How Substitutable Are Workers? Evidence from Worker Deaths*

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This Version: June 2024

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 as positive wage effects are concentrated among coworkers in the same occupation as the deceased. We quantify the deviation from a frictionless benchmark model and structurally estimate the size of replacement costs implied by the reduced-form evidence. Our estimates imply large costs of replacing incumbent workers and thus point to substantial frictions in the labor market, especially in thin markets for skill.

* The authors are grateful for many comments from seminar and conference audiences and colleagues and especially thank Raj Chetty, Edward Glaeser, Lawrence Katz, and Andrei Shleifer. This study uses the factually anonymous data sampled from the Integrated Employment Biographies (IEB) database of the Institute for Employment Research (IAB). Data access is provided by IAB in accordance with the regulations of section 75 of the German Social Code, Book III . We are very grateful to Stefan Bender for his support in making this project possible and to the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at IAB, Peter Brown, Daniela Hochfellner, and Wolfram Klosterhuber for their support with the data. Carolin Baum, Nikhil Basavappa, David Poth, and especially William Chen and Chantal Pezold provided excellent research assistance.

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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, and 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

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

a worker in treatment group firms, suggesting that 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 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% 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 €4,241 (2010 CPI), corresponding to about 13% 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 in the years after the coworker death. We investigate heterogeneity across occupations, skill groups, and local labor markets. We find that 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. 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.

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. Incumbent workers in the model are particularly valuable to the firm because they are hard to replace, e.g., due to hiring and training costs. The share of this value that they capture is determined by their outside options, posted wages at other firms drawn from an exogenous distribution. If markets were frictionless, firms would respond to a worker exit by increasing hiring, with no effect on incumbents. But because hiring is costly, incumbents become more valuable and therefore firms offer higher wages to retain them. We estimate the model with method of 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, ranging from 65% to 278% of 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 and Zwiebel, 1996a,b; Cahuc, Marque, and 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 and Piore, 1971; Baker, Gibbs, and 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. We find larger effects in constellations where the deceased ranked above the incumbent, but also estimate positive, though not statistically significant wage effects in the reverse constellation. We conclude that our results are broadly consistent with internal labor market models, but are harder to reconcile with models of slot constraints and vacancy chains as the only 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, which are arguably less directly related to work, 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 do not see significant shifts from part- to full-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 of 65% 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 (Hamermesh, 1996; Manning, 2011; Kuhn and Yu, 2021). 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 and von Wachter, 2011; Hall, 2011), and that larger post-match turnover costs (Silva and Toledo, 2009) or imperfect substitutability between new hires and incumbent workers (Mercan, Schoefer, and Sedláček, 2024) 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. We also show how our estimated wage increases in responses to worker deaths are evidence that replacement costs create a wedge between the marginal product and wages, contributing to a literature on monopsony power in the labor market. This is in line with Isen's 2013 findings that workers' wages are lower than their marginal revenue product, as revenue drops by more than labor costs in response to a worker death. 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 in a bargaining model (see, e.g., Kline et al., 2019, 2021). Our findings also inform the policy debate around skilled labor shortages (Sauer and Wollmershäuser, 2021; Causa et al., 2022) and highlight the costs of employee turnover and the association of market thickness with lower replacement costs. 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, and von Wachter, 2021; Bianchi et al., 2022; Boeri, Garibaldi, and 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% of employment.

Relative to the OECD average, Germany has a relatively high manufacturing share at 22.6% of GDP (OECD: 15.0%, US: 12.7%, 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 and 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 and 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, and Schoefer, 2023). 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, and Maier, 2011; Bispinck, Dribbusch, and Schulten, 2010; Ellguth and Kohaut, 2020). As a consequence, firms have a fair amount of scope to set wages in response to shocks such as the ones we study.

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 for our main analysis. 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 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% 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, and Schönberg, 2009; Card, Heining, and 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 €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 €27,000, i.e., about half of the 2011 earnings maximum. In the sample we work with, 6.0% of earnings observations are censored. As our analysis focuses primarily on within-worker and within-establishment variation in wages, imputation procedures based on lagged or current individual or employer-level information would not add additional information for the analysis. We therefore do not impute earnings above the Social Security earnings maximum, and instead set wages to the earnings maximum if they are top-coded. Our analysis thus does not capture variation in wages above the earnings maximum. Excluding workers with initial earnings at or above the maximum leads to effect sizes on wages about 2% 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 and 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 would be classified as different establishments even when they are located in the same municipality. In all cases, our analysis will be conducted at a within-firm level and all coworkers will be employed by the same firm. The analysis may not capture all employment at a firm in the case of multi-establishment firms. However, for the sample that we consider, an estimated 84% of establishments correspond to single-establishment firms (Antoni, Laible, and Schild, 2015).

[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 craftsperson, 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. Hethey-Maier and Schmieder (2013) use a worker flow methodology to document that only about 35 to 40% of new or disappearing establishment IDs in the German Social Security data correspond to actual establishment entries or exits. Due to the uncertainty surrounding the continued operation of an establishment when the establishment ID disappeared, 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.

In keeping with convention (Dustmann, Ludsteck, and Schönberg, 2009; Card, Heining, and 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% 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% 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% 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

[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.

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, and 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.

Matched Sampling to Select Comparison Group. We implement a matched sampling procedure separately for each event year d. For each deceased worker-firm pair in the treatment group, we select a worker-firm pair from the comparison group pool with similar lagged characteristics. This approach is motivated by Rosenbaum and Rubin (1985) and Imbens and Rubin (2015, chapter 15), who describe how matched sampling can be used to find a comparison group of similar size and with similar observed characteristics as the treatment group and follows the precedent in the literature (Azoulay, Wang, and 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 7.6 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 95.91% of worker-firm pairs in the treatment group. When no exact match can be found, i.e., in the remaining 4.09% 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 sample to employers with between 3 and 30 full-time employees four years before death, which comprise 30.6% 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

[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.

[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*).

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 2 report summary statistics for the 33,983 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 46 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 €31,753 in the treatment and €31,818 in the comparison group, respectively. Both groups of workers have an average tenure of about 9 years at the firm. The similarity between actual and placebo deceased workers is not a mechanical effect of the matched sampling, as the matching relied on variables in $k=-4$.

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 2 report summary statistics for these incumbent workers who are slightly younger than the actual and placebo deceased workers with an average age of 38 and are more likely to be female (26%). Incumbent workers have average earnings in $k=-1$ of about €28,000 (€27,999 in the treatment, €27,933 in the comparison group), an average level of education of 10.9 years, and have about 7 years of tenure with

[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-3.3 in the Appendix, we also report results for two additional groups of incumbents: the sample of part-time coworkers and individuals who were apprentices.

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 3 in the year before the worker death. The average establishment in the treatment group has 14.32 employees (14.38 in the comparison group), of which about 16% are new employees, and has been observed in the data for about 14.8 years. About 3% of firms are in the primary sector (agriculture, mining), 50% in the secondary sector (manufacturing), and 47% in the tertiary sector (services). Since we do not match exactly on industry, occupation of the deceased, and the location of the firm, a potential concern could be that there is substantial imbalance in these dimensions. We assess this concern by regressing treatment status on industry fixed effects (3 digit), fixed effects for the occupation of the deceased (5 digit), and labor market region fixed effects (50 regions based on Kropp and Schwengler, 2011) and find that these variables are jointly insignificant in predicting treatment status in our sample ($p = 0.175$).

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:

$$y_{jdt} - y_{jd,d-1} = \sum_{\substack{k=-3 \\ k\neq -1}}^{5} \delta_k \times \mathbb{1}(t - d_{jd} = k) + \sum_{\substack{k=-3 \\ k\neq -1}}^{5} \beta_k \times \mathbb{1}(t - d_{jd} = k) \times \text{Treated}_{jd} + \varepsilon_{jdt}, \quad (1)$$

where y_{jdt} denotes the outcome y for firm j observed in year t in which the worker death (or matched placebo death) occurring in year d. The model includes fixed effects for relative time, δ_k.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. Treated_{jd} is an indicator function for treatment status, that is, whether firm j actually experienced a death in year d or was chosen as a matched control. The coefficients of interest, β_k, capture the effect of an actual worker death in year $k = t - d$ in the treatment group and are normalized to zero in $k = -1$ given the difference specification. We define the *short-run treatment effect* as the effect in the first post-death year, β_1, and a *long-run treatment effect* as a pooled coefficient $\beta 1 \leq k \leq 5$ (keeping individual year relative time fixed effects δ_k).

Due to the stacked nature of our difference-in-differences design with a perfectly balanced panel (similar to, e.g., Cengiz et al., 2019), our research design does not suffer from recent problems that the literature on two-way fixed effects with staggered treatment and heterogeneous treatment effects has pointed out (see, e.g., De Chaisemartin and d'Haultfoeuille, 2020; Sun and Abraham, 2021; Goodman-Bacon, 2021). Our main specifications have perfect balance in terms of the number of treated and control units. Moreover, when we run heterogeneity analyses, we reweight observations to maintain balance in the weights on treated units and their matched controls, which addresses remaining concerns with the stacked difference-in-differences design (Gardner, 2022; Wing, Freedman, and Hollingsworth, 2024).[9]

We cluster standard errors at the matched-pair level, as suggested by Abadie and Spiess (2022) to account for correlated shocks within observables used in the matching.[10] We have also explored clustering at the firm level to address potential concerns of serial correlation of outcomes across firms experiencing multiple deaths (Bertrand, Duflo, and Mullainathan, 2004). This leads to 0.1% larger standard errors than clustering at the matched-pair level (the matched-pair level includes one treated and one control firm for a fixed year of death). The difference is so small because there are 63,926 unique firms among the 71,966 firms in the sample, and some of the firms that appear multiple times appear quite far apart in the 27-year sample period.

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 being retained could itself be affected by a worker death.

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

[9]We drop firms whose matched pair has, e.g., no workers in the same occupation when we do heterogeneity by occupation. For the 10% of the sample where there are multiple treated and control firms that match on all the characteristics, if we drop one of two treated firms for having no workers in the same occupation, we downweight the two control firms by 50% to retain year balance.

[10]If we drop a firm from a match with more than two firms while doing heterogeneity analysis, as described in the previous footnote, we then cluster at the level of the match.

$$y_{ijdt} - y_{ijd,d-1} = \sum_{\substack{k=-3 \\ k \neq -1}}^{5} \delta_k \times \mathbb{1}(t - d_{jd} = k) + \sum_{\substack{k=-3 \\ k \neq -1}}^{5} \beta_k \times \mathbb{1}(t - d_{jd} = k) \times \text{Treated}_{jd} + \varepsilon_{ijdt}, \quad (2)$$

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 similarly estimates things in differences to absorb unobserved heterogeneity across incumbent workers.[11] As before, the model includes leads and lags around event time, $\mathbb{1}(\text{period}_k)$, and the coefficients of interest are the β_k. 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 matched pair level. Short-run and average long-run treatment effects are also defined analogously as β_1 and $\sum_{k=1}^{5} \beta_k$, 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

[11] A similar specification would be to include worker × year of death fixed effects, γ_{jd} and estimate the regression with the level of outcome variables on the left-hand side instead of in differences. This would give the exact same point estimates and standard errors with a balanced panel, so estimating the "differences-in-differences" specification with fixed effects is standard. For the establishment-level outcomes, we have a perfectly balanced panel and estimates with establishment fixed effects are identical to the differences specification. For the incumbent worker outcomes, however, because we restrict the sample to individuals aged between 25 and 64 in a given year, the panel is unbalanced, and the worker fixed effects approach is thus inconsistent. Even with the unbalanced panel, the differences specification still estimates the ATT for the coworkers that are working age at the time.

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).[12]

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-3.1 reveals that firms in the treatment and comparison group have an identical probability (about 1.2%) of experiencing a worker death in a given future period, as the indicator for treatment status is statistically insignificant 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.

In the heterogeneity analyses, we further keep only matches that maintain within-match balance on the relevant heterogeneity dimension. For example, when focusing on manager vs. non-manager deaths, we only keep matches where both the deceased and the placebo deceased worker were managers or non-managers, respectively. When doing heterogeneity by characteristics that vary among workers within a firm (for instance occupation), we reweight observations so that the weight on coworkers in, for instance, the same occupation, sum to one within a firm and keep only matched pairs where both the treatment and control firm had a positive number of workers in the same or the other occupation. We therefore run the occupation heterogeneity splits as separate regressions.

[12] 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."

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 1a 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 -0.87 (SE 0.003) in the first post-death period and is equal to -0.566 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 1a 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 -0.34 (SE 0.034) workers in the first period after death and remains at -0.12 (SE 0.04) fewer workers in the second period. 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 1b 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 0.40 (SE 0.03) new workers and an additional 0.23 and 0.08 workers in the subsequent two periods. Figure A-3.1 decomposes the hiring effect into two components: the hiring of workers who work in the same 1-digit occupation as the deceased and hiring of workers in other occupations. Only about 11% of the hiring response to worker exits is due to hiring in other occupations (the average deceased worker has 33% of their colleagues in other 1-digit occupations, for comparison).

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-3.2). The upper panel uses individual incumbent workers' labor earnings as the outcome variable and documents a statistically significant increase of €186.53 (SE 35.78) in the first post-death period, $k = 1$. Compared to incumbent workers' average yearly earnings of €27,999 in $k = -1$, this corresponds to a real increase of about 0.66%. Wages of incumbent workers in the treatment group stay elevated for several years and remain statistically significant up to the third post-death period, $k = 3$.

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 regardless of work hours as the outcome variable. On average, the sum of coworker earnings increases by €1670.79 (SE 391.17) 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 €4241 so that the increase in incumbent worker earnings corresponds to about 13% of a deceased worker's average annual earnings (€31,753 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-3.2, 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.

As an additional robustness check, we also separately investigate effects on workers who stayed in the establishment and on those who left (see Appendix Table A-3.17). We find that positive wage effects are concentrated among the stayers only and, in this group, remain statistically significant and positive at about €200 in all post-event periods we consider. In contrast, we find negative effects on workers who left the firm. As a caveat to the interpretation of the robustness check, we note that the decision to stay or leave a firm is endogenous, e.g., to idiosyncratic productivity shocks.

In Table 4, 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 0.43 percentage point (SE 0.12 ppt) higher probability of remaining employed at the same firm. Incumbents in the treatment group are, however, also likely to be employed anywhere in the short run. However, the long-run effect on full-time employment is zero while the probability of staying employed at the same establishment remains positive, stable, and statistically significant.

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 a reduction in the share of workers working part-time suggests that the intensive margin hours response may be limited. 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-3.3, though effects for these smaller subsamples are less precise. For part-time incumbents, we find statistically insignificant, negative effects. Wage earnings decrease by €52.61 (SE 117.27) in the short run, and we find similar long-term declines (€170.80, SE 128.43). We find no effects on the probability to remain employed at the same establishment or to switch into full-time employment. These results further suggest that hours margins may not drive our overall results as part-time workers may have more scope to adjust hours upwards compared to full-time workers (which constitute our main sample).

