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How substitutable are workers? Evidence from worker deaths

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Articles on labour market issues

26|2022 How Substitutable Are Workers? – Evidence from Worker Deaths

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How Substitutable Are Workers? – Evidence from Worker Deaths

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Mit der Reihe „IAB-Discussion Paper“ will das Forschungsinstitut der Bundesagentur für Arbeit den Dialog mit der externen Wissenschaft intensivieren. Durch die rasche Verbreitung von Forschungsergebnissen über das Internet soll noch vor Drucklegung Kritik angeregt und Qualität gesichert werden.

The “IAB-Discussion Paper” is published by the research institute of the German Federal Employment Agency in order to intensify the dialogue with the scientific community. The prompt publication of the latest research results via the internet intends to stimulate criticism and to ensure research quality at an early stage before printing.

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Abstract

We estimate how exogenous worker exits affect firms' demand for incumbent workers and new hires. Drawing on administrative data from Germany, we analyze 34,000 unexpected worker deaths, which, on average, raise the remaining workers' wages and retention probabilities. The average effect masks substantial heterogeneity: Coworkers in the same occupation as the deceased see positive wage effects; coworkers in other occupations experience wage decreases when a high-skilled or specialized worker dies. Our findings imply substantial replacement costs, which are larger in thin markets and when skills are specialized.

Zusammenfassung

Wir schätzen, wie exogene Austritte von Arbeitnehmern die Nachfrage der Firmen nach etablierten Arbeitnehmern und Neueinstellungen beeinflussen. Basierend auf administrativen Daten aus Deutschland analysieren wir 34.000 unerwartete Todesfälle von Arbeitnehmern. Diese führen im Durchschnitt zu einer Erhöhung der Löhne und Verbleibwahrscheinlichkeiten der verbleibenden Arbeitnehmer. Hinter dem durchschnittlichen Effekt verbergen sich erhebliche Heterogenitäten: Mitarbeiter im gleichen Beruf wie der Verstorbene sehen positive Lohneffekte. Dagegen erfahren Kollegen in anderen Berufen Lohnkürzungen, wenn ein hochqualifizierter oder spezialisierter Arbeiter stirbt. Unsere Ergebnisse implizieren erhebliche Ersatzkosten, die in dünnen Märkten und bei spezialisierten Qualifikationen größer sind.

JEL

J20, J30, J63

Keywords

Hiring Costs, Human Capital Specificity, Labor Market Thickness

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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 the presence of rents and with Becker's 1962 idea that human capital has firm-specific components (Jacobson/LaLonde/Sullivan, 1993; Lazear, 2009). 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 replacement costs (Slichter, 1919; Oi, 1962; Manning, 2011) and the mechanisms underlying rent sharing (Kline et al., 2019) to the importance of labor pooling as a source of agglomeration (Marshall, 1890)—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 incumbent workers. In a frictionless, competitive model, worker exits do not affect the firm's demand for incumbent workers: the firm can simply hire a suitable new worker in response to a worker exit. In contrast, when outsiders are only imperfect substitutes for insiders—for instance because the replacement of workers is costly—worker exits can affect the firm's labor demand for incumbent workers.

Our empirical answer leverages a quasi-experimental research design to estimate the causal effect of unexpected worker deaths on hiring and on incumbent workers' wages 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.¹ 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. 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

¹ The use of deaths as a source of variation builds on previous work in Jones/Olken (2005); Bannedsen/Perez-Gonzalez/Wolfenzon (2020); Bannedsen et al. (2007); Azoulay/Wang/Zivin (2010); Oettl (2012); Becker/Hvide (2021); Isen (2013); Jaravel/Petkova/Bell (2018); and Fadlon/Nielsen (2019).

outcomes in comparison group firms can be used to gauge what would have happened to 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 statistically significant earnings increase of about 0.6 Percent in the year after the death. 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 Euro 3,300 (2010 CPI), corresponding to about 13 percent 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.

We document substantial heterogeneity in the effects across skill groups, occupational boundaries, and labor markets. Positive wage effects of worker exits are concentrated among incumbent workers in the same occupation group as the deceased. For deaths of managers and workers in high-skilled or specialized occupations, we estimate negative effects on the wages of incumbent workers in other occupations. Overall, effects of worker deaths are larger in magnitude for exits of workers with more specific human capital, e.g., with longer tenure or from more specialized occupations. Further corroborating the role of specialized skills, we also find larger effects on incumbent wages when the external labor market is thin in the deceased's occupation, i.e. when the local labor market's relative share of workers in the deceased's occupation is low. This difference is driven by deaths of workers in specialized occupations.

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. To quantify the deviation from a frictionless labor market, we draw on a simple wage posting model (Kline et al., 2019) with imperfect substitutability of incumbent workers and new hires and extend it to incorporate dynamics and multiple worker types. As in Becker (1964), firms in the model share rents with incumbent workers who are hard to replace, e.g., due to hiring and training costs. Following the intuition we describe above, firms will respond to the exit of a worker by increasing pay for incumbent workers to increase their retention only if worker replacement is costly. If markets are frictionless, firms respond only on the hiring margin. We estimate the model with the method of simulated moments to pick parameter values such that the model closely matches our reduced-form moments.

We find that firms in our sample face substantial replacement costs on an order of magnitude of about two annual salaries of a deceased worker. Our estimates further

indicate that replacement costs are convex: the marginal cost of hiring an additional worker increases in the hiring rate. Interpreting the effect heterogeneity through the lens of the model indicates higher replacement costs for workers in thin labor markets.

While our estimation of replacement costs draws on a model with wage posting, we also assess several other models that could account for our reduced-form findings and probe alternative interpretations of our findings. First, we find qualitatively similar predictions in a multi-worker bargaining model (Stole/Zwiebel, 1996a,b; Cahuc/Marque/Wasmer, 2008) or when we extend the Kline et al. (2019) model to incorporate bargaining. Second, we interpret our results through the lens of an internal labor market model with promotions (Doeringer/Piore, 1971; Baker/Gibbs/Holmstrom, 1994a). The model we estimate could nest an internal labor market interpretation if promotions are simply labels for wage increases. However, wage increases in response to a worker exit might also reflect a chain of promotions of the remaining workers to fill the shoes of the worker who exited, irrespective of replacement costs. We test several predictions of this view. For example, we test whether wage increases are concentrated among workers ranking below the deceased worker but find, if anything, larger long-run wage effects for workers ranking above the deceased in the salary distribution. We conclude that our results are consistent with some internal labor market models, but are harder to reconcile with models of slot constraints and vacancy chains as important drivers of the effects we document.

We also assess the extent to which compensating differentials or changes in work hours can account for our findings. 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 purely 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, speak against 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. We also conduct other robustness checks, e.g., studying deaths on weekends, and find similar results in this subsample. We also study the extent to which hours changes could account for our findings, for example, if workers increase work hours after a coworker's death. We find no effect on changes from full- to part-time work, and also do not detect significant changes in hours. However, our analysis of hours is limited to a short sample period and overtime may only be imperfectly captured in this sample. To complement our analysis, we thus extend the Kline et al. (2019) analysis to incorporate an intensive margin and estimate it under the assumption that some of the wage earnings changes we document may reflect hours changes. Our results indicate that even with such intensive margin changes, firms face substantial costs of replacing workers on the order of magnitude of an incumbent's annual salary.

Our paper contributes to several additional strands of the literature. First, we quantify a core determinant of labor demand and wages: the frictions that firms face in replacing workers. Existing evidence, frequently drawn from surveys of firms' recruitment, hiring, and training costs, typically finds hiring and training costs on the order of magnitude of one month of workers' pay (see Hamermesh, 1996; Manning, 2011: for surveys). However, previous work has pointed out that such low hiring or replacement costs are hard to reconcile with the documented large costs of job loss to workers (Davis/von Wachter, 2011; Hall, 2011), and that larger post-match turnover costs (Silva/Toledo, 2009) or imperfect substitutability between new hires and incumbent workers (Mercan/Schoefer/Sedláček, forthcoming) can generate more realistic volatility of vacancies and unemployment. Our estimates point to substantially larger estimates than what is implied by surveys and thereby help to alleviate these tensions. An additional difference between our revealed-preference approach and existing work is that it also captures the additional costs of retaining coworkers in response to a worker exit as well as the costs associated with losing an incumbent worker with high human capital or match specificity. Second, our paper complements the extensive literature on rent-sharing (see Card et al., 2018; Jäger et al., 2020: for overviews) by providing direct evidence for a mechanism—human capital specificity leading to imperfect substitutability between insiders and outsiders—that gives rise to such rent sharing. Moreover, low replacement costs are also hard to square with existing rent sharing estimates (see, e.g., Kline et al., 2019, 2021). Third, our finding of local labor market thickness associated with lower replacement costs provide direct evidence for the importance of labor pooling as an agglomeration force, especially in the context of specialized labor (Moretti, 2011). Our findings also inform the policy debate around skilled labor shortages (Sauer/Wollmershäuser, 2021; Causa et al., 2022) and highlight the costs of employee turnover and the association of market thickness with lower replacement costs. Fourth, our findings of imperfect substitutability inside the firm, e.g., across skill groups, provides 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., Katz/Murphy, 1992; Card, 2009; Dustmann/Ludsteck/Schönberg, 2009), and point to the important role of managerial human capital (Lucas, 1978; Rosen, 1982). Our analysis of worker deaths complements studies leveraging alternative identification strategies, e.g., the forced dismissal of researchers (Waldinger, 2012) or retirement reforms (Carta/D'Amuri/von Wachter, 2021; Bianchi et al., 2022; Boeri/Garibaldi/Moen, 2022).

2 Empirical Setting and Data

2.1 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. 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 our analysis, we focus on a sample of firms with less than 30 employees which account for about 30 percent of employment.

Relative to the OECD average, Germany has a relatively high manufacturing share at 22.6 percent of GDP (OECD: 15.0%, US: 12.7%, World Bank National Accounts Data, 2012). 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 (see, e.g., Acemoglu, 1997; Acemoglu/Pischke, 1998).

The German labor market model combines sectoral bargaining between unions and employer associations at the industry-region level with institutions allowing for increasingly local wage-setting. Traditionally, collective bargaining agreements (CBAs) between employer associations and unions have played an important role in the wage setting process, although even historically less so in smaller firms (Ellguth/Kohaut, 2020). In the last decades, the wage setting processes in the German labor market have become increasingly decentralized (Hassel, 1999; Dustmann et al., 2014; Jäger/Noy/Schoefer, forthcoming). While employers could always raise wages beyond CBA levels, opening and hardship clauses, which give firms more flexibility to negotiate with their workers directly and to pay below-CBA wages, have become increasingly common along with a lower overall coverage rate of collective bargaining agreements (Brändle/Heinbach/Maier, 2011; Bispinck/Dribbusch/Schulten, 2010; Ellguth/Kohaut, 2020).

