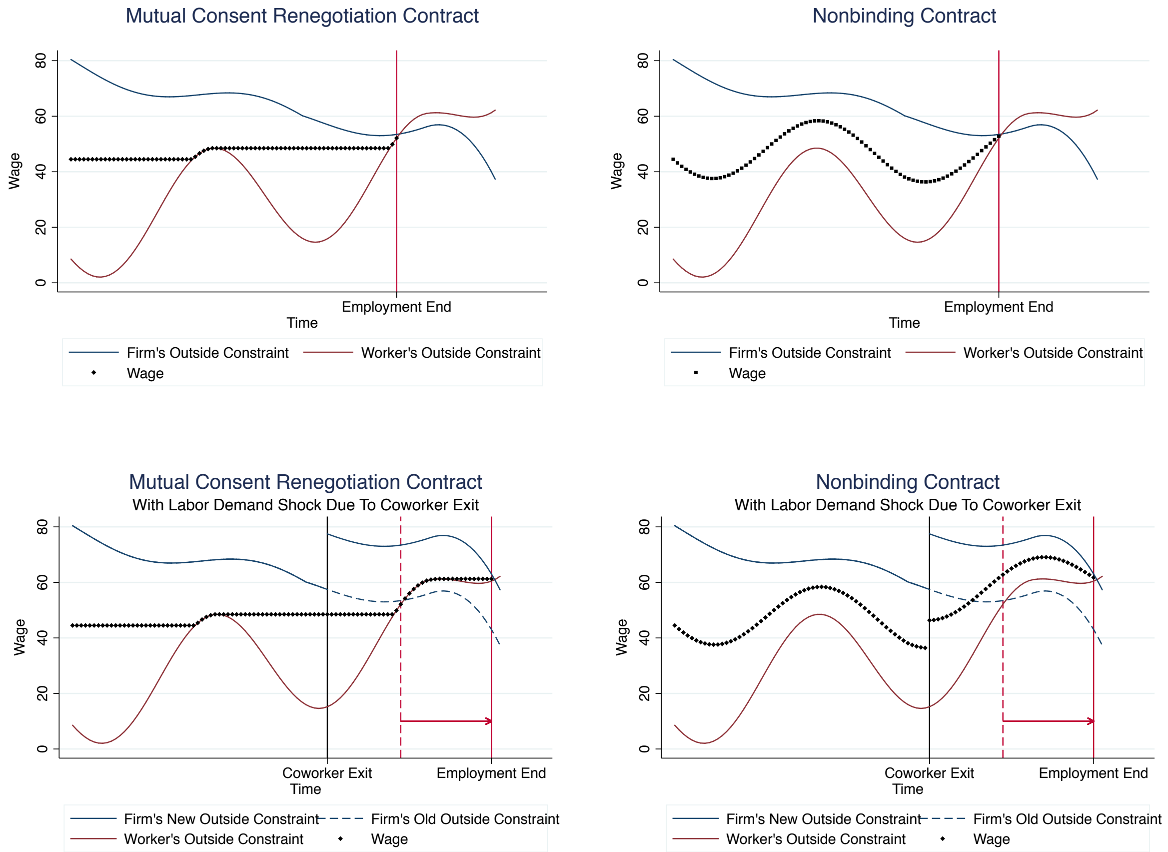
Note: The table shows heterogeneity of the treatment based on the difference-in-differences framework in 18. Short-run effects refer to the treatment effects in year $k = 1$ post-death; long-run effects refer to the average treatment effects in years $k = 1$ through $k = 5$. Covariates that are included as interactions with treatment status are also included as baseline effects, i.e. as an interaction of the baseline period effect $1(\text{period}_k)$ with the covariate. Same Occupation and Other Occupation are dummy variables indicating whether an incumbent worker was in the same 1-digit occupation group as the deceased or in a different one in the year before a worker death. Low, medium, and high tenure are categorized as 1 to 5 years (low), 5 to 10 years (medium), and greater than 10 years of tenure (high). To calculate a specialization measure for the occupation of the deceased worker, I follow Bleakley and Lin (2012) and calculate returns to experience for each 5-digit occupation. I then use the estimated occupation-specific returns to experience to classify occupations as follows: occupations with returns to experience below the 20th percentile are classified as low specialization occupations, occupations with returns to experience between the 20th and 80th percentile are classified as medium specialization, and occupations above the 80th percentile of returns to experience as high specialization occupations. Observations are weighted inversely by the number of incumbent workers at the firm of the deceased. Standard errors are clustered at the firm level. Levels of significance: * 10%, ** 5%, and *** 1% level.