For apprentices, we find positive but statistically not significant increases in earnings of around €197 per year as well as a statistically significant increase of 0.3ppt 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-3.4, we document a large increase in new hires' wages of €427 in the first year. 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-3.5, 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. In Column (2) of Appendix Table A-3.4, we residualize new hires' wages, taking out the component explained by observable characteristics like age, education, and occupation, and still find a substantial increase in new hires' wages.

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 €224.10 in the short run and €140.30 in the long run (see Table 5). In contrast, the average effect on wages of workers in other occupation groups is about 32% smaller and only statistically significant in the short run. There is no statistically detectable effect on the retention of workers in other occupations.

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 (a) and Table 6. 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 effects in the short run of worker deaths at medium and low levels of education and (€207.50, SE 39.57, and €179.72, SE 89.49, respectively), and negative though not statistically significant effects of worker deaths in the high education group (€-84.03, SE 181.50). In the long run, effects of worker deaths in the medium skill group are the largest (€129.22, SE 43.71), while effects in the other two education groups are no longer statistically significant. For low- and high-education deaths, we find negative effects on workers in other occupations.

We next analyze heterogeneity based on the skill intensity of the deceased's occupation and report qualitatively similar results in Figure 4 Panel (b) and Table 6. 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% 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 again find positive wage effects in the short run for deaths in the two lower-skill groups and negative effects of deaths in the highest skill group. In the long run, only deaths in the medium-skill group are associated with statistically significant, positive wage effects. We again find negative effects of deaths in the lowest and highest skill group on wages of workers in occupations other than the deceased's.

Managerial Status As another dimension of skill, we explore heterogeneity in the deceased worker's managerial status (see Figure 4 Panel (c) and Table 6). 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.[13] 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. In contrast, manager deaths lead to sizeable negative effects on incumbent workers, in particular in other occupations. However, we note that confidence intervals are wide due to the smaller number of observations.

[13] 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.

Tenure We investigate treatment effect heterogeneity by tenure of the deceased worker. Columns (7) and (8) of Table 6 reports 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). In the short run, worker deaths in all tenure groups are associated with positive wage effects, though effects are only statistically significant in the medium- and long-tenure group. In the long run, only the effect of worker deaths in the long tenure category is statistically significant.

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 and 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 6 report treatment effects on incumbent worker wages by occupational specialization of the deceased worker. In the short run, we find small, negative, and not statistically significant effects in the low-tenure group and larger and significant treatment effects around €200 in the medium- and high-tenure group. In the long run, only deaths in the medium-tenure group are associated with positive, statistically significant wage effects.

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 cannot 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 density. To proxy for labor market thickness, we measure the relative agglomeration of workers in the deceased's occupation (or industry) in the local labor market. To delineate local labor markets, we focus on 50 commuting zones (CZs), following Kropp and Schwengler (2011). We measure thickness at the 5-digit occupation (or 3-digit industry) × CZ level as the share of employment in the relevant occupation in that CZ relative to the nationwide share of employment in that occupation.[14] We then classify 5-digit occupation × CZ or 3-digit industry × CZ 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 larger short-run effects of worker deaths in thin labor markets on the wages of incumbents in the same occupation as the deceased. We report results in Table 7 (and for all coworkers in Appendix Table A-3.6); Panel A reports results for the occupational thickness definition, Panel B for the industry-based thickness definition. In the occupational thickness heterogeneity analysis, short-run effects on incumbent wages are €186.79 (SE 102.24) in low-thickness CZs and €121.65 (SE 93.99) in high-thickness CZs. For the industry-based thickness heterogeneity analysis, we find a short-run effect of €247.28 (SE 98.63) in low-thickness CZ and of €217.67 (SE 89.75) in high-thickness CZs. However, we note that differences are not statistically significant and less pronounced in the longer run.

If the differences in estimates were 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 (see Columns (3) though (6)). We find no strong evidence in favor of this prediction as heterogeneity between high- and low-thickness CZs appears broadly similar for deaths in high- and low-specialization occupations.

As an additional measure of thickness, we consider employment density (Panel (C) of Table 7). We find larger wage effects in less dense CZs (€249.84, SE 92.23) compared to denser CZ (€105.44, SE 105.26).

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. The analysis reveals larger differences between

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

thin and thick labor markets when focusing high specialization occupations, though standard errors are wide and do not reject equality of effects across labor markets.

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 7, illustrates, short-run 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 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 and 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-3.7 and Appendix Table A-3.8). We find mixed results for heterogeneity of hiring effects across local labor markets.

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 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 $j \in \{1, ..., J\}$ 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:

$$G(\omega) = \left(\frac{\omega - w^m}{\overline{w} - w^m}\right)^\eta, \qquad \omega \in [w^m, \overline{w}]. \tag{3}$$

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 $w^m = w^m$. 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:

$$L = G(w^I)I + N, \tag{4}$$

and total labor costs are:

$$c\left(\frac{N}{I}\right)I + w^m N + w^I G(w^I)I. \tag{5}$$

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:

$$\Pi(w^I, N, I) = P(L)L - c\left(\frac{N}{I}\right)I - w^m N - w^I G(w^I)I. \tag{6}$$

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

$$MRP = w^I + \frac{w^I - w^m}{\eta}, \tag{7}$$

$$MRP = w^m + c'\left(\frac{N}{I}\right), \tag{8}$$

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:

$$w^I - w^m = \frac{\eta}{1+\eta}c'\left(\frac{N}{I}\right). \tag{9}$$

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 (9) also offers one way of measuring replacement costs $c'(\frac{N}{I})$ 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:

$$\frac{dw^I}{dI} = \frac{\eta}{1+\eta} \frac{\frac{dN}{dI} - \frac{N}{I}}{I} c''\left(\frac{N}{I}\right). \tag{10}$$

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 respond 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).[15]

Rearranging equation (10) also demonstrates how the empirical moments we identify in the data identify replacement costs:

$$c''\left(\frac{N}{I}\right) = \frac{1+\eta}{\eta} \frac{dw^I}{dI} \frac{I}{\frac{dN}{dI} - \frac{N}{I}}, \tag{11}$$

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 ($\eta = \frac{d \log G(w^I)}{d \log w^I} \frac{w^I - w^m}{w^I}$).

Dynamics. We then 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:

$$V(I_t) = \max_{w_t^I, N_t} \Pi(w_t^I, N_t, I_t) + \beta V(I_{,t+1}) \quad \text{s.t.} \quad I_{t+1} = G(w_t^I) I_t + N_t. \tag{12}$$

The only change to the FOCs (7) and (8) is the addition of $\beta V'(I_{t+1})$ on the LHS. Note that the firm is forward-looking in our extension to the dynamic case while workers are not.[16]

[15] In a typical monopsony model, firms face an upward sloping labor supply curve (that does not distinguish between incumbents and new hires) and therefore have to raise wages for incumbents and new hires to increase employment (see Sokolova and Sorensen, 2021, for a survey of recruitment elasticity estimates). In the model we consider, there is no margin for adjustment of new hire wages, but the cost of new hire labor to the firm is still upward sloping if c is convex. This certainly misses a dimension of realism, where firms can also increase recruitment by offering higher wages, something we find suggestive evidence for in Table A-3.4. The hiring costs in the model will capture all of the costs of increasing hiring, whether they take the form of increased search effort from firms or increased new hire wages to speed recruitment.

[16] Workers do not take potential future rents into account and will not accept jobs with wages below w^m in the first period (e.g., because w^m constitutes a reference point and wages below it are viewed as unfair, similar to minimum wages in Falk, Fehr, and Zehnder, 2006). Relatedly, contracts with entry-level wages below w^m may not be incentive compatible as firms would have an incentive to fire workers after the first period (see Daruich, Di Addario, and Saggio, 2023, for related evidence).

Extensions with New Hires Probabilistically Becoming Incumbents and Multiple Worker Types. The baseline model assumes that, after hiring costs are paid, new hires and incumbents are homogeneous in production and all workers are perfect substitutes in production within the firm, though incumbents are imperfectly substitutable with outsiders. We extend the model in two ways that more realistically describe worker heterogeneity. First, we add a hazard rate $\delta < 1$ with which newly hired workers become incumbents (where our benchmark would have $\delta = 1$, see Appendix A-1.4). This increased propensity of new workers to exit captures a variety of reasons why new hires are more likely to exit than longer tenured workers, such as the match quality being lower or the match surplus being lower because new hires have less firm-specific human capital (Jovanovic, 1979a,b).

Second, we incorporate two types of workers to shed light on substitutability of workers within the firm. In this case, production is a constant-elasticity-of-substitution (CES) aggregate of the labor of the two types, with elasticity of substitution ρ. Production then takes the following form, with type-specific productivities A_s, for workers in the same occupation as the deceased,[17] and A_o for workers in other occupations:

$$\begin{aligned} Q_j &= (\alpha(A_s L_s)^\rho + (1-\alpha)(A_w L_w)^\rho)^{\frac{1}{\rho}}, \\ L_k &= G(w_k^I) I_k + N_k,\ k \in s, w, \end{aligned} \tag{13}$$

with demand and hiring as before. The firm has a two-dimensional value function over the numbers of incumbent managers and workers, respectively. We can then estimate the elasticity of substitution implied by the empirical results. If we find $\rho < \frac{\epsilon-1}{\epsilon}$, wages will fall for a worker of one type if a worker of the other type exits. See Section A-1.4 for derivations.

5.2 Model Identification and Estimation

Identification We adopt the following functional form for hiring costs:

$$c\left(\frac{N}{I}\right) = \frac{\gamma}{1+\lambda}\left(\frac{N}{I}\right)^{1+\lambda}. \tag{14}$$

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

Given (14), the model has eight parameters: γ, λ, w^m, η, $\overline{w}$, P^0, ϵ, β. The first six parameters are estimated, and we set the remaining two. Because each period represents one year, we set $\beta = 0.96$ to target a 4% discount rate. Finally, we set w^m to the sample average earnings for new hires.

To identify the parameters, we target the retention, hiring, and earnings response one

[17] We normalize A_s to 1 and α, because A_s, A_o, α and P^0 are not all identified.

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 14.55 incumbents, retains 82.7% of its incumbents, and pays its incumbents €31661. We set the number of hires to 2.51 new workers to ensure that a firm with 14.55 incumbents and an 82.7% retention rate is in a steady state.[18] Our reduced-form results indicated a 0.43 percentage point increase in retention, 0.4 additional hires, and a €186.53 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 83.2% of its incumbents, pay them €31848, and hire 2.91 new workers. These moments identify the model parameters because they imply a system of six equations in six unknowns given by:

$$\frac{\epsilon-1}{\epsilon}P^0 L_0^{-1/\epsilon} + \beta V'(I_0) = w_0^I + \frac{w_0^I - w^m}{\eta}, \tag{15}$$

$$\frac{\epsilon-1}{\epsilon}P^0 L_0^{-1/\epsilon} + \beta V'(I_0) = w^m + \gamma\left(\frac{N_0}{I_0}\right)^\lambda, \tag{16}$$

$$\left(\frac{w_0^I - w^m}{\overline{w} - w^m}\right)^\eta = G_0, \tag{17}$$

$$\frac{\epsilon-1}{\epsilon}P^0 L_1^{-1/\epsilon} + \beta V'(I_1) = w_1^I + \frac{w_1^I - w^m}{\eta}, \tag{18}$$

$$\frac{\epsilon-1}{\epsilon}P^0 L_1^{-1/\epsilon} + \beta V'(I_1) = w^m + \gamma\left(\frac{N_1}{I_1}\right)^\lambda, \tag{19}$$

$$\left(\frac{w_1^I - w^m}{\overline{w} - w^m}\right)^\eta = G_1, \tag{20}$$

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

Table 1: Model Targets

	I_k	L_k	N_k	G_k	w_k^I
$k = 0$	14.55	14.55	2.51	0.827	31661
$k = 1$	13.55	14.18	2.91	0.832	31848

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.[19]

[18]This is also close to matching the sample mean of hires, 2.26, though mean hires are not exactly equal to the level of hiring consistent with the mean retention rate because mean hiring gives more weight to large firms with large absolute levels of hiring.

[19]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

We estimate the model using the method of 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 8. 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 the cost of hiring if $N = I$, so the hiring cost per worker if a firm wished to double its level of employment. Values ranging from €67,000 to €97,000. Second, we find moderate convexity of hiring costs, with λ being 0.06. 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 values for η, the elasticity of incumbent retention to the incumbent wage premium, ranging between 0.3 and 0.4. 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 and Sorensen, 2021).

Our preferred specification is Column (1) of Table 8, with ϵ fixed and equal to 2. Column (3) shows that if we fix w^m, setting it to the average wage of workers in their first three years at the firm (the definition of new hire wages used in Kline et al. (2019)), we get economically meaningless values for product demand, with $\epsilon < 1$. Product market markups are given by $\frac{\epsilon}{\epsilon-1}$; as ϵ approaches 1, product demand becomes perfectly inelastic. What we find is that the model implied w^m is decreasing in ϵ. If we were to fix $\epsilon = 5$, which would be consistent with estimates of markups in Germany over this period from De Loecker and Eeckhout (2018), we find $w^m = -63,000$ and replacement costs of 440% of an incumbent's salary. The challenge here is that w^m represents the lower endpoint of the wage offer distribution as well as the new

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-3.2, and Table A-3.9 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 the reduced-form results, which measure the wages coworkers receive, may underestimate the increases in the wages that firms post (especially 5 years out when only 30% of workers are still at their original firm, if increases in posted wages are not passed through to workers at subsequent firms, then the effect on coworkers should be scaled up to get the change in offered wages).

hire wage, and it's not clear whether that is the right support for the incumbent wage offer distribution. For instance, it might be the case that incumbents receive no wage offers for a share of periods, and the model captures that by imagining incumbents receiving wage offers between $-63,000$ and 0. If new hire wages were $-63,000$, the only way to rationalize the level of hiring we see would be high hiring costs. If we could identify an additional parameter, it would be helpful to have a parameter governing the share of years in which incumbents receive outside offers, or generally to separate the offer distribution from new hire wages. We choose $\epsilon = 2$ because it gives plausible, positive values for w^m and because the intensive margin calibration, with w^m fixed to the empirical value, finds $\epsilon = 2.23$ (see Column (7) of Table 8).

