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# How Substitutable Are Workers? Evidence from Worker Deaths\*

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## Abstract

To assess the substitutability between workers within a firm, and between incumbent workers and outsiders, we estimate how exogenous worker exits affect a firm's demand for incumbent workers and new hires. Using matched employer-employee data from Germany, we analyze the effects of 34,000 unexpected worker deaths and show that these worker exits on average raise the remaining workers' wages and retention probabilities for a period of several years. The average effect masks substantial heterogeneity: Coworkers in the same occupation as the deceased see positive wage effects; coworkers in other occupations instead experience wage decreases when a high-skilled worker or manager dies. Finally, when the external labor market in the deceased's occupation is thin, incumbents' wages respond more and external hiring responds less to a worker death. Taken together, our findings imply that incumbent workers are closer substitutes to one another compared to outsiders, that high-skilled workers and managers are complements to coworkers in other occupations, and that thin external markets for skills lead to higher firm-specificity of human capital and lower replaceability of incumbents.

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# 1 Introduction

The fluidity of labor markets 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 adequate substitutes for their current workers. An extensive body of empirical literature sheds light on the workers' perspective and finds that workers who are displaced from their jobs suffer persistent earnings losses—consistent with Becker's (1962) idea that human capital has firm-specific components.<sup>1</sup> 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 through hiring and how do such worker exits affect the firm's demand for its remaining workers? Several debates—ranging from the role of labor pooling as a source of agglomeration (Marshall, 1890) to the importance of intrafirm bargaining (Stole and Zwiebel, 1996a,b)—hinge directly on the answer to this question.

We offer an empirical answer to this question by estimating the effects of exogenous worker exits on hiring, and on the firm's demand for the labor of the remaining workers. We then use the results to adjudicate between different models of the labor market, in particular different assumptions about the substitutability of workers. We illustrate the intuition underlying our approach in a simple conceptual framework that demonstrates how different assumptions about worker substitutability alter the predictions for the sign and magnitude of the effects of worker exits. The competitive labor market model assumes that outside workers are perfect substitutes for incumbent workers and thus predicts that the effect of worker exits on the firm's labor demand for the remaining insiders is zero: the firm can simply hire a suitable new worker in response to a worker exit so that its demand for the labor of the remaining 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 incumbent workers. In bargaining models that incorporate such imperfect substitutability (see, e.g., Stole and Zwiebel 1996a,b), the sign of the effect identifies the substitutability of the exiting worker's skills with those of the remaining workers: the firm's labor demand rises for substitutes of the worker who exited and, in contrast, falls for complements.

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

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<sup>1</sup>See, e.g., Jacobson, LaLonde, and Sullivan (1993) 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).

and retention rates based on the universe of German Social Security records. In a dynamic difference-in-differences design, we 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 a worker death that year. The research design relies on deaths as a source of variation to circumvent the endogeneity of worker exits.<sup>2</sup> The sample excludes the deaths of workers who experienced a hospitalization or longer sickness spell in the five years before their death in order to exclude deaths preceded by debilitating diseases.<sup>3</sup> The outcomes in the treatment and comparison group follow parallel trends in the years prior to the death of a worker in treatment group 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, we show that worker deaths affect firms' demand for the labor of their remaining workers. On average, incumbent workers in the treatment group experience a highly statistically significant earnings increase of about 0.6% in the year after the death.<sup>4</sup> Over the course of the five years after the death, the average cumulative effect on the earnings of all incumbent workers in a treatment group firm is close to 6,000 EUR (2010 CPI), corresponding to about 18% of an average deceased worker's annual earnings. Moreover, incumbent workers in the treatment group are more likely to retain employment at the same firm and are less likely to be employed at other firms; their probability of (any) employment does not change in response to a worker death. Worker deaths do not affect incumbents' working hours at the part-time versus full-time margin.<sup>5</sup>

In a next step, we leverage the research design to estimate within-firm heterogeneity across occupation and skill groups and find substantial heterogeneity, shedding 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.<sup>6</sup> For deaths of workers in high-skilled occupations, we

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<sup>2</sup>The use of deaths as a source of variation builds on previous work in Jones and Olken (2005); Bennedsen, Pérez-González, and Wolfenzon (2006); Bennedsen et al. (2007); Azoulay, Wang, and Zivin (2010); Oettl (2012); Becker and Hvide (2013); Isen (2013); Jaravel, Petkova, and Bell (2015); and Fadlon and Nielsen (2015). We discuss the relationship to this line of work in more detail in Section 6.2.

<sup>3</sup>See Section 4.1 for details.

<sup>4</sup>The average firm in our sample has 14.5 employees in the year before a worker death.

<sup>5</sup>As a complement to our main analysis, we also analyze the impact of worker deaths on coworker hours using a novel dataset on work hours from 2010 to 2015. The analysis reveals that worker deaths are not associated with detectable increases in work hours. Even if the treatment effect on wage earnings was in part due to changes in working hours, nonzero wage effects of worker exits indicate that the firm cannot costlessly hire perfect replacements for incumbents.

<sup>6</sup>In our main specifications, we 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

estimate statistically significant, *negative* effects on the wages of incumbent workers in other occupations. Similarly, wage effects on incumbent workers in other occupations are negative in the case of deaths of managers.<sup>7</sup> Turning the focus to measures of human capital specificity of the deceased, we find evidence suggesting that longer-tenured workers and workers in specialized occupations are harder to replace with outsiders.<sup>8</sup>

Since the evidence indicates that worker exits affect firms' demand for incumbents, our findings are hard to reconcile with frictionless labor markets and perfect substitutability between incumbents and outsiders. Instead they point to a set of models in which firms face frictions in replacing workers externally. In particular, the findings accord with Becker's (1964) conjecture that firms share rents with workers to keep workers with specific human capital from quitting.<sup>9</sup> The finding of positive wage effects on coworkers in the same occupation as the deceased supports this view, because workers in the same occupation are arguably closer substitutes than workers in different occupations and therefore become more valuable to the firm as a consequence of a coworker exit. The finding of negative wage effects of worker deaths in high-skilled occupations on incumbents in other occupations indicates imperfect substitutability between high- and low-skilled labor.<sup>10</sup> Our findings thereby support a key assumption of models positing that skilled workers raise the productivity of other workers at the same firm (see, e.g., Lucas, 1978; Rosen, 1982; Murphy, Shleifer, and Vishny, 1991), 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).<sup>11</sup>

The validity of our interpretation of the empirical results as evidence regarding the substitutability of workers depends on whether alternative mechanisms can account for our findings.

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other occupations as the complement of that group.

<sup>7</sup> We 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). In our analysis of hours, we find evidence consistent with some of the negative effects of manager and high-skilled worker deaths on wage earnings operating through reduced work hours.

<sup>8</sup> We proxy for 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).

<sup>9</sup>Our 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.

<sup>10</sup>Our analysis of work hours suggests that the complementarity between managers and workers in high-skilled occupations with workers in other occupations may in part be mediated through work hours.

<sup>11</sup>Katz and Murphy (1992), for example, provide evidence that college- and high-school-educated workers are imperfect substitutes and show that changes in the aggregate supply of college graduates are associated with in opposite-signed changes the college premium.

We 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) search frictions without human capital specificity. None of the alternative mechanisms matches all of the evidence. The first alternative explanation builds on the hypothesis 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. While such labor supply-driven explanations could explain why wages increase, they would simultaneously predict that workers’ probability of staying with the firm should decrease. The data, however, reject this explanation as both wages and the probability of staying at the firm go up. Therefore, positive shifts in firms’ labor demand dominate any 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 are consistent with the evidence presented in this paper.

To shed light on the sources of frictions in replacing workers, we study heterogeneity by external labor market conditions and find that firms in thicker markets for specialized skills respond to a worker death with smaller increases in incumbent wages and more external hiring. 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) theory according to which the specificity of human capital depends on the thickness of the market.<sup>12</sup> We investigate the role of market thickness by estimating heterogeneity across labor markets which vary in the relative agglomeration of workers in the deceased’s occupation.<sup>13</sup> Wage effects are smaller in labor markets with a higher con-

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<sup>12</sup>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. This view of human capital specificity contrasts with a dichotomous distinction of purely firm-specific and purely general skills.

<sup>13</sup> We measure thickness at the 5-digit occupation  $\times$  commuting zone level as the share of employment in the relevant occupation in that commuting zone relative to the nationwide share of employment in that occupation. We then classify 5-digit occupation  $\times$  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

centration of workers 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 shows that firms in thicker labor markets are more likely to hire a new worker externally when a worker in a specialized occupation dies. Taken together, our findings support Lazear’s (2009) theory of firm-specific human capital and suggest that frictions in replacing workers are larger in thin markets, in which workers’ skills are more firm-specific.

This paper contributes to several additional strands of the literature. Its results provide direct evidence supporting the key assumption of intrafirm bargaining models (Stole and Zwiebel 1996a,b)—imperfect substitutability between incumbent workers and outsiders—and thereby resolve an open debate in the literature.<sup>14</sup> By shedding light on the frictions that firms face in replacing workers externally, our study adds to a literature—going back to Slichter (1919) and Oi (1962)—that estimates the costs of worker turnover.<sup>15</sup> While this literature focuses on gauging firms’ expenditure for recruiting, hiring, and training, our 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, our research design complements the extensive 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; and Card, Devicienti, and Maida, 2013) as it provides direct evidence for a mechanism—human capital specificity leading to imperfect substitutability between insiders and outsiders—that gives rise to such rent sharing. Finally, our findings provide 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.<sup>16</sup>

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the overall share of mechanical engineers in the German labor market.

<sup>14</sup>The canonical intrafirm bargaining model of Stole and Zwiebel (1996a,b) relies crucially on the assumption 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”.

<sup>15</sup>See also Section 5.2 and overview of estimates of hiring costs in Manning (2011).

<sup>16</sup>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., Lazear (1992); Baker, Gibbs, and Holmstrom (1994a,b); Lazear and Oyer (2004b,a); relatedly, Bertrand (2004) provides 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,

The remainder of the paper is organized as follows. Section 2 presents 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 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 matched sampling procedure to select the comparison group. Section 5 presents the results of our paper. In section 6, we assess alternative mechanisms to explain our findings and discuss further implications of our study. The last section concludes.

## 2 Conceptual Framework

To structure our empirical analysis, this section presents a conceptual framework that demonstrates the link between the effects of worker exits on incumbent wages and the substitutability between workers within a firm, and between incumbent workers and outsiders, under different modeling assumptions. We then illustrate the relationship between wage effects and the substitutability of workers in three benchmark models of the labor market: first, 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 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), which we discuss in Appendix A.1.<sup>17</sup>

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

1. Changes in firms’ demand for incumbent workers as a consequence of a worker exit are inconsistent with perfect substitutability between incumbent workers and outsiders.
2. The sign of the wage effect on incumbents identifies the substitutability between the skills of the worker who exited and those of the remaining incumbent workers. Intuitively, an exit of a hard-to-replace worker raises the firm’s demand for the labor of the

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1991). For overviews, see the surveys in Gibbons and Waldman (1999); Lazear and Oyer (2013); Oyer and Scott (2011); and Waldman (2013). In recent work, Jäger et al. (2018) provide further evidence that wages inside a firm are shielded from shifts in workers’ nonemployment values.

<sup>17</sup>This model is closely related to work in Wolinsky (2000); Elsbey 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.



remaining incumbent workers with substitutable skills. Analogously, worker exits lead to negative effects on incumbent workers who are complements of the worker who left.

3. The magnitude and duration of wage effects is proportional to the frictions that the firm faces in hiring suitable replacement workers.

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

We 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), which consists of 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 human capital. A more realistic interpretation of this assumption is the idea that human capital specificity or turnover costs lead to rents arising from continuing the employment relationship, thus creating a bilateral monopoly between the firm and each worker.<sup>18</sup> 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.<sup>19</sup> We 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 firms' production functions have decreasing returns to scale and lower 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 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.

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<sup>18</sup>Alternatively, incumbent workers could be hard to replace if 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.

<sup>19</sup>See Malcomson (1999) for an overview in the context of employment contracts. In Appendix A.2, we discuss wage renegotiation in a model with partially binding but incomplete contracts which leads to some wage rigidity.

Wages are determined in pairwise negotiations between the firm and each worker in which the surplus is split equally.<sup>20</sup> 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 with equal bargaining power.<sup>21</sup> Labor contracts are assumed to be non-binding in the sense that no long-term contracts can be written.<sup>22</sup> 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  $\underline{w}$ :

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

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:

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

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  $\tilde{\pi}(2) = F(2) - 2 \cdot \tilde{w}(2)$ ; 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:

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

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

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

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<sup>20</sup>The setup can be easily extended to situations with asymmetric bargaining power as in section A.1.

<sup>21</sup>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.

<sup>22</sup>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:<sup>23</sup>

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

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 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:

$$\underbrace{\tilde{w}(N-1) - \tilde{w}(N)}_{\text{Wage Change}} = \frac{1}{N+1} \left( \underbrace{\sum_{i=0}^{N-1} \frac{2i}{N(N-1)} \Delta F(i)}_{\text{Weighted Marginal Product of } i\text{th worker}} - \underbrace{\Delta F(N)}_{\text{Marginal Product of } N\text{th worker}} \right). \quad (6)$$

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  $N - 1$ .<sup>24</sup> For a single-factor production function with decreasing returns to scale,  $F'(N) > 0$ ,  $F''(N) < 0$ , i.e., substitutability among incumbents, the wages of remaining incumbent workers thus rise following the exit of a coworker from the firm, since  $\Delta F(i) > \Delta F(N)$ ,  $\forall i < N$ . For a constant-returns-to-scale production function, the wage effect is zero. If the production function features increasing returns to scale—implying that incumbent workers are complements to each other—the wage effect of a worker exit is negative because  $\Delta F(i) < \Delta F(N)$ ,  $\forall i < N$ . In the Cahuc, Marque, and Wasmer (2008) model with heterogeneous labor, that we discuss in Appendix A.1, a similar logic arises with wages of substitutes rising and complements falling after a worker exit.

<sup>23</sup>See equations (2) and (3) in Stole and Zwiebel (1996). Note that this solution is only feasible if  $\Delta F(i) \geq \underline{w}$ ,  $\forall i \leq N$ .

<sup>24</sup>Note that the weights sum up to 1:  $\sum_{i=0}^{N-1} \frac{2i}{N(N-1)} = 1$ .

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

We now illustrate wage effects in a model with a pool of workers on the external labor market from which the firm can hire as in de Fontenay and Gans (2003), which relaxes the assumption that workers cannot be replaced externally. The model nests the Stole and Zwiebel model as well as the competitive labor market as corner cases and documents that wage effects on incumbent workers are zero in labor markets with a large pool of suitable workers available on the external market. More generally, wage effects become smaller in magnitude 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 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 bioengineer quits, a firm that hires a similar engineer with industry experience can continue the production process without much disruption but would not be able to do so if it hires 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  $\underline{w}$ :

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

This setup nests the competitive labor market case when the number of replacement workers on the outside labor market becomes large, which results in wages paid by the firm corresponding to workers' outside wages and no rents earned by workers ( $\lim_{\bar{N} \rightarrow \infty} \tilde{w}_{\bar{N}-N}(N) = \underline{w}$ ). It also nests the case with irreplaceable workers when no outsiders are available and  $\bar{N} = N$ , and the firm pays wages according to (5) as in Stole and Zwiebel.

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. 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:

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

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), we can directly test two hypotheses regarding the fluidity of labor markets using our 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  $\lim_{\bar{N} \rightarrow \infty} \tilde{w}_{\bar{N}-1-N}(N) - \tilde{w}_{\bar{N}-N}(N) = 0$ .