distinction matters for the determination of the renegotiated wage which is either set to split the surplus from continuing the employment relationship or set to the outside option of the party who prefers their outside option to the negotiated wage.

The left panels of Figure C.1 illustrate a dynamic version of such an employment contract. The navy and maroon lines are a stylized example of a dynamic path for the firm's and the worker's outside options. Employment continues as long as there are positive rents and wages are only renegotiated when either the firm's or the worker's outside option binds. To illustrate the contrast to nonbinding contracts as in Stole and Zwiebel (1996a,b), the right panels illustrate how wages are continuously renegotiated as a consequences of outside option changes in a setup with nonbinding contracts.

The lower two panels demonstrate how a positive shock to a firm's labor demand for a given worker—e.g., due to the exit of a substitutable coworker—translates into wage and employment changes under the two contracting regimes. In both contracting regimes, the employment relationship lasts longer and turnover is lower as a consequence of the positive demand shock which shifts the firm's outside option upward. With a nonbinding contract, changes in firm's labor demand translate immediately into wage changes. However, when wages can be renegotiated only under mutual consent, shocks to the firm's labor demand for a given worker do not need to immediately affect wages and only do so when the worker's outside constraint bind, i.e. when the worker would prefer her outside option relative to the previously negotiated wage. This illustrates how a contracting setup in which wages can only be renegotiated by mutual consent leads to some wage rigidity which mute the wage response to a coworker exit relative to a setup with nonbinding contracts.

Figure C.1: Wage and Employment Dynamics Under Different Contracting Regimes




Note: The figures display wage and employment outcomes over time under different contracting regimes (see Appendix C) for a stylized example of two dynamic paths of the firm's and the worker's outside options. The left panels show wage setting and employment under the assumption that contracts can only be renegotiated under mutual consent (see, e.g., MacLeod and Malcomson 1993a,b). The right panels show wage setting and employment under completely nonbinding contracts (see, e.g., Stole and Zwiebel 1996a,b). The key difference between the two contracting regimes is that renegotiation under mutual consent leads to more rigid wages relative to the nonbinding contracts benchmark. The lower two panels demonstrate how a positive shock to a firm's labor demand for a given worker—e.g., due to the exit of a substitutable coworker—translates into wage and employment changes under the two contracting regimes. In both contracting regimes, the employment relationship lasts longer as a consequence of the demand shock. With a nonbinding contract, changes in firm's labor demand translate immediately into wage changes. However, when wages can be renegotiated only under mutual consent a positive shock to a firm's labor demand for a given worker does not need to immediately affect wages and only does so when the worker's outside constraint bind, i.e. when the worker would prefer her outside option relative to the negotiated wage.

# D Additional Tables

Table D.1: Treatment Effects on Wages By Establishment Size

| Outcome: | Incumbent Wages | |
|---|---|---|
| | Short-Run Effect | Long-Run Effect |
| Treated × (Employment ≤ 10) | 204.21** | 154.94** |
| | (65.21) | (70.95) |
| Treated × (10 < Employment ≤ 20) | 175.93*** | 113.51** |
| | (50.76) | (56.77) |
| Treated × (20 < Employment ≤ 30) | 82.88 | 73.27 |
| | (71.67) | (81.42) |
| No. of Observations | 6,845,994 | 6,845,994 |
| No. of Incumbent Workers | 760,666 | 760,666 |
| No. of Clusters | 67,710 | 67,710 |

Note: The table displays results of diff-in-diff specifications by initial establishment size. Observations are weighted inversely by the number of incumbent workers at the deceased's establishment. Levels of significance: * 10%, ** 5%, and *** 1% level.