2.2 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. First, wages are directly reported as part of administrative procedures. 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 compared to many existing estimates of replacement costs, which often leverage surveys or 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 percent of employment in Germany (own calculations for 1981 to 2011 based on Mikrozensus). 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. 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/Ludsteck/Schönberg, 2009; Card/Heining/Kline, 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, draw on novel data on hours worked reported by the Statutory Accident Insurance from 2010 to 2015, and also estimate a model assuming that earnings responses may, at least partially, reflect hours changes.

A drawback of the earnings data is that—as in many administrative datasets—earnings are top-coded above the Social Security earnings maximum. For example, in 2011, the earnings

maximum was at Euro 66,000 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 Euro 27,000, i.e., about half of the 2011 earnings maximum. In the sample we work with, 6.0 percent 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. Excluding workers with initial earnings at or above the maximum leads to effect sizes on wages about 5 percent larger than without this restriction. Another drawback is that we do not have access to suitable data on revenue or productivity.²

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 and its predecessors (Klassifikation der Berufe 2010, see Paulus/Matthes, 2013: for an overview). 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 (1-digit occupation). 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.³

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.⁴ The assignment of establishment IDs implies, for example, that two bakeries operated by the same firm in the same city would be reported as one establishment. In contrast, a bakery and a mill operated by the same firm

² We do not draw on firm-level data on revenue or productivity, e.g., from the IAB Establishment Panel or Orbis-ADIAB, due to an insufficient sample size of establishments in the size category we study.

³ 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 craftsman, graduation from a professional academy, or university studies (see Bundesagentur für Arbeit (2011)).

⁴ The Social Security system issues a new establishment ID after an ownership change and other reorganizations. Hethy-Maier/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.

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 percent of establishments correspond to single-establishment firms (Antoni/Laible/Schild, 2015). In keeping with convention (Dustmann/Ludsteck/Schönberg, 2009; Card/Heining/Kline, 2013)), we will use the terms establishment and firm interchangeably throughout.

3 Empirical Strategy

3.1 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 percent of reported deaths, the reported death date corresponds to the latest date for which an employment or unemployment spell is reported in the data. Most of the remaining observations with spells with end dates after the reported death date end within weeks after death, suggesting that in these cases there are some minor inconsistencies in the exact date of reporting. To rule out spurious death notifications, 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 percent 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 the 42 percent of

employer-reported deaths with a sickness leave in the five years before. 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. (Shorter sickness leaves are mandatorily covered by employers and are typically not observed in the data.) Receipt of 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.⁵ So even though 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.

3.2 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. We use a matched sampling procedure—similar to the approach in Azoulay/Wang/Zivin (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 (death) 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. A death is defined to occur in event year d if it occurs between July 1 of d and June 30 of $d + 1$ so that a death occurs between $k = 0$ and $k = 1$.

Treatment Group. For each event year d from 1980 to 2007, we identify the set of worker deaths in d for whom the restrictions described in 3.1 are met. Death notifications are reported from 1980 onwards and we require a sufficiently long post-death period. 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.

⁵ 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.

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/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/Wang/Zivin, 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 (i) no apprenticeship training (low), (ii) workers with an apprenticeship training (medium), and (iii) workers with a qualification for university studies (*Abitur*) or a university-level education (high)], deciles of earnings in $d - 4$
- Firm characteristics: number of employees in $d - 4$, deciles of average earnings at the firm in $d - 4$.

These variables are chosen to create a comparison group with similar observed characteristics as the treatment group, in particular age and gender, as deceased workers in the sample are on average 7.4 years older and more likely to be male than workers in the pool for the comparison group (86% vs. 62% men). An exact match is found for 95.81 percent of worker-firm pairs in the treatment group. When no exact match can be found, i.e., in the remaining 4.19 percent 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.⁶

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

⁶ 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.

sample to employers with between 3 and 30 full-time employees four years before death, which comprise about 30.5 percent of employment subject to Social Security in Germany.⁷ 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, thus preventing an analysis of sharp shocks. Second, the effect of a worker death on average coworker wages decreases with firm size so that it will be hard to detect in larger firms. We drop establishments that are part of the government or the social insurance system, churches and other non-profits (industry code larger than 870 in the 1973 edition of the German Classification of Economic Activities), and keep establishments in the service, manufacturing and agricultural sector. 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 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 $d - 4$.

3.3 Summary Statistics

This section provides summary statistics for workers and firms in the treatment and comparison group 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. (Our difference-in-differences design permits differences in average levels of outcome variables between the treatment and comparison group and instead relies on a common trend assumption.)

Characteristics of Actual and Placebo Deceased Workers. Columns (1) and (2) of Table 1 report summary statistics for the 33,786 actual and the same number of placebo deceased workers in the treatment and comparison group, respectively, in the year before the worker death. 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, actual and placebo deceased workers earned a wage corresponding to an annual salary of Euro 26,099 in the treatment and Euro 26,413 in the comparison group, respectively. Both groups of workers have an average tenure of about 9.5 years at the firm. The similarity between actual

⁷ 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*)).

and placebo deceased workers is not a mechanical effect of the matched sampling, as the matching relied on variables in $k = -4$.

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 .⁸ Columns (3) and (4) of Table 1 report summary statistics for these incumbent workers who are slightly younger than the actual and placebo deceased workers with an average age of 39 and are more likely to be female (26 percent). Incumbent workers have average earnings in $k = -1$ of about Euro 28,000 (Euro 27,770 in the treatment, Euro 27,840 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 the year before the worker death. The average establishment in the treatment group has 14.45 employees (14.51 in the comparison group), of which about 15 percent are new employees, and has been observed in the data for about 14.8 years. About 3 percent of firms are in the primary sector (agriculture, mining), 50 percent in the secondary sector (manufacturing), and 47 percent 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/Schwengler, 2011) and find that these variables are jointly insignificant in predicting treatment status in our sample ($p = 0.336$).

3.4 Estimating Equations and Identification

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:

⁸ 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 A-4.3 in the Appendix, we also report results for two additional groups of incumbents: the sample of part-time coworkers and individuals who were apprentices.

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

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, γ_j , and leads and lags around event time, $\mathbb{1}(\text{period}_k)$.⁹ Treated_j is an indicator function for treatment status. The coefficients of interest, $\beta_k^{Treated}$, capture the effect of an actual worker death in year $k = t - d$ in the treatment group and are normalized to zero in $k = -1$ ($\beta_{-1}^{Treated} = 0$). We define the *short-run treatment effect* as the effect in the first post-death year, $\beta_1^{Treated}$, and a *long-run treatment effect* as the average of treatment effects in the five-year post-period, $\frac{1}{5} \sum_{k=1}^5 \beta_k^{Treated}$. 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 (Bertrand/Duflo/Mullainathan, 2004).

The model allows for average differences between the treatment and the comparison group as they are absorbed by the firm fixed effects, γ_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.

Estimating Equations for Incumbent Worker Outcomes. The estimating equation in (1) 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 . 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:

⁹ 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 (1) 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.

$$y_{ijk} = \alpha + \gamma_{ij} + \sum_{k=-3}^5 \beta_k \times \mathbb{1}(\text{period}_k) + \sum_{k=-3}^5 \beta_k^{Treated} \times \mathbb{1}(\text{period}_k) \times \text{Treated}_{ij} + \epsilon_{ijk}. \quad (2)$$

Here, y_{ijk} denotes the outcome y for incumbent worker i at firm j in year $k = t - d$ relative to the worker death occurring in year d . The model includes incumbent worker-firm effects 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 (1) and (2). 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.

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, e.g., if deaths of workers reflect additional stress from an uptick in firm performance that results in higher wages. Alternatively, the positive estimates could be downward-biased if deaths occur as a consequence of negative shocks to the firm. However, when pre-trends are parallel, such shocks would have to be sudden in onset but, at the same time, large enough to be associated with worker deaths. This, in turn, makes some potential threats to identification less compelling: coronary heart disease, for instance, develops over a long time span and is caused by chronic rather than short-term stress levels (Kivimäki et al., 2006).¹⁰

¹⁰ In a 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.”

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. Appendix Table A-4.1 reveals that firms in the treatment and comparison group have an identical probability (about 1.2 percent) of experiencing a worker death in a given future period, as the indicator for treatment status is statistically insignificant and small. 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.

Heterogeneity of Treatment Effects. In order to assess heterogeneity in the treatment effects, we estimate variations of the econometric models in (1) and (2) 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 covariates.

4 Effects of Worker Deaths on Employment, Hiring, and Coworker Wages

4.1 Effects of Worker Exits on Firm Employment and Hiring

In a first step, we document that a worker death constitutes a shock to a firm's labor supply and affects 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.

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 with a red dashed line. 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 -0.866 (SE 0.0027) in the first post-death period and is equal to -0.565 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 solid, 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 -0.340 (SE 0.035) 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 would hire a replacement worker instantaneously.

Figure 1 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 0.401 (SE 0.028) new workers and an additional 0.224 and 0.080 workers in the subsequent two periods. Figure A-4.1 in the Online Appendix 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.

4.2 How Do Worker Exits Affect Incumbent Worker Wages and Employment Outcomes?

This section examines the average effects of worker exits on incumbents' wages and employment. Figure 2 documents the dynamics of the treatment effect on the earnings of incumbents (see also Appendix Table A-4.2). The upper panel uses individual incumbent workers' labor earnings as the outcome variable and documents a statistically significant increase of Euro 173.40 (SE 37.47) in the first post-death period, $k = 1$. Compared to incumbent workers' average yearly earnings of Euro 27,856 in $k = -1$, this corresponds to a real increase of about 0.6 percent. 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 2 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 average, the sum of coworker earnings increases by Euro 1582.93 (SE 430.78) in the year following a worker death. The treatment effect then gradually decreases over time and remains statistically significant for the first two post-death periods. The total effect on the sum of coworker earnings in the first five post-death years is Euro 3,288 so that the increase in incumbent worker earnings corresponds to about 13 percent of a deceased worker's average annual earnings (Euro 26,099 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 $k = 1$. 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 Appendix Figure A-4.2, 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.

In Table 3, we document treatment effects on several employment outcomes. First, turnover of incumbent workers in treatment group firms is lower: each incumbent worker has, on average, about a 0.3 percentage point higher probability of remaining employed at the same firm. Incumbents in the treatment group are, however, not more likely to be employed at all, as the long-run effect on full-time employment is zero.