As a summary measure, we calculate the implied marginal replacement cost $c'(\frac{N}{I})$ for firms in our sample and find values ranging between €46,000 and €88,000. As a benchmark, we compare these to the wages of incumbents in our worker death sample. This calculation reveals a marginal replacement cost between 1.5 and 2.8 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. Survey responses may capture explicit training costs, for instance, but firms might find replacement more costly due to other costs and delays as new workers acquire firm-specific human capital, or because it takes time to identify whether new workers are as good matches (Jovanovic, 1979a,b). We find larger replacement costs compared to typical adjustment costs from the literature on employment adjustment (see, e.g., Hamermesh and Pfann, 1996; Bond and Van Reenen, 2007; Bloom, 2009). A potential explanation is that we study unexpected, discrete shocks to incumbent employment in relatively small firms facing convex replacement costs,[20] and Appendix Table A-3.18 shows that the effects are strongest at the smallest firms. 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. Our results also accord with results in Isen (2013) who studies wage bill and revenue effects in response to worker deaths in the United States, concluding that wages are marked down by 15 to 25 percent relative to workers' marginal product due to search frictions and human capital specificity.

[20]Several papers in that literature provide evidence in line with non-convex adjustment costs (Bond and Van Reenen, 2007; Bloom, 2009). Our estimation points to convexity as indicated by estimates of $\lambda > 0$. As a potential caveat, we note that our structural model builds on average hiring and wage effects of worker deaths and thus does not detect potential lumpiness of hiring responses across firms.

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 €534 more to each incumbent and get 0.022 more worker-years from each worker from retention (in total over a period of five years). The implied expenses for retaining a full incumbent are hence equal to $534/0.022 \approx 24300$ 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. When we extend the model to have new hires be less likely to become incumbents, we can explain 25% of the gap between the model and the data. We also gauge the explanatory power of the second hypothesis, wage rigidity. 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-3.10. 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).

Extension with New Hires Probabilistically Becoming Incumbents Column (8) of Table 8 shows the results from the extension in which new hires do not become incumbents with certainty. To match the empirical observation that new hire retention is 72% and incumbent retention is 88%, we choose $\delta = 0.82$, so that 82% of new hires begin the next period as incumbents and the remainder exit. The immediate effect of this on the equilibrium is to make hiring less attractive; if the parameters were held fixed, the steady-state level of employment would fall. Given that we keep the employment target fixed, hiring costs fall to offset this. Figure A-3.3 shows the event study of wages in the baseline model and the model with $\delta = 0.82$. The predicted wage response is €81 in the second year after a worker death, compared to €57 in the baseline model, while the point estimate is €152 so 25% of the gap can be explained by greater difficulty in retaining new hires.

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 (4) and (5) of Table 8). 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 worker 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 5, we had shown that the wage and especially hiring responses are concentrated on the same occupation as the deceased (workers in the same 1-digit occupation as the deceased make up 63% of the average firm, but 86% 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 $\rho < \frac{\epsilon-1}{\epsilon}$ ($\frac{1}{2}$ given that we fix

$\epsilon = 2$), there would be a decrease. We report results in Appendix Table A-3.19. We estimate $\rho = 0.95$, which implies an elasticity of substitution between workers in different occupations of 20. 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% of workers are in the same 1-digit occupation as the deceased. On average, they receive a wage increase of €224, while the point estimate for workers in other occupations is €152. This means that $\frac{0.63*224}{0.63*224+0.37*152} = 71.5\%$ 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 expenditures 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 87% 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.[21]

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

[21] 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 88% of the hiring expenditures being on workers in the same occupation. Incorporating convex hiring costs, that share would be higher.

model). Relative to the static baseline model, the key difference is that (7) will be amended as incumbents can demand higher wages. The solution to the Nash bargaining problem is then characterized by:

$$\frac{w^I - w^m}{\eta} + w^I - MRP = \frac{\phi}{1-\phi}\frac{1}{g(w^I)I}\frac{\Pi - \underline{\Pi}}{w^I - w^m} \tag{21}$$

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-1.5. 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. As before, a key mechanism underlying the wage effects in response to a worker exit is that the marginal revenue product falls in the number of incumbents when hiring costs are convex.

Second, an alternative framework are multi-worker firm models with intrafirm bargaining (Stole and Zwiebel, 1996a,b; Cahuc and Wasmer, 2001; Acemoglu and 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 (see working paper version of this paper in Jäger and Heining, 2022). 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 and 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 and 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 several 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 Table 9). We calculate separate treatment effects for the remaining coworkers who earned more and those who earned less than the deceased worker. We do find larger wage effects in constellations where the deceased worker ranked above the remaining incumbent workers. For example, in the short run, we find that incumbents in the same occupation as the deceased get a €290 (SE 73) wage increase when the deceased ranked higher while the effects are only €104 (SE 82) for deceased workers ranking below the incumbent. However, in the long run, wage effects are more similar regardless of whether the deceased had ranked below or above the incumbent worker (€98, SE 92, and €141, SE 77, respectively). Second, as a complement, we also study deaths of workers by whether they are in the top quartile of the firm's wage distribution (Columns (3) and (4) of Table 9). Focusing on effects on incumbents in the same occupation group, we find larger effects for high-wage earners (€512, SE 165), though effects of workers ranking below the top quartile are also substantial (€195, SE 63). Overall, the pattern of results does not reject the slot constraints view of internal labor markets, though the point estimates for wage effects on incumbents when the deceased ranked below them or was not in the top quartile of earnings provide evidence for wage adjustments not mediated by promotions.

In addition, 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 (Panel D of Table 9).[22] 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

[22]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.

Table 9). 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. In Appendix Table A-3.3 we find increases in occupational mean wages of €63 (SE 31), three 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, and 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. However, if promotions take place within five-digit occupation codes, or changes in occupation due to promotion are not recorded in the administrative data, it is possible that the true explanatory power of promotions is greater.

Overall, we conclude that our evidence documents that internal labor markets are important and firms draw differently on internal workers than on external workers, which is consistent with our main findings that external workers are imperfect substitutes for incumbents. We find some evidence supporting a slot constraints view, but also results less in line with such a perspective. We also find smaller, or even negative, baseline effects of deaths of high-skilled workers 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, for instance 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, and Holmstrom, 1994a, who document that "job levels are important to compensation, but there is also substantial individual variation in pay within levels").

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 and 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 5), 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 weekdays, 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-3.11). 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 and Trenkle, forthcoming; Dustmann et al., 2022). We separately analyze effects for this period and report results in Appendix A-2. We find no average effects on hours worked, even though effects on earnings are positive and sizeable (€95.41, SE 122.00). While we find no average 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 addition, confidence intervals do not allow us to reject that the hours response could account for the wage effects (as wage effects in the sample of workers not missing hours are statistically not significant).

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-1.6 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).[23] We report results in Table A-3.19 and Figures 6 and A.1 through A.3. Our point estimates from this extension suggest that if hours were held constant, we would see an €86.73 increase in annual wages instead of the €186.53 increase we observe. As Figure 6 shows, given the fixed hours elasticity we take from Chetty et al. (2013), there is relatively little uncertainty across bootstrap draws in the magnitude of hours adjustment, and most of the uncertainty, when bootstrap draws offer higher or lower point estimates for wage adjustments, is in terms of hourly wages. However, the hourly wage effects are still statistically significant in the model. The estimates also point to substantial replacement costs, but those costs fall to less than one annual salary for an incumbent (65%). Thus, once we incorporate an hours margin in our model, estimated replacement costs are smaller than in our main specification but still indicative of large costs of replacing workers. We similarly posit that if there were other omitted variables, besides increased working hours, that were leading our empirical results to overestimate the extent of the change in realized wages, e.g., compensating differentials that we discussed in the previous section, then we would likely also overestimate replacement costs.

Another reason that the observed wage effects likely correspond to increases in hourly wages is that we see positive retention effects. Full-time workers in Germany would mostly like to work fewer hours at their current wage than they do (see Wanger and Weber, 2023). With upwards sloping hours supply curves, if firms responded to worker deaths by increasing the hours of their workforce, this would impose a utility cost on workers and likely lead to increased exit. Additionally, as the model illustrates, firms would not entirely respond on the hours margin because paying a higher wage to increase employment through retention is valuable when workers' marginal product is higher, so long as hiring costs are not linear, making outsiders imperfect substitutes. If hiring costs were linear, firms would not need to adjust hours or wages.

[23]The studies used to estimate the intensive margin elasticity in Chetty et al. (2013) all include adjustments of both hours per day and days per year. This therefore could represent an upper bound on the intensive margin adjustment in the form of hours per day, which is all that could confound our results because we measure daily wages. The US and Scandinavian labor markets used for that estimate also may offer greater hours flexibility than the German labor market.

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 the external market for them is thin, thereby supporting specific human capital or match specificity as important correlates of replacement costs. Our evidence 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 and 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, and 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 and 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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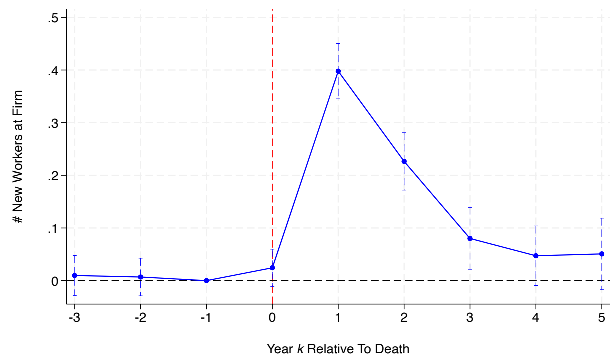
Figures

Figure 1: Effect of Worker Death on Employment and Hiring

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

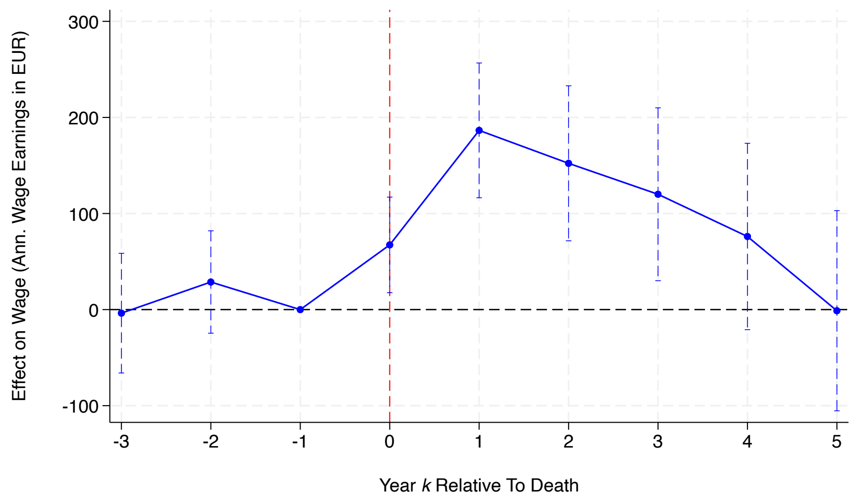
(b) Effects of Worker Deaths on Hiring



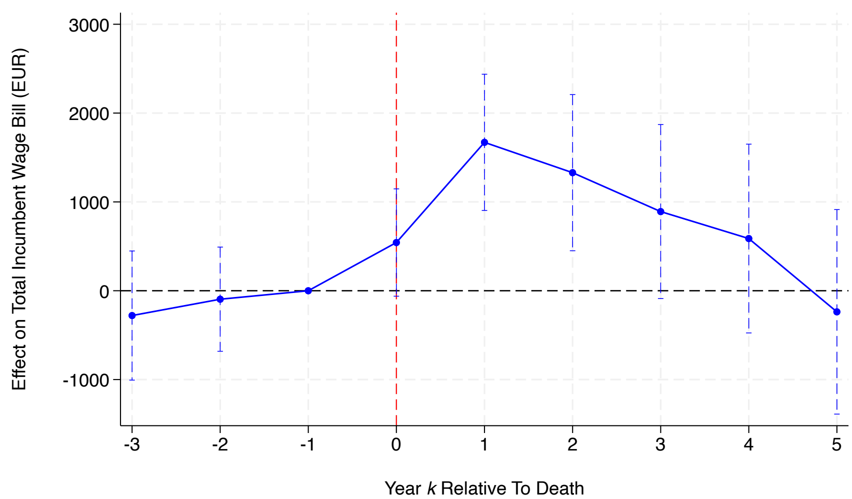
Note: The figures show regression coefficients and associated confidence intervals for the difference between treatment and comparison group in a given year k relative to the death of a worker in the treatment group firms, i.e., the β_k from the difference-in-differences model in (1). The coefficient in $k = -1$ is normalized to zero. The first outcome variable in Panel (a) measures the overall employment at a firm (full- or part-time). The comparison group mean for employment in $k = -1$ is 14.4. 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 (full- or part-time). The comparison group mean of the number of new workers in $k = -1$ is 2.3. The dashed vertical lines denote 95% confidence intervals based on standard errors are clustered at the firm level.