The second hypothesis delivers a comparative static to test the importance of labor pooling and labor market thickness.<sup>25</sup> 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 strong agglomeration force.<sup>26</sup> Moretti (2011) describes a potential labor pooling channel of particular relevance for our study positing 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,” while noting that most evidence on the importance of this channel is indirect. Based on the research design in this paper, we can directly assess the importance of this labor pooling channel by estimating the incumbent worker wage response to worker exits in different labor markets. If this force matters, firms that are located in local labor markets with an agglomeration of similar types of labor will be able to hire replacement workers more easily, leading to smaller wage effects on incumbents.

### 3 Empirical Setting and Data

#### 3.1 Empirical Setting: German Labor Market

To provide context for the following analysis, we briefly highlight several relevant characteristics of the German labor market. Our analysis of the effect of worker exits focuses on small firms.<sup>27</sup> 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, such firms with less than 250

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<sup>25</sup>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.

<sup>26</sup>See, e.g., Helsley and Strange (1990) and Rotemberg and Saloner (2000) for formalizations of labor market pooling as an agglomeration force.

<sup>27</sup>See sample restrictions in Section 4.2.

employees accounted for 99.5% of firms and 61.3% of employment.<sup>28</sup> In comparison, employment in similar-sized firms in the United States in 2012 accounted for 43.3% of employment.<sup>29</sup> In our analysis, we focus on a sample of firms with less than 30 employees which account for about 30% of employment. Relative to the OECD average, Germany has a relatively high manufacturing share at 22.6% of GDP (OECD: 15.0%, US: 12.7%).<sup>30</sup>

A key feature of the German education system is apprenticeship training offered by firms. As part of an apprenticeship training, a worker receives training in occupation- and industry-specific skills at a particular firm and a vocational school.<sup>31</sup> 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, which give firms more flexibility to negotiate with their workers directly, in particular to pay below-CBA wages, have become increasingly common. The prevalence of opening clauses started to increase in the 1990s (Brändle et al., 2011); by 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; and Dustmann et al., 2014).

## 3.2 Primary Data Source: Social Security Records

We 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 allows us 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.

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<sup>28</sup>Source: Eurostat, information for 2012. According to the EU definition, small- and medium-sized enterprises are defined as enterprises with fewer than 250 employees, with sales not exceeding EUR 50 million and an annual balance sheet not exceeding EUR 43 million.

<sup>29</sup>Source: Own calculations based on 2012 employment data from the Longitudinal Business Database 1977-2013, United States Census Bureau.

<sup>30</sup>Source: World Bank National Accounts Data, information for 2012.

<sup>31</sup>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.

First, wages are directly reported as part of administrative procedures and, as a result, measurement error is low, which further increases the reliability and precision of estimates. Second, the dataset is large covering all employment subject to Social Security in Germany, which allows for a relatively precise estimation of effects and enables an analysis of wage effects for different types of firms and workers to shed light on the mechanisms 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 our analysis covers about 82% of employment in Germany.<sup>32</sup> 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.

We use data on workers' daily earnings as the primary outcome variable. The earnings variable reports gross earnings which are reported as daily earnings associated with a specific employment spell.<sup>33</sup> For the analysis, we 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. The main dataset does not contain information on the exact hours worked but does 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. We follow the existing literature using this data source (see, e.g., Dustmann et al., 2009; and Card et al., 2013) and use the terms earnings and wages interchangeably. In our analysis, we also assess whether hours of work are affected at the part-time versus full-time margin. In addition, we leverage novel data on hours worked reported by the Statutory

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<sup>32</sup>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).

<sup>33</sup>The reporting of earnings occurs typically per employment spell or, for spells lasting longer than a calendar year, as an average for the calendar year. In the IEB database, earnings are reported as average daily earnings average over the reporting period.

Accident Insurance from 2010 to 2015 to directly assess effects on hours for that time period.

A drawback of the earnings data is that—as in many administrative datasets—earnings are top-coded above the Social Security earnings maximum.<sup>34</sup> In the sample we work with, 6.0% of earnings observations are censored. As our analysis focuses primarily on within-worker and within-establishment variation in wages, imputation procedures based on lagged or current individual or employer-level information would not add additional information for the analysis. We therefore do not impute earnings above the Social Security earnings maximum and instead set wages to the earnings maximum if they are top-coded. Our analysis thus does not capture variation in wages above the earnings maximum.<sup>35</sup>

To assess the interdependencies between workers inside the firm and understand heterogeneity in the effect of worker exits, we 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*).<sup>36</sup> 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. We then use this horizontal classification to identify groups of workers inside a firm who work in jobs with a similar or distinct thematic focus.<sup>37</sup> Second, occupations are classified vertically based on the skill requirements of the occupation. We use this vertical categorization to identify workers in managerial and supervisory roles.<sup>38</sup>

Our 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.<sup>39</sup> The assignment of establishment IDs implies, for example, that two bakeries operated by the same firm in the same city would

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<sup>34</sup>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 our sample is around 30,000 EUR, i.e., about half of the 2011 earnings maximum.

<sup>35</sup> We have checked that the results are robust to dropping observations of individuals who at some point in the study period earned above the earnings maximum.

<sup>36</sup>See Paulus and Matthes (2013) for a detailed overview.

<sup>37</sup>The horizontal classification is based on a worker's 1-digit occupation group.

<sup>38</sup>We 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. They typically require a qualification as master craftsperson, graduation from a professional academy, or university studies (see Bundesagentur für Arbeit (2011)) .

<sup>39</sup>The Social Security system issues a new establishment ID after an ownership change and other reorganizations. Hethy-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, we 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.



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, our 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 we consider, an estimated 84% of establishments correspond to single-establishment firms.<sup>40</sup>

## 4 Empirical Strategy

We implement a dynamic difference-in-differences design in which we 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—that have similar lagged characteristics but did not experience the death of a worker that year. The first part of this section describes the identification of unexpected worker deaths in the Social Security data. Next, we describe how we 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. We then provide summary statistics for the treatment and comparison group. Finally, we 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, we leverage deaths of workers as a source of variation in a firm’s labor supply. We 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. We 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

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<sup>40</sup>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., Card et al., 2013), we will use the terms establishment and firm interchangeably throughout.

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, we restrict our 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.

We focus on deaths that are arguably premature and unexpected. First, we restrict the sample to deaths of individuals who are younger than 65 at the time of death. Second, we 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, we drop individuals who had a sickness leave in the five years before their death.<sup>41</sup> 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.<sup>42</sup> Receipt of such wage replacement benefits is reported in the data, which allows us to restrict the sample to individuals who did not experience a hospitalization or longer sickness leave before their death.<sup>43</sup> 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 unanticipated deaths, such as those 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 et al., 2010).<sup>44</sup> To circumvent this problem, we 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 treatment group worker-firm pairs in which the worker died.

**Time Notation.** We let  $t$  denote calendar years,  $d$  event years, and  $k = t - d$  the year relative to an event. For a given year  $t$ , we measure outcomes on July 1 of that year.<sup>45</sup> A

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<sup>41</sup>This restriction leads us to drop 42% of employer-reported deaths.

<sup>42</sup>Shorter sickness leaves are mandatorily covered by employers and are typically not observed in the data.

<sup>43</sup>The data do not distinguish between the different kinds of wage replacement benefits (“Entgeltersatzleistungen”) which also include maternity benefits. As we 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.

<sup>44</sup>See also Borusyak and Jaravel (2016) for a discussion of identification challenges in event study designs.

<sup>45</sup>Employment is reported at a daily frequency. As noted in 3.2, earnings are reported as an annual average for the typical employee. We follow the reporting of other variables in the IEB and IAB data and choose

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, we identify the set of worker deaths in  $d$  for whom the restrictions described in 4.1 are met.<sup>46</sup> For each worker who died in  $d$  and for their employer at the time of death, we 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 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, we record baseline characteristics in  $d - 4$  for this comparison group pool.

**Matched Sampling to Select Comparison Group.** We implement a matched sampling procedure *separately for each event year  $d$* . For each deceased worker-firm pair in the treatment group, we 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) who 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  $d$ , we select placebo deceased worker-firm pairs from the comparison group pool of worker-firm pairs that did not experience a death in  $d$  to match exactly the following characteristics of actual deceased worker-firm pairs in the treatment group:

- Worker characteristics: age in years, gender, education group<sup>47</sup>, 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

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July 1 as the relevant reference date.

<sup>46</sup>The time period chosen ranges from 1980 to 2007 as death notifications are reported from 1980 onwards and as we require a sufficiently long post-death period exists as employment and wage data are available until 2011.

<sup>47</sup> We 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.

comparison group (86% vs. 62% men).<sup>48</sup> An exact match is found for 95.81% of worker-firm pairs in the treatment group. When no exact match can be found, i.e., in the remaining 4.19% of cases, the deceased worker-firm pair is not included in the sample. When multiple potential matches for a deceased worker-firm pair are available, we 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.<sup>49</sup>

The matched sampling procedure implies that the comparison between the treatment and the comparison group is between coworkers and establishments of actual and placebo 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, we 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, we 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.<sup>50</sup> 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.<sup>51</sup> We 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.<sup>52</sup> Finally, we 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 and focus on a balanced panel of

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<sup>48</sup>We have also verified that we 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, 2018), we implement an exact matching approach. We 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.

<sup>49</sup>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.

<sup>50</sup>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*)).

<sup>51</sup>In Appendix Table B.1, we document that the treatment effect of a worker exit on the remaining incumbent workers' wages decreases with establishment size.

<sup>52</sup>Specifically, we drop all establishments with an industry code larger than 870 in the 1973 edition of the German Classification of Economic Activities.

firms. In both the treatment and the comparison group, we 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. To include workers with short tenure in our analysis, we do not condition on employment at the *same* firm in the years before  $d$ , only on full-time employment at any firm in  $d$  and in  $d - 4$ .

### 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 we implement permits differences in average levels of outcome variables between the treatment and comparison group and instead relies on a common trend assumption (see Section 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 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, respectively. 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  $k = -4$ .<sup>53</sup> 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, we define a sample of incumbent workers as the set of full-time coworkers of the deceased in event year  $d$ .<sup>54</sup> Columns (3) and (4) of Table 1 report summary statistics for these

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<sup>53</sup>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  $k = -1$ .

<sup>54</sup>Similar to the sample restriction for the actual and placebo deceased workers, we 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, we set their earnings to zero. In Table B.2 in the Appendix, we also report results for two additional groups of

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.** We 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 we 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. We assess 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 our sample ( $p = 0.336$ ).

#### 4.4 Estimating Equations and Identification

We 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, retention rates, and hours. Two advantages of this approach are (1) that the research design allows for a direct, graphical assessment of treatment effects over time, and (2) that 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, we describe the econometric framework and discuss and test the identification assumptions.

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

$$y_{jk} = \alpha + \gamma_j + \sum_{k=-3}^5 \beta_k \times \mathbb{1}(\text{period}_k) + \sum_{k=-3}^5 \beta_k^{Treated} \times \mathbb{1}(\text{period}_k) \times \text{Treated}_j + \epsilon_{jk}, \quad (9)$$

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

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incumbents: the sample of part-time coworkers and individuals who were apprentices.

time,  $\mathbb{1}(\text{period}_k)$ .<sup>55</sup>  $\text{Treated}_j$  is an indicator function for treatment status. The coefficients of interest,  $\beta_k^{\text{Treated}}$ , capture the effect of an actual worker death in year  $k = t - d$  in the treatment group and are normalized to zero in  $k = -1$  ( $\beta_{-1}^{\text{Treated}} = 0$ ). We define the *short-run treatment effect* as the effect in the first post-death year,  $\beta_1^{\text{Treated}}$ , and a *long-run treatment effect* as the average of treatment effects in the five-year post-period,  $\frac{1}{5} \sum_{k=1}^5 \beta_k^{\text{Treated}}$ . We 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).<sup>56</sup>

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 we do not assume that the treatment and comparison group would have the same average outcomes in the absence of treatment. Rather, the variation we 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 treatment group 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 follow 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 treatment group. Given that the estimated effects on coworker wages are on average positive, a potential threat to identification 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

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<sup>55</sup>Formally, we 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. The model in (9) does not include calendar year fixed effects as calendar time is balanced between the comparison and treatment group as a consequence of the matched sampling procedure which we implement separately for each event year  $d$ . We have also run all specifications allowing for calendar year fixed effects which, mechanically, does not change the point estimates of the treatment effects and, in addition, leads to very similar standard errors.

<sup>56</sup>As a robustness check, we also estimate specifications with standard errors clustered at the match level (see Abadie and Spiess, 2016) and standard errors clustered at the firm level treating a firm sampled in different event years as one firm. Both alternative procedures lead to virtually identical standard errors.

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 chronic rather than short-term stress levels (Kivimäki et al., 2006).<sup>57</sup>

In addition to analyzing pre-trends, we implement a further test to gauge the importance of these potential challenges to identification, and document 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  $k > 0$ . We 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 (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 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 (9) above describes specifications to estimate treatment effects on firm-level outcomes such as employment and hiring. To analyze treatment effects on outcomes for incumbent workers, e.g., wages, we 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.

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

$$y_{ijk} = \alpha + \gamma_{ij} + \sum_{k=-3}^5 \beta_k \times \mathbb{1}(\text{period}_k) + \sum_{k=-3}^5 \beta_k^{Treated} \times \mathbb{1}(\text{period}_k) \times \text{Treated}_{ij} + \epsilon_{ijk}. \quad (10)$$

Here,  $y_{ijk}$  denotes the outcome  $y$  for incumbent worker  $i$  at firm  $j$  in year  $k = t - d$  relative

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<sup>57</sup>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.”



to the worker death occurring in year  $d$ . The model includes incumbent worker-firm effects that absorb unobserved heterogeneity across incumbent workers. As before, the model includes leads and lags around event time,  $\mathbb{1}(\text{period}_k)$ , 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 specifications (9) and (10). 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, we estimate variations of the econometric models in (9) and (10) that include interactions between the post-period treatment effects, i.e., the interaction of  $\mathbb{1}(\text{period}_k)$  and treatment status, and some covariates, e.g., the skill level of the deceased worker. Whenever such interaction terms are included, the model also includes a set of interactions of the baseline period effects,  $\mathbb{1}(\text{period}_k)$ , with the relevant covariate.

## 5 Results

Our main results show that worker deaths lead to increases in both 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, we estimate *negative* effects on the wages of workers in other occupation groups. Next, we document that firms in thicker markets for skill hire more externally and change wages of incumbents by less in response to a worker exit. As a robustness check, we also draw on newly available data on hours worked and detect no increase in incumbent hours after a coworker death. Finally, we complement our quantitative analysis with our own survey to provide further context for firms' responses to worker exits.

Taken together, our results 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, we 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 treatment group firms is temporarily lowered. Hiring rises sharply and some hiring occurs in occupations other than the one of the deceased, thus providing evidence consistent with the notion that workers who 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. We 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 turnover was so high 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) workers 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.<sup>58</sup>

To give an overview of the employment effects, we decompose the labor supply shock due to a worker death into three effects: hiring, retention, and a residual employment effect. Figure B.1 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%. Since worker exits affect the retention of incumbents, the next section will analyze the effects on incumbents’ wage and employment outcomes more closely.

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

This section examines the average effects on incumbents of worker exits due to unexpected death and shows that such exits, on average, raise incumbent worker wages. The finding of nonzero wage effects is hard to reconcile with completely fluid labor markets and perfect substitutability between outsiders and incumbent workers, which implies that firms face frictions in replacing workers 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 4 documents the dynamics of the treatment effect on the earnings of incumbents.<sup>59</sup> 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 a real 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 4 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. On

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<sup>58</sup>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”, i.e., as an apprentice.

<sup>59</sup>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 B.3 in the Appendix.

average, 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. The positive effect in  $k = 0$  is entirely driven by worker deaths that occur in the same calendar year as wage measurement in  $k = 0$  and is not affected by deaths that occur in the first half of the subsequent calendar year. In Figure 5, we 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.