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. However, we also note that the base probability of switching to part-time work is low in our sample, thereby rendering the part-time/full-time margin overall less relevant. We revisit the evidence on intensive margin changes and how they change our interpretation of results in Section 6.4.

Effects Among Other Groups: Part-Time Incumbents, Apprentices, and New Hires.

We also analyze the effects on outcomes of part-time workers and apprentices in Appendix Table A-4.3. For part-time incumbents, we find qualitatively similar results. Wage earnings increase by Euro 159.66 (SE 83.47) in the short run, and we find a similar long-term effect (Euro 150.70, SE 85.21). We find positive but statistically insignificant effects on the probability to remain employed at the same establishment or to switch into full-time employment. For apprentices, we find positive but statistically insignificant increases in earnings of around Euro 118 per year as well as a sharp and statistically significant increase of 1.26ppt (SE 0.5ppt) to 1.49ppt (SE 0.4ppt) in the probability to remain employed at the same establishment.

We also study how a worker death affects wages and characteristics of new hires. In Appendix Table A-4.4, we document a large increase in new hires' wages of Euro 1,201 in the first year and Euro 490 in the long run. Taken at face value, these results would be consistent with a bargaining model or with some monopsony power in the market for new hires. This would contrast with the conclusions in Kline et al. (2019) and Garin/Silverio et al. (2018) who find no rent sharing among new workers. However, we caution that the effects on the wages of new hires are conceptually harder to interpret than the effects on wages of incumbents. That is, for the analysis of incumbents, we draw on a well-defined treatment and control group while for new hires the identity of the new hires potentially changes. Therefore, compositional changes in new hires' characteristics could explain at least some of the effects we document. Indeed, we find that characteristics of new hires change in response to a worker death. In Appendix Table A-4.5, we document that while education levels do not change on average, new hires are older and more experienced compared to the workers the firm would have hired in the absence of the worker death. Thus, they resemble the worker who passed away more closely than the cohorts of new workers the firm would have hired otherwise.

4.3 Heterogeneity Within And Across Occupations

We estimate the effect on wages of incumbent workers in the same occupation group as the deceased versus on incumbents in other occupation groups. 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. Figure 3 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 Euro 239.17 in the short run and 148.11 in the long run (see Table 4). In contrast, the average effect on workers in other occupation groups is about 32 percent smaller and only statistically significant in the short run.

4.4 Heterogeneity by Workers' Skills

We next analyze heterogeneity in the effect depending on the deceased's skill levels to investigate whether workers with more or more specific skills might be harder to replace. We focus on three measures: (i) education levels, (ii) skill intensity of the occupation, and (iii) managerial or supervisory status. We then focus on two measures of specificity by studying heterogeneity in the deceased's tenure and a measure of human capital specificity of the deceased's occupation.

Education Levels and Occupational Skill Intensity

As a first skill measure, we study heterogeneity in the deceased's education level and report results in Figure 4 Panel (b) and Table 5. We categorize education levels as low (no apprenticeship training), medium (apprenticeship training), or high (workers with a university entrance exam (*Abitur*) or a college degree). We find positive average effects in the low- and medium-education categories and negative average effects in the high-education category. 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. We also distinguish between effects within and across occupation boundaries, as in Section 4.3. The analysis reveals positive point estimates for low and medium education levels on coworkers in the same occupation as the deceased. Strikingly, we find large and negative effects of high-education worker deaths on workers in other occupations, with effects of Euro -521.38 (SE €272.43) in the short-run and Euro -273.86 (Euro 301.98) in the long-run.

We also analyze heterogeneity based on the skill intensity of the deceased's occupation and report qualitatively similar results in Figure 4 Panel (a) and Table 5. We focus on the skill intensity of the occupation level, as 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 percent sample of IEB biographies and then classify occupations as low- (below 20th percentile), as medium- (between 20th and 80th percentile), and high-skilled (above 80th percentile). Here, we find positive wage effects across the skill distribution of deceased workers on workers in the same occupation group as the deceased. Again, we find negative wage effects of workers in high-skill occupations on workers in other occupations, with a short-run effect of Euro -119.70 (Euro 150.93) and a long-run effect of Euro -54.37 (Euro 167.89).

Managerial Status

As another 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 Figure 4 Panel (c) and Table 5). We classify workers 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.¹¹ Based on this distinction, we find that deaths of workers in non-manager occupations are associated with positive effects on incumbent wages across and within the deceased's occupation group as are deaths of managers on workers in other occupations. In contrast, the effect of manager deaths on incumbents in other occupations is negative at Euro -136.51 (Euro 168.94) in the short-run and remains large in the longer run (Euro -172.24, SE Euro 185.75).

Tenure

We investigate treatment effect heterogeneity by tenure of the deceased worker. Columns (7) and (8) of Table 5 present treatment effects separately by tenure of the deceased worker: short (one to five years), medium (five to ten years), and long tenure (more than ten years). We find slightly smaller treatment effects associated with deaths of workers with short tenure and larger and more precisely estimated effects of deaths of workers with medium or long tenure. However, the point estimates are not estimated precisely enough to reject equality of the coefficients.

¹¹ 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.

Occupational Specialization: Returns to Experience

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/Lin (2012) who classify occupations as relying on more specific skills when the returns to experience are high. 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 as having low (below 20th percentile), medium (between 20th and 80th percentile), or high (above 80th percentile) degrees of specialization.

Columns (9) and (10) of Table 5 report treatment effects on incumbent worker wages by occupational specialization of the deceased worker. The baseline effects of specialization appear to be non-monotonic in specialization. However, as in the case of heterogeneity by skills, the average effects mask heterogeneity in the effect on coworkers in the same occupation versus coworkers in other occupations. In columns (3) and (4), we document that short-run 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. For deaths of workers in occupations with high degrees of specialization, we find a short-run effect of Euro 321.97 (SE Euro 131.68) on coworkers in the same occupation and of Euro -363.93 (SE Euro 138.04) on coworkers in other occupations.

4.5 Heterogeneity By Labor Market Thickness

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. For example, Moretti (2011) describes a potential benefit of labor market thickness for firms noting that “thick labor markets reduce the probability that a firm can't fill a vacancy, following an idiosyncratic shock to the labor supply of an employee” and points out that “this argument applies particularly to workers with specialized skills” (see also Lazear, 2009: for a similar argument).

Motivated by these considerations, we explore 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 50 commuting zones, following Kropp/Schwengler (2011). 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.¹² 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.

We find that incumbent wages respond less to a worker death and the differential is particularly pronounced for specialized occupations. We report results on coworkers in the same occupation as the deceased in Table 6 (and for all coworkers in Appendix Table A-4.6) and visualize results in Figure 4. As Figure 4 illustrates, for the sample of all worker deaths, the point estimate for the wage effect is larger in thin labor markets compared to thick ones; however, the difference is not statistically significant. If the 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 following Section 4.4). The analysis reveals substantially larger differences between thin and thick labor markets with point estimates for the short-run wage effect of Euro 439.78 in thin and Euro 111.25 in thick labor markets, respectively; the difference in the effect between thick and thin labor markets is statistically significant ($p = 0.02$).

We find similar patterns when using different measures of labor market thickness to estimate heterogeneity in the treatment in panels (B) and (C) of Table 6. 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, consistent with the hypothesis that labor market thickness mediates the response to worker exits.

We also study heterogeneity by local unemployment, as tightness (as opposed to thickness) is a key driver of matching in search-and-matching models. However, as panel (D) of Table 6, illustrates, wage effects are, if anything, larger when unemployment is high. Taken at face value, these results would be inconsistent with the predictions from a standard search-and-matching model intuition whereby firms would be able to recruit more easily

¹² 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.

when unemployment is high. However, they would be consistent with models in which higher unemployment raises the costs for firms to find a good match, as they need to select from a larger and less selective applicant pool (Hall, 2005; Engbom, 2021; Hall/Kudlyak, 2022).

To shed further light on the relevance of labor market thickness, we assess differences in the treatment effect on hiring across labor markets (Appendix Table A-4.7 and Appendix Table A-4.8). There, we find some suggestive evidence that firms hire more externally in thick markets. For example, a death of a worker in a specialized occupation leads to 0.36 (SE 0.06) additional hires in thin markets and to 0.48 (SE 0.05) in thick markets. We find similar differences (0.35 vs. 0.50 additional hires) when measuring thickness at the industry level. For density or unemployment, we do not find a similar pattern with deaths in specialized occupations leading to similar replacement hiring.

4.6 Summary

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 percent and 0.4 percent, respectively, 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. Wage responses are larger when the external labor market in the deceased's occupation is thin.

5 Estimation of Implied Replacement Costs

Our reduced-form 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. A key question that arises from the reduced-form evidence is how large the frictions are that firms face in replacing workers.

To provide an empirical answer to the question, we draw on a simple model with replacement costs (Kline et al., 2019). We then estimate the model parameters with the

method of simulated moments, allowing us to gauge the implied replacement costs of workers relative to the benchmark cases of no frictions and perfect substitution across workers within the firm.

5.1 Model Sketch

Static case.

Our static model follows Kline et al. (2019) and we report additional derivations in Appendix A-1. Each firm $j \in \{1, \dots, J\}$ starts with I_j incumbents. To reduce notation, we will omit j subscripts but note that choices and prices are firm-specific.

The firm chooses a wage w^I for incumbent workers. Incumbent workers then choose between staying at the firm or accepting an outside offer whose wage equivalent value is drawn from a distribution with CDF:

$$G(\omega) = \left(\frac{\omega - w^m}{\bar{w} - w^m} \right)^\eta, \quad \omega \in [w^m, \bar{w}]. \quad (1)$$

The firm therefore expects to retain $G(w^I)I$ workers. The parameter η captures the elasticity of worker retention to the incumbent wage premium $w^I - w^m$, relative to the market wage w^m .

After the uncertainty in retention is resolved, the firm can hire new workers in the outside labor market at market wage $w^m = w^m(A)$, where A represents the amenities provided by the firm. In addition to wage costs, hiring N new workers incurs an additional recruitment cost of $c(N, I)$, which exhibits constant returns to scale so that $c(N, I) = c(N/I)I$.

The total labor employed by the firm is:

$$L = G(w^I)I + N, \quad (2)$$

and total labor costs are:

$$c \left(\frac{N}{I} \right) I + w^m N + w^I G(w^I) I. \quad (3)$$

The firm produces one unit of output per worker at the end of the period and sells it in a monopolistically competitive product market with inverse product demand curve $P(L) = P^0 L^{-1/\epsilon}$ where $\epsilon > 1$ is the demand elasticity and P^0 is a demand shifter.