Figure 2: Effect of Worker Deaths on Incumbent Worker Wages

(a) Incumbent Worker Wage Earnings

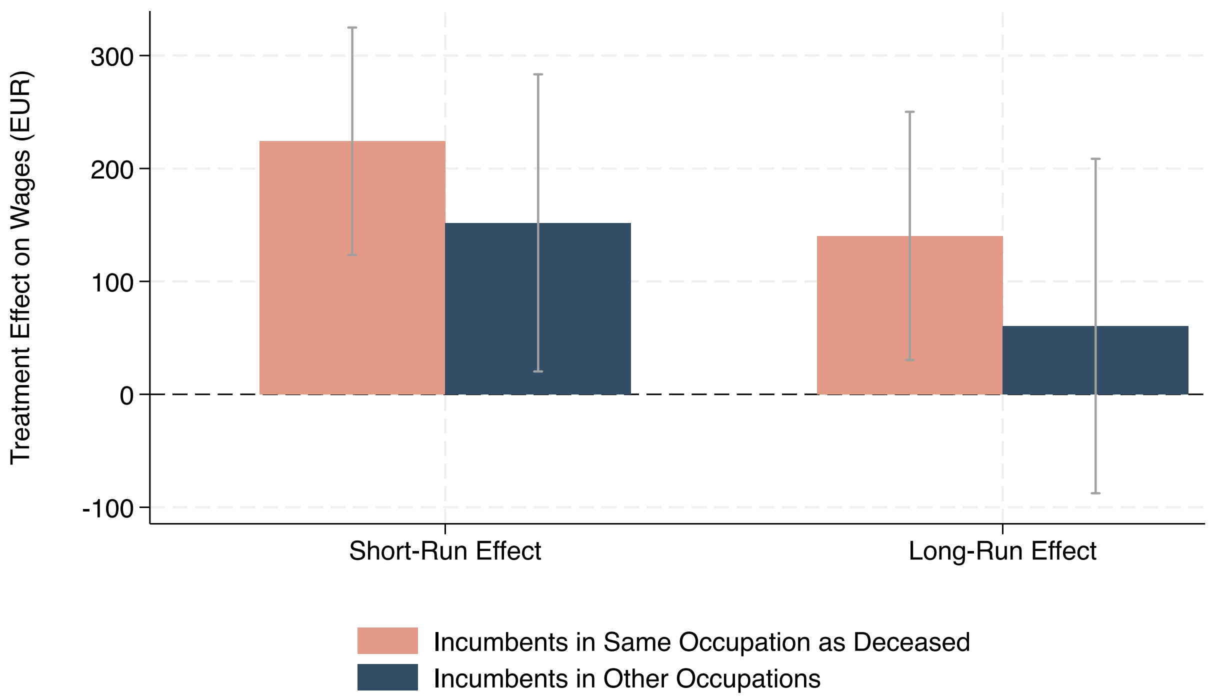


(b) Sum of Incumbent Wages in a Given Year



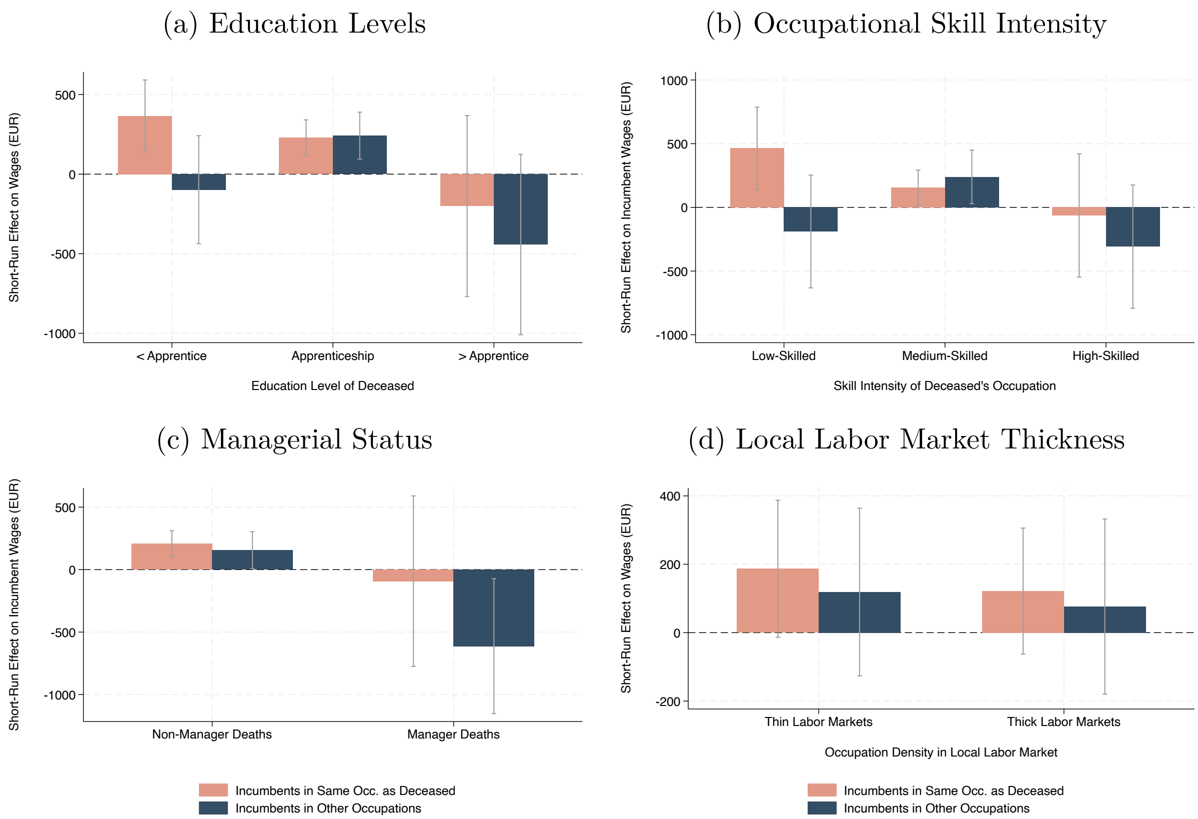
Note: The two panels display regression coefficients and associated 95% confidence intervals for the difference between incumbent worker in the treatment and comparison group, i.e., the β_k from equation (2). The coefficients in $k = -1$ are normalized to zero. In the first panel, the outcome variable is the wage of an incumbent worker (scaled to correspond to yearly earnings, CPI 2010). Incumbent workers are defined as full-time working-age 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 €31,818 so that the €186.53 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 all incumbent workers regardless of their work hours or age. The dashed vertical lines denote 95% confidence intervals based on standard errors clustered at the firm level. See Appendix Table A-3.2 for additional information.

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 6 for additional information.

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

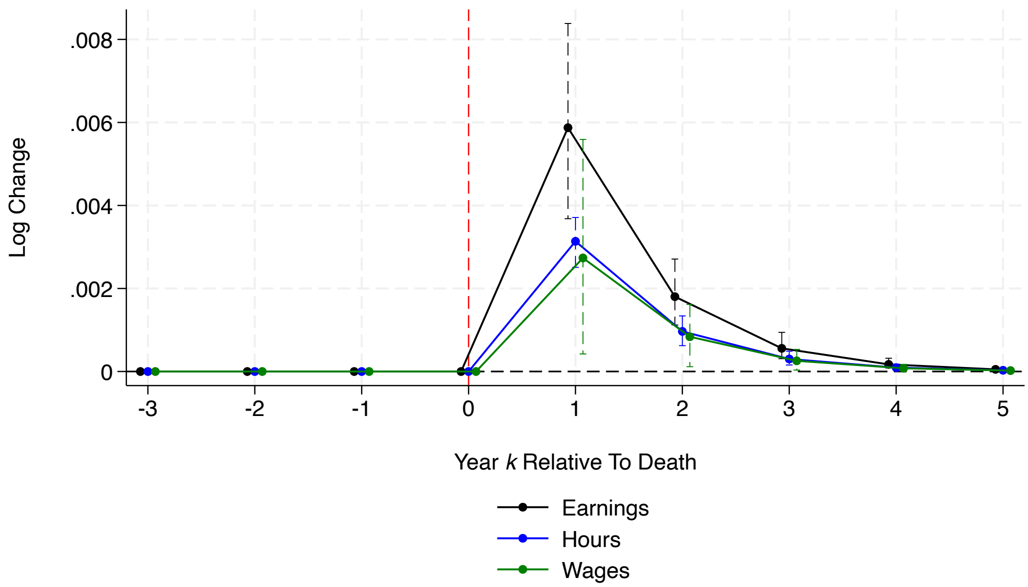


Note: The figures display short-run treatment effects of worker exits on the wages of incumbents in the same 1-digit occupation group as the deceased and, in panels (a) through (c), on incumbents in other 1-digit occupation groups for different measures of the skill level of the deceased worker. 1-digit 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 coworkers in the same occupation as the deceased; the second two bars show the effect for all coworkers. Thickness is measured at the 5-digit occupation×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×commuting zone cells are characterized as thick or thin based on a median split. In all panels, the vertical lines indicate 95% confidence intervals based on standard errors clustered at the firm level. See Tables 6 and 7 and Section 4.5 for additional information.

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 moments. See Section 5 for more details. Confidence intervals on the values from the data are clustered at the matched-pair level. Confidence intervals on the model values are 95% bootstrap confidence intervals, computed by drawing targets with replacement from the matched firm pairs, recomputing the target wage, retention and hiring moments on the bootstrap sample, and then recalibrating the model to the new targets, for 400 bootstrap draws. Wages are the model-implied coworker event study differences between a treatment firm that experienced a worker death and a control firm that remained in steady state, accounting for the fact that some coworkers exit the treated firm due to better outside offers, so the magnitude of the change in offered wages or hours by the firm is larger than the realized change following all coworkers, including those who leave.

Figure 6: Log Change in Earnings, Wages, and Hours in Response to Worker Death (Model)



Note: The figure displays the log changes in earnings, hours, and hourly wages from the model in Section A-1.6.2. The model is calibrated to match the short-run earnings response, so the evolution of annual earnings corresponds to that in Figure 5(c). The firm is able to adjust on both the hours margin, increasing the hours demands on incumbent workers, which decreases retention, and the wage margin, which increases retention. Because the model is calibrated to our short-run retention point estimate, which is positive, wages and worker utility necessarily rise. Confidence intervals are 95% bootstrap confidence intervals, computed by drawing targets with replacement from the matched firm pairs, recomputing the target wage, retention and hiring moments on the bootstrap sample, and then recalibrating the model to the new targets, for 400 bootstrap draws. Earnings, hours, and wages are the model-implied coworker event study differences between a treatment firm that experienced a worker death and a control firm that remained in steady state, accounting for the fact that some coworkers exit the treated firm due to better outside offers, so the magnitude of the change in offered wages or hours by the firm is larger than the realized change following all coworkers, including those who leave.

Tables

Table 2: Individual-Level Summary Statistics

	Actual and Placebo Deceased Workers		Incumbent Workers	
	Treatment Group	Comparison Group	Treatment Group	Comparison Group
Age	46.38	46.38	38.38	38.46
	(9.82)	(9.82)	(11.30)	(11.31)
Female	0.14	0.14	0.26	0.26
	(0.34)	(0.34)	(0.44)	(0.44)
Earnings (€, 2010 CPI)	31,753	31,818	27,999	27,933
	(12,410)	(12,523)	(13,669)	(13,707)
Years of Education	10,62	10.63	10.89	10.90
	(1.49)	(1.54)	(1.81)	(1.83)
Tenure	8.96	9.01	7.09	7.05
	(5.96)	(5.97)	(5.49)	(5.47)
N	35,983	35,983	407,626	406,697

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.

Table 3: Firm-Level Summary Statistics

	Treatment Group	Comparison Group
Total Number of Employees	14.32	14.38
	(7.39)	(7.42)
Number of New Workers	2.27	2.26
	(2.40)	(2.39)
Number Part-Time Workers	1.11	1.10
	(2.16)	(2.16)
Number Apprentices	0.82	0.86
	(1.50)	(1.55)
Firm Age	14.79	14.82
	(6.75)	(6.75)
Primary Sector	0.03	0.03
	(0.17)	(0.17)
Secondary Sector (Manufacturing)	0.50	0.50
	(0.50)	(0.50)
Tertiary Sector (Service)	0.47	0.48
	(0.50)	(0.50)
N	35,983	35,983

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

Table 4: Treatment Effect on Incumbent Worker Employment Outcomes

	Short-Run Effect	Long-Run Effect
	Outcome: Employed at Same Establishment	
Treated	0.0043	0.0044
	(0.0012)	(0.0013)
Comparison Group Mean in $k = 1$: 0.827		
	Outcome: Full-Time Employment	
Treated	0.0034	0.0010
	(0.0012)	(0.0012)
Comparison Group Mean in $k = 1$: 0.896		
	Outcome: Part-Time Employment	
Treated	0.0002	0.0005
	(0.0005)	(0.0005)
Comparison Group Mean in $k = 1$: 0.012		
No. of Observations	7,328,907	
No. of Firms	63,926	

Note: 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 $k = 0$. The first row presents results from a regression comparing the propensity of workers at treatment firms to be at same establishment in year $k = 1$ or $1 \leq k \leq 5$ to control firms (which is not a differences-in-differences specification because it doesn't compare this difference to the difference in year $k = -1$, which would be the share of coworkers who were new hires at the time of the death and not a baseline level of retention). The second and third rows display treatment effects on several employment outcomes based on difference-in-differences regressions. Treated refers to the Post $\times$ 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. 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 35,983 clusters at the matched pair level.

Table 5: 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	224.10	140.30	0.0048	0.0013	0.35	0.14	-0.39	-0.18
	(51.38)	(56.07)	(0.0017)	(0.0008)	(0.02)	(0.02)	(0.02)	(0.03)
No. of Observations	4335570		2975700		647694		647694	
No. of Firms	55430		55430		63926		63926	
Treated × Other	151.82	60.48	0.0016	0.0007	0.05	0.02	0.05	0.06
	(67.13)	(75.55)	(0.0021)	(0.0011)	(0.02)	(0.01)	(0.02)	(0.03)
No. of Observations	2355750		1604848		647694		647694	
No. of Firms	42070		42070		63926		63926	

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.

Table 6: Heterogeneity of Wage Effects By Deceased's Skill Levels and Specialization

	Outcome: Incumbent Worker Wages									
Dimension of Heterogeneity:	Education		Occupational Skill		Managerial Status		Tenure		Specialization	
(Deceased Characteristic)	Short Run	Long Run	Short Run	Long Run	Short Run	Long Run	Short Run	Long Run	Short Run	Long Run
Treated × Low	179.72	45.72	210.92	75.79	199.54	116.54	249.00	120.54	-57.39	-71.13
	(89.48)	(96.94)	(118.07)	(125.36)	(38.61)	(42.32)	(156.05)	(167.37)	(139.76)	(157.70)
Treated × Medium	207.50	129.22	206.87	123.10			194.60	82.41	197.31	112.77
	(39.65)	(43.71)	(52.77)	(57.15)			(79.82)	(87.77)	(53.40)	(58.60)
Treated × High	-84.03	-55.96	-88.72	26.49	-329.87	-169.34	155.12	132.26	201.20	-5.22
	(181.50)	(196.57)	(151.87)	(168.96)	(199.14)	(217.91)	(70.61)	(78.67)	(176.91)	(196.43)
No. of Observations	7328907		4243230		6237621		3644514		3927951	
No. of Firms	63926		39299		54962		33092		36219	
Treated × Low × Same Occupation	365.98	342.79	463.14	358.44	207.16	96.15	214.49	222.77	317.22	167.69
	(115.08)	(125.63)	(165.13)	(177.63)	(52.86)	(57.13)	(231.11)	(237.33)	(224.91)	(257.89)
Treated × Low × Other Occupations	-97.62	-298.03	-189.47	-202.90	155.87	39.87	212.47	-200.22	-281.87	-177.27
	(173.22)	(190.01)	(225.81)	(242.93)	(75.56)	(85.10)	(317.49)	(342.91)	(237.43)	(268.16)
Treated × Medium × Same Occupation	228.86	138.43	154.76	76.77			197.30	5.11	213.74	139.08
	(57.23)	(62.40)	(70.07)	(75.48)			(112.65)	(123.67)	(71.04)	(77.53)
Treated × Medium × Other Occupations	241.58	144.11	239.32	79.70			350.87	258.78	285.10	136.08
	(75.22)	(85.14)	(107.15)	(120.43)			(152.44)	(172.02)	(108.58)	(122.70)
Treated × High × Same Occupation	-200.47	-342.50	-64.14	-2.96	-92.32	-175.69	274.14	143.06	9.60	-11.20
	(290.04)	(318.64)	(246.88)	(275.87)	(348.15)	(385.70)	(100.70)	(112.15)	(280.55)	(298.61)
Treated × High × Other Occupations	-442.58	-160.35	-308.23	-7.55	-612.58	-226.81	67.28	39.92	-102.50	-487.41
	(289.08)	(321.45)	(247.06)	(282.15)	(275.39)	(315.45)	(126.06)	(145.12)	(283.05)	(322.38)
No. of Observations (Same Occupation)	4335570		2632617		3798756		2167929		2442069	
No. of Firms (Same Occupation)	55430		34646		48222		28748		31815	
No. of Observations (Other Occupation)	2355750		1269171		1928178		1169118		1165752	
No. of Firms (Other Occupation)	42070		25137		36031		21933		22822	

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-3.12. Standard errors are clustered at the firm level.