**Benchmarks for Magnitude of Effects.** We offer three empirical benchmarks to gauge the magnitude of the average effects on incumbent wages.<sup>60</sup> 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 the magnitude of returns to experience. Based on the same data source, Dustmann and Meghir (2005) estimate returns of about 0.7% per year of industry experience for a sample of workers comparable to a typical incumbent worker in our sample (with

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<sup>60</sup>Note that the following calculations will understate the overall impact as there is substantial heterogeneity in effects and treatment effects are negative for some subgroups (see Section (5.3)).

apprenticeship training and more than five years of experience). This suggests that, on average, worker deaths raise incumbent worker wages for a period of several years by about the amount as would having an additional year of industry experience.

Third, to provide a benchmark of the magnitude from the firm’s perspective, we compare the treatment effect on the sum of incumbent worker wages to estimates of standard turnover costs.<sup>61</sup> 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 et al. 2008, and equation (15) in Appendix A.1). 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. In relative terms, the estimated magnitude of 18% of the deceased worker’s average annual earnings is comparable in magnitude to the estimates in Boushey and Glynn (2012) who report a median estimate for turnover costs of 21 percent of an employee’s annual salary based on a survey of 27 case studies.<sup>62</sup> The fact that the wage effects are on the same order of magnitude as consensus estimates of turnover costs—which are thought to be the main 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 friction that firms face in replacing workers.

**Additional Incumbent Worker Outcomes.** In Figure 7, we 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.<sup>63</sup> Turnover of incumbent workers in treatment group firms is lower: each incumbent worker has, on average, about a 0.5 percentage point higher probability of remaining employed at the same firm.<sup>64</sup> 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

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<sup>61</sup>For the following comparison, it is important to bear in mind that the wage effects are estimated for the incumbent sample and incumbents remain in the sample regardless of whether they switch firms.

<sup>62</sup>See also Manning (2011), Table 2, for an overview of hiring cost estimates.

<sup>63</sup>See also Appendix Table 4.

<sup>64</sup> We also analyze the effects on outcomes of part-time workers and apprentices in Table B.2. We do not find evidence for a treatment effect on the retention of part-time incumbents. In contrast, the effect on the retention of apprentices is three to four times larger than the retention effect on full-time coworkers of the deceased: individuals who are apprentices in a firm in which a worker dies unexpectedly have about a 1.9 percentage point higher probability of staying employed at the same firm in the subsequent five years.

up employment with other firms.

The treatment effect on the probability of part-time employment is a precisely estimated zero. Even though our primary dataset does 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.<sup>65</sup> In addition, we analyze novel data on hours worked for a sampling period from 2010 to 2015 and do not find positive hours effects in response to coworker deaths (see Section 5.6). Even if some of the wage effect were driven by an intensive margin response, the finding of nonzero effects of a worker exit on incumbents' earnings would still indicate that the worker who exited cannot be costlessly replaced on the external market.

Further evidence on the treatment effect on the probability of a promotion also suggests that factors besides 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 7). To obtain a proxy for internal promotions, we first calculate average wages at the 5 digit occupation level by drawing on a 10% sample of individuals from the IEB and regress individuals' log wages on occupation dummies and individual fixed effects. We 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 than 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 being promoted is also very small at 0.8%.<sup>66</sup> The probability of an internal promotion of an incumbent worker in the treatment group is therefore 10% higher than in the comparison group.<sup>67</sup>

Figure 6 shows that the positive shift in the wage distribution for the treatment relative to the comparison group goes along with a higher fraction of individuals receiving positive nominal earnings increases. The outcome variable in Figure 6 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  $X = 10\%$ , implying that the fraction of incumbent workers who experience

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<sup>65</sup>In Table B.2, we also report treatment effects for the samples of part-time incumbents and apprentices and do not find evidence for an hours response at the full- vs. part-time margin.

<sup>66</sup>The baseline magnitudes are relatively small as they cannot reflect promotions within an occupation which richer, firm-level personnel records may report.

<sup>67</sup>Further evidence indicates that the positive effect on promotions is driven by positive effects on coworkers in occupations with lower average wages than that of the deceased.

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.

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

Having established that worker exits affect wages of incumbent workers, we next assess heterogeneity in the effect 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 high-skilled workers and managers are complements to other workers inside the firm.

In a first step, we estimate the effect on wages of incumbent workers in the same occupation group as the deceased versus on incumbents in other occupation groups.<sup>68</sup> Figure 8 shows that the effect of a worker death on incumbent workers in the same occupation group as the deceased is 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 not statistically significant. The results support the premise that the research design can identify the within-firm substitutability of insiders to the extent that occupation is a natural measure of similarity and substitutability of workers.

Next, we 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 from deaths of high-skilled workers and managers on other workers. We 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, we 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 9).<sup>69</sup> The reason for focusing on the skill intensity of the occupation level rather than on

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<sup>68</sup> We 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.

<sup>69</sup> See also Table 5 for additional information.

education levels directly is that the modal education level is an apprenticeship training and apprenticeship programs differ widely in the skill level of the targeted occupation. To measure the skill level of an occupation we calculate the average years of education at the 5-digit level based on a 20% sample of IEB biographies. We 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 for average years of education in the sample of deceased workers. As panel (A) of Figure 9 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, we focus on education levels directly and find a similar pattern of effects (see panel (B) of Figure 9).<sup>70</sup> We 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).<sup>71</sup> Since the overwhelming majority of workers in our sample have an apprenticeship training (79.5%), 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 significant ( $p < 0.1$ ), providing evidence suggestive of complementarities.

As a third dimension of skill, we 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 9 and Table 7). We proxy for 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.<sup>72</sup> Based on this distinction, we 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).

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<sup>70</sup>See also Table 6 for additional estimation results by education levels.

<sup>71</sup>Further formal education refers to a university entrance exam (*Abitur*) or a college degree.

<sup>72</sup>Specifically, we 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.



**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 since 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 Lucas (1978) model posits a two-factor production function with decreasing returns of low-skilled labor and complementarity between high- and low-skilled labor:

$$Y = h \cdot F(L), \tag{11}$$

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, 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 worker deaths on incumbent workers in similar occupations and negative wage effects of manager or higher-skilled worker deaths on the wages of incumbents in other occupations. Our 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 occupations. The Lucas (1978) model’s feature that output is sensitive to managerial human capital, but less so to lower-skilled workers’ 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 evidence on 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

We investigate treatment effect heterogeneity by tenure of the deceased worker and by the specialization of the deceased worker’s occupation to assess whether workers with more specific human capital are harder to replace. 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).<sup>73</sup>

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, we assess treatment effect heterogeneity based on a measure of specialization at the occupation level. To proxy for specialization, we 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 capturing 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, we calculate returns to experience based on Mincer equations estimated separately for each 5-digit occupation. We then use 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 mask heterogeneity in the effect on coworkers in the same occupation versus coworkers in other occupations. In columns (5) and (6), we document that treatment effects on incumbent worker wages in the same occupation as the deceased 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. Thus, for both worker-level measures of human capital specificity, we find larger wage effects on incumbent workers suggesting that workers with

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<sup>73</sup>See also recent evidence in Shaw and Lazear (2008) who document that worker *productivity* increases with tenure.

more specific human capital are harder to replace externally.

## 5.5 Do Thick Markets Make Workers More Replaceable?

The results in the previous subsection imply that human capital specificity lowers the substitutability between incumbents and outsiders. In this section, we investigate this mechanism further and assess whether incumbents and outsiders are more substitutable in thick labor markets, where there is a relative agglomeration of workers with the relevant skills. We 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. This lends 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) theory in which 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, our 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.<sup>74</sup> 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

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<sup>74</sup>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). Our 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.

skills". As our research design analyzes the effects of idiosyncratic shocks to workers' labor supply, we can directly assess the importance of this particular labor pooling channel.

We operationalize this test by assessing heterogeneity in the effect of worker deaths by measures of labor market thickness and occupational specialization. To proxy for labor market thickness, we measure the relative agglomeration of workers in the deceased's occupation in the local labor market. To delineate local labor markets, we 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 (10) shows the 50 German labor market regions based on the categorization in Kropp and Schwengler (2011) that we follow.<sup>75</sup> We measure thickness at the 5-digit occupation  $\times$  commuting zone level as the share of employment in the relevant occupation in that commuting zone relative to the nationwide share of employment in that occupation.<sup>76</sup> We then classify 5-digit occupation  $\times$  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 we 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.

In thick labor markets, incumbent wages respond less to a worker death and the differential is particularly pronounced for specialized occupations. Figure 11 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.<sup>77</sup> For the sample of all worker deaths, the point estimate for the wage effect is twice as large in thin labor markets compared to thick ones; 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, we 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

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<sup>75</sup>An advantage of the Kropp and Schwengler (2011) categorization is that the classification into labor market regions is relatively stable over time.

<sup>76</sup>Formally, we calculate labor market thickness for 5-digit occupation  $o$  in labor market (commuting zone)  $l$  in year  $d$  as  $T_{old} = \frac{\sum_{o' \in \mathcal{O}} \frac{e_{o'ld}}{e_o}}{\sum_{o' \in \mathcal{O}} \frac{e_{o'}}{e_o}}$ , 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.

<sup>77</sup>See also Panel (A) of Table B.5.

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 ( $p = 0.02$ ).

We find qualitatively similar patterns when using different measures of labor market thickness to estimate heterogeneity in the treatment.<sup>78</sup> Two additional measures of thickness that we 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, we 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 the relative agglomeration of worker in the same occupation. One interpretation of this finding is that the relative agglomeration in an occupation may be a better proxy for local labor market thickness. However, since the analysis relies on observational variation in thickness, the results do not constitute definite evidence favoring one thickness measure over another.

To shed further light on the relevance of labor market thickness, we 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 12 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, we find a substantial differential between thick and thin 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 (1) that workers are more replaceable in thick labor markets, and (2) that the substitutability of incumbent workers and outsiders decreases with human capital specificity.<sup>79</sup> Our 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

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<sup>78</sup>See panels (B) and (C) of Table B.5. In Table B.5, we also report differences by the local unemployment rate which we discuss in Section 6.1.

<sup>79</sup>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.

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.

## 5.6 Evidence on Hours Response

We draw on data based on unique information from the German Statutory Accident Insurance to assess the effect of worker deaths on coworkers’ work hours. For the years 2010 to 2015, information on workers’ hours as reported by the firms are included in the IEB database.<sup>80</sup> We apply our research design for wages using hours-per-week as outcome variable and find that the positive average effect of a worker death on coworker earnings does not appear to operate through an hours response. However, we find evidence consistent with some of the negative effects of manager and high-skilled worker deaths operating through reduced work hours.

Before analyzing potential effects on hours, we assess the reliability of the hours data with several validation tests. First, we tabulate hours per week by gender and benchmark it against data from the Structure of Earnings Survey (*Verdienststrukturerhebung*) 2014 (see Statistisches Bundesamt, 2016). As Appendix Table B.6 documents, the summary statistics for the work hours in the administrative data line up very closely with the information from the Structure of Earnings Survey. The average hours per week in the administrative data are 31.55 while the survey average is at 30.91 (both including overtime). Both the administrative and the survey data show a pattern of higher work hours per week for men (34.33 vs. 34.66) compared to women (28.44 vs. 26.95). We also plot the distribution of hours per week in Appendix Figure B.2. We next follow a validation test from Lachowska, Mas, and Woodbury (2018) who assess the reliability of administrative hours measures using data from Washington state. Building on their procedure, we test whether changes log hours from year to year predict changes in log earnings. As Appendix Figure B.3 shows, we find that changes in log hours within individual over time are positively correlated with changes in log earnings ( $p < 0.001$ ), providing support for the reliability of the earnings measures. In addition, we run several other tests of worker-level predictors of work hours and, e.g., find that part-time workers work 11.16 (se 0.003) fewer hours per week.<sup>81</sup>

Figure B.4 and columns 1 and 2 of Table B.9 extend our main specification to the new

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<sup>80</sup>Information on workers’ hours became available for this limited time period because of a temporary change in the administrative reporting process.

<sup>81</sup>We also note that Gudgeon and Trenkle (2016), based on the same administrative data sources, report evidence documenting hours responses to a tax notch.

sample, using hours per week as outcome variable.<sup>82</sup> On average, we find no evidence for hours increases in response to coworkers deaths. The short- and long-run effects on incumbent work hours are -0.42 (se 0.29) and -0.49 (se 0.32), respectively. That is, point estimates are, on average, *negative* but we cannot rule out that the average effect on work hours are zero. We next assess the effect within and across coworkers in the same occupation (columns 3 and 4 of Table B.9) and find effects close to zero for coworkers in the same occupation. In contrast, we find negative effects on workers in other occupations with estimates of -0.70 (se 0.36) and -0.84 (se 0.41) at the short- and long-term horizon, respectively. When we further dissect this finding across skill groups, we find evidence that the negative hours effects on coworkers in other occupations are driven by workers in high-skilled occupations or managers.

The analysis of hours thus reveals that the positive main effect of worker deaths on coworker earnings cannot be explained through an hours response. One potential factor explaining the absence of a positive hours response could be the institutional setup in Germany where labor law, agreements and contracts put sharp upper limits on work hours.<sup>83</sup> The analysis of the hours response to worker deaths in other occupations would not be directly affected by those upper limits. The negative hours response for coworker pairs in different occupations, especially in those cases where the deceased was a manager or in a high-skilled occupation, could be consistent with downward nominal wage rigidity and a complementarity between, e.g., managers and workers in other occupations: the labor demand for workers in other occupations falls while the firm cannot nominally lower wages, thus leading the firm to demand fewer hours.

## 5.7 Qualitative Evidence: Survey

To complement our analysis of administrative data, we conducted a survey among small businesses with less than 30 employees in Germany.<sup>84</sup> Firms were invited to participate in a survey about worker exits and their consequences with regard to the job position, replacements, and effects on the remaining workers. 36 firms took part in the survey of which 33 reported at least one worker exit during the past five years. The firms were asked to provide information on the last exit of a worker in each of the three categories: manager, high-skilled and low-skilled worker. We only include observations that contain information on wages and thereby obtain a sample of 33 exits. As reason for the exit, firms predominantly state

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<sup>82</sup>We also report summary statistics for the new sample in Tables B.7 and B.8.

<sup>83</sup>The Hours of Work Act (ArbZG) puts an eight-hour-per-working-day limit on hours; temporary exceptions to ten hours are permissible if the average work shift balances to eight hours within six months through compensatory time off. In addition, collective bargaining agreements lead to further regulation of working hours.

<sup>84</sup>The firms were sampled by infas Institute for Applied Social Sciences.

quits (69%). Other reasons include poaching, sickness, and death. Several firms report that the quit had financial reasons (e.g., “she wanted to earn more”). The survey provides some evidence for the challenges of finding a replacement for workers: in our sample, 34% did not find any replacement, 44% hired a new worker and 22% replaced the worker internally.

In response to a worker exit, 33% of firms change the remaining workers’ wages, with a wage change being more common within the old worker’s occupation. Several firms provide reports of mechanisms that corroborate our findings from the analysis of administrative data. One example is provided by a firm that lost a low-skilled worker to a firm that offered a higher wage. The firm did not find an external replacement within 12 months and states that it raised the remaining workers’ wages significantly. Another example stems from a firm describing that “[the old worker] possessed a lot of knowledge and skills. We had to split her tasks internally between two other workers.” As a consequence of the exit, the firm raised the wages of workers within the same occupation.

## 6 Discussion

### 6.1 Alternative Mechanisms

Our 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, we investigate to what extent our results could be rationalized through alternative explanations.