The firm's profits are given by:

$$\Pi(w^I, N, I) = P(L)L - c \left(\frac{N}{I} \right) I - w^m N - w^I G(w^I) I. \quad (4)$$

The first-order conditions characterizing the firm's optimal choice of incumbent wages w^I and new hires N are:

$$MRP = w^I + \frac{w^I - w^m}{\eta}, \quad (5)$$

$$MRP = w^m + c' \left(\frac{N}{I} \right), \quad (6)$$

where the marginal revenue product of labor is defined as $MRP \equiv \frac{dP(L)L}{dL} = \frac{\epsilon-1}{\epsilon} P(L)$.

Equating the two first-order conditions and re-arranging yields an expression for the incumbent wage premium:

$$w^I - w^m = \frac{\eta}{1 + \eta} c' \left(\frac{N}{I} \right). \quad (7)$$

The markup of incumbent wages over market wages arises from positive marginal hiring costs and equals a fraction $\frac{\eta}{1+\eta}$ of the marginal hiring cost. In turn, equation (7) also offers one way of measuring replacement costs $c' \left(\frac{N}{I} \right)$ as a function of the wages paid to incumbents and new hires and the shape parameter of the outside offer distribution η .

The model further illuminates the factors guiding the incumbent wage response to changes in the number of respondents, e.g., due to a worker death, with the following comparative

static:

$$\frac{dw^I}{dI} = \frac{\eta}{1 + \eta} \frac{\frac{dN}{dI} - \frac{N}{I}}{I} c'' \left(\frac{N}{I} \right). \quad (8)$$

In the model, the incumbent wage response thus depends on the convexity of hiring costs. We show in Appendix A-1 that $\frac{dN}{dI} - \frac{N}{I} < 0$. As a consequence, incumbent wages rise in response to a negative shock to the number of incumbents, e.g., due to a worker death, if and only if hiring costs are convex. If marginal hiring costs are constant, then all adjustment in response to a worker exit happens on the hiring margin (rather than through retention of incumbent workers).

Rearranging equation (8) also demonstrates how the empirical moments we identify in the data identify replacement costs:

$$c'' \left(\frac{N}{I} \right) = \frac{1 + \eta}{\eta} \frac{dw^I}{dI} \frac{I}{\frac{dN}{dI} - \frac{N}{I}}, \quad (9)$$

which depend on the incumbent wage response, $\frac{dw^I}{dI}$, the hiring rate, $\frac{N}{I}$, changes in the hiring rate, $\frac{dN}{dI}$, as well as η , which we identify from scaling the retention elasticity ($\eta = \frac{d \log G(w^I)}{d \log w^I} \frac{w^I - w^m}{w^I}$).

Extension to dynamic case.

We extend the model to incorporate dynamics by assuming that new hires become incumbents in the subsequent period. Each period represents one year. Letting β denote the firm's discount factor, the firm's problem is now characterized by the Bellman equation:

$$V(I_t) = \max_{w_t^I, N_t} \Pi(w_t^I, N_t, I_t) + \beta V(I_{t+1}) \quad \text{s.t.} \quad I_{t+1} = G(w_t^I)I_t + N_t. \quad (10)$$

The only change to the FOCs (5) and (6) is the addition of $\beta V'(I_{t+1})$ on the LHS.

5.2 Model Identification and Estimation

Identification

We adopt the following functional form for hiring costs:

$$c\left(\frac{N}{I}\right) = \frac{\gamma}{1+\lambda} \left(\frac{N}{I}\right)^{1+\lambda}. \quad (11)$$

The parameter γ determines the steady-state marginal hiring cost while λ determines the degree of convexity.

Given (11), the model has eight parameters: $\gamma, \lambda, \eta, \epsilon, \bar{w}, P^0, \beta, w^m$. The first six parameters are estimated, and we set the remaining two. Let θ refer to the estimated parameters. Because each period represents one year, we set $\beta = 0.96$ to target a 4 percent discount rate. Finally, we set w^m to the sample average earnings for new hires.

To identify θ , we target the retention, hiring, and earnings response one year after an incumbent's death. Letting k denote the year relative to an incumbent death, we assume the firm is in steady state until an incumbent dies between $k = 0$ and $k = 1$, and the firm responds to the incumbent's death in year $k = 1$. We draw on the sample means before the worker death and posit that the firm has 14.51 incumbents, retains 82.6 percent of its incumbents, and pays its incumbents Euro 28176. We set the number of hires to 2.17 new workers to ensure the firm is in a steady state (and also closely matching the sample mean). Our reduced-form results indicated a 0.3 percentage point increase in retention, 0.4 additional hires, and a Euro 173 increase in incumbent earnings in response to a worker death. Therefore, in year $k = 1$ the treated firm starts with 13.48 incumbents and should retain 82.9 percent of its incumbents, pay them Euro 28349, and hire 2.57 new workers. These moments identify the model parameters because they imply a system of six equations in six unknowns given by:

$$\frac{\epsilon - 1}{\epsilon} P^0 L_0^{-1/\epsilon} + \beta V'(I_0) = w_0^I(\theta) + \frac{w_0^I - w^m}{\eta}, \quad (12)$$

$$\frac{\epsilon - 1}{\epsilon} P^0 L_0^{-1/\epsilon} + \beta V'(I_0) = w^m + \gamma \left(\frac{N_0}{I_0}\right)^\lambda, \quad (13)$$

$$\left(\frac{w_0^I - w^m}{\bar{w} - w^m}\right)^\eta = 0.826, \quad (14)$$

$$\frac{\epsilon - 1}{\epsilon} P^0 L_1^{-1/\epsilon} + \beta V'(I_1) = w_1^I + \frac{w_1^I - w^m}{\eta}, \quad (15)$$

$$\frac{\epsilon - 1}{\epsilon} P^0 L_1^{-1/\epsilon} + \beta V'(I_1) = w^m + \gamma \left(\frac{N_1}{I_1} \right)^\lambda, \quad (16)$$

$$\left(\frac{w_1^I - w^m}{\bar{w} - w^m} \right)^\eta = 0.829, \quad (17)$$

where the values of I_k , L_k , N_k , and w_k^I are given by:

	I_k	L_k	N_k	w_k^I
$k = 0$	14.48	14.38	2.168	28175
$k = 1$	13.48	14.04	2.569	28380

The subscripts indicate the year k , so the first three equations represent the steady-state moments, and the last three represent the moments one year after an incumbent death.¹³ The value function is obtained by solving the Bellman equation (10) with collocation, using either regular or Chebyshev polynomials.

We estimate the model using the method of simulated moments. Then we use the estimated model to simulate the event study and calculate statistics which measure the magnitude of labor market frictions.

5.3 Results

Parameter Estimates

We report results in Table 7. Column 1 reports results based on short-run effects (one year after a worker death), column 2 based on long-run results (over five years).

Several clear results emerge that are consistent across specifications and point towards substantial replacement costs. First, we find high values of γ , the parameter determining

¹³ In the model, we target the wage response in the year after the death compared to the year before. In the data, we observe wages at an annual frequency, so we use the year ending 6–18 months before the worker death for $k = 0$ and data from the year beginning 6 months after to 6 months before the worker death for $k = 1$ response, leaving out the intervening year where there is already partial adjustment (see Table A-4.2, and Table A-4.9 for an approach breaking it out by month of death that yields results in line with the model and the no-anticipation assumption). Also, in the data we observe the wages of all coworkers; in the model 17% of workers are inframarginal leavers who have a high outside offer and will leave the firm whether it pays w_0^I or w_1^I . We compute the implied wage changes for all coworkers in the model so that it matches what we observe in the data. This logic also implies that our results may underestimate the increases in posted wages, especially 5 years out when only 30% of workers are still at their original firm (if posted wages are not passed through to workers at subsequent firms).

the steady-state marginal hiring costs, with values ranging from Euro 76,000 to Euro 98,000. Second, we find moderate convexity of hiring costs, with λ being 0.09. A result of $\lambda = 0$ would have implied that all adjustment to a worker death occurred on the hiring (rather than retention) margin. Third, we find low value for η , the elasticity of incumbent retention to the incumbent wage premium, ranging between 0.2 and 0.3. These can be transformed into retention elasticities and are consistent with the reduced-form retention elasticity of 0.62. The estimate is at the lower end but within the range of estimates for the retention elasticity surveyed in meta-analyses (Manning, 2021; Sokolova/Sorensen, 2021).

As a summary measure, we calculate the implied marginal replacement cost $c'(\frac{N}{T})$ for firms in our sample and find values ranging between Euro 65,000 and Euro 84,000. As a benchmark, we compare these to the wages of incumbents in our worker death sample. This calculation reveals a marginal replacement cost between 2.3 and 3 annual salaries of an incumbent. Our estimates of replacement costs are substantially higher than standard estimates in the literature based on firm surveys (see, e.g., Manning, 2011). An important distinction of our results from ones based on firm surveys is that our results draw on actual employment and wage responses of firms in response to worker exits. Our results are in line with the results in Kline et al. (2019, 2021), which point to marginal replacement costs of 1.27 times the annual earnings of an incumbent and who use a similar framework but different empirical strategy with identification stemming from wage differences between new workers and incumbents and rent sharing elasticities.

As a complement to our structural estimation, we also offer a simple back-of-the-envelope calculation to assess firms' willingness to pay to retain incumbents. We had documented that, in response to a worker death, firms pay an average of Euro 527 to their incumbents and get 0.018 more worker-years from their retained workers (in total over a period of five years). The implied expenses for retaining a full incumbent are hence equal to $527/0.018 = 28364$ or roughly one annual salary. This does not provide an upper bound on replacement costs because convex costs would make further incumbents more costly to retain. Nor does it provide a lower bound because the average cost of adjusting through hiring could be lower; it is only the marginal costs that are equalized. However, the exercise gives a sense of magnitudes of replacement costs on the retention margin. Our structural estimation gives estimates about for overall replacement costs that are substantially larger, though we find quantitatively more similar results once we incorporate the intensive margin response (which we report on in Section 6.4).

We further gauge the plausibility of the results of our dynamic structural model by tracing the paths of hiring and incumbent wages implied by our parameter estimates and compare them to reduced-form findings in Figure 5. In Panel (a), we show that the model almost perfectly replicates the observed short-run employment response to a worker death in the first three years after a worker death. In the subsequent years, we see a slight divergence

with model employment fully converging while observed employment remains slightly lower. However, the difference between the model prediction and the data is not statistically significant. Panel (b) reports results for hiring in the model and the data. The model matches the overall pattern of hiring responses very well with a sharp increase in the year after the worker death and a subsequent decline. Again, the long-run differences are not statistically distinguishable even though the point estimates for hiring in the data remain slightly elevated compared to the model. Finally, we show the wage response in panel (c). Here, we see a perfect match in the first year after the event (and had noted before that period 0 for wages is muddled due to the data reporting periods). However, we see a divergence in years two through four where the observed wage response in the data remains more elevated while wages in the model converge more quickly.