Table 7: 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	Long-Run Effect	Short-Run Effect	Long-Run Effect	Short-Run Effect	Long-Run Effect
	(1)	(2)	(3)	(4)	(5)	(6)
(A) Thickness Measured at Occupation Level						
Treated × Low Thickness (Occupation)	186.79	105.69	79.33	-62.70	199.66	117.55
	(102.24)	(110.43)	(202.30)	(213.74)	(179.08)	(197.78)
Treated × High Thickness (Occupation)	121.65	94.28	-104.44	-247.50	294.13	300.25
	(93.99)	(104.22)	(187.46)	(207.32)	(156.03)	(177.07)
No. of Observations	2330946		456138		935028	
No. of Firms	31404		7368		12002	
(B) Thickness Measured at Industry Level						
Treated × Low Thickness (Industry)	247.28	127.90	388.68	78.50	119.97	245.55
	(98.63)	(107.09)	(195.42)	(206.68)	(176.29)	(194.44)
Treated × High Thickness (Industry)	217.67	124.29	60.55	-191.91	224.89	196.28
	(89.75)	(98.38)	(180.00)	(195.18)	(149.21)	(167.70)
No. of Observations	2541375		511200		995391	
No. of Firms	34255		8383		12582	
(C) Density of Local Labor Market						
Treated × Low Density	249.84	184.05	453.27	228.02	129.95	301.20
	(92.23)	(100.71)	(180.66)	(194.66)	(163.67)	(179.81)
Treated × High Density	105.44	-21.51	206.54	-1.73	167.16	18.41
	(105.26)	(115.09)	(217.95)	(229.28)	(172.70)	(194.72)
No. of Observations	2345040		441846		937386	
No. of Firms	31273		7213		11734	
(D) Local Unemployment Rate						
Treated × Low Unemployment	259.52	177.15	378.36	62.82	348.06	277.58
	(105.02)	(115.79)	(215.00)	(230.58)	(173.14)	(195.81)
Treated × High Unemployment	290.89	124.17	162.03	-169.50	291.78	115.99
	(98.53)	(109.35)	(186.46)	(200.81)	(178.50)	(201.74)
No. of Observations	2980395		725742		1349973	
No. of Firms	29772		7002		11186	

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 $1(\text{period}_k)$ with the covariate. The sample is restricted to incumbent workers in the same 1-digit occupation group as the deceased. To calculate a specialization measure for the occupation of the deceased worker, we follow Bleakley and Lin (2012) and calculate returns to experience for each 5-digit occupation. We then use the estimated occupation-specific returns to experience to classify occupations into high- and low-specialization occupations based on a median split. All external labor market characteristics are measured at the commuting zone level based on median splits of the relevant measure. Thickness measured at the occupation level is used to categorize 5-digit occupation × commuting zone cells as thick or thin based on the relative share of workers in the 5-digit occupation in the commuting zone relative to the overall share of workers in that occupation in the labor market. Thickness measured at the industry level is defined analogously for the share of workers in the 3-digit industry × commuting zone level. Density of the local labor market refers to the number of workers in a commuting zone divided by that commuting zone's area. The unemployment rate is calculated as the number of unemployed workers in the commuting zone divided by the number of workers. Observations are weighted inversely by the number of incumbent workers at the firm of the deceased. Standard errors are clustered at the firm level.

Table 8: Estimation of Model Parameters and Implied Replacement Costs

	Short-Run Estimation	Long-Run Estimation	Short-Run Estimation (Empirical w^m value)	Thick Labor Markets (Short-Run)	Thin Labor Markets (Short-Run)	Intensive Margin (Short-Run)	Intensive Margin (Short-Run, Empirical w^m value)	Adding Hazard Rate of Becoming an Incumbent δ (Short-Run)
γ	66556	96899	71633	33742	68931	26297	28997	62415
	[43585, 125247]	[51310, 280318]	[43230, 150554]	{17257, 122281}	{29816, 318332}	[13165, 56715]	[21501, 45485]	[41487, 119707]
λ	0.0621	0.0551	0.248	0.122	0.0642	0.144	0.115	0.0852
	[0.0344, 0.112]	[0.0322, 0.1]	[0.142, 0.344]	{0.0377, 0.374}	{0.0193, 0.197}	[0.0565, 0.869]	[0.0709, 0.161]	[0.0569, 0.164]
η	0.382	0.334	0.0958	0.19	0.299	0.165	0.206	0.234
	[0.162, 0.779]	[0.146, 0.584]	[0.0455, 0.154]	{0.0369, 0.704}	{0.0714, 0.977}	[0.0265, 0.509]	[0.123, 0.293]	[0.11, 0.453]
$\overline{w}$	42268	48485	56958	38602	44465	37906	37770	39208
	[38121, 55216]	[40608, 108254]	[41626, 280756]	{35002, 178950}	{37523, 163208}	[35411, 201124]	[35361, 46362]	[37408, 54959]
w^m	15178	9627	27608	26777	17835	28772	27608	21589
	[-3647, 25677]	[-13430, 23472]	[27513, 27707]	{5827, 30543}	{-26788, 29298}	[17205, 31610]	[27514, 27707]	[10049, 27682]
P^0	304962	339272	-4757430	271512	310998	265850	212705	288766
	[280909, 378592]	[288241, 579932]	[-60007217, 4333245]	{248211, 364515}	{265294, 536785}	[244274, 300195]	[77232, 945458]	[273354, 356552]
ε	2	2	0.62	2	2	2	2.23	2
			[0.335, 1.44]				[1.36, 4.69]	
Marginal Replacement Cost ($c'(\frac{N}{I})$)	59677	87968	46377	27225	61559	20429	23702	53109
	[36270, 116877]	[43010, 261757]	[30347, 93223]	{8149, 109073}	{20169, 288976}	[2443, 48943]	[17903, 37400]	[32812, 107349]
(Expressed as % of incumbent salary)	188	278	146	87	192	64.5	74.9	168
	[115, 370]	[136, 828]	[96.1, 294]	{26, 350}	{63, 899}	[7.72, 155]	[56.5, 118]	[104, 339]
Retention Elasticity	0.733	0.48	0.748	1.36	0.676	1.81	1.61	0.736
	[0.347, 1.18]	[0.131, 0.93]	[0.355, 1.2]	{0.359, 3.89}	{0.152, 1.82}	[0.783, 13.3]	[0.949, 2.3]	[0.348, 1.18]

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 (4) and (5) split the sample by labor market thickness and report replacement costs for both specifications separately. Confidence intervals are computed by bootstrapping; we draw with replacement from all matched pairs and recompute all empirical moments and then parameter estimates. We then calculate and confidence intervals as the 2.5th and 97.5th percentile of distribution of parameter estimates. For columns (4) and (5), we report 80% confidence intervals. The sample of pairs in thick or thin labor markets is smaller, so 5 to 10% of estimates feature negative wage or retention point estimates, which the model cannot rationalize after a worker death. See Section 5 for additional information. In columns (3) and (7), w^m is fixed in the estimation; the confidence intervals are the empirical bootstrap confidence intervals on the value of new hire wages.

Table 9: Wage Effect Heterogeneity by Relative Ranking of Deceased

Dimension of Heterogeneity	Deceased Wage Rank Relative to Incumbent		Deceased Ranked in Top 25% of Workers at the Firm		Deceased's Occupation's Average Pay Relative to Incumbent	
	Short Run	Long Run	Short Run	Long Run	Short Run	Long Run
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Incumbent Wages						
Treated × Lower	22.12	87.28	199.36	102.58	148.48	185.19
	(66.35)	(74.84)	(45.57)	(50.17)	(103.86)	(116.55)
Treated × Same					233.63	109.85
					(64.05)	(70.29)
Treated × Higher	210.84	130.76	366.10	211.11	281.08	179.66
	(51.82)	(54.47)	(93.78)	(103.64)	(77.72)	(87.04)
No. of Observations			7,328,907			
No. of Firms			63,926			
No. of Observations (Lower)	2,958,921				1,406,331	
No. of Firms (Lower)	49,345				28,181	
No. of Observations (Same)					2,842,479	
No. of Firms (Same)					45,769	
No. of Observations (Higher)	4,055,049				1,613,934	
No. of Firms (Higher)	55,787				29,883	
Panel B: Incumbent Wages						
Treated × Lower × Same Occupation	104.25	98.03	195.09	131.66		
	(81.89)	(92.26)	(62.76)	(68.83)		
Treated × Lower × Other Occupation	-55.71	-226.54	143.56	-10.36		
	(123.24)	(140.77)	(85.92)	(97.03)		
Treated × Higher × Same Occupation	290.24	141.04	512.31	233.37		
	(73.28)	(76.82)	(165.25)	(186.91)		
Treated × Higher × Other Occupation	169.51	114.80	203.96	1.00		
	(84.97)	(92.33)	(167.93)	(192.68)		
Panel C: Promotions						
Treated × Lower					-0.0007	-0.0000
					(0.0005)	(0.0005)
Treated × Same					0.0001	0.0008
					(0.0005)	(0.0005)
Treated × Higher					0.0012	0.0017
					(0.0012)	(0.0012)
Panel D: Promotions						
Treated					0.0010	0.0014
					(0.0003)	(0.0003)

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 $1(\text{period}_k)$ with the covariate. In Column (1) and (2) *Lower* restricts to the set of incumbents relative to whom the deceased worker earned a strictly lower wage, while *Higher* means that the deceased worker earned an equal or higher wage. For Column (3) and (4) *Lower* and *Higher* indicate whether the deceased worker was ranked low (in the bottom 75%) or within the top 25% of the firm in terms of salary. In Column (5) and (6) *Lower*, *Same* and *Higher* refer to the ranking of the deceased relative to the incumbent worker in terms of the average pay in their respective 5-digit occupations. 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. Observations are weighted inversely by the number of incumbent workers at the firm of the deceased. Standard errors are clustered at the firm level. Number of observations for the occupation splits in column 1 are 1,645,281, 2,242,773, 815,211, and 1,202,130, and number of firms is 36,464, 43,162, 22,288, and 29,728, respectively. In column 2, because whether the deceased is in the top 25% of the wage distribution is fixed at the firm level, we can specify the higher and lower values as interaction terms in one large regression without needing to adjust the clustering structure. The same occupation regression has 3,227,454 and 42,763 firms, while the other occupation regression includes 1,777,347 observations from 32,507 firms from pairs that matched on whether the deceased or placebo deceased worker was in the top 25% of the wage distribution at the firm and both had workers in other occupations from the deceased. In columns (5) and (6), the point estimates on same occupational pay differ slightly from those in Table 5 because the occupational pay definitions used here are at the 5-digit level, while those are at the 1-digit level. The number of observations for the four promotion regressions (three interacted with occupational pay ranking and then one overall) are 1,406,331, 2,842,479, 1,613,934, and 7,328,907, respectively. The number of firms in those regressions are 28,181, 45,769, 29,883, and 63,926.

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

Simon Jäger, Jörg Heining and Nathan Lazarus

Contents

A-1 Structural Model and Estimation

We provide derivations and additional details following and building on the model in Kline et al. (2019).

A-1.1 Model

A-1.1.1 FOCs

To derive (7) and (8), we compute partial derivatives of the profits function with respect to the choice variables:

$$
\begin{aligned}
\frac{\partial \pi}{\partial w^I} &= MRP\frac{\eta}{w^I - w^m}G(w^I)I - w^I\frac{\eta}{w^I - w^m}G(w^I)I - G(w^I)I,\\
\frac{\partial \pi}{\partial N} &= MRP - w^m - c'\left(\frac{N}{I}\right).
\end{aligned}
$$

Re-arranging then yields (7) and (8).

For the dynamic model, the FOCs become:

$$MRP + \beta V'(G(w_t^I)I_t + N_t) = w_t^I + \frac{w_t^I - w^m}{\eta}, \tag{22}$$

$$MRP + \beta V'(G(w_t^I)I_t + N_t) = w^m + c'\left(\frac{N_t}{I_t}\right). \tag{23}$$

A-1.1.2 Comparative Statics

We develop intuition about the model's behavior by studying comparative statics in the simpler case of the static model. Proposition A-1.1 summarizes these results. See Appendix A-1.7 for proofs.

Proposition A-1.1. *The responses of incumbent wages and hiring to a change in the number of incumbents are summarized by the following system of equations*

$$
\begin{aligned}
\frac{dMRP}{dI} &= \frac{1+\eta}{\eta}\frac{dw^I}{dI},\\
\frac{dMRP}{dI} &= \frac{c''\left(\frac{N}{I}\right)}{I}\left(\frac{dN}{dI} - \frac{N}{I}\right),\\
\frac{dMRP}{dI} &= -\frac{1}{\epsilon}MRP\frac{dL}{dI}\frac{1}{L},\\
\frac{dL}{dI} &= \frac{dN}{dI} + I\eta G(w^I)\frac{1}{w^I - w^m}\frac{dw^I}{dI} + G(w^I).
\end{aligned} \tag{24}
$$

From this system of equations, we deduce the following results.

(i) Hiring costs are strictly convex if and only if incumbent wages decrease with I.

(ii) If hiring costs are linear, then incumbent wages do not vary with I, and hiring strictly decreases with I.

(iii) The change in hiring satisfies $dN/dI - N/I < 0$.

(iv) If we neglect the scale effect from a change in I to hiring costs, then hiring strictly decreases with I.

Proposition A-1.1 highlights how the model requires strictly convex hiring costs to match the data. Without strictly convex costs, incumbent wages would not change after an incumbent death. The firm instead responds by fully replacing the incumbent with outsiders. According to claim (iii), we should also expect the hiring of outsiders to increase after an incumbent death when the scale effect to hiring costs is not too large.

Connection with Empirical Results. The observed wage response implies that hiring costs are convex. The identified responses of wages and hiring to an incumbent death are therefore consistent with each other.

A-1.2 Identification

With six target moments and six endogenous parameters, our baseline model is exactly identified. The model has a block diagonal structure, and the following explains which target moments from the data identify which parameters in the model. Denote the wages paid in the period before a death, -1, and the period after a death, 1, w^I_{pre} and w^I_{post}.[24] Denote the number of new hires N_{pre} and N_{post} and the retention rates G_{pre} and G_{post}. We also take I_{pre} to be exogenous, using the number of workers in period -1, and $I_{post} = I_{pre} - 1$ due to the death. Consider a fixed value for w^m and β, with the parameters to estimate being η, $\overline{w}$, γ, λ, P^0 and $\overline{w}$.