First, we consider whether changes in the incumbent workers’ amenity value of working at the firm could explain our findings. *Prima facie*, the positive wage effect could be driven by increases in incumbent workers’ compensating differential of working at the firm (Rosen, 1974; Thaler and Rosen, 1976): 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 treatment 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 we 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.



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 that incentivize workers to put forth efforts earlier in their careers to reap later rewards (Lazear, 1979), or models of job assignment and promotions based on seniority which induce workers to invest in specific human capital (Carmichael, 1983, and Prendergast, 1993).<sup>85</sup> 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, our 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, we explore whether the source of frictions that firms face in replacing insiders—which we 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, we find no evidence for long-term effects of worker deaths on employment but estimate wage and retention effects that persist for several years.<sup>86</sup> Our 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 to accumulate specific human capital and to

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<sup>85</sup>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.

<sup>86</sup>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.

become 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*.<sup>87</sup> In an additional contrast to a model in which wage effects are purely driven by search frictions, we find no evidence that wage effects are attenuated in labor markets with a high number of unemployed jobseekers.<sup>88</sup> Taken together with our results on the role of labor market thickness and human capital specificity, the findings 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].

## 6.2 Discussion of Related Literature

Our paper speaks to several additional 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).<sup>89</sup> Our 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 A.2). 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, in which firms agree to renegotiate wages when workers have a credible outside option that they prefer over the contracted wage and prefer to continue the employment relationship. Our 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 renegotiate their wages.

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<sup>87</sup>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 we 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.

<sup>88</sup>See Panel D in Table B.5.

<sup>89</sup>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).

In addition, our results relate to work by Manning (2003) who advocates a view of the labor market in which firms have monopsony power over workers. Our 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 our results do not accord with a model in which firms can simply raise wages by an infinitesimally small amount to hire a suitable new worker externally. Specifically, our estimates are consistent with a very low elasticity of incumbents' labor supply to the firm of less than one.<sup>90</sup> However, our results are less consistent 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 (Manning, 2003, pp. 14-15), while matching models assume that wages are determined through ex-post bargaining after a worker and a firm have met. Our 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 continued renegotiation of wages.

The empirical strategy in our 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 et al., 2006), superstar scientists (Azoulay et al., 2010), or inventors (Jaravel et al., 2015) and documents negative effects on outcomes such as firm performance, collaborator productivity, and co-inventor productivity and earnings.<sup>91</sup> The negative average effects on productivity in Azoulay et al. (2010), and on productivity and wages in Jaravel et al. (2015) imply complementarities among collaborators and co-inventors and are consistent with the negative point estimates that we find for deaths of highly-educated workers.<sup>92</sup> 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 our study, Isen (2013) does not focus on the substitutability of a firm's workers with outsiders or the substitutability among incumbents.

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<sup>90</sup>We use a simple back-of-the-envelope calculation to arrive at a measure of the elasticity of incumbents' labor supply to the firm:

$$\frac{\underbrace{\frac{\Delta \text{Employment}_{at\_Firm_{L=1}}}{\text{Employment}_{at\_Firm_{L=-1}}}}_{0.52\%}}{\underbrace{\frac{\Delta \text{Wage}_{L=1}}{\text{Wage}_{L=-1}}}}_{0.6\%}} = 0.83.$$

<sup>91</sup>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.

<sup>92</sup>Additionally, Jaravel et al. (2015) find positive average effects on inventors at the same firm that are not co-inventors.

At a broader level, our 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 (forthcoming), for instance, investigate spillover effects between researchers in academia. Hayes, Oyer, and Schaefer (2006) find evidence for complementarities between members of top management teams. Mas and Moretti (2009) and Cornelissen, Dustmann, and Schönberg (2017) 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 (2016) find that firms that 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 (2015) estimate large supervisor effects on worker productivity in a setting of technology-based service workers.

## 7 Conclusion

Analyzing shocks to firm-specific labor supply due to unexpected deaths of workers, we demonstrated that firms face frictions in replacing workers externally as such worker deaths affect firms' labor demand for the remaining workers. We also shed light on the sources of frictions in replacing workers and documented that the effects on labor demand are larger when workers' human capital is more firm-specific. We argued that our findings can be interpreted through a simple bargaining model (Stole and Zwiebel 1996a,b) in which human capital specificity leads to imperfect substitutability of insiders and outsiders, and provided evidence at odds with several alternative explanations for our findings. Our research design also allowed us to shed light on the within-firm substitutability of workers. The analysis revealed that deaths of high-skilled workers and managers lead to negative effects on wages of workers in other occupations and thus supports the key assumption of models positing that managerial human capital is a complement to the labor of other workers inside the firm (Lucas, 1978; Rosen, 1982). A fruitful avenue for future research will be an investigation of how different types of production hierarchies (Caliendo, Monte, and Rossi-Hansberg, 2015) amplify or weaken the effects of worker exits and to what extent turnover of key employees triggers reorganizations of such hierarchies.

While our empirical analysis considered the effects of worker exits due to death, it seems plausible that our findings could be used to understand the effects of separations and quits more generally, e.g., the poaching of a worker by another firm. These other settings are potentially highly important: in the case of Germany, more than half of all vacancies are posted to replace workers who quit (Mercan and Schoefer, 2018). Clearly, our estimates—taken at face value—cannot be directly extrapolated to these other settings, as circumstances and

samples differ. However, the survey evidence we provided suggests that similar mechanisms to the ones we have uncovered could be at play for replacing workers due to circumstances other than death. Conceptually, our analysis therefore contributes to our understanding of the factors that make workers hard to replace, although clearly more work is needed to systematically assess the consequences of worker turnover due to other circumstances. By demonstrating that firms hire more externally and raise incumbent workers' wages by less in thick markets, our empirical analysis offers new insights into the black box of matching frictions in the labor market and shows that human capital specificity lowers the fluidity of labor markets.

Our discovery of such external hiring frictions provides evidence that supports the key assumptions of models in which the supply of skilled workers affects firms' technology adoption due to the presence of matching frictions (see Acemoglu, 1996, 1997).<sup>93</sup> For firms considering whether to invest in a new technology that is complementary to specialized 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 choose to invest in a new technology when the pool of skilled workers is large enough. Our paper provides evidence supporting two key assumptions underlying such models by showing that firms face frictions in replacing workers and by documenting that these frictions appear greater when human capital is firm-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 and organizational structures by firms.

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<sup>93</sup>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 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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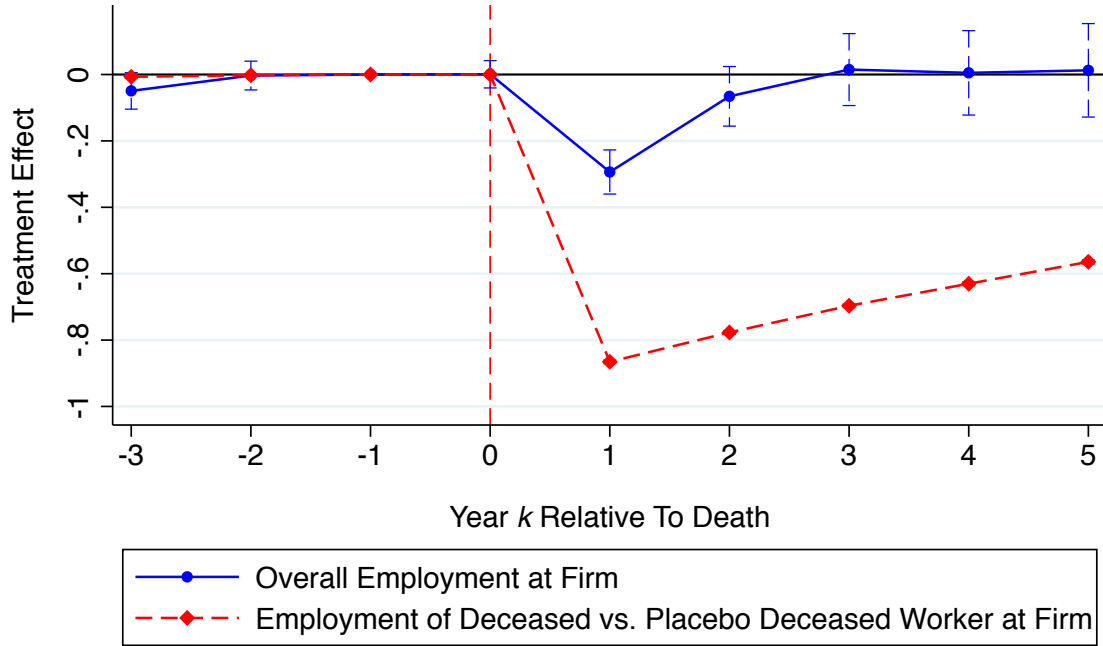
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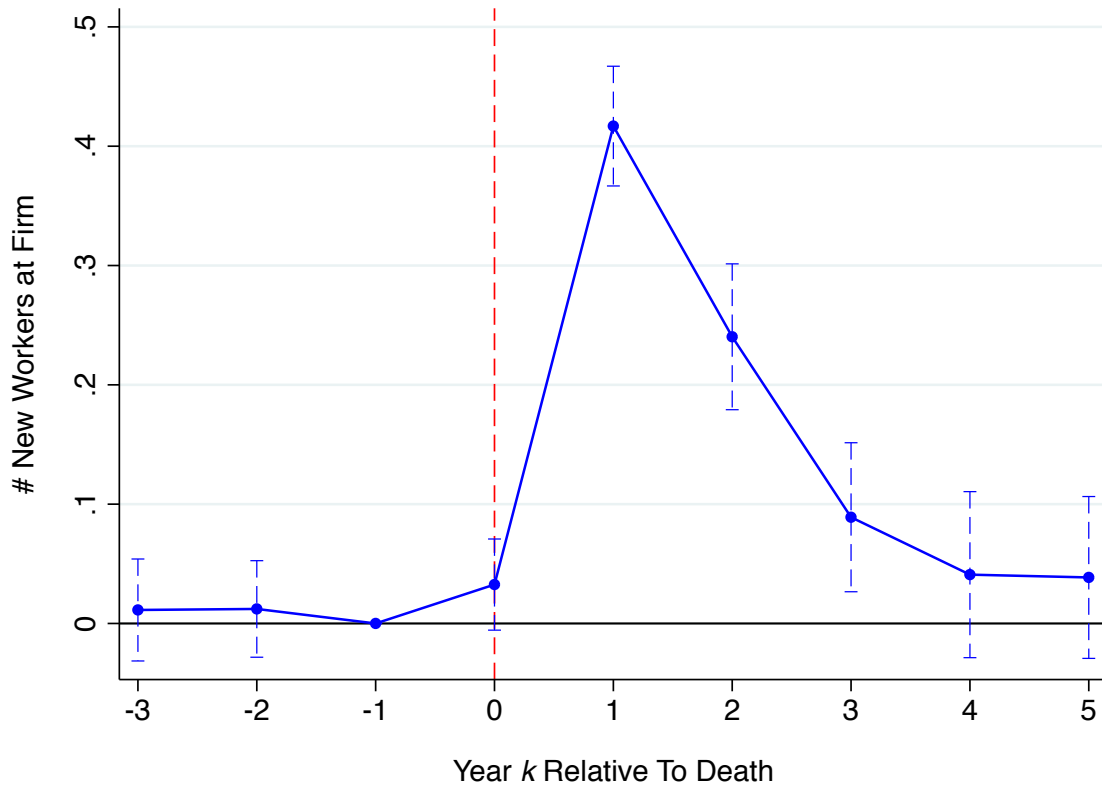
# Figures

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



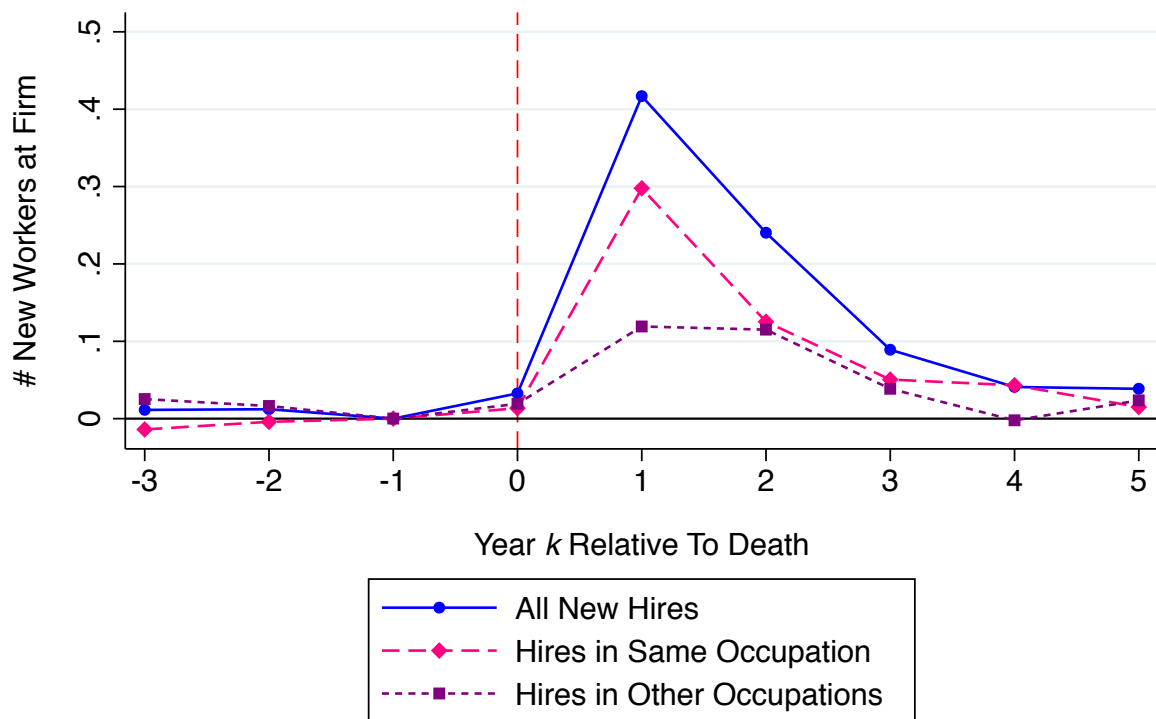
*Note:* The figure shows regression coefficients and associated confidence intervals for the difference between treatment and comparison group in a given year  $k$  relative to the death of a worker in the treatment group firms, i.e., the  $\beta_k^{Treated}$  from the difference-in-differences model in (9). The coefficient in  $k = -1$  is normalized to zero. The first outcome variable measures the overall employment at a firm. The comparison group mean for employment in  $k = -1$  is 14.5. 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 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 treatment and comparison group firms in a given year  $k$  relative to the death of a worker in the treatment firms, i.e., the  $\beta_k^{Treated}$  from the difference-in-differences model in (9). 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.