Two potential hypotheses for the divergence are (i) that it might take more than one period for new workers to become incumbents (so that the effective number of incumbents remains depressed for longer), or (ii) there could be frictions in wage setting, e.g., wage rigidity, so that a firm cannot easily take back raises it granted. Our results on heterogeneity by specialization and external labor market thickness provide some support for the first hypothesis. We also gauge the explanatory power of the second hypothesis. To that end, we split our sample into firms with less or more wage flexibility (proxied by the standard deviation of period-to-period wage changes as in Jäger et al., 2020) and report results in Appendix Table A-4.10. We find that firms with more flexible wages have, on average, lower long-term wage effects. However, once we zoom into heterogeneity within and across occupations, we also detect large (absolute) effects among firms with more flexible wage policies. We therefore conclude that specialization of skills, which takes time, has more support in the data to help explain the longer-term wage effects we observe (although we leave a more definitive test to future research).

Labor Market Heterogeneity

We also estimate the model separately by thickness of the external labor markets and find that estimated replacement costs are roughly twice as large in thin labor markets compared to thick ones (see columns (3) and (4) of Table 7). This is a consequence not only of the larger wage response in thin markets, but also, somewhat surprisingly, by a smaller increase in retention in thin markets (0.24 percentage points to 0.46), despite the larger wage change. This means that the change in wages required to retain an additional workers is substantially larger in thin markets.

Extension to Imperfect Substitution Within the Firm

We estimate the model with two types of workers, considering those in the same occupation as the deceased as one type, and those in other occupations as another type. In Table 4, we had shown that the wage and especially hiring responses are concentrated on the same occupation as the deceased (workers in the same one-digit occupation as the deceased make up 63 percent of the average firm, but 86 percent of the new hires). However, there is an increase in hiring for workers in other occupations, whereas if workers were so imperfectly substitutable such that $\rho < \frac{\epsilon-1}{\epsilon}$, there would be a decrease. We report results in Appendix Table A-4.14. We estimate $\rho = 0.85$, which implies an elasticity of substitution between workers in different occupations of 6.5. We also find that hiring costs are much higher for workers in other occupations, which would rationalize firms' choice to increase those workers' wages but not engage in much hiring. This could be because these occupations are disproportionately specialized or outside of the firms' area of expertise.

As a complement to our analysis of imperfect substitution between workers in different occupations, we also conduct a simple back-of-the-envelope calculation. If workers could frictionlessly move between occupations, then hiring and retention efforts should roughly track the share of workers in the deceased's occupation. However, our results show that most retention and hiring efforts occur within the deceased's occupation. That is, we observe that 63 percent of workers are in the same 1-digit occupation as the deceased. On average, they receive a wage increase of Euro 239, while the point estimate for workers in other occupations is Euro 162. This means that $\frac{0.63*239}{0.63*239+0.37*162} = 71.5\%$ of the retention expenditures in wages go towards workers in the same occupation. Therefore, about $71.5/63 - 1 \approx 13.5\%$ more of retention costs go into the same occupation. Similarly, we also calculate the share of the total wage bill increase among incumbents that goes into the same occupation and find that about 94 percent of the incumbent worker wage bill increase in response to a worker death occurs in the deceased's occupation, leading us to a similar quantitative conclusion.¹⁴

¹⁴ We also estimate that the firm hires 0.35 new workers in the same occupation and 0.05 in other occupations; with linear hiring costs those correspond to 86% of the hiring expenditures being on workers in the same occupation. Incorporating convex hiring costs, that share would be higher.

6 Alternative Models and Interpretations of Results

The framework in Section 5 drew on a wage posting model. Here, we discuss and evaluate several alternative models of wage determination as well as alternative explanations of our results. We first discuss bargaining and internal labor market models and then discuss two alternative interpretations of our findings, compensating differentials and hours changes.

6.1 Bargaining Models

Wage posting assumes zero bargaining power for workers. Here, we sketch two alternative models where workers also hold some bargaining power. As in the model in Section 5, the effects of incumbent exits depend on replacement costs.

First, we extend the model from Section 5 by giving workers bargaining power through a union. We assume that workers and the firm bargain over incumbent wages and that, after bargaining, the firm can hire as many outsiders as it wants (a version of the right-to-manage model). Relative to the static baseline model, the key difference is that (5) will be amended as incumbents can demand higher wages. The solution to the Nash bargaining problem is then characterized by:

$$\frac{w^I - w^m}{\eta} + w^I - MRP = \frac{\phi}{1 - \phi} \frac{1}{g(w^I)I} \frac{\Pi - \underline{\Pi}}{w^I - w^m} \quad (1)$$

where $\underline{\Pi}$ denote the firm's profits when using only outsiders hired from the market and ϕ denotes bargaining power of the union. We provide a more comprehensive description of this extension as well as comparative statics with respect to the number of incumbents in Appendix A-1.3. If there are no replacement costs, the wage response will be zero.

Incumbent wages will increase in response to a worker exit for sufficiently small values of worker bargaining power ϕ .

Second, an alternative framework are multi-worker firm models with intrafirm bargaining (Stole/Zwiebel, 1996a,b; Cahuc/Wasmer, 2001; Acemoglu/Hawkins, 2014). In this class of models, worker replacement on the external labor market is costly. Firms engage in pairwise negotiations with workers, taking into account that their outside option, if negotiations with

an individual worker break down, is to continue negotiating with the remaining workers. Compared to the wage posting framework we adopt in Section 5, the model shares several qualitative predictions for the relationship between replacement costs and the effects of worker exits. As we show in Appendix A-2, wages of incumbent workers rise following the exit of a coworker from a firm with decreasing returns to scale and wage effects become smaller in magnitude when firms face fewer search frictions, e.g., because more outsiders are available. In the limit, wage effects of a worker exit become zero as frictions go to zero.

6.2 Internal Labor Market Models

A separate framework that could account for our findings are internal labor market models in which hiring of new workers is largely restricted to lower-level “ports of entry,” higher-level vacancies are typically filled internally, and wages track seniority and job titles (Doeringer/Piore, 1971). 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 and lead to a vacancy chain of promotions.

Our model in Section 5 could accommodate an internal labor market interpretation. For example, job titles might simply be labels for wage levels and firms might promote workers whenever they change their wages (e.g., to increase retention).

A version of an internal labor markets model that would be harder to square with the model in Section 5 is one in which wages sharply track positions and firms face “slot constraints” (see, e.g., Lazear/Rosen, 1981; Bianchi et al., 2022). For example, suppose a firm has two slots for staff engineers and one slot for a senior staff engineer and little leeway to adjust wages in a given position. In that case, the firm could raise incumbent wages in response to a worker exit only if the senior staff engineer exits (and one of the more junior staff engineers gets a promotion and a wage increase). In contrast, if one of the non-senior staff engineers leaves, the firm could not increase wages for the remaining engineers due to the slot constraints preventing a second senior staff engineer and having little leeway to raise wages without job title changes.

We test two key implications of this view. First, we analyze effect heterogeneity by the relative salary ranking of the deceased and the remaining incumbent workers (Column 1 and 2 of Panel A in Appendix Table A-4.11). We calculate separate treatment effects for the remaining coworkers who earned more and those who earned less than the deceased worker. In the short run, we find slightly larger effects where the deceased ranked higher than the incumbent worker (Euro 212, SE 53) compared to cases where the deceased ranked lower (€135, SE 67). In the long run, we find effects of Euro 115 (SE 55) where the deceased

ranked higher than the incumbent worker and of Euro 254 (SE 74) where the deceased worker ranked lower than the incumbent. We find a similar pattern of results when zooming into workers in the same occupation group as the deceased (Columns 1 and 2 of Panel B in Appendix Table A-4.11). As a complement, we also study deaths of workers by whether they are among the top 25 percent of a firm's salary ranking (Columns 3 and 4 of Appendix Table A-4.11). We find larger effects for deaths of workers outside the top salary group. Overall, the pattern of results is at odds with predictions of the slot constraints view of internal labor markets driving our main results.

Second, we also test to what extent promotions can account for the wage changes we observe. As one proxy for promotions, we test whether worker deaths trigger changes into higher-paying occupations among the remaining incumbent workers and find a small but precisely estimated 0.1 ppt (SE 0.03 ppt) increase in the probability of a promotion in the short run (Column (5) of Appendix Table A-4.3).¹⁵ This increase in promotions is driven by those incumbent workers who were in a lower-paying occupation than the deceased (Columns 5 and 6 of Appendix Table A.4-11). To gauge to what extent such promotions can account for the wage changes we observe, we estimate specifications assigning the average wage in an occupation as outcome variable. For these specifications, we find small and significant effects indicating a wage increase of 0.12 percent (SE 0.15%), i.e., about four times smaller than the overall wage effect we observe. As previous work has documented that the wage effects of promotions are small relative to the average differences between jobs (Baker/Gibbs/Holmstrom, 1994b), we believe that this likely constitutes an upper bound for the share of wage increases accounted for by promotions. Our evidence therefore does not point to promotions as the main mechanism for the wage changes we observe.

Overall, we conclude that our evidence documents the importance of internal labor markets as idiosyncratic shocks to firm-specific labor supply shape wages. However, we find less support for slot constraints driving our results, as we find wage effects among workers who initially ranked above the deceased worker in the firm's salary distribution and who do not experience higher rates of promotion. We also had already documented negative effects of deaths of high-skilled worker and managers on the wages of workers in other occupations, which an internal labor market model with promotions would not predict. Our overall results can be explained in a model where internal labor markets matter, e.g., because costs of replacing workers lead to a role for idiosyncratic shocks to labor supply in shaping wages, but are harder to square with models of slot constraints and little leeway in wage setting conditional on a position (consistent with Baker/Gibbs/Holmstrom, 1994a: who document that "job levels are important to compensation, but there is also substantial individual variation in pay within levels").

¹⁵ We have also assessed whether a specific type of promotion, to foreman (*Meister*), may drive our results but find precisely estimated zero effects. We thank one of our referees for this suggestion.