First, recall that the CDF of outside offers is:

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

So a fraction $G(w^I_{pre})$ workers do not get an offer better than w^I_{pre} and are therefore retained. Plugging in the functional form of G we get the following equations to hit the retention moments.

[24] To be precise, let w^I_{post} denote the target post-period wage. In the actual calibration, we account for the fact that observed coworker increases in the period after a death are an underestimate of changes in offered wages; see Section 5.2. What we do specifically is to solve out for the model implied path of coworker wages, supposing they're on a fixed job ladder, receiving offers between w^m and $\overline{w}$, and take better offers whenever they receive them. We assume that in the absence of treatment, firm wage offers would be w^I_{pre} for every year, if treated, firms instead offer a path of wages w^I_t. Workers are naive, and stay if offered a higher wage than their outside offer, even though in a firm that experiences a death, $w^I_1 > w^I_2$ because the increase in labor demand is the highest in the first period after the death, so a sophisticated worker who knew the future path of w_I would have a slightly lower threshold for exiting.

$$
\begin{aligned}
G(w^I_{pre}) &= \left(\frac{w^I_{pre} - w^m}{\overline{w} - w^m}\right)^\eta = G_{pre} \\
G(w^I_{post}) &= \left(\frac{w^I_{post} - w^m}{\overline{w} - w^m}\right)^\eta = G_{post}
\end{aligned}
$$

We can solve out for both η and $\overline{w}$ in closed form,

$$
\begin{aligned}
\eta &= \frac{\log\left(\frac{G_{pre}}{G_{post}}\right)}{\log\left(\frac{w^I_{pre}-w^m}{w^I_{post}-w^m}\right)} \\
\overline{w} &= w^m + \left(\frac{\left(w^I_{pre} - w^m\right)^{\log(G_{post})}}{\left(w^I_{post} - w^m\right)^{\log(G_{pre})}}\right)^{\frac{1}{\log\left(\frac{G_{post}}{G_{pre}}\right)}},
\end{aligned}
$$

but the more important insight, from Kline et al., is that η is identified from the retention elasticity. Then, $\overline{w}$ can be chosen to rationalize the level of the retention rate, given η:

$$
\frac{dG(w^I)I}{dw^I}\frac{w^I}{G(w^I)I} = \eta\frac{w^I}{w^I - w^m}.
$$

With values for η and $\overline{w}$, we can then equate the wage and hiring FOCs (22) and (23), and plug in our functional form for the cost function $c\left(\frac{N_t}{I_t}\right) = \gamma\left(\frac{N_t}{I_t}\right)^\lambda$ to get:

$$
\begin{aligned}
w^I_{pre} + \frac{w^I_{pre} - w^m}{\eta} &= w^m + \gamma\left(\frac{N_{pre}}{I_{pre}}\right)^\lambda \\
w^I_{post} + \frac{w^I_{post} - w^m}{\eta} &= w^m + \gamma\left(\frac{N_{post}}{I_{post}}\right)^\lambda.
\end{aligned}
$$

We can again solve out for γ and λ in closed form:

$$
\begin{aligned}
\gamma &= \frac{1+\eta}{\eta}\left(\frac{\left(w^I_{post} - w^m\right)^{\log\left(\frac{N_{pre}}{I_{pre}}\right)}}{\left(w^I_{pre} - w^m\right)^{\log\left(\frac{N_{post}}{I_{post}}\right)}}\right)^{\frac{1}{\log\left(\frac{N_{pre}}{I_{pre}}\Big/\frac{N_{post}}{I_{post}}\right)}} \\
\lambda &= \frac{\log\left(\frac{w^I_{pre}-w^m}{w^I_{post}-w^m}\right)}{\log\left(\frac{N_{pre}}{I_{pre}}\Big/\frac{N_{post}}{I_{post}}\right)} = \frac{\log\left(\frac{G_{pre}}{G_{post}}\right)}{\eta\log\left(\frac{N_{pre}}{I_{pre}}\Big/\frac{N_{post}}{I_{post}}\right)}
\end{aligned}
$$

Then, we can plug these back into the wage FOC, letting $L_t = G(w_t^I)I_t + N_t = I_{t+1}$ be total labor employed:

$$\frac{\epsilon - 1}{\epsilon} P^0 L_t^{-\frac{1}{\epsilon}} + \beta V'(I_{t+1}) = w_t^I + \frac{w_t^I - w^m}{\eta}.$$

We then use the envelope theorem to characterize the value function in closed form. The static profit function is:
$$\Pi(I_t, w_t^I, N_t) = P^0 L_t^{\frac{\epsilon-1}{\epsilon}} - I_t G(w_t^I) w^I - w^m N_t - \gamma \frac{1}{1+\lambda} I_t \left(\frac{N_t}{I_t}\right)^{1+\lambda}.$$

The envelope theorem gives:

$$V'(I_{t+1}) = \frac{\partial \Pi}{\partial I_{t+1}}\bigg|_{I_{t+2} = I_{t+2}^*(I_{t+1})},$$

where the constraint,

$$I_{t+2} = G(w_{t+1}^I)I_{t+1} + N_{t+1} = I_{t+2}^*(I_{t+1}),$$

holds the policy function $I^*_{t+2}(I_{t+1})$ constant at its optimal level. But since the wage and hiring first order conditions can be equated, it is without loss to assume that $w^I_{t+1} = {w^I_{t+1}}^*$ and the marginal adjustment takes place entirely on the hiring margin, which is much easier than implicitly differentiating the system with both wage and hiring adjustment in optimal proportion. So, letting $N_t = I^*_{t+1} - G\left(w^{I*}_t\right) I_t$, we have:

$$\begin{aligned}
\Pi(I_t, w^{I*}_t, I^*_{t+1} - G\left(w^{I*}_t\right) I_t) &= P^0 {I^*_{t+1}}^{\frac{\epsilon-1}{\epsilon}} - I_t G\left(w^{I*}_t\right) w^{I*}_t - w^m \left(I^*_{t+1} - G\left(w^{I*}_t\right) I_t\right) \\
&\quad - \gamma \frac{1}{1+\lambda} I_t \left(\frac{I^*_{t+1} - G\left(w^{I*}_t\right) I_t}{I_t}\right)^{1+\lambda} \\
\frac{\partial \Pi}{\partial I_t} = G\left(w^{I*}_t\right)(w^m - w^{I*}_t) &- \gamma \left(\frac{I^*_{t+1} - G\left(w^{I*}_t\right) I_t}{I_t}\right)^{\lambda} \left(G\left(w^{I*}_t\right) + \frac{I^*_{t+1} - G\left(w^{I*}_t\right) I_t}{I_t}\right) \\
&- \gamma \frac{1}{1+\lambda} \left(\frac{I^*_{t+1} - G\left(w^{I*}_t\right) I_t}{I_t}\right)^{1+\lambda} \\
V'\left(I_{pre}\right) = G_{pre}(w^m - w^I_{pre}) &- \gamma \left(\frac{I_{pre} - G_{pre} I_{pre}}{I_{pre}}\right)^{\lambda} \left(G_{pre} + \frac{I_{pre} - G_{pre} I_{pre}}{I_{pre}}\right) \\
&- \gamma \frac{1}{1+\lambda} \left(\frac{I_{pre} - G_{pre} I_{pre}}{I_{pre}}\right)^{1+\lambda} \\
V'\left(I_{pre}\right) = G_{pre}(w^m - w^I_{pre}) &- \gamma \left(\frac{N_{pre}}{I_{pre}}\right)^{\lambda} \left(G_{pre} + \frac{N_{pre}}{I_{pre}}\right) - \gamma \frac{1}{1+\lambda} \left(\frac{N_{pre}}{I_{pre}}\right)^{1+\lambda}.
\end{aligned}$$

Here, we use the steady-state assumption to substitute $I_{pre+1} = I_{pre}$ and $I_{pre} - G_{pre} I_{pre} = N_{pre}$ and rely on the death being entirely unexpected.

We can proceed similarly for the post period, but there, while we know $I_{post+1} = G(w^I_{post}) I_{post} + N_{post}$, we do not know the policy functions $w^I\left(I_{post+1}\right)$ and $I_{post+2}\left(I_{post+1}\right)$

$$\begin{aligned}
V'\left(I_{post+1}\right) = G\left(w^I\left(I_{post+1}\right)\right)&(w^m - w^I_{post+1}) \\
&- \gamma \left(\frac{I_{post+2} - G(w^I_{post+1}) I_{post+1}}{I_{post+1}}\right)^{\lambda} \left(G(w^I_{post+1}) + \frac{I_{post+2} - G(w^I_{post+1}) I_{post+1}}{I_{post+1}}\right) \\
&- \gamma \frac{1}{1+\lambda} \left(\frac{I_{post+2} - G(w^I_{post+1}) I_{post+1}}{I_{post+1}}\right)^{1+\lambda}.
\end{aligned}$$

Empirically, the linear policy function,

$$I_{t+1}(I_t) = L_{pre} + \frac{I_t - I_{pre}}{I_{post} - I_{pre}} \left(L_{post} - L_{pre}\right),$$

comes very close to the value for I_{post+2} that we get from the policy function using a sixth degree polynomial approximation to the value function. If we assumed a linear policy function for I_{t+1} that implies values for w_t^I and N_t from the first order conditions and I transition function, we have:

$$w_t^I + \frac{w_t^I - w^m}{\eta} = w^m + \gamma \left(\frac{N_t}{I_t}\right)^{\lambda}$$
$$I_{t+1} = G\left(w_t^I\right) I_t + N_t$$

So, with the caveat that I_{post+2} does not have an easy closed-form expression (it could be written as an infinite sum given its dependence on $V'\left(I_{post+2}\right)$ which in turn depends on I_{post+3}), we can now solve out for ϵ and P^0 from the first order conditions

$$\frac{\epsilon-1}{\epsilon} P^0 L_{pre}^{-\frac{1}{\epsilon}} + \beta V'(I_{pre}) = w_{pre}^I + \frac{w_{pre}^I - w^m}{\eta}$$
$$\frac{\epsilon-1}{\epsilon} P^0 L_{post}^{-\frac{1}{\epsilon}} + \beta V'(L_{post}) = w_{post}^I + \frac{w_{post}^I - w^m}{\eta}$$

This gives

$$\epsilon = \log\left(\frac{L_{post}}{L_{pre}}\right) \Big/ \log\left(\frac{w_{pre}^I + \frac{w_{pre}^I - w^m}{\eta} - \beta V'(L_{pre})}{w_{post}^I + \frac{w_{post}^I - w^m}{\eta} - \beta V'(L_{post})}\right)$$
$$P^0 = \left(\frac{\left(w_{pre}^I + \frac{w_{pre}^I - w^m}{\eta} - \beta V'(L_{pre})\right)^{\log(L_{post})}}{\left(w_{post}^I + \frac{w_{post}^I - w^m}{\eta} - \beta V'(L_{post})\right)^{\log(L_{pre})}}\right)^{\frac{1}{\log\left(\frac{L_{post}}{L_{pre}}\right)}} \frac{\log\left(\frac{L_{post}}{L_{pre}}\right)}{\log\left(\frac{L_{post}}{L_{pre}}\right) - \frac{w_{pre}^I + \frac{w_{pre}^I - w^m}{\eta} - \beta V'(L_{pre})}{w_{post}^I + \frac{w_{post}^I - w^m}{\eta} - \beta V'(L_{post})}}.$$

We implement calibrations where we allow w^m to be free, which means we do not get these closed form solutions, and where we target all five periods after a death or fix ϵ and w^m and do not hit the moments exactly. Nevertheless, this provides useful intuition to see what variation identifies what parameters.

A-1.3 Estimation Strategy

To implement the estimation, we adopt the mathematical program with equilibrium constraints (MPEC) approach proposed by Su and Judd (2012). The typical approach for estimating equilibrium models is the following procedure.

1. Solve the model accurately given a fixed set of parameters.

2. Use an optimization algorithm (either derivative-free or with finite difference approximations to the derivatives) to update the parameters.

3. Iterate until a solution is found.

The issue with this approach is that step 1 is usually time-consuming. MPEC bypasses this issue by solving directly for the parameter values with a constrained optimization approach. The targeted moments comprise the objective to minimize while equilibrium conditions are imposed as constraints. This approach speeds up computation by only solving the model accurately for the final set of parameters. Most algorithms for constrained optimization problems allow constraints to be violated during the parameter search and are robust to these violations. As a result, the algorithm does not inefficiently and repeatedly solve the model for parameters that are not close to hitting the targeted moments.

We also avoid solving for value function coefficients by using the envelope characterization described above in Section A-1.2. Computationally, to deal with the fact highlighted there that I_{post+2} cannot be solved for in closed-form, we fix $I_{post+10}$ to be equal to the steady state value, and then the problem backward from there. Convergence is fast enough that after 10 periods, optimal values are very close to the steady state (lengthening this to 20 periods makes no difference for our estimates), and by the turnpike theorem, adjusting the chosen value for $I_{post+10}$ actually has little impact on our estimates for the first 5 periods; we have also explored robustness to large changes in the terminal condition.

A-1.3.1 Baseline Dynamic Model

Suppose the firm is in a steady state, and at time $t = 0$ it experiences an unexpected decrease in the number of its incumbent workers. Note that this is not an innocuous assumption because it is possible many firms were still on a transition path across the time horizon for which we have data. We take the simple steady-state assumption and then match the adjustments over time of wages, hiring, and total employment to those estimated by the paper's event study.

We estimate six parameters because we have six moments, hence the model is exactly identified. The six parameters to estimate are γ, λ, η, $\overline{w}$, P^0, and either w^m or ϵ, depending on the calibration. We normalize labor productivity to $T = 1$ since it is not separately identified from P^0. We set $\beta = 0.96$ to match a 4% annual discount rate, which is standard in the literature.

In the extension to two types discussed in Section A-1.4, we add six additional moments (steady state and 1-year changes to wages, retention and hours for workers in other occupations), so we can identify four more type specific parameters, $\gamma_{\text{other occ}}$, $\lambda_{\text{other occ}}$, $\eta_{\text{other occ}}$, and $\overline{w}_{\text{other occ}}$. But because we take there to be one common ϵ and P^0 at the firm-level, we can identify $\frac{A_{\text{other occ}}}{A_{\text{same occ}}}$ and ρ. As discussed in the note to Table A-3.19, we cannot use the empirical new hire wage values for w^m and get a feasible value for ϵ, so we scale the w^m values by a factor of 0.44 to target $\epsilon = 2$; $\frac{A_{\text{other occ}}}{A_{\text{same occ}}}$ and ρ would not be identified if the ratio between $w^m_{\text{same occ}}$ and $w^m_{\text{other occ}}$ were free instead of fixed at its empirical value through this constant scaling approach.