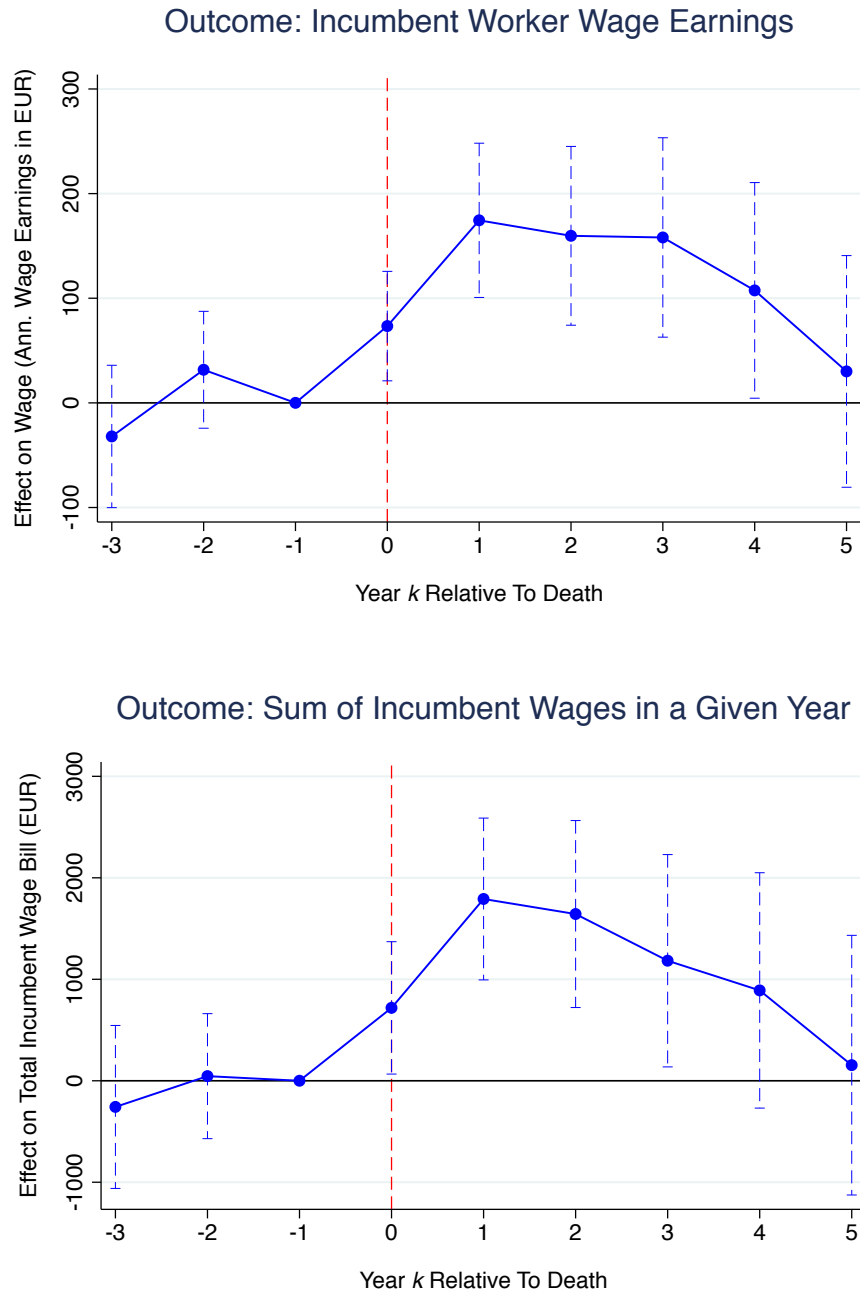
Figure 3: Decomposition of Effects of Worker Death on Hiring



*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  $k = -1$ .

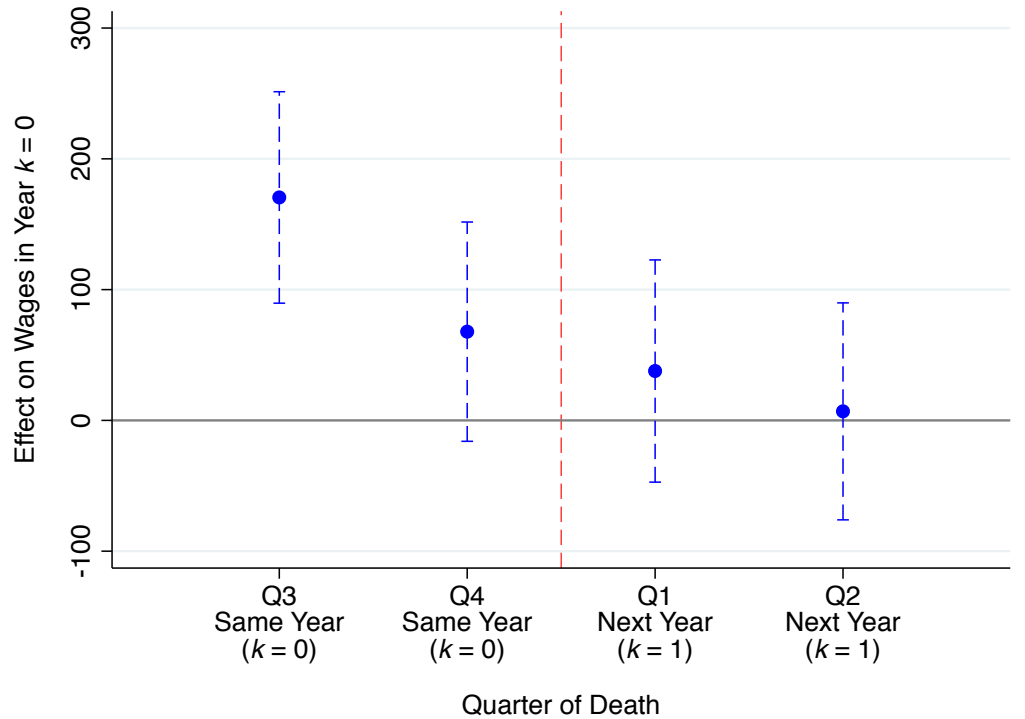


Figure 4: Effect of Worker Deaths on Incumbent Worker Wages



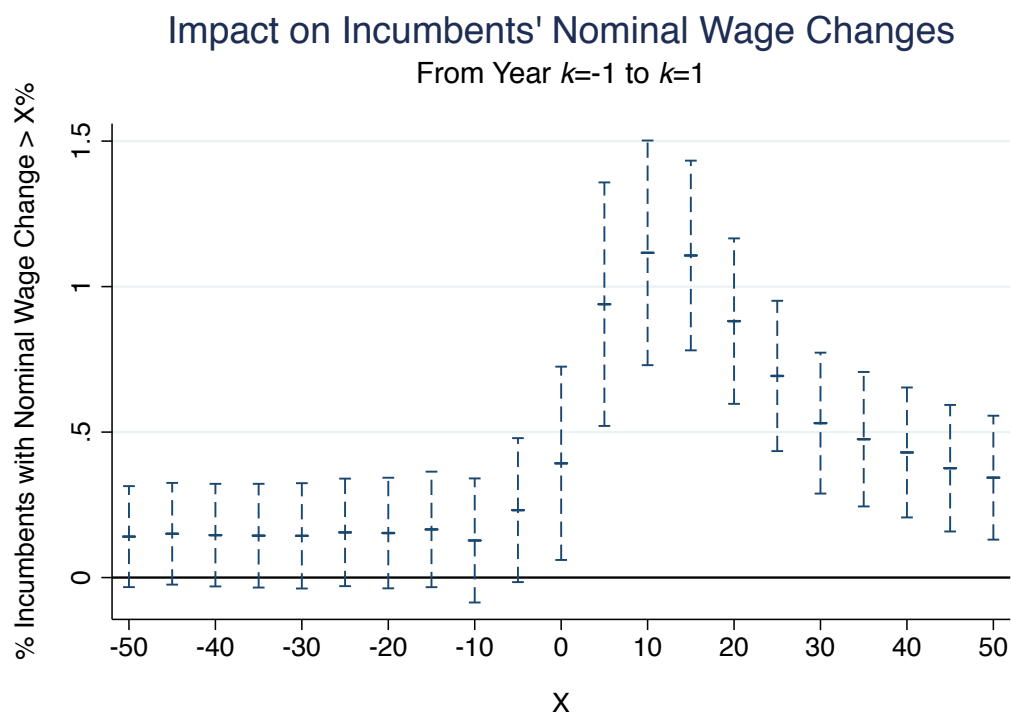
*Note:* The two panels display regression coefficients and associated 95% confidence intervals for the difference between incumbent worker in the treatment and comparison group, i.e., the  $\beta_k^{Treated}$  from equation (10). The coefficients in  $k = -1$  are normalized to zero. In the first panel, the outcome variable is the wage of an incumbent worker (scaled to correspond to yearly earnings, CP We 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 is 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 B.3 for additional information.

Figure 5: Effects in on Incumbent Worker Wages in Year  $k = 0$  By Quarter of Death



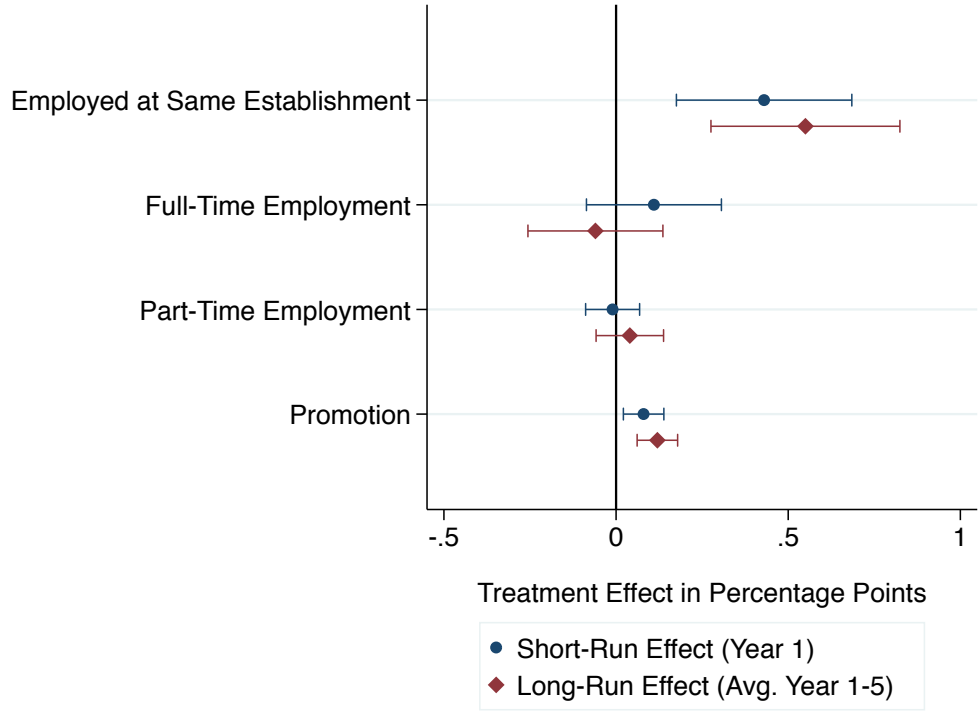
*Note:* The figure presents results of a difference-in-differences regression of wages in year  $k=0$  on treatment status interacted with dummies for the quarter of death of the deceased worker in the treatment group. The positive and statistically significant coefficients for wage effects in year 0 of deaths that occur in Q3 (July, August, and September) document that the positive wage effects in year  $k = 0$  (see, e.g., Figure 4) 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 B.4.

Figure 6: Treatment Effect on Distribution of Incumbents' Nominal Wage Changes



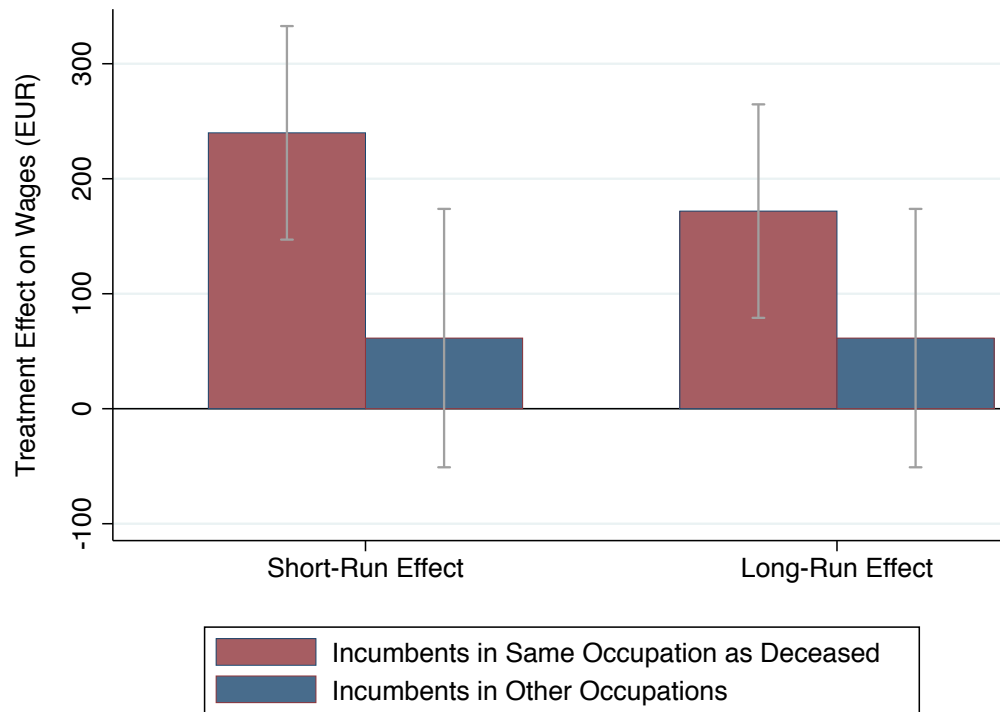
*Note:* The figure shows the treatment effect on outcomes variables that measure whether a worker has experienced a nominal wage change of more than  $X\%$ . Each point estimate is based on a separate regression of  $1((y_1 - y_{-1})/y_{-1} \cdot 100 > X)$  on an indicator for treatment status, where 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 7: Treatment Effect on Incumbent Worker Employment Outcomes



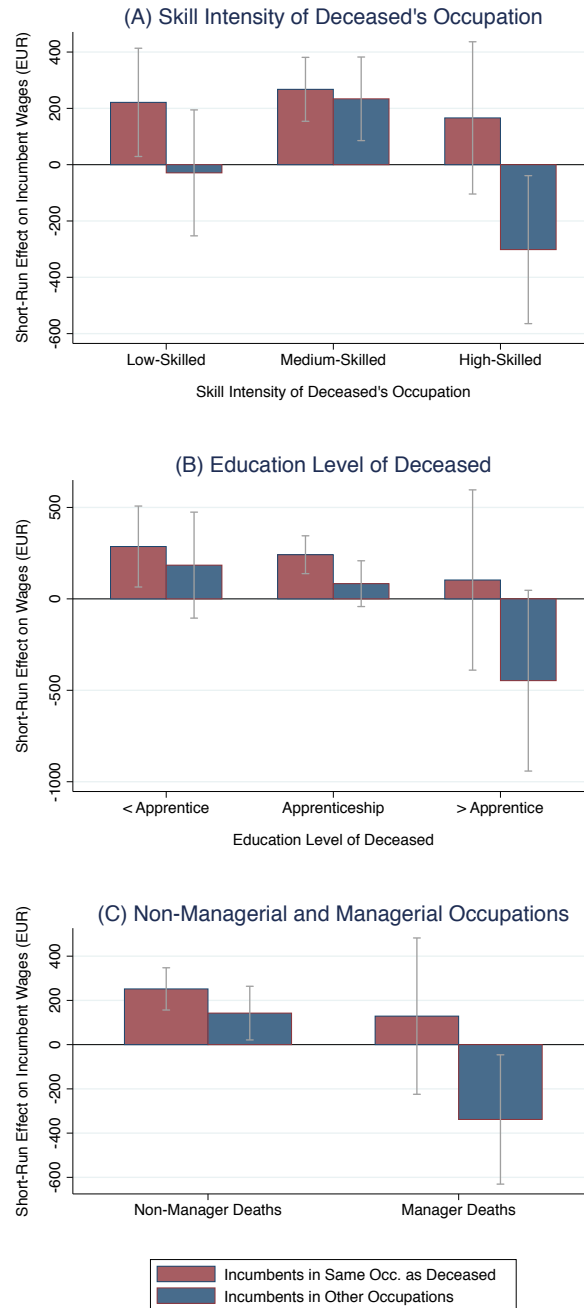
*Note:* The figure displays treatment effects on several employment outcomes of incumbent workers. The mid-points of each interval denote the point estimate of the treatment effect; the range of the interval corresponds to the 95% confidence interval of the treatment effect based on standard errors clustered 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, we draw a 10% sample of individuals from the IEB and regress individuals' log wages on occupation dummies and individual fixed effects. We use the estimated occupation effects to measure promotions. See Table 4 for additional information.