6.3 Compensating Differentials

We also 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/Rosen, 1976): for instance, the perception of job hazards could have increased as a consequence of a death (even though we had documented that the risk of future deaths is not increased in treatment firms). Alternatively, the amenity value of working at the firm and interacting with coworkers is lower after having lost a colleague. These explanations have in common that worker 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 in response to a worker death. Moreover, we also find that the retention rates go up only in the same occupation as the deceased (Table 4), i.e. exactly where we found positive wage effects and further casting doubt on a compensating differential explanation. We also separately assess effects on weekends and week days, as weekend deaths are arguably less directly related to work events. If anything, we find larger effects for deaths that occur on weekends (Appendix Table A-4.12). The overall results therefore imply that shifts in firms' labor demand are indeed the driving force underlying the effects that we estimate.

6.4 Hours Changes

Our analysis of wage changes draws on wage earnings of full-time incumbent workers. Unlike other social security datasets, our data feature exact days worked so that employment duration does not affect our outcome variable mechanically. However, a key open question is to what extent the effects on wage earnings we document reflect changes in the wage rate vs. changes in work hours. Here, we revisit to what extent the effects on wage earnings may reflect hours changes and, if so, how that would change the interpretation of our findings.

We have already investigated whether worker exits affect incumbent hours at the part-time/full-time margin and found precisely estimated zero effects (although the base probability of a switch to part-time is low). The IAB administrative data generally do not feature detailed data on hours beyond the full- and part-time margin (e.g., on paid overtime work). For a limited time period, 2010 to 2015, we can draw on hours data from the accident insurance (see also Gudgeon/Trenkle, forthcoming; Dustmann et al., 2022). We analyze

effects for this subperiod and report results in Appendix A-3. We find no average effects on hours worked and also no effects on hours when we analyze effects separately within and across the same occupation as the deceased. For this sample, we detect no average effect but find large positive long-run wage effects in the same occupation as the deceased and large negative effects among workers in other occupations. While we find no effects on work hours for this sample, we caution that the data may imperfectly capture actual rather than regular or contractual hours and in particular overtime and the estimates of earnings effects are imprecise for this sample.

In order to assess how the interpretation of our results would change if the wage earnings response in our main analysis also partially reflects hours changes in ways we cannot measure with the data for our main sample, we extend our model to explicitly feature an intensive margin of labor supply for incumbent workers (see Appendix A-1.4 for details). We incorporate an intensive margin by assuming that workers receive disutility from working additional hours beyond their scheduled hours and that firms will compensate them for the disutility. We derive comparative statics, showing that for sufficiently convex costs of working additional hours, worker exits would raise hours but still also lead to wage rate increases among the remaining incumbents (beyond an overtime premium). We also estimate the model and target a Hicksian elasticity of labor supply following the literature (Chetty et al., 2013). We report results in Table A-4.14 and Figures A.1 through A.4. The results imply that about 70 percent of the earnings response one period after an incumbent death is due to wages and the remainder due to hours. The estimates also point to substantial replacement costs on the order of magnitude of one annual salary for an incumbent (134%). Thus, once we incorporate an hours margin in our model, the results are still indicative of large costs of replacing 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 interpreted our results quantitatively through the lens of a model with replacement frictions. We also assessed our reduced-form results through the lens of alternative frameworks, which might be operating in tandem, and found our overall conclusions to be robust (although we acknowledge the difficulty of accounting for multiple alternative explanations operating at the same time). A key take-away of our study is that the replacement costs implied by our findings are substantially larger than most estimates in the literature (see Manning, 2011: for a

survey).

A key difference of our study relative to most estimates from the literature is that we leverage a revealed-preference approach to measure wage and employment responses to employee exits rather than measuring recruitment and training costs through surveys. Why does our approach imply substantially larger costs of employee turnover than measured in firm surveys? We argue that such surveys miss three crucial and related dimensions of turnover costs. First, they miss higher costs of retaining incumbent employees who become more valuable in response to coworker turnover—a novel mechanism largely overlooked in the previous literature. Second, they miss the component of firm-specific human capital acquisition that is not embedded in worker training. If “we know more than we can tell” (Polanyi, 1967; Autor, 2014), acquisition of firm-specific human capital and, in particular, its tacit components, takes time to acquire—costs only insufficiently captured by explicit training costs. Third, they miss the costs of replacing incumbent workers with high match quality, which takes time to be revealed (Jovanovic, 1979a,b). Our evidence pointed to larger replacement costs when workers skills are specialized and when the external market for them is thin, thereby supporting specific human capital or match specificity as important correlates of replacement costs. Our evidence also pointed to longer lasting wage relative to employment effects, consistent with the idea that it takes time for newly hired workers to become insiders. We leave a deeper investigation and modeling of the processes through which newly hired workers become insiders to future research.

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/Schoefer, 2020). Nonetheless, we also caution that several differences exist between worker deaths in the smaller firms we consider and other types of worker exits. For example, a worker getting poached might recruit some of their former colleagues to the new employer. In addition, poaching constitutes useful information, e.g., about relative wages and working conditions for the original firm as well as the remaining workers who may be imperfectly informed about market wages (Jäger et al., 2022). A research design with exogenous variation in non-death worker exits would shed light on such mechanisms.

Our findings point towards several fruitful directions for future work. One promising avenue for future work will be an investigation of how different types of production hierarchies (Caliendo/Monte/Rossi-Hansberg, 2015) amplify or decrease replacement costs, and to what extent turnover of key employees triggers reorganizations of such hierarchies. Our findings also raise the question of whether and under what conditions workers can change their own replaceability through entrenchment (Shleifer/Vishny, 1989), and what

consequences such entrenchment may entail. Finally, our paper provided evidence supporting two key assumptions of models in which the supply of skilled workers affects firms' technology adoption (Acemoglu, 1996, 1997): firms facing frictions in replacing workers and these frictions appearing greater when human capital is firm-specific. Investigating how changes in replacement costs affect the adoption of new technologies and organizational structures by firms would thus be a natural next step.

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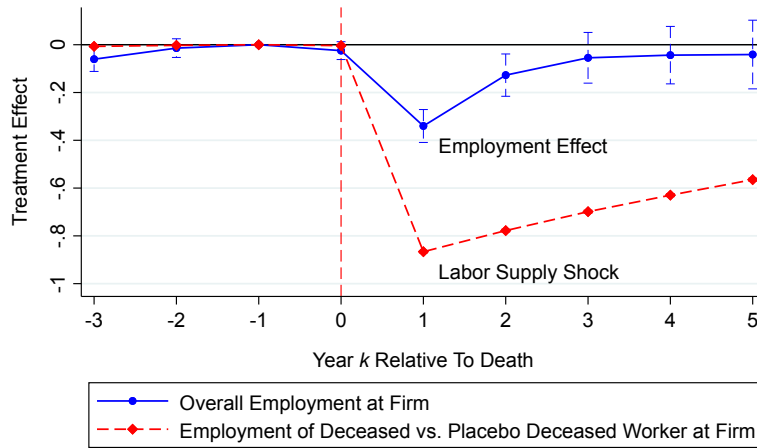
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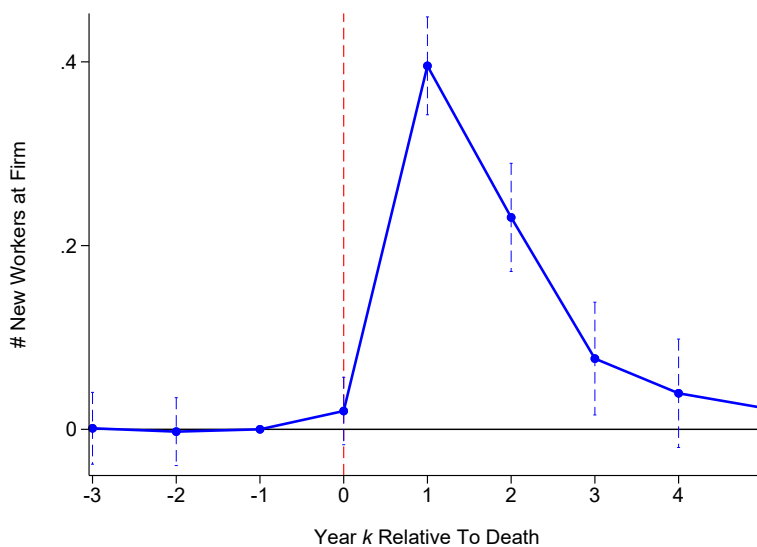
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Figures

Figure 1: Effect of Worker Death on Employment and Hiring



(a) Labor Supply Shocks Due to Worker Deaths and Employment Effects

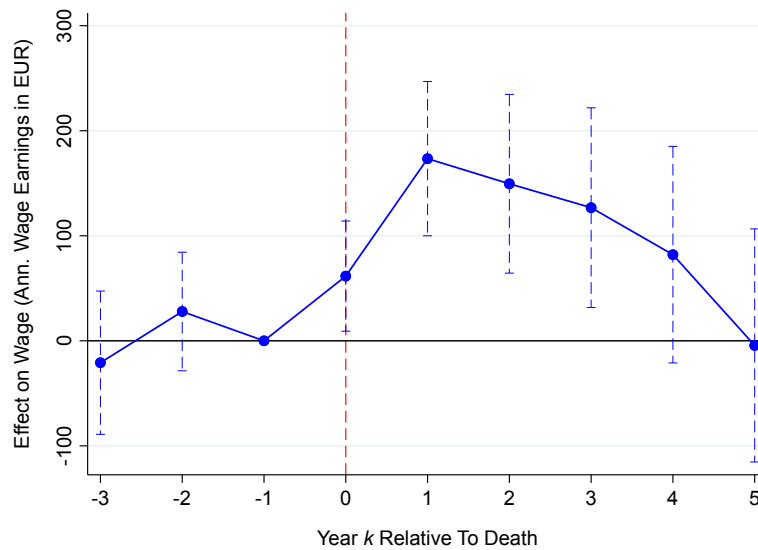


(b) Effects of Worker Deaths on Hiring

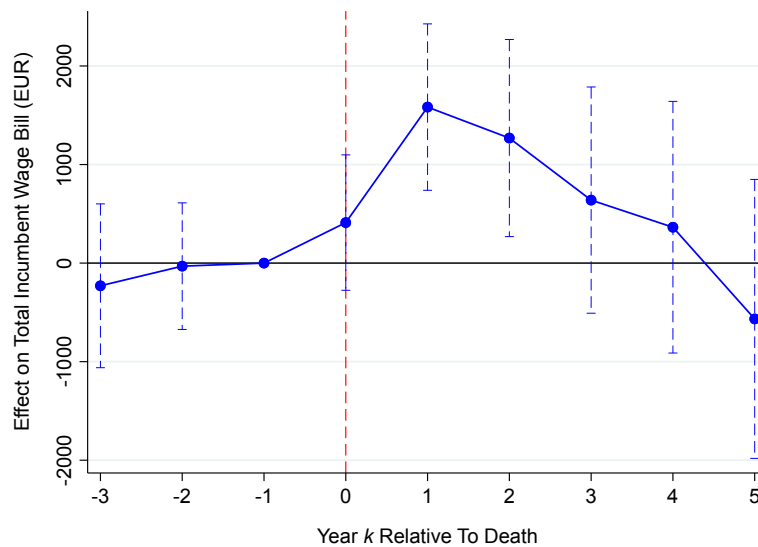
Note: The figures show regression coefficients and associated confidence intervals for the difference between treatment and comparison group in a given year k relative to the death of a worker in the treatment group firms, i.e., the $\beta_k^{Treated}$ from the difference-in-differences model in (1). The coefficient in $k = -1$ is normalized to zero. The first outcome variable in Panel (a) measures the overall employment at a firm. The comparison group mean for employment in $k = -1$ is 14.5. The labor supply shock is captured by an indicator variable that is equal to 1 if the deceased or placebo deceased is employed at the firm under study. The outcome variable in Panel (b) 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.