A-1.4 Extensions with New Hires Probabilistically Becoming Incumbents and Imperfect Substitution

In the version of the model in which not all hires become incumbents, we just change the transition function for the stock of incumbents from:

$$I_{t+1} = G\left(w^I_t\right) I_t + N_t$$

to

$$I_{t+1} = G\left(w^I_t\right) I_t + \delta N_t.$$

With probability δ, a new hire becomes an incumbent and stays with the firm, but with probability $1-\delta$, the new hire does not advance to become an incumbent. We calibrate $\delta = 0.82$ to match the empirical difference in retention rates between new hires (using the definition from Kline et al. (2019) of workers in their first three years at the firm) and incumbents. A new hire at time t will therefore produce output with certainty at time t, and contribute to production in $t+1$ with probability $G\left(w^I_{t+1}\right)\delta$.

We also extend the model to incorporate two types of workers who are imperfectly substitutable, which we use to interpret our results on occupation heterogeneity. Denote occupations $k \in \{s, o\}$, where s is the same occupation as the deceased and o is another occupation. The firm arrives into the period with I_k incumbents. Workers of type s and o produce output according to the CES production function:

$$Q_j = (\alpha(A_s L_{sj})^\rho + (1-\alpha)(A_o L_{oj})^\rho)^{\frac{1}{\rho}},$$
$$L_{kj} = G_k(w^I_{kj} I_{kj} + N_{kj}.$$

We have type-specific retention function parameters,

$$G(w^I_{kj}) = (\frac{w^I_{kj} - w^m_k}{\overline{w}_k - w^m_k})^{\eta_k}$$

and hiring cost function parameters

$$c\left(\frac{N_k}{I_k}\right)=\frac{\gamma_k}{1+\lambda_k}\left(\frac{N_k}{I_k}\right)^{1+\lambda_k}$$

And product demand, profits, and the type-specific transition functions are otherwise the same. The first order conditions are therefore

$$\mathrm{MRP}_j(L_{kj})=w_k^m+\gamma_k N_{kj}^{\lambda_k}+\frac{d}{dI_k}V_{I_k}(I_k,I_{-k})$$

$$\mathrm{MRP}_j(L_{kj})=w_{kj}^I-\frac{w_{kj}^I+w_k^m}{\eta_k}+V_{I_k}(I_k,I_{-k}),$$

where the value function now is two-dimensional, defined over the stocks of each type of worker. The moments used for identification are then the wage, hiring, and retention responses of workers in the same occupation as the deceased to the death, as well as the steady state (computed from the control group in the year of the death) averages of those values, and similarly the responses and steady state values for workers in other occupations (as in the main estimation, for steady state values we use the average employment and retention to back out steady state hiring, but for the post-period response we use the observed hiring estimates).

A-1.5 Extension to Bargaining

In this extension, we relax the wage posting assumption in the baseline static model to allow Nash bargaining over incumbent wages.

A-1.5.1 Firm's Problem

Given a wage w^I, firms solve

$$\max_N P^0(G(w^I)I+N)^{1-1/\epsilon}-c\left(\frac{N}{I}\right)I-w^m N-w^I G(w^I)I. \tag{25}$$

As before, the FOC is

$$MRP=w^m+c'(N/I).$$

This equation implicitly defines N as a function of w^m, w^I, and I. Let $\nu(w^I)$ denote that dependence. We suppress w^m and I because they are irrelevant to the Nash bargaining problem.

The Nash bargaining problem for wages is:

$$\max_{w^I}\left(w^I-w^m\right)^{\phi}\left(\Pi(w^I,I)-\underline{\Pi}\right)^{1-\phi}, \tag{26}$$

where $\Pi(w^I, I)$ is firm j's profits after setting a wage of w^I for incumbents and arriving into the period with I incumbents; and $\underline{\Pi}$ are the profits when the firm has no incumbents and chooses N to maximize their profits. We assume that a union negotiates on behalf of incumbents; the union cares equally about every incumbent; every incumbent receives the same wage; and the union bargains under the assumption that all incumbents remain rather than account for the probability that some incumbents will leave. We assume that if bargaining fails, then all incumbents leave, and the firm must produce with only newly hired labor. On the other hand, incumbents can find a job at the competitive market wage. Transform the objective function by taking logs. The FOC is:

$$0 = \phi \frac{1}{w^I - w^m} + (1-\phi) \frac{\partial_{w^I} \Pi(w^I, I)}{\Pi(w^I, I) - \underline{\Pi}}. \tag{27}$$

The profits function is

$$\Pi(w^I, I) = P^0 (G(w^I) I + \nu(w^I))^{1-1/\epsilon} - c\left(\frac{\nu(w^I)}{I}\right) I - w^m \nu(w^I) - w^I G(w^I) I,$$

so the partial w.r.t. w^I is

$$\begin{aligned}
\partial_{w^I} \Pi(w^I, I) = {} & P_0^j T^{1-1/\epsilon} \left(1 - \frac{1}{\epsilon}\right) L^{-1/\epsilon} \left(g(w^I) I + \frac{\partial \nu}{\partial w^I}\right) \\
& - (c'(\nu(w^I)/I) + w^m) \frac{\partial \nu}{\partial w^I} - G(w^I) I - w^I g(w^I) I \\
= {} & \left(MRP - \frac{G(w^I)}{g(w^I)} - w^I\right) g(w^I) I + \left(MRP - c'(\nu(w^I)/I) - w^m\right) \frac{\partial \nu}{\partial w^I} \\
= {} & \left(MRP - \frac{w^I - w^m}{\eta} - w^I\right) g(w^I) I + \left(MRP - c'(\nu(w^I)/I) - w^m\right) \frac{\partial \nu}{\partial w^I}.
\end{aligned}$$

Recognize that the FOC for N implies

$$MRP - w^m - c'(\nu(w^I)/I) = 0,$$

so the partial derivative of profits simplifies further to

$$\partial_{w^I} \Pi(w^I, I) = \left(MRP - \frac{w^I - w^m}{\eta} - w^I\right) g(w^I) I.$$

Intuitively, the partial derivative of profits with respect to w^I is the gain in sales net of the wages paid to inframarginal and marginal workers multiplied by the marginal change in retention probability. When $\phi = 0$, this derivative is set to zero. When $\phi \in (0,1)$ the optimal solution features $w^I > w^m$ and $\Pi(w^I, I) > \underline{\Pi}$. It must be the case (for an interior solution) that the partial derivative of profits to wages is negative, i.e.,

$$MRP < \frac{w^I - w^m}{\eta} + w^I.$$

The marginal revenue product decreases in w^I, hence it is the case that w^I is higher with worker bargaining power.

When $I = 0$, the marginal product simplifies to

$$MRP = \left(1 - \frac{1}{\epsilon}\right) P^0 N^{-1/\epsilon},$$

so that when $\underline{N}$ solves

$$\left(1 - \frac{1}{\epsilon}\right) P^0 \underline{N}^{-1/\epsilon} = w^m + c'(\underline{N}),$$

profits are

$$\underline{\Pi} = P^0 \underline{N}^{1-1/\epsilon} - c(\underline{N}/I)I - w^m \underline{N}.$$

The surplus profits from retaining incumbents is

$$\begin{aligned}
\Pi - \underline{\Pi} &= P^0((G(w^I)I + \nu(w^I))^{1-1/\epsilon} - \underline{N}^{1-1/\epsilon}) \\
&\quad - (c(\nu(w^I)/I) - c(\underline{N}/I))I - w^m(\nu(w^I) - \underline{N}) - w^I G(w^I)I \\
&= \frac{\epsilon}{\epsilon - 1}(MRPL - \underline{MRPN}) \\
&\quad - (c(\nu(w^I)/I) - c(\underline{N}/I))I - w^m(\nu(w^I) - \underline{N}) - w^I G(w^I)I
\end{aligned}$$

Re-arrange the bargaining FOC to acquire

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

i.e., equation (21) stated in the main text.

A-1.5.2 Comparative Statics

In this section, we partially characterize the comparative statics with respect to the number of incumbents. When worker bargaining power is sufficiently low, wages will increase after an incumbent death, and greater worker bargaining power tends to reduce how much wages increase. If hiring costs are zero, then incumbent exits do not change wages. Lemma A-1.1 derives the system of equations characterizing the equilibrium response to an incumbent death while Proposition A-1.2 signs the wage response.

Lemma A-1.1. *The responses of wages and hiring to a change in the number of incumbents satisfy the system of equations*

$$\frac{dMRP}{dI} = c''\left(\frac{N}{I}\right) I^{-1}\left(\frac{dN}{dI} - \frac{N}{I}\right) \tag{28}$$

$$\frac{dMRP}{dI} = \frac{1+\eta}{\eta}\frac{dw^I}{dI} - \frac{d\mathcal{B}}{dI} \tag{29}$$

$$\mathcal{B} \equiv \frac{\phi}{1-\phi}\frac{1}{g(w^I)I}\frac{\Pi(\cdot) - \underline{\Pi}}{w^I - w^m} \tag{30}$$

$$\frac{d\mathcal{B}}{dI} = \mathcal{B}\left(\frac{1}{\Pi(w^I, I) - \underline{\Pi}}\frac{d\Pi}{dI} - \eta\frac{1}{w^I - w^m}\frac{dw^I}{dI}\right) \tag{31}$$

$$\frac{dMRP}{dI} = -\frac{1}{\epsilon}MRP\frac{dL}{dI}\frac{1}{L} \tag{32}$$

$$\frac{dL}{dI} = \frac{dN}{dI} + Ig(w^I)\frac{dw^I}{dI} + G(w^I). \tag{33}$$

Proof. Equilibrium is characterized by the conditions

$$MRP = \left(1 - \frac{1}{\epsilon}\right) P^0 L^{-1/\epsilon}$$

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

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

The derivative of MRP w.r.t. I is

$$\begin{aligned}\frac{dMRP}{dI} &= -\frac{1}{\epsilon}\left(1 - \frac{1}{\epsilon}\right) P^0 L^{-1/\epsilon - 1}\frac{dL}{dI} \\ &= -\frac{1}{\epsilon}MRP\frac{dL}{dI}\frac{1}{L} \\ \frac{dL}{dI} &= \frac{dN}{dI} + Ig(w^I)\frac{dw^I}{dI} + G(w^I).\end{aligned}$$

Total differentiation of the FOCs implies

$$\frac{dMRP}{dI} = c''\left(\frac{N}{I}\right) I^{-1}\left(\frac{dN}{dI} - \frac{N}{I}\right)$$

$$\frac{dMRP}{dI} = \frac{1+\eta}{\eta}\frac{dw^I}{dI} - \frac{d\mathcal{B}}{dI},$$

where

$$
\begin{aligned}
\frac{d\mathcal{B}}{dI} &= \frac{\phi}{1-\phi}\frac{1}{g(w^I)I(w^I-w^m)}\left(\frac{d\Pi}{dI} - \frac{\Pi(\cdot)-\underline{\Pi}}{g(w^I)I(w^I-w^m)}\left(g'(w^I)I(w^I-w^m)+g(w^I)I\right)\frac{dw^I}{dI}\right)\\
&= \frac{\phi}{1-\phi}\frac{1}{g(w^I)I(w^I-w^m)}\left(\frac{d\Pi}{dI} - \frac{\Pi(\cdot)-\underline{\Pi}}{(w^I-w^m)}\left(\frac{g'(w^I)}{g(w^I)}(w^I-w^m)+1\right)\frac{dw^I}{dI}\right).
\end{aligned}
$$

Recognize that

$$
\begin{aligned}
g'(w^I) &= \frac{1}{\overline{w}-w^m}\eta\left(1-\frac{1}{\eta}\right)(G(w^I))^{-1/\eta}g(w^I)\\
\frac{g'(w^I)}{g(w^I)} &= \frac{1}{\overline{w}-w^m}\eta\left(1-\frac{1}{\eta}\right)(G(w^I))^{-1/\eta}\\
&= \frac{1}{\overline{w}-w^m}\eta\left(1-\frac{1}{\eta}\right)\left(\frac{w^I-w^m}{\overline{w}-w^m}\right)^{-1}\\
&= \frac{\eta-1}{w^I-w^m}.
\end{aligned}
$$

It follows that

$$
\begin{aligned}
\frac{d\mathcal{B}}{dI} &= \frac{\phi}{1-\phi}\frac{1}{g(w^I)I}\frac{1}{w^I-w^m}\left(\frac{d\Pi}{dI} - \frac{\Pi(\cdot)-\underline{\Pi}}{w^I-w^m}(\eta-1+1)\frac{dw^I}{dI}\right)\\
&= \frac{\phi}{1-\phi}\frac{1}{g(w^I)I}\frac{1}{w^I-w^m}\frac{d\Pi}{dI} - \frac{\eta}{w^I-w^m}\mathcal{B}\frac{dw^I}{dI}.
\end{aligned}
$$

Use the definition of $\mathcal{B}$ to derive (31). □

Proposition A-1.2. *Suppose the hiring cost function $c(N/I)$ is strictly convex.*

(i) MRP strictly decreases with I.

(ii) If ϕ is sufficiently small, then wages strictly decrease with I.

(iii) If ϕ is sufficiently small, then positive worker bargaining power ($\phi > 0$) reduces how much wages increase after an incumbent death given the same $\frac{dMRP}{dI}$.

Proof. By the envelope theorem, profits increase with the number of incumbents. Conjecture $\frac{dMRP}{dI} > 0$. This implies $\frac{dN}{dI} > 0$ and $\frac{dw^I}{dI} > 0$. This leads to a contradiction as argued in the proof of Proposition A-1.1 when the hiring cost function is strictly convex. We may also rule out the zero derivative case by contradiction due to the strict convexity of $c(\cdot)$. If the derivative was zero, then $\frac{dN}{dI} = \frac{N}{I} > 0$ and $\frac{dw^I}{dI} > 0$ (by inspection and positivity of $\frac{d\Pi}{dI}$). This would imply $\frac{dL}{dI} > 0$, contradicting the zero response of MRP. Therefore, we must have $\frac{dMRP}{dI} < 0$.

To show (ii), notice that by Proposition A-1.1 wages strictly decrease with I when $\phi = 0$. By continuity, within a neighborhood of $\phi = 0$, i.e., for sufficiently small ϕ, this result remains true. We leave a fuller characterization of the comparative static to future work.

Finally, we prove (iii). Using (ii) and the fact that profits increase with I, we know that $\frac{d\mathcal{B}}{dI} > 0$. Further, $\frac{dMRP}{dI}$ is strictly negative under strictly convex hiring costs. For the same $\frac{dMRP}{dI}$, the only way to maintain the equality in (29) when $\phi > 0$ is for $\frac{dw^I}{dI}$ to become less negative. $\square$

A-1.6 Intensive Margin: Hours

One reason earnings may rise in response to a worker death is that firms make their incumbents work longer hours rather than pay them higher wages. To shed light on this mechanism, we extend the baseline model with an intensive margin. We begin with an analytically tractable extension to the static model, with which we can prove comparative statics, and conclude with a more realistic quantitative dynamic model. In the analytical model, we show that if it is costly for the firm to increase hours worked by incumbents, then firms will increase earnings mostly by increasing wages. In the numerical example, we estimate the model to match existing evidence on the intensive-margin elasticity of labor supply and find that a majority of the earnings response to an incumbent death is due to wage increases.