Table D.2: Dynamics of Average Treatment Effect on Incumbent Worker Wages

| Outcome: | Incumbent Worker Wages | Sum of Incumbent Worker Wages |
|---|---|---|
| Treated × $k = -3$ | -32.09 | -258.09 |
| | (34.71) | (409.65) |
| Treated × $k = -2$ | 31.64 | 45.49 |
| | (28.50) | (314.21) |
| Treated × $k = -1$ | omitted | omitted |
| Treated × $k = 0$ | 73.37*** | 718.12*** |
| | (26.69) | (332.73) |
| Treated × $k = 1$ | 174.47*** | 1791.14*** |
| | (37.60) | (406.74) |
| Treated × $k = 2$ | 159.66*** | 1642.80*** |
| | (43.59) | (469.99) |
| Treated × $k = 3$ | 158.08*** | 1182.79*** |
| | (48.62) | (533.71) |
| Treated × $k = 4$ | 107.50*** | 890.33 |
| | (52.57) | (591.64) |
| Treated × $k = 5$ | 30.05 | 153.68 |
| | (56.48) | (652.66) |
| No. of Observations | 6,845,994 | 6,845,994 |
| No. of Incumbent Workers | 760,666 | 760,666 |
| No. of Clusters | 67,710 | 67,710 |

Note: The table reports results based on the dynamic difference-in-differences model in (18). $k$ denotes the year relative to the death of the worker. The mean of incumbent worker wages in year $k = -1$ in the control group is EUR 27,856 (2010 CPI). Observations are weighted inversely by the number of incumbent workers at the firm. Standard errors clustered at the firm level. Levels of significance: * 10%, ** 5%, and *** 1% level.

Table D.3: Effects in on Incumbent Worker Wages in Year $k = 0$ By Quarter of Death

| Outcome: | Wage in Year $k = 0$ |
|---|---|
| Treated × Death in July, August, September of $k = 0$ | 170.48*** |
| | (41.25) |
| Treated × Death in October, November, December of $k = 0$ | 67.83 |
| | (42.78) |
| Treated × Death in January, February, March of $k = 1$ | 37.75 |
| | (43.35) |
| Treated × Death in April, May, June of $k = 1$ | 6.89 |
| | (42.33) |
| No. of Incumbent Workers | 760,666 |
| No. of Clusters | 67,710 |

Note: The table displays results of a difference-in-differences regression of wages in year $k = 0$ on treatment status interacted with dummies for the quarter of death of the deceased worker in the treated group. The positive and statistically significant coefficients for wage effects in year 0 of deaths that occur in July, August, and September or October, November, and December document that the positive wage effects in year $k = 0$ (see, e.g., Figure 5) are driven by deaths that occur in the same calendar year, as wages for most employees correspond to average wages calculated over a calendar year horizon so that deaths in, e.g., August will have an effect on average wages in that year. The table also demonstrates that deaths in the first quarter of the following calendar year do not have a statistically detectable effect on incumbent worker wages in the previous calendar year. Standard errors are based on 67,710 clusters at the worker death level. Observations are weighted inversely by the number of incumbent workers at the firm of the deceased. Levels of significance: * 10%, ** 5%, and *** 1% level.