Source: Own calculations.

Figure 2: Effect of Worker Deaths on Incumbent Worker Wages



(a) Incumbent Worker Wage Earnings

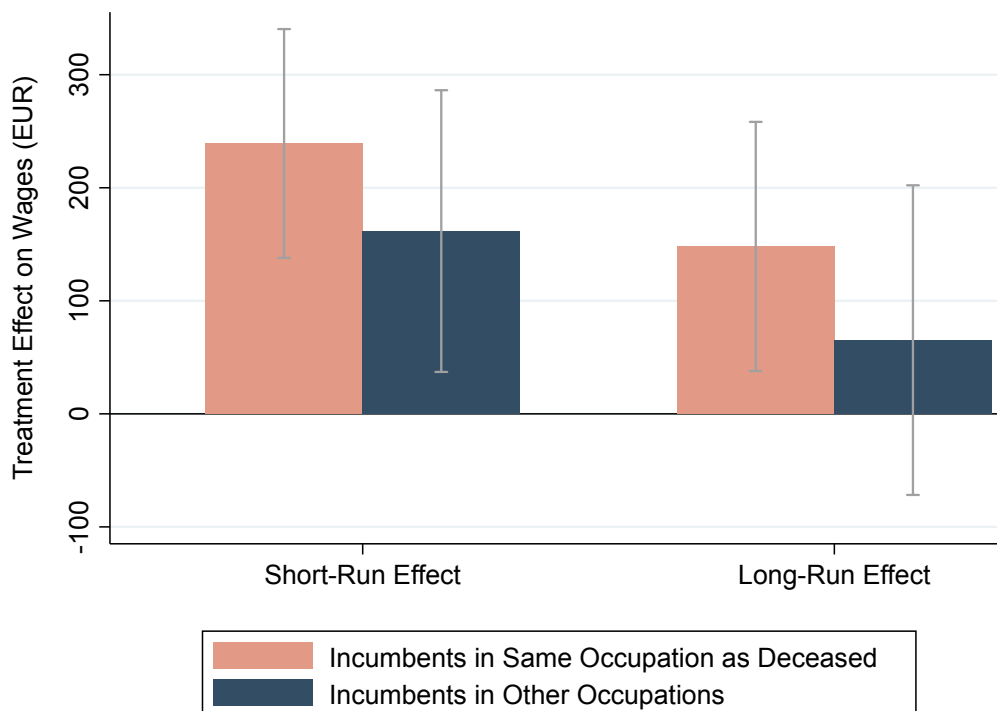


(b) Sum of Incumbent Wages in a Given Year

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 (2). The coefficients in $k = -1$ are normalized to zero. In the first panel, the outcome variable is the wage of an incumbent worker (scaled to correspond to yearly earnings, CPI 2010). Incumbent workers are defined as full-time coworkers of the deceased or placebo deceased in the year before death. The comparison group mean of incumbent worker wages in year $k = -1$ is €27,839 so that the €173.40 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 dashed vertical lines denote 95% confidence intervals based on standard errors clustered at the firm level. See Appendix Table A-4.2 for additional information.

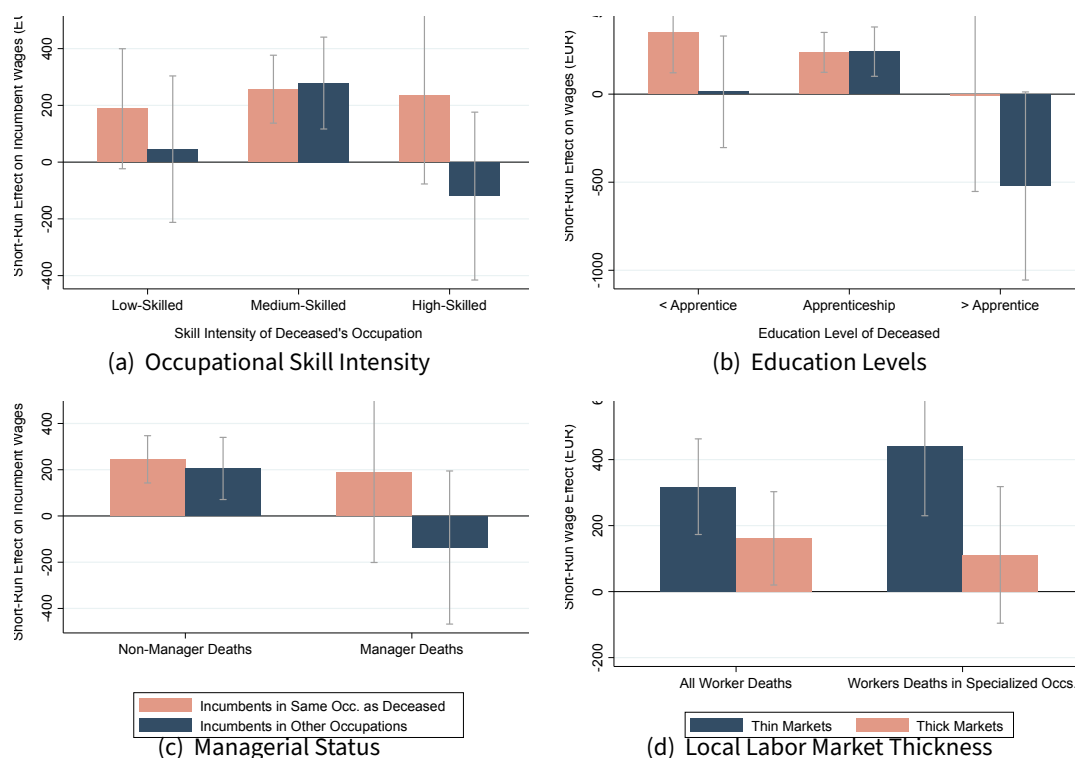
Source: Own calculations.

Figure 3: Incumbent Wage Effects in Same vs. Other Occupations



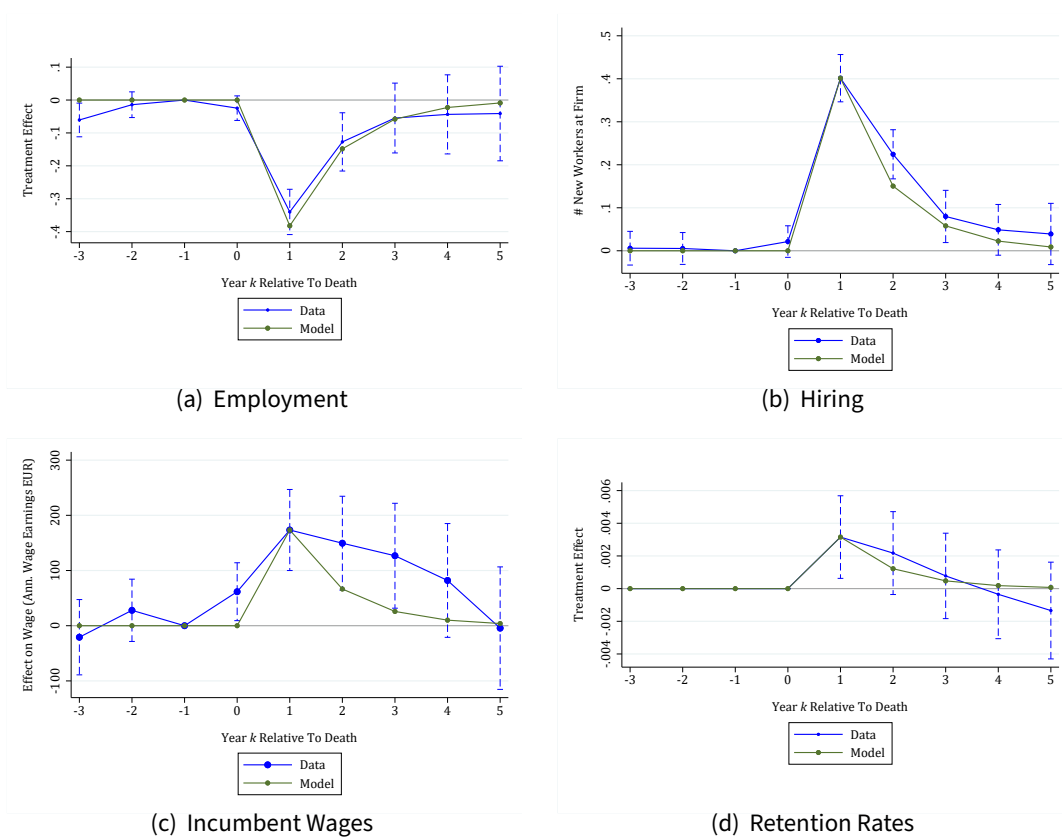
Note: The figure displays treatment effects of worker exits on the wages of incumbents in the same 1-digit occupation group as the deceased and on incumbents in other 1-digit occupation groups. 1-digit occupation groups stratify occupations horizontally based on the thematic focus of the work, e.g., 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.
Source: Own calculations.

Figure 4: Heterogeneity in Incumbent Wage Effects by Skill Level of Deceased and Labor Market Thickness



Note: The 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, in panels (a) through (c), 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 panel (d), we show heterogeneity by local labor market characteristics. The first two bars plot effect heterogeneity for all worker deaths; the second two bars restrict the sample to workers in specialized occupations. 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. 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 5 for more details). In all panels, the vertical lines indicate 95% confidence intervals based on standard errors clustered at the firm level. See Tables 5 and 6 and Section 4.5 for additional information. **Source:** Own calculations.

Figure 5: Model Prediction vs. Reduced-Form Effects



Note: The figure displays effects of worker deaths on several firm and incumbent worker outcomes. The blue lines report the measured effect in the data. The gray and green lines report predictions based on an estimation of the modified Kline et al. (2019) model using the method of simulated moments. See Section 5 for more details.
Source: Own calculations.