Figure 8: Incumbent Wage Effects in Same vs. Other Occupations



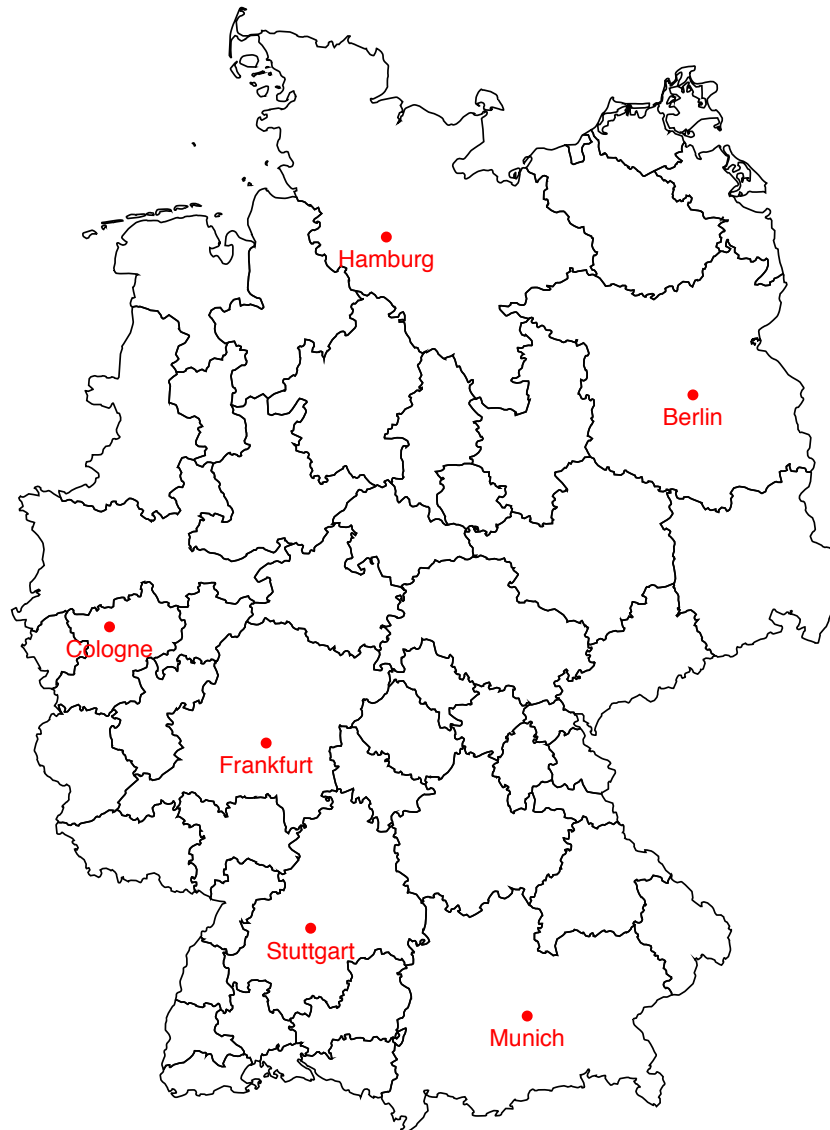
*Note:* The figure displays treatment effects of worker exits on the wages of incumbents in the same 1-digit occupation group as the deceased and on incumbents in other 1-digit occupation groups. 1-digit occupation groups stratify occupations horizontally based on the thematic focus of the work, e.g., 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 9: Incumbent Wage Effects by Skill Level of Deceased



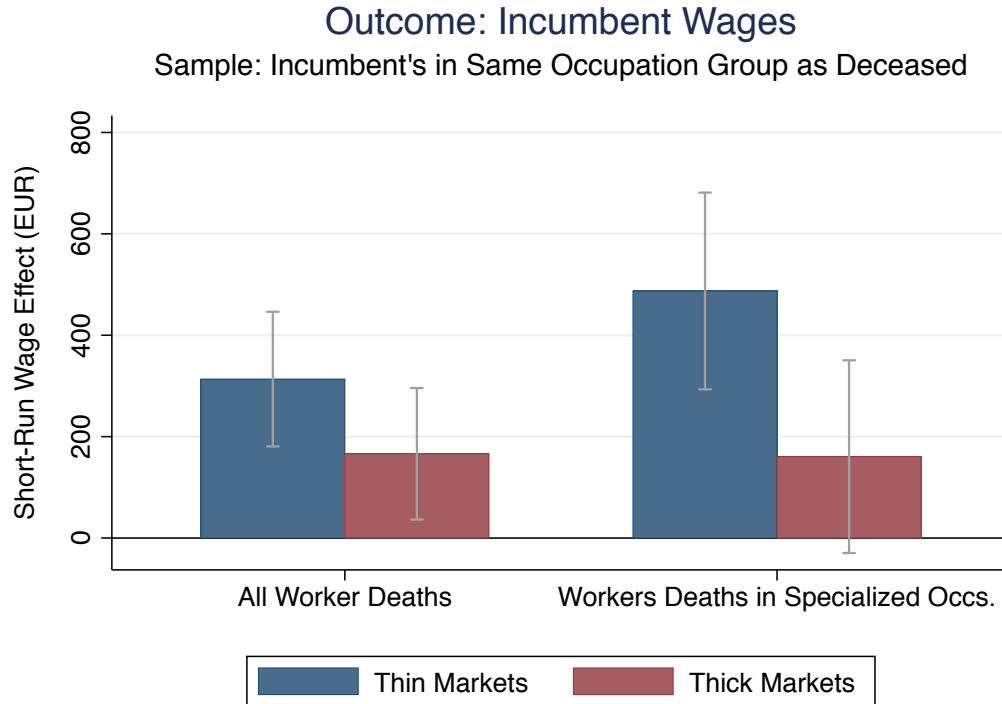
*Note:* The three figures display short-run treatment effects of worker exits on the wages of incumbents in the same 1-digit occupation group as the deceased and on incumbents in other 1-digit occupation groups for different measures of the skill level of the deceased worker. 1-digit occupation groups stratify occupations horizontally based on the thematic focus of the work, e.g., production and manufacturing vs. accounting. In panel (A), we 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), we 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), we 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 10: 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, we 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 11: 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 10 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 gray vertical lines denotes 95% confidence intervals.



Figure 12: 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  $\times$  commuting zone level as the share of employment in the relevant occupation in that commuting zone relative to the nationwide share of employment in that occupation (see Figure 10 for an overview of labor market regions). 5-digit occupation  $\times$  commuting zone cells are characterized as thick or thin based on a median split. Occupations are classified as specialized if they have an above-median return to occupational experience (see Table 8 for more details). The gray vertical lines denotes 95% confidence intervals.

# 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 (9.90)	47.22 (9.90)	39.44 (11.30)	39.33 (11.29)
Female	0.14 (0.35)	0.14 (0.35)	0.26 (0.44)	0.26 (0.44)
Earnings (EUR, 2010 CPI)	31,458 (12,313)	31,536 (12,451)	27,788 (13,651)	27,856 (13,631)
Years of Education	10.6 (1.5)	10.6 (1.5)	10.9 (1.9)	10.9 (1.9)
Tenure	9.52 (6.15)	9.53 (6.14)	7.04 (5.48)	7.06 (5.47)
<i>N</i>	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 (7.38)	14.50 (7.40)
Number of New Workers	2.27 (2.40)	2.23 (2.41)
Number Part-Time Workers	1.19 (2.24)	1.20 (2.25)
Number Apprentices	0.83 (1.51)	0.86 (1.52)
Firm Age	14.77 (6.77)	14.79 (6.77)
Primary Sector	0.029 (0.167)	0.029 (0.169)
Secondary Sector (Manufacturing)	0.500 (0.500)	0.494 (0.500)
Tertiary Sector (Service)	0.472 (0.499)	0.477 (0.499)
<i>N</i>	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.

Table 4: Treatment Effect on Incumbent Worker Employment Outcomes

	Short-Run Effect	Long-Run Effect
	<u>Outcome: Employed at Same Establishment</u>	
Treated	0.0043 (0.0013)	0.0055 (0.0014)
Comparison Group Mean in $k = 1$ : 0.825		
	<u>Outcome: Full-Time Employment</u>	
Treated	0.0011 (0.001)	-0.0006 (0.001)
Comparison Group Mean in $k = 1$ : 0.894		
	<u>Outcome: Part-Time Employment</u>	
Treated	-0.0001 (0.0004)	0.0004 (0.0005)
Comparison Group Mean in $k = 1$ : 0.0121		
	<u>Outcome: Promotion</u>	
Treated	0.0008 (0.0003)	0.0012 (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  $\text{Post} \times \text{Treated}$  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 than the occupation he or she worked in in year  $k = -1$ . To calculate average wages at the 5 digit occupation level, we draw a 10% sample of individuals from the IEB and regress individual's log wage on occupation dummies and individual fixed effects. We 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.

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 $\times$ Same Occupation	239.91 (47.37)	171.86 (51.66)				
Treated $\times$ Other Occupations	61.42 (57.32)	47.56 (62.95)				
Treated $\times$ Low Skilled Occupation			118.74 (76.48)	85.07 (83.29)		
Treated $\times$ Medium Skilled Occupation			256.68 (47.32)	182.80 (52.01)		
Treated $\times$ High Skilled Occupation			-45.47 (100.00)	-17.64 (111.28)		
Treated $\times$ Low Skilled Occupation $\times$ Same Occupation					221.31 (98.08)	143.35 (105.57)
Treated $\times$ Low Skilled Occupation $\times$ Other Occupations					-29.08 (114.10)	2.12 (123.95)
Treated $\times$ Medium Skilled Occupation $\times$ Same Occupation					267.60 (57.93)	197.66 (63.01)
Treated $\times$ Medium Skilled Occupation $\times$ Other Occupations					233.92 (75.72)	151.33 (83.27)
Treated $\times$ High Skilled Occupation $\times$ Same Occupation					166.15 (137.96)	119.22 (153.29)
Treated $\times$ High Skilled Occupation $\times$ Other Occupations					-301.78 (134.05)	-181.73 (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 equation (10). 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 occupation 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.

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 (47.37)	171.86 (51.66)				
Treated × Other Occupations	61.42 (57.32)	47.56 (62.95)				
Treated × Low Education			249.42 (92.39)	119.45 (101.07)		
Treated × Medium Education			184.03 (41.92)	142.75 (46.19)		
Treated × High Education			-131.33 (184.87)	-73.48 (201.98)		
Treated × Low Education × Same Occupation					286.18 (112.93)	187.75 (124.16)
Treated × Low Education × Other Occupations					184.43 (147.85)	-1.90 (158.14)
Treated × Medium Education × Same Occupation					241.40 (52.79)	174.92 (57.50)
Treated × Medium Education × Other Occupations					83.06 (63.88)	84.74 (70.56)
Treated × High Education × Same Occupation					103.16 (251.50)	74.15 (274.00)
Treated × High Education × Other Occupations					-447.42 (252.18)	-278.07 (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 equation (10). 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 occupation 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.

Table 7: Wage Effects and Manager Status 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 × Same Occupation	239.91 (47.37)	171.86 (51.66)				
Treated × Other Occupations	61.42 (57.32)	47.56 (62.95)				
Treated × Deceased Non-Manager			214.29 (39.35)	155.38 (43.28)		
Treated × Deceased Manager			-108.82 (120.54)	-78.57 (133.16)		
Treated × Deceased Non-Manager × Same Occupation					251.95 (48.71)	178.58 (53.06)
Treated × Deceased Non-Manager × Other Occupations					142.59 (61.86)	110.11 (67.93)
Treated × Deceased Manager × Same Occupation					128.95 (180.28)	109.80 (198.45)
Treated × Deceased Manager × Other Occupations					-338.31 (149.12)	-262.42 (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 equation (10). 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 occupation in the year before a worker death. We 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.

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 (104.59)	81.52 (112.50)				
Treated × Medium Tenure of Deceased Worker	140.10 (61.46)	66.03 (66.89)				
Treated × Long Tenure of Deceased Worker	178.57 (56.96)	161.22 (63.93)				
Treated × Low Specialization Occupations			82.24 (87.38)	132.79 (96.07)		
Treated × Medium Specialization Occupations			216.61 (47.07)	120.13 (51.92)		
Treated × High Specialization Occupations			122.13 (88.87)	140.99 (96.75)		
Treated × Low Specialization Occupations × Same Occupation					161.79 (115.00)	162.20 (125.59)
Treated × Low Specialization Occupations × Other Occupations					-26.81 (124.03)	89.88 (135.70)
Treated × Medium Specialization Occupations × Same Occupation					220.32 (57.42)	138.70 (62.88)
Treated × Medium Specialization Occupations × Other Occupations					209.12 (75.43)	81.79 (83.17)
Treated × High Specialization Occupations × Same Occupation					427.11 (121.60)	333.38 (129.45)
Treated × High Specialization Occupations × Other Occupations					-244.26 (124.61)	-92.97 (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 equation (10). 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 occupation 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, we follow Bleakley and Lin (2012) and calculate returns to experience for each 5-digit occupation. We 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.



# Online Appendix of: How Substitutable Are Workers? Evidence from Worker Deaths

Simon Jäger and Jörg Heining

## A Appendix to Conceptual Framework

### A.1 Incumbent Worker Wage Effects With Heterogeneous Labor and Search Frictions

In the main part of the paper, we discussed multiworker bargaining models in section 2 and considered static models with homogenous labor. Here, we illustrate the relationship between worker substitutability and wage effects of worker exits in a dynamic search-and-matching model Pissarides (2000) with intrafirm bargaining and heterogeneous labor following Cahuc et al. (2008). Abandoning the assumption of homogenous labor allows for a characterization of wage effects across worker types. As in the static model with homogenous labor, the sign of the wage effect of a worker exit identifies the substitutability between different worker types inside the firm with substitutes associated with positive and complements associated with negative wage effects. Similar to the intuition in Section 2.2, the magnitude of the wage effect is proportional to the search frictions that the firm faces.

Consider a production function  $F(N_1, \dots, N_n)$  with  $n \geq 1$  types of labor, indexed by  $i = 1, \dots, n$ , and let  $\mathbf{N} = (N_1, \dots, N_n)$  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  $q_i(\theta_i) = h_i(u_i, V_i)/V_i$ .<sup>94</sup> Existing jobs are destroyed at an exogenous destruction rate of  $s_i$ . The wage of workers of type  $i$  is denoted by  $w_i(\mathbf{N})$  as it can depend on the vector of labor inputs  $\mathbf{N}$  and is determined as the result of Nash bargaining as in Stole and Zwiebel with worker's bargaining power denoted by  $\beta$ .