## Table D.4: Wage Effects and External Labor Market Characteristics

| Outcome: Wages of Incumbent Workers in Same Occupation Group as Deceased | | | | | | |
|---|---|---|---|---|---|---|
| Sample: | All Worker Deaths | | Worker Deaths in High Specialization Occupations | | Worker Deaths in Low Specialization Occupations | |
| | Short-Run Effect | Long-Run Effect | Short-Run Effect | Long-Run Effect | Short-Run Effect | Long-Run Effect |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *(A) Thickness Measured at Occupation Level* | | | | | | |
| Treated × Low Thickness (Occupation) | 313.49*** | 216.91** | 487.31*** | 291.28** | 181.50** | 161.67 |
| | (67.77) | (73.81) | (99.03) | (107.07) | (92.52) | (101.17) |
| Treated × High Thickness (Occupation) | 166.19** | 127.50* | 160.51* | 171.91 | 169.27* | 91.85 |
| | (66.16) | (72.25) | (96.94) | (105.68) | (90.00) | (98.18) |
| *(B) Density of Local Labor Market* | | | | | | |
| Treated × Low Density | 301.82*** | 203.20** | 387.15*** | 265.28** | 236.20** | 152.35 |
| | (65.82) | (71.20) | (95.01) | (102.88) | (89.87) | (97.77) |
| Treated × High Density | 177.08** | 141.18* | 264.41** | 198.39* | 113.57 | 103.49 |
| | (68.45) | (74.90) | (101.10) | (110.04) | (92.65) | (101.55) |
| *(C) Thickness Measured at Industry Level* | | | | | | |
| Treated × Low Thickness (Industry) | 267.59*** | 249.62*** | 347.18*** | 258.48** | 204.01** | 244.17** |
| | (67.92) | (73.61) | (97.23) | (105.76) | (94.34) | (101.91) |
| Treated × High Thickness (Industry) | 217.24*** | 104.70 | 306.62** | 211.07** | 153.49* | 27.00 |
| | (65.87) | (71.98) | (98.51) | (105.82) | (88.20) | (97.10) |
| *(D) Local Unemployment Rate* | | | | | | |
| Treated × Low Unemployment | 193.78** | 160.17* | 268.51** | 248.71** | 136.47 | 93.86 |
| | (75.75) | (83.54) | (111.35) | (121.26) | 103.07 | 114.51 |
| Treated × High Unemployment | 276.38*** | 186.49** | 379.30*** | 262.01** | 199.04** | 127.06 |
| | (72.04) | (78.51) | (103.62) | (112.75) | 99.23 | 108.16 |

Note: The table shows heterogeneity of the treatment effect based on the difference-in-differences framework in 18. Short-run effects refer to the treatment effects in year $k = 1$ post-death; long-run effects refer to the average treatment effects in years $k = 1$ through $k = 5$. Covariates that are included as interactions with treatment status are also included as baseline effects, i.e. as an interaction of the baseline period effect $1(\text{period}_k)$ with the covariate. The sample is restricted to incumbent workers in the same 1-digit occupation group as the deceased. To calculate a specialization measure for the occupation of the deceased worker, I follow Bleakley and Lin (2012) and calculate returns to experience for each 5-digit occupation. I then use the estimated occupation-specific returns to experience to classify occupations into high- and low-specialization occupations based on a median split. All external labor market characteristics are measured at the commuting zone level (see Figure 11 for an overview of commuting zones) based on median splits of the relevant measure. Thickness measured at the occupation level is used to categorize 5-digit occupation × commuting zone cells as thick or thin based on the relative share of workers in the 5-digit occupation in the commuting zone relative to the overall share of workers in that occupation in the labor market. Thickness measured at the industry level is defined analogously for the share of workers in the 3-digit industry × commuting zone level. Density of the local labor market refers to the number of workers in a commuting zone divided by that commuting zone's area. The unemployment rate is calculated as the number of unemployed workers in the commuting zones divided by the number of workers. Observations are weighted inversely by the number of incumbent workers at the firm of the deceased. Standard errors are clustered at the firm level. Levels of significance: * 10%, ** 5%, and *** 1% level.