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 (9.91)	47.22 (9.91)	39.42 (11.30)	39.33 (11.30)
Female	0.14 (0.35)	0.14 (0.35)	0.26 (0.44)	0.26 (0.44)
Earnings (€, 2010 CPI)	26,098.68 (9,264.10)	26,412.74 (9,130.92)	27,770.17 (13,656.44)	27,839.64 (13,617.7)
Years of Education	10.62 (1.52)	10.63 (1.56)	10.91 (1.86)	10.90 (1.84)
Tenure	9.51 (6.14)	9.55 (6.12)	7.03 (5.47)	7.06 (5.48)
<i>N</i>	33,786	33,786	379,728	379,728

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 €(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.

Source: Own calculations.

Table 2: Firm-Level Summary Statistics

	Treatment Group	Comparison Group
Total Number of Employees	14.45 (7.37)	14.51 (7.39)
Number of New Workers	2.16 (2.30)	2.16 (2.32)
Number Part-Time Workers	1.19 (2.24)	1.19 (2.25)
Number Apprentices	0.82 (1.49)	0.85 (1.51)
Firm Age	14.78 (6.77)	14.80 (6.78)
Primary Sector	0.029 (0.167)	0.029 (0.168)
Secondary Sector (Manufacturing)	0.500 (0.500)	0.495 (0.500)
Tertiary Sector (Service)	0.472 (0.499)	0.477 (0.499)
<i>N</i>	33,786	33,786

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*).

Source: Own calculations.

Table 3: Treatment Effect on Incumbent Worker Employment Outcomes

	Short-Run Effect	Long-Run Effect
<u>Outcome: Employed at Same Establishment</u>		
Treated	0.0032 (0.0013)	0.0036 (0.0014)
Comparison Group Mean in $k = 1$: 0.826		
<u>Outcome: Full-Time Employment</u>		
Treated	0.0007 (0.0010)	-0.0015 (0.0010)
Comparison Group Mean in $k = 1$: 0.895		
<u>Outcome: Part-Time Employment</u>		
Treated	0.00004 (0.0004)	0.0005 (0.0005)
Comparison Group Mean in $k = 1$: 0.012		
No. of Observations	6807673	6807673

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. Standard errors are based on 67,726 clusters at the firm level.

Source: Own calculations.

Table 4: Effects on Outcomes Within Deceased Worker's Occupation and in Other Occupations

Outcome:	Incumbent Worker Wages		Retention		Hiring		Employment	
	Short Run	Long Run	Short Run	Long Run	Short Run	Long Run	Short Run	Long Run
Treated × Same	239.17 (51.64)	148.11 (56.21)	0.0049 (0.0017)	0.0017 (0.0008)	0.35 (0.02)	0.13 (0.02)	-0.39 (0.03)	-0.19 (0.03)
Treated × Other	161.69 (63.57)	65.22 (69.87)	0.0002 (0.0020)	-0.0004 (0.0010)	0.05 (0.02)	0.03 (0.02)	0.05 (0.02)	0.06 (0.03)
No. of Observations	6807673		4928792		608148		608148	
No. of Clusters	67572		67572		67572		67572	

Note: The table shows heterogeneity of the treatment based on the difference-in-differences framework in equation (2). 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. For hiring and employment, they refer to the numbers of workers hired and the stock of workers in the same occupation and in all other occupations within the firm. The retention rate is defined as the probability of exit for a worker who is still in the same employment spell as they were at the time of the coworker death. The sample size is smaller for retention outcomes because we restrict our analysis to retention of workers who are employed at the firm at the time of the death.

Source: Own calculations.

Table 5: Heterogeneity of Wage Effects By Deceased's Skill Levels and Specialization

Outcome: Incumbent Worker Wages										
Dimension of Heterogeneity: (Deceased Characteristic)	Education		Skill		Managerial Status		Tenure		Specialization	
	Short Run	Long Run	Short Run	Long Run	Short Run	Long Run	Short Run	Long Run	Short Run	Long Run
Treated × Low	195.48 (91.50)	94.31 (101.41)	120.54 (77.69)	56.38 (85.81)	205.51 (39.19)	133.25 (43.29)	117.21 (104.60)	36.82 (113.23)	119.24 (86.93)	115.33 (96.32)
Treated × Medium	186.60 (41.88)	118.97 (46.29)	236.62 (46.73)	152.00 (51.52)			194.65 (60.99)	86.35 (66.72)	197.43 (46.78)	96.62 (51.82)
Treated × High	-52.73 (181.46)	-45.49 (199.55)	7.52 (100.36)	-2.69 (111.85)	-59.19 (120.75)	-93.57 (133.95)	137.52 (56.82)	114.58 (64.06)	144.25 (89.63)	122.60 (97.88)
Treated × Low × Same Occupation	350.84 (117.18)	310.80 (129.25)	188.17 (107.91)	126.58 (114.61)	244.96 (52.14)	136.02 (56.36)	134.61 (146.08)	174.33 (154.76)	221.32 (129.85)	223.30 (143.63)
Treated × Low × Other Occupations	13.27 (161.61)	-194.94 (175.87)	45.74 (131.64)	-36.94 (141.84)	205.46 (68.58)	100.02 (75.38)	-12.80 (182.26)	-164.95 (197.84)	282.30 (136.59)	125.53 (150.51)
Treated × Medium × Same Occupation	237.26 (57.73)	136.65 (62.73)	257.11 (61.07)	129.94 (66.42)			197.71 (83.25)	45.91 (90.60)	223.63 (61.51)	107.10 (67.20)
Treated × Medium × Other Occupations	241.32 (71.45)	140.77 (78.64)	278.66 (82.60)	133.70 (91.12)			141.08 (102.78)	18.24 (112.03)	271.73 (83.57)	155.54 (92.09)
Treated × High × Same Occupation	-10.47 (276.58)	-108.78 (300.48)	233.94 (158.62)	246.62 (175.80)	189.16 (199.20)	241.57 (222.93)	279.09 (78.11)	213.63 (86.61)	321.97 (131.68)	219.61 (138.09)
Treated × High × Other Occupations	-521.38 (272.43)	-273.86 (301.98)	-119.70 (150.93)	-54.37 (167.89)	-136.51 (168.94)	-172.74 (185.75)	209.86 (96.28)	115.83 (107.12)	-363.93 (138.04)	-317.15 (149.62)
No. of Observations	6,807,673		6,807,673		6,807,673		6,337,990		6,807,673	
No. of Coworkers	711,969		711,969		711,969		711,969		711,969	
No. of Clusters	67,572		67,572		67,572		63,233		67,572	

Note: The table shows heterogeneity of the treatment based on the difference-in-differences framework in equation (2). 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. 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 tenure are categorized as 1 to 5 years (low), 5 to 10 years (medium), and greater than 10 years of tenure (high). 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. In the manager column, low refers to workers we identify as non-managers and high refers to managers. We calculate a specialization measure for the occupation of the deceased worker (returns to occupation experience) and classify workers into three groups (below 20th percentile, 20th to 80th percentile, above 80th percentile). We also report heterogeneity of effects on hiring and employment in Table A-4.13. Standard errors are clustered at the firm level.

Source: Own calculations.

Table 6: Heterogeneity of Wage Effects and External Labor Market Characteristics

Outcome: Wages of Incumbent Workers in Same Occupation Group as Deceased Co-Worker Sample:	All Worker Deaths		Worker Deaths in High Specialization Occupations		Worker Deaths in Low Specialization Occupations	
	Short-Run Effect (1)	Long-Run Effect (2)	Short-Run Effect (3)	Long-Run Effect (4)	Short-Run Effect (5)	Long-Run Effect (6)
<i>(A) Thickness Measured at Occupation Level</i>						
Treated × Low Thickness (Occupation)	317.96 (73.93)	199.08 (80.59)	439.78 (107.06)	225.46 (113.99)	221.79 (101.75)	183.14 (112.62)
Treated × High Thickness (Occupation)	161.44 (72.13)	97.66 (78.45)	111.25 (105.60)	29.84 (113.74)	196.95 (98.15)	143.87 (107.18)
<i>(B) Density of Local Labor Market</i>						
Treated × Low Density	293.33 (71.03)	216.64 (77.88)	358.69 (102.59)	204.80 (111.12)	242.38 (97.89)	225.94 (108.12)
Treated × High Density	184.95 (74.98)	78.55 (81.12)	197.37 (110.21)	53.71 (116.75)	176.89 (101.87)	101.21 (111.46)
<i>(C) Thickness Measured at Industry Level</i>						
Treated × Low Thickness (Industry)	279.25 (74.06)	250.78 (80.52)	342.06 (105.59)	208.83 (112.88)	228.33 (103.24)	286.74 (113.45)
Treated × High Thickness (Industry)	204.16 (71.91)	57.17 (78.09)	217.05 (106.89)	59.28 (113.80)	195.46 (96.75)	57.13 (106.11)
<i>(D) Local Unemployment Rate</i>						
Treated × Low Unemployment	181.64 (82.69)	122.69 (90.56)	170.01 (121.35)	119.64 (129.71)	190.66 (112.76)	123.99 (125.42)
Treated × High Unemployment	274.17 (78.78)	168.69 (86.45)	318.71 (112.92)	170.83 (122.38)	240.19 (108.87)	166.13 (120.25)
N	4248678		1689241		2559437	

Note: The table shows heterogeneity of the treatment effect based on the difference-in-differences framework in equation (2). 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 $1(\text{period}_t)$ 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/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 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.

Source: Own calculations.

Table 7: Estimation of Model Parameters and Implied Replacement Costs

	Short-Run Estimation	Long-Run Estimation	Thick Labor Markets (Short-Run)	Thin Labor Markets (Short-Run)
γ	76054	97944	45692	115518
λ	0.09	0.09	0.07	0.10
η	0.20	0.15	0.35	0.13
\bar{w}	45448	56310	35307	64726
ϵ	1.33	1.31	1.87	1.01
P^0	1147740	1394109	297754	61400900
Marginal Replacement Cost ($c'(\frac{N}{L})$)	65449	84203	40402	127148
(Expressed as % of incumbent salary)	(232%)	(299%)	(146%)	(455%)
Retention Elasticity Reduced Form	0.621		1.111	0.391

Note: The first column is estimated to match the wage, retention, and employment responses in the first year after a worker death. The second column matches the entire path of responses over a five-year horizon; see Section 5 for more information. Columns (3) and (4) split the sample by labor market thickness and report replacement costs for both specifications separately. See Section 5 for additional information.

Source: Own calculations.

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