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

---

<sup>94</sup>The firm takes the filling rate  $q_i(\theta_i)$  as given, i.e., the firm should be thought of as small relative to the market.

$$\Pi(\mathbf{N}) = \max_{\mathbf{V}} \left( \frac{1}{1+r} \frac{d\Pi}{dt} \right) \left\{ \left[ F(\mathbf{N}) - \sum_{j=1}^n (w_j(\mathbf{N})N_j - \gamma_j V_j) \right] dt + \Pi(\mathbf{N}^+) \right\}, \quad (12)$$

subject to the law of motion for employment

$$N_i^+ = N_i(1 - s_i dt) + V_i q_i dt, \forall i \in \{1, \dots, n\}. \quad (13)$$

Here,  $\mathbf{V}$  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:

$$\underbrace{\frac{F_i(\mathbf{N}) - w_i(\mathbf{N}) - \sum_{j=1}^n N_j \frac{\partial w_j(\mathbf{N})}{\partial N_i}}{r + s_i}}_{\text{Marginal Benefit of Employment of Type } i} = \underbrace{\frac{\gamma_i}{q_i}}_{\text{Marginal Cost of Hiring}}. \quad (14)$$

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

$$\underbrace{F_i(\mathbf{N})}_{\text{Marginal Product}} = \underbrace{w_i(\mathbf{N})}_{\text{Wage}} + \underbrace{\frac{\gamma_i(r + s_i)}{q_i}}_{\text{Turnover Costs}} + \underbrace{\sum_{j=1}^n N_j \frac{\partial w_j(\mathbf{N})}{\partial N_i}}_{\text{Employment Wage Effect}}. \quad (15)$$

The last term is absent in standard search models without intra-firm bargaining. For constant-returns-to-scale production functions, the employment wage 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 wage effect. Based on our research design, we 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:

$$\beta \underbrace{\frac{\partial \Pi(\mathbf{N})}{\partial N_i}}_{\text{Firm's Marginal Profit}} = (1 - \beta) \underbrace{\frac{w_i(\mathbf{N}) - rU_i}{r + s_i}}_{\text{Worker's Surplus}}, \quad (16)$$

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.<sup>95</sup> Cahuc, Marque, and Wasmer

<sup>95</sup>For ease of exposition, we only discuss the case with constant bargaining power. Cahuc, Marque, and

(2008) derive the wage  $w_i(N)$  earned by workers of type  $i$ :

$$w_i(N) = (1 - \beta)rU_i + \int_0^1 z^{\frac{1-\beta}{\beta}} F_i(Nz) dz. \quad (17)$$

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,  $rU_i$  or the flow value of unemployment, 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  $\beta = \frac{1}{2}$ .

Equation (17) demonstrates that the sign of the effect of a change in the employment of worker type  $j$  at the firm on the wages of workers of type  $i$  at the firm identifies which worker types are complements or substitutes in production:

$$\frac{\partial w_i(N)}{\partial N_j} = \int_0^1 z^{\frac{1}{\beta}} F_{ij}(Nz) dz. \quad (18)$$

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 vacancies to converge back to its pre-exit steady state employment level. Therefore, any wage effects will also converge back to zero.

While firms in the model are assumed to post vacancies to instantaneously converge back to the steady state, we can also think of perturbations of the steady state in which the firm posts finite vacancies so that the speed of convergence will be inversely related to the search friction that the firm faces. 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  $-\frac{\partial w_i(N)}{\partial N_j}$  as employment of worker type  $j$  has changed by  $-1$ ; in the next period, the wage effect will be  $-\frac{\partial w_i(N)}{\partial N_j}(1 - q_j(\theta_j))$ , in expectation, as the vacancy will have been filled with

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Wasmer (2008) also derive solutions with heterogeneous bargaining weights for each worker type  $i$ .

probability  $q_j(\theta_j)$ .<sup>96</sup> 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:

$$\sum_{t=0}^{\infty} \frac{\partial w_i(N)}{\partial N_j} \Delta N_{jt} = - \sum_{t=0}^{\infty} \frac{\partial w_i(N)}{\partial N_j} (1 - q_j(\theta_j))^t = - \frac{\partial w_i(N)}{\partial N_j} \frac{1}{q_j(\theta_j)}. \quad (19)$$

According to (19), 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 (19) documents that the speed of hiring and the speed of wage adjustment are identical in the perturbation of 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 if, for instance, new workers are hired relatively fast but wage effects persist for longer. 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.

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<sup>96</sup>Note that this illustration ignores higher order terms, e.g., of additional workers leaving the firm.

## A.2 Wage Effects With Renegotiation Under Mutual Consent

The analysis in Section 2.1 assumed that contracts are completely nonbinding for either party. Here, we 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:

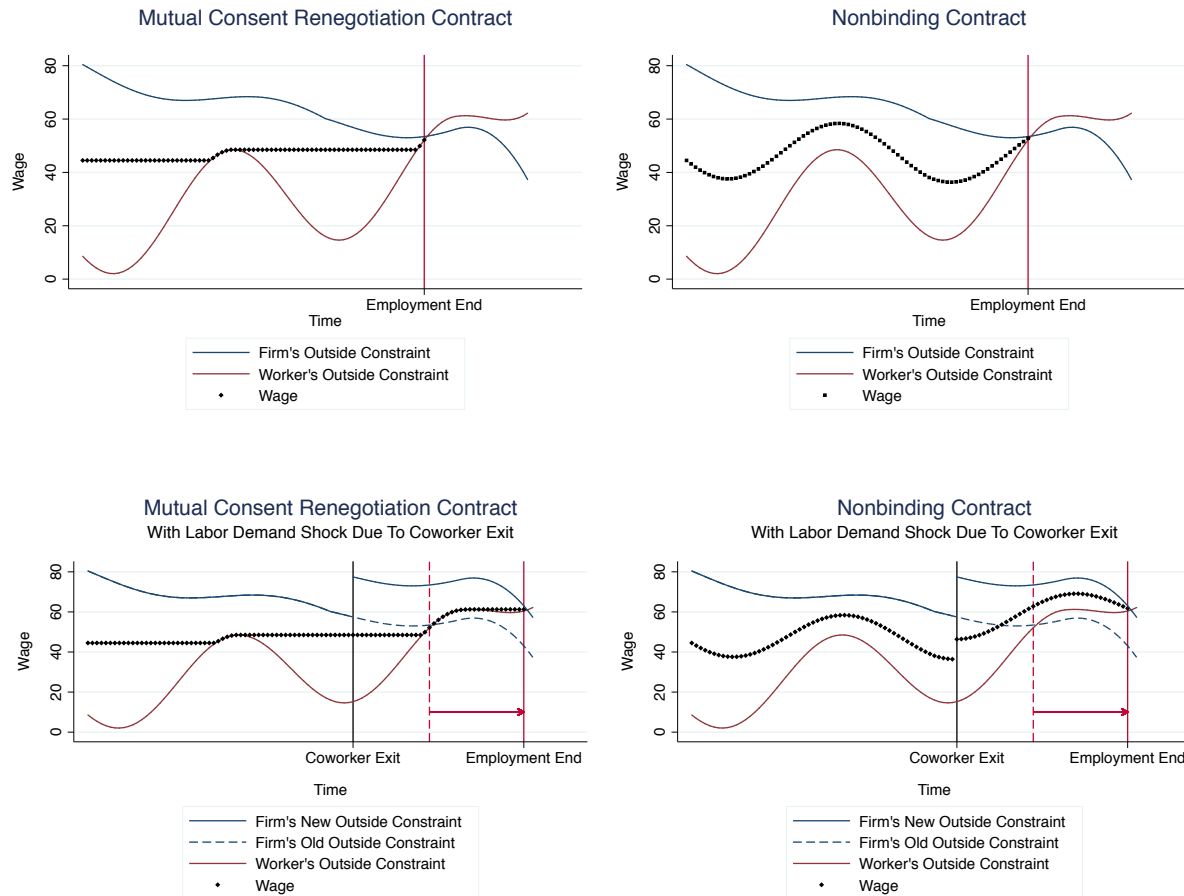
1. **Efficient separation:** If the rents from continuing the employment relationship are negative, for instance, because of a negative shock to the worker's specific productivity, the firm and the worker separate and receive their outside option.
2. **Continued employment with no renegotiation:** If 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 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 A.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 binds, 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 mutes the wage response to a coworker exit relative to a setup with nonbinding contracts.

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



*Note:* The figures display wage and employment outcomes over time under different contracting regimes (see description in Appendix A.2) 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 previously negotiated wage.

## B Additional Tables and Figures

### B.1 Additional Figures

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

<u>Outcome:</u>	Incumbent Wages	
	<u>Short-Run Effect</u>	<u>Long-Run Effect</u>
Treated $\times$ (Employment $\leq$ 10)	204.21 (65.21)	154.94 (70.95)
Treated $\times$ (10 < Employment $\leq$ 20)	175.93 (50.76)	113.51 (56.77)
Treated $\times$ (20 < Employment $\leq$ 30)	82.88 (71.67)	73.27 (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.



Table B.2: Treatment Effects for Additional Samples: Part-Time Incumbents and Apprentices

Sample:	Part-Time Incumbents		Apprentices		Main Sample: Full-Time Incumbents	
	Short-Run Effect	Long-Run Effect	Short-Run Effect	Long-Run Effect	Short-Run Effect	Long-Run Effect
<u>Outcome: Wages</u>						
Treated	260.46 (83.24)	236.59 (84.36)	112.68 (82.65)	100.10 (84.90)	174.47 (37.60)	128.54 (38.53)
<u>Outcome: Employed at Same Establishment</u>						
Treated	0.0022 (0.0038)	0.004 (0.004)	0.0162 (0.005)	0.0185 (0.004)	0.0043 (0.0013)	0.0055 (0.0014)
<u>Outcome: Full-Time Employment</u>						
Treated	0.0032 (0.0027)	0.003 (0.003)	0.001 (0.005)	-0.001 (0.003)	0.0011 (0.001)	-0.0006 (0.001)
<u>Outcome: Part-Time Employment</u>						
Treated	-0.0037 (0.0039)	-0.003 (0.004)	-0.001 (0.001)	0.000 (0.001)	-0.0001 (0.0004)	0.0004 (0.0005)
<u>Outcome: Promotion</u>						
Treated	0.0019 (0.001)	0.001 (0.001)	-0.001 (0.001)	0.000 (0.001)	0.0008 (0.0003)	0.0012 (0.0003)

*Note:* The table displays treatment effects on several employment outcomes based on difference-in-differences regressions. The sample of part-time incumbents is defined as the set of part-time coworkers of the deceased in the year before death. Apprentices are defined as apprentices at the incumbent's firm in the year before death. The full-time incumbent sample is the main sample used for the analysis in the paper and included here as a benchmark. Treated refers to the  $\text{Post} \times \text{Treated}$  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 than the occupation he or she worked in in year  $k = -1$ . To calculate average wages at the 5 digit occupation level, we draw a 10% sample of individuals from the IEB and regress individual's log wage on occupation dummies and individual fixed effects. We use the estimated occupation effects to measure promotions. Observations are weighted inversely by the number of incumbent workers at the firm of the deceased.

Table B.3: Dynamics of Average Treatment Effect on Incumbent Worker Wages

Outcome:	Incumbent Worker Wages	Sum of Incumbent Worker Wages
Treated $\times k = -3$	-32.09 (34.71)	-258.09 (409.65)
Treated $\times k = -2$	31.64 (28.50)	45.49 (314.21)
Treated $\times k = -1$	omitted	omitted
Treated $\times k = 0$	73.37 (26.69)	718.12 (332.73)
Treated $\times k = 1$	174.47 (37.60)	1791.14 (406.74)
Treated $\times k = 2$	159.66 (43.59)	1642.80 (469.99)
Treated $\times k = 3$	158.08 (48.62)	1182.79 (533.71)
Treated $\times k = 4$	107.50 (52.57)	890.33 (591.64)
Treated $\times k = 5$	30.05 (56.48)	153.68 (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 (10).  $k$  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.

Table B.4: Effects in on Incumbent Worker Wages in Year  $k = 0$  By Quarter of Death

Outcome:	Wage in Year $k = 0$
Treated $\times$ Death in July, August, September of $k = 0$	170.48 (41.25)
Treated $\times$ Death in October, November, December of $k = 0$	67.83 (42.78)
Treated $\times$ Death in January, February, March of $k = 1$	37.75 (43.35)
Treated $\times$ 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 Q3 or Q4 of  $k = 0$  document that the positive wage effects in year  $k = 0$  (see, e.g., Figure 4) 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.

Table B.5: 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)
<i>(A) Thickness Measured at Occupation Level</i>						
Treated × Low Thickness (Occupation)	313.49 (67.77)	216.91 (73.81)	487.31 (99.03)	291.28 (107.07)	181.50 (92.52)	161.67 (101.17)
Treated × High Thickness (Occupation)	166.19 (66.16)	127.50 (72.25)	160.51 (96.94)	171.91 (105.68)	169.27 (90.00)	91.85 (98.18)
<i>(B) Density of Local Labor Market</i>						
Treated × Low Density	301.82 (65.82)	203.20 (71.20)	387.15 (95.01)	265.28 (102.88)	236.20 (89.87)	152.35 (97.77)
Treated × High Density	177.08 (68.45)	141.18 (74.90)	264.41 (101.10)	198.39 (110.04)	113.57 (92.65)	103.49 (101.55)
<i>(C) Thickness Measured at Industry Level</i>						
Treated × Low Thickness (Industry)	267.59 (67.92)	249.62 (73.61)	347.18 (97.23)	258.48 (105.76)	204.01 (94.34)	244.17 (101.91)
Treated × High Thickness (Industry)	217.24 (65.87)	104.70 (71.98)	306.62 (98.51)	211.07 (105.82)	153.49 (88.20)	27.00 (97.10)
<i>(D) Local Unemployment Rate</i>						
Treated × Low Unemployment	193.78 (75.75)	160.17 (83.54)	268.51 (111.35)	248.71 (121.26)	136.47 (103.07)	93.86 (114.51)
Treated × High Unemployment	276.38 (72.04)	186.49 (78.51)	379.30 (103.62)	262.01 (112.75)	199.04 (99.23)	127.06 (108.16)

*Note:* The table shows heterogeneity of the treatment effect based on the difference-in-differences framework in equation (10). 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, we follow Bleakley and Lin (2012) and calculate returns to experience for each 5-digit occupation. We 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 10 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 zone 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.

Table B.6: Summary Statistics on Hours per Week in Administrative Data and Structure of Earnings Survey

All Workers			
	Administrative Data	Survey (Excluding Overtime)	Survey (Overtime)
Mean	31.55	30.62	0.29
Standard Deviation	11.39	12.53	1.49
Women			
	Administrative Data	Survey (Excluding Overtime)	Survey (Overtime)
Mean	28.44	26.81	0.14
Standard Deviation	10.93	12.77	0.99
Men			
	Administrative Data	Survey (Excluding Overtime)	Survey (Overtime)
Mean	34.33	34.24	0.42
Standard Deviation	11.05	11.15	1.83

*Note:* The table reports hours per week based on administrative data from the German Statutory Accident Insurance as well as data from the Structure of Earnings Survey 2014 (*Verdienststrukturerhebung*, p. 118). The German Statutory Accident Insurance required all firms to report information on workers' hours of work as part of their administrative reporting processes in the time period from 2010 to 2015. We drop outlier observations below the 1st and above the 99th percentile. The administrative data include overtime measures while the survey separately asks for hours (excluding overtime) and overtime hours.

Table B.7: Individual-Level Summary Statistics (Hours Sample)

	Actual and Placebo Deceased Workers		Incumbent Workers	
	Treatment Group	Comparison Group	Treatment Group	Comparison Group
Age	50.9 (8.6)	50.9 (8.6)	43.1 (11.5)	43.1 (11.4)
Female	0.13 (0.34)	0.13 (0.34)	0.23 (0.42)	0.24 (0.43)
Earnings (EUR, 2010 CPI)	25,533 (10,836)	25,124 (10,699)	28,808 (14,028)	28,593 (14,023)
Years of Education	10.6 (1.1)	10.6 (1.1)	10.6 (1.2)	10.7 (1.3)
Tenure	7.57 (3.30)	7.53 (3.34)	5.84 (3.3)	5.85 (3.3)
<i>N</i>	2,328	2,328	22,250	22,370

*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 B.8: Firm-Level Summary Statistics (Hours Sample)

	Treatment Group	Comparison Group
Total Number of Employees	15.14 (7.37)	15.17 (7.42)
Number Part-Time Workers	2.93 (3.3)	2.84 (3.1)
Number Apprentices	0.59 (1.11)	0.66 (1.26)
Firm Age	10.0 (1.43)	10.0 (1.43)
Primary Sector	0.02 (0.14)	0.02 (0.14)
Secondary Sector (Manufacturing)	0.47 (0.50)	0.47 (0.50)
Tertiary Sector (Service)	0.50 (0.50)	0.51 (0.50)
<i>N</i>	2,328	2,328

*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).

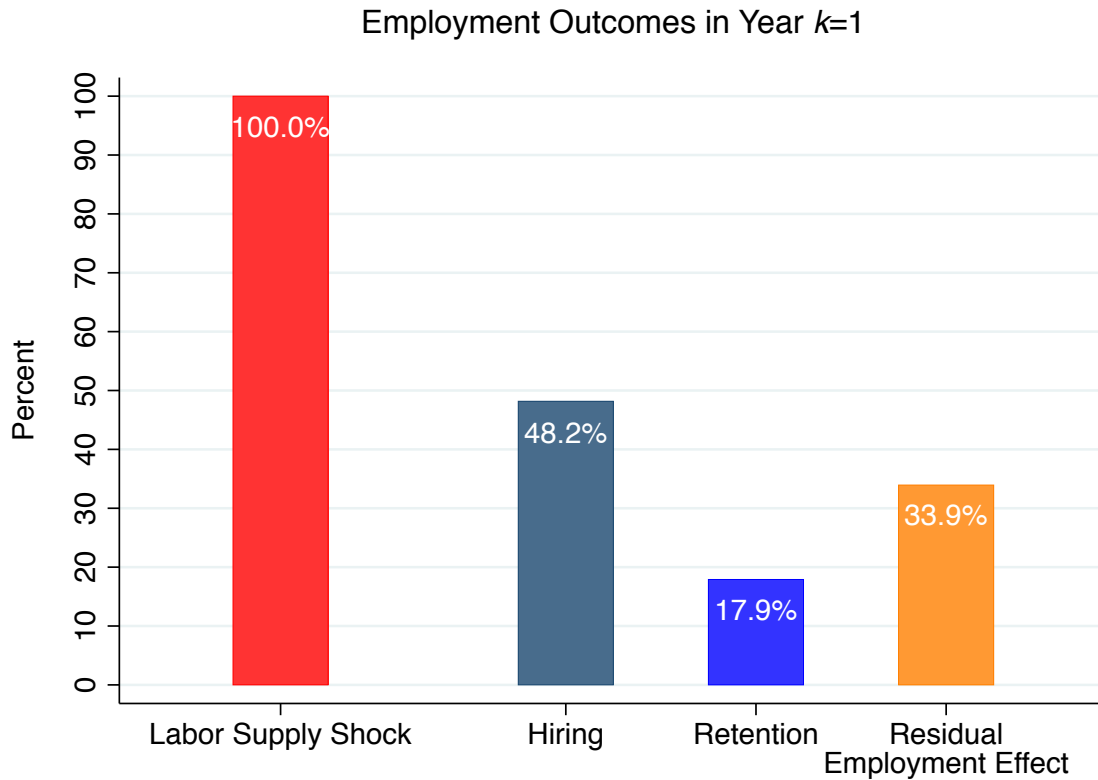
Table B.9: Effects on Hours per Week

Outcome: Incumbent Worker Hours										
	Short-Run Effect	Long-Run Effect	Short-Run Effect	Long-Run Effect	Short-Run Effect	Long-Run Effect	Short-Run Effect	Long-Run Effect	Short-Run Effect	Long-Run Effect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treated	-0.42 (0.29)	-0.49 (0.32)								
Treated × Same Occupation			0.06 (0.45)	0.14 (0.45)						
Treated × Other Occupations			-0.70 (0.36)	-0.84 (0.41)						
Treated × Low Skilled Occupation					-0.71 (0.80)	-0.93 (0.89)				
Treated × Medium Skilled Occupation					-0.01 (0.39)	-0.01 (0.42)				
Treated × High Skilled Occupation					-1.25 (0.45)	-1.38 (0.46)				
Treated × Low Education							-0.18 (0.67)	-0.68 (0.75)		
Treated × Medium Education							-0.39 (0.34)	-0.31 (0.38)		
Treated × High Education							-1.23 (0.85)	-2.28 (0.83)		
Treated × Deceased Non-Manager									-0.12 (0.35)	-0.15 (0.38)
Treated × Deceased Manager									-1.61 (0.49)	-1.86 (0.51)
No. of Observations	216,795	216,795	216,795	216,795	216,795	216,795	216,795	216,795	216,795	216,795
No. of Clusters	4,767	4,767	4,767	4,767	4,767	4,767	4,767	4,767	4,767	4,767

*Note:* The table shows heterogeneity of the treatment based on the difference-in-differences framework in equation (10). The outcome variable are hours per week among incumbent workers. The German Statutory Accident Insurance required all firms to report information on workers' hours of work as part of their administrative reporting processes in the time period from 2010 to 2015. 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 occupation 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. 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. We 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.

## B.2 Additional Figures

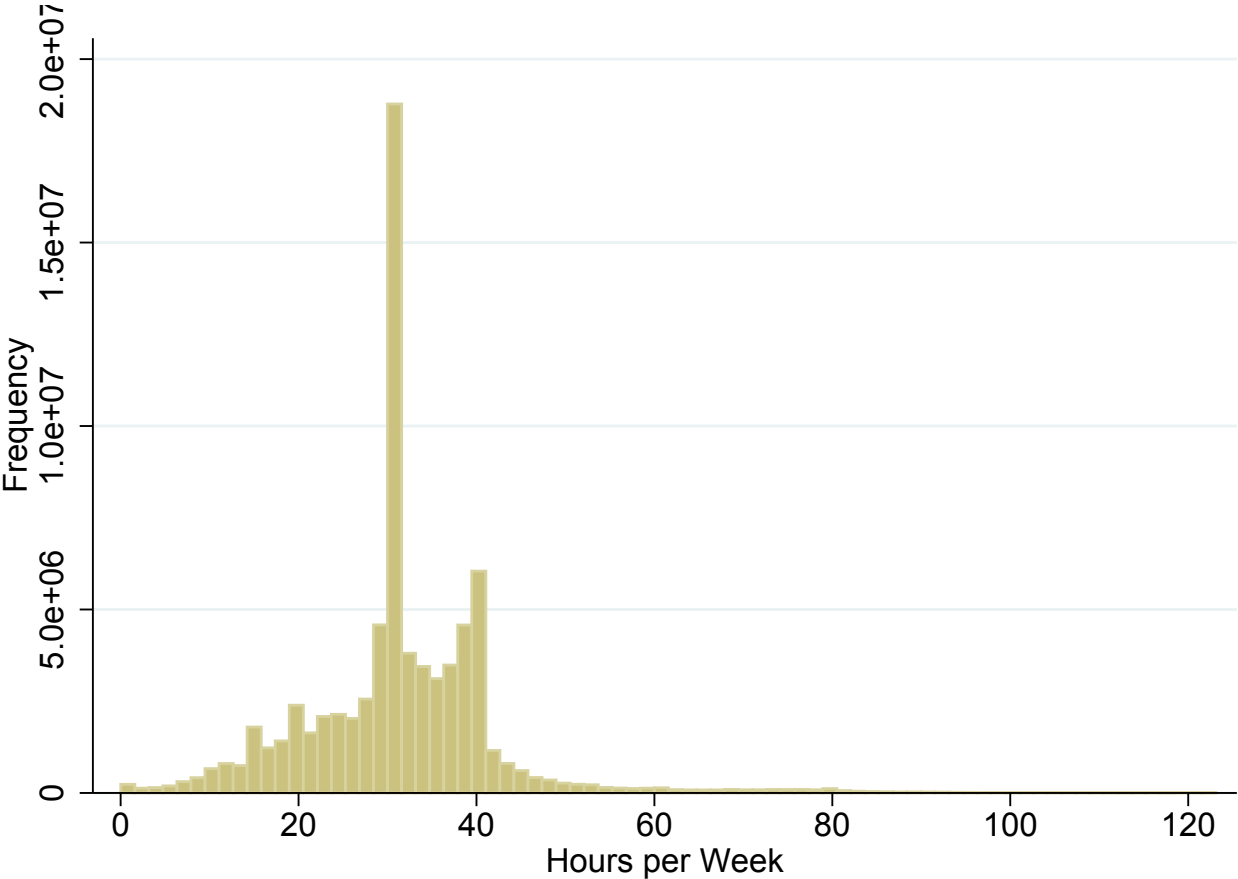
Figure B.1: Decomposition of Employment Effects of Labor Supply Shock Due to Worker Death



*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. The residual employment effect is the effect on employment at the firm in  $k = 1$ .

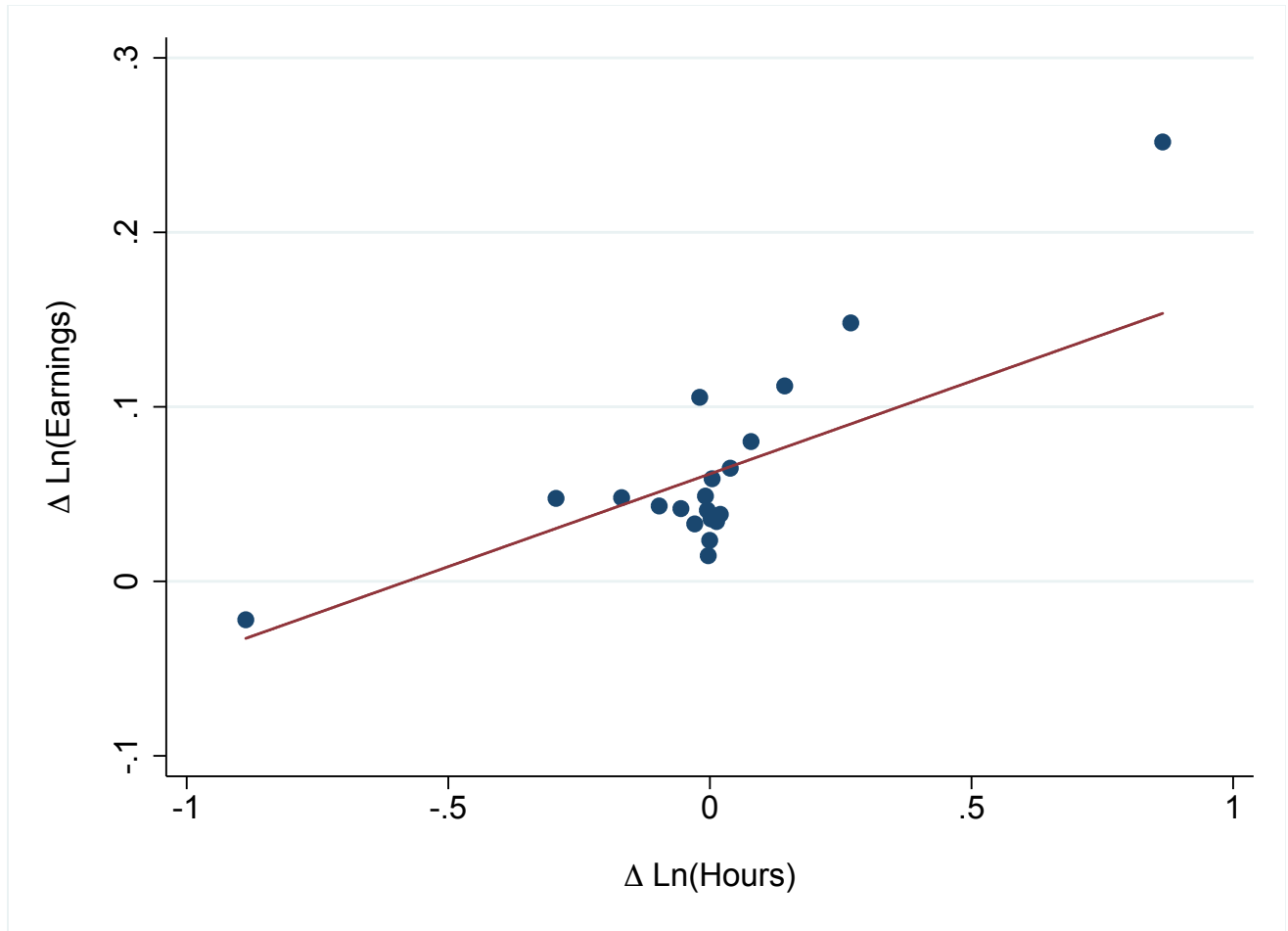


Figure B.2: Distribution of Hours Per Week



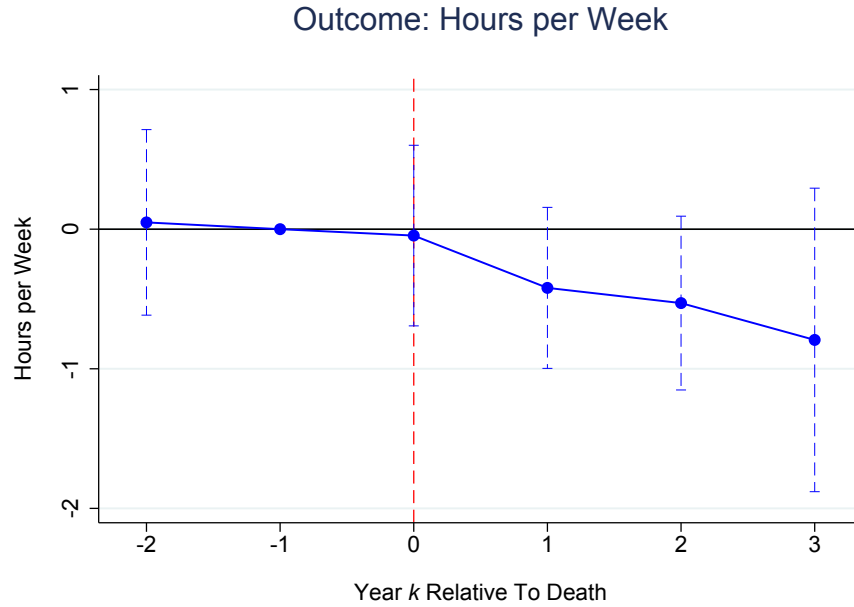
*Note:* The figure shows a histogram of hours per week based on administrative data from the German Statutory Accident Insurance, which required all firms to report information on workers' hours of work as part of their administrative reporting processes in the time period from 2010 to 2015. We drop outlier observations below the 1st and above the 99th percentile.

Figure B.3: Changes in Log Earnings and Log Hours Within Individuals



*Note:* The figure shows a binned scatter plot of within-worker, year-to-year changes in the log of average weekly earnings on the  $y$ -axis and the log of average weekly hours on the  $x$ -axis. The hours information is based on administrative data from the German Statutory Accident Insurance, which required all firms to report information on workers' hours of work as part of their administrative reporting processes in the time period from 2010 to 2015. We drop outlier observations below the 1st and above the 99th percentile. A regression of changes in log average weekly earnings on the change in the log of average weekly hours reveals a coefficient of 0.2175 (se 0.0028).

Figure B.4: Effect of Worker Deaths on Incumbent Worker Hours



*Note:* The figure displays regression coefficients and associated 95% confidence intervals for the difference between incumbent worker in the treatment and comparison group, i.e., the  $\beta_k^{Treated}$  from equation (10). The coefficients in  $k = -1$  are normalized to zero. The outcome variable are the reported hours per week of incumbent workers. Incumbent workers are defined as full-time coworkers of the deceased or placebo deceased in the year before death. The data on hours per week stem from administrative data from the German Statutory Accident Insurance, which required all firms to report information on workers' hours of work as part of their administrative reporting processes in the time period from 2010 to 2015. We drop outlier observations below the 1st and above the 99th percentile of hours per week.