Setup The labor force size L now represents the number of full-time equivalent (FTE) workers employed by the firm. Let 1 FTE equal ϕ hours of work. Newly hired workers can only work ϕ hours, but the firm can control the number of hours h^I worked by incumbents. Higher hours increases the size of the effective labor force, but higher hours are not a free lunch. The subsequent analytical and quantitative sections differ in exactly how higher hours affects the firm's problem.

A-1.6.1 Analytical Model

We first assume that firms must pay additional costs if incumbents work more than ϕ hours. Without loss of generality, we set $\phi = 40$ in this section. Profits are given by

$$\begin{aligned} &P^0 Q^{\frac{\epsilon-1}{\epsilon}} - c\left(\frac{N}{I}\right) I - w^m N \\ &- \left(w^I \frac{h^I}{40} + \frac{\chi}{40}\frac{1}{1+\psi}((h^I)^{1+\psi} - 40^{1+\psi})\right) G(w^I) I, \end{aligned} \tag{34}$$

where $\psi > 0$. In addition to paying incumbents a wage w^I, the firm pays additional costs that are in convex in the number of hours worked by incumbents. The subtraction of $40^{1+\psi}$ centers these costs around 40 hours of work so that a firm choosing $h^I = 40$ is not penalized. These costs could be interpreted as additional compensation demanded by incumbents in order to work more than 40 hours, i.e., an overtime premium.

The response of wages, hours, and earnings to a change in the number of incumbents are characterized by the following proposition. The proof is in Appendix A-1.7.

Proposition A-1.3. *Assume hiring costs are strictly convex, and assume χ is chosen so that the firm sets $h^I = 40$ in equilibrium.*

(i) If $\psi > 1$, then $\frac{dw^I}{dI} < 0$, $\frac{dh^I}{dI} < 0$, and incumbent earnings decrease with I. The larger η, ψ, and χ are, the more the response is along the wage dimension.

(ii) If $\psi = 1$, then $\frac{dw^I}{dI} = 0$, $\frac{dh^I}{dI} < 0$, and incumbent earnings decrease with I.

(iii) If $\psi < 1$ and $\eta(1-\psi) < \psi$, then $\frac{dw^I}{dI} > 0$, $\frac{dh^I}{dI} < 0$, and incumbent earnings decrease with I.

(iv) If $\psi < 1$, $\eta(1-\psi) > \psi$, and $\eta > (1-\psi)^{-1}$, then $\frac{dw^I}{dI} < 0$, $\frac{dh^I}{dI} > 0$, and incumbent earnings increase with I.

To summarize, when the convexity of costs from hours is sufficiently large, wages, hours, and earnings will all increase in response to an incumbent death. This case is also the only one in which the earnings and wage response have the same sign. The more costly it is for h^I to deviate from 40, the more wages will change compared to hours. In the subsequent quantitative model, similar results will hold.

A-1.6.2 Quantitative Model

Setup and Estimation Specifying the trade-off in (34) as additional costs to the firm renders the model analytically tractable but misses an additional trade-off that h^I may affect the probability of retention.

Define

$$r(w^I, h^I) = (1-\tau)w^I - \frac{\chi}{1+\psi}((h^I)^{1+\psi} - \phi^{1+\psi}) \tag{35}$$

to be an incumbent's "reservation earnings level", where τ is the effective tax rate, w^I is now interpreted as a worker's earnings rather than wage, and the parameters χ and ψ capture a worker's disutility from labor. The disutility is zero when hours equal the steady-state level. We include labor income taxes to account for the fact that in the background of the empirical evidence in Chetty et al. (2013) is existing labor income taxes, which affect the relative valuation of labor income and leisure. Incumbents receive offers at other firms drawn from the distribution:

$$G(\omega) = \left(\frac{\frac{\omega}{1-\tau} - w^m}{\overline{w} - w^m} \right)^{\eta}. \tag{36}$$

The division of ω by $1-\tau$ indicates that ω is the pre-tax level of earnings and that incumbents make decisions based on the post-tax level. Unlike before, incumbents accept any offer if $\omega \geq r(w^I, h^I)$ rather than $\omega \geq (1-\tau)w^I$.

Equilibrium is now characterized by the profit function

$$\Pi(I, w^I, h^I) = P^0 Q^{\frac{\epsilon-1}{\epsilon}} - c\left(\frac{N}{I}\right) I - w^m N - w^I G(r(w^I, h^I))I$$
$$Q = T\left(N + \frac{h^I}{\phi} G(r(w^I, h^I))I\right),$$

and the four following equilibrium conditions.

$$MRP_t = \frac{\epsilon - 1}{\epsilon} P_0 T^{\frac{\epsilon-1}{\epsilon}} \left(N_t + \frac{h_t^I}{\phi} G(r(w_t^I, h_t^I))I_t\right)^{-1/\epsilon}$$
$$MRP_t + \beta V'(I_{t+1}) = w^m - c'\left(\frac{N_t}{I_t}\right)$$
$$MRP_t + \beta V'(I_{t+1}) = \left(\frac{h_t^I}{\phi}\right)^{-1} \left(\frac{(w_t^I - \frac{\chi}{1+\psi}(h_t^I)^{1+\psi}) - (w^m - \frac{\chi}{1+\psi}\phi^{1+\psi})}{\eta} + w_t^I\right)$$
$$MRP_t + \beta V'(I_{t+1}) = w_t^I \left(\frac{h_t^I}{\phi}\right)^{-1} \frac{\frac{\eta\chi}{1+\psi}(h_t^I)^{1+\psi}}{\frac{\eta\chi}{1+\psi}(h_t^I)^{1+\psi} - ((w_t^I - \frac{\chi}{1+\psi}(h_t^I)^{1+\psi}) - (w^m - \frac{\chi}{1+\psi}\phi^{1+\psi}))}$$

For ψ, we have a worker utility function of the form $(1-\tau)w_t^I - \chi\frac{1}{1+\psi}\left({h^I}^{1+\psi} + \phi^{1+\psi}\right)$, which is the same quasi-linear form as the simple utility function discussed in Chetty et al. (2013). There, the authors' preferred point estimate is an intensive-margin elasticity of 0.33, which means $\frac{1}{\psi} = 0.33$ or $\psi = 3.03$. To estimate χ, we target $h^I = 33.63$, the average value among full-time workers in the data from the Statutory Accident Insurance, before the incumbent shock. The firm does not choose a point on the workers' labor supply curve because they're freely able to choose a bundle of offered hours and wages, and workers can only respond through exit. We therefore pin down the steady-state level of hours as optimal from the firm's first-order conditions, and just use the workers' utility function to understand which outside offers then dominate the firm's offer.

We calibrate the remaining parameters. We set $\tau = 0.15$ so that the average labor income tax rate is 15% and $\phi = 33.63$, so that there is no penalty for choosing the steady-state level of hours.

Results Table 8 reports the estimated parameters. Figure 6 plots the log change in earnings relative to steady state and decomposes the change into wages and hours. The figure shows that slightly more of the earnings response can be attributed to hours, although changes in hourly wages account for almost half. The wage change explains 46.5% of the log earnings change in the first year after an incumbent death, the hours change explains 55.3%, and the remaining 0.2% is the interaction of increased hours times increased wages.

For completeness, we also reproduce the empirical event studies and calculate measures of replacement costs. Figures A.1 - A.3 show the event studies. Figure A.3 also shows the model-implied earnings path if either hours did not move or wages did not move after an incumbent death, but hourly wages or hours evolved as in the model.

Figure A.1: Labor Supply Shock and Employment Effects Of Worker Death

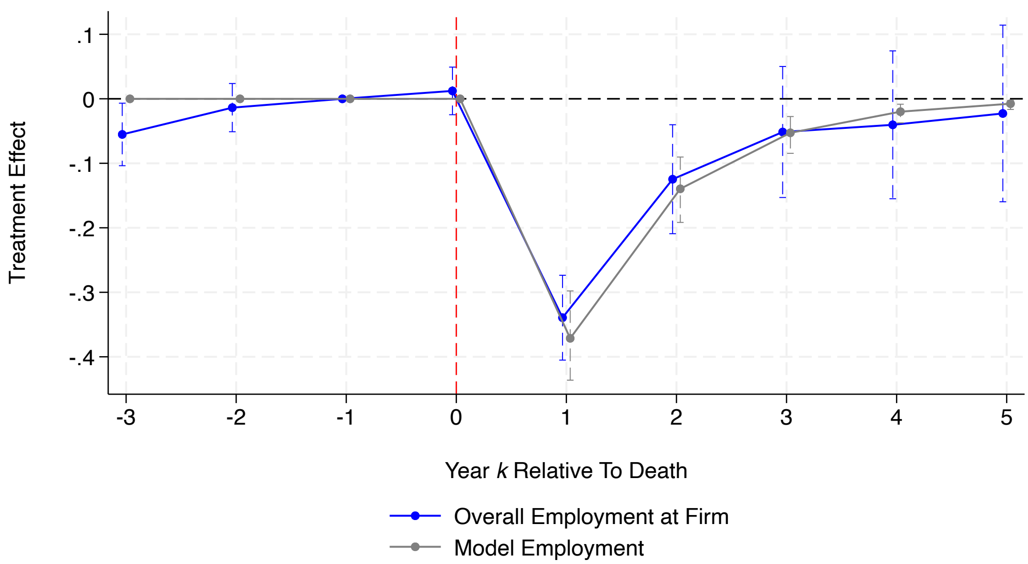


Figure A.2: Effect of Worker Death on Hiring

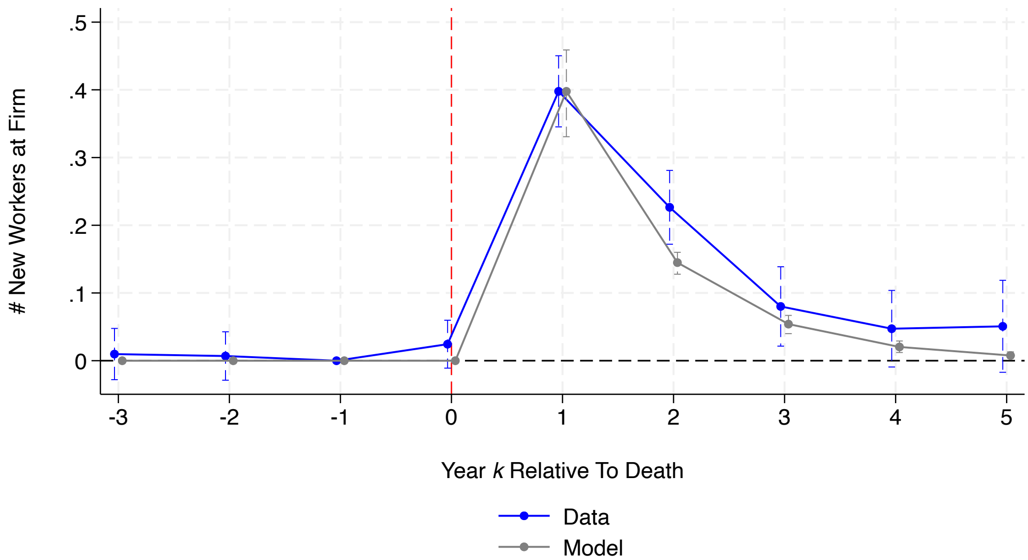


Figure A.3: Earnings, Wages and Hours Responses

A-1.7 Proofs

A-1.7.1 Proof of Proposition A-1.1

Proof. To obtain (24), we implicitly differentiate the definition of MRP and the two FOCs (7) and (8). The derivatives of MRP and L are given by:

$$\begin{aligned}
\frac{dMRP}{dI} &= -\frac{1}{\epsilon}\left(\frac{\epsilon-1}{\epsilon}\right)P^0 T^{1-1/\epsilon}L^{-1/\epsilon-1}\frac{dL}{dI} \\
&= -\frac{1}{\epsilon}MRP\frac{dL}{dI}\frac{1}{L} \\
\frac{dL}{dI} &= \frac{dN}{dI} + Ig(w^I)\frac{dw^I}{dI} + G(w^I) \\
&= \frac{dN}{dI} + I\eta G(w^I)\frac{1}{w^I - w^m}\frac{dw^I}{dI} + G(w^I).
\end{aligned}$$

We then use these results to differentiate the two FOCs and simplify.

Consider claim (i). Suppose hiring costs are strictly convex. We prove that $\frac{dMRP}{dI} < 0$ by contradiction. Suppose not.

First consider the case of $\frac{dMRP}{dI} > 0$. Then $\frac{dL}{dI} < 0$ from the derivative of MRP, $\frac{dw^I}{dI} > 0$ from the wage FOC, and $\frac{dN}{dI} > 0$ from the hiring FOC, as $c''(\cdot) > 0$. The latter two signs, however, imply $\frac{dL}{dI} > 0$, a contradiction.

Now consider the case of $\frac{dMRP}{dI} = 0$. Then $\frac{dw^I}{dI} = 0$, and $\frac{dN}{dI} = N/I$, as hiring costs are strictly convex. It follows that $\frac{dL}{dI} > 0$, but this sign contradicts a zero marginal product response.

To finish claim (i), we proceed by contraposition and suppose hiring costs are linear (it is assumed $c(\cdot)$ is weakly convex). Then $c''(\cdot) = 0$, so $\frac{dMRP}{dI} = 0$. This implies $\frac{dw^I}{dI} = 0$. The former equality completes the proof.

Claim (ii) follows from the previous argument. Linear hiring costs imply $\frac{dMRP}{dI} = 0$, hence $\frac{dL}{dI} = 0$. For this latter equality to hold, $\frac{dN}{dI} < 0$.

Claim (iii) follows from Claim (i)'s argument. In particular, when hiring costs are strictly convex, $\frac{dMRP}{dI} < 0$ implies the result. When hiring costs are linear, $\frac{dN}{dI} < 0$.

The hypothesis of Claim (iv) means that

$$\frac{dMRP}{dI} = \frac{c''(\frac{N}{I})}{I}\frac{dN}{dI}.$$

The conclusion of Claim (iv) follows from Claim (i). $\square$

A-1.7.2 Proofs for Model with Intensive Margin

The equilibrium conditions are

$$MRP = w^m + c'\left(\frac{N}{I}\right) \tag{37}$$

$$MRP = w^I + \frac{w^I - w^m}{\eta} + \frac{\chi}{1+\psi}((h^I)^\psi - 40^{1+\psi}/h^I) \tag{38}$$

$$MRP = w^I + \chi(h^I)^\psi \tag{39}$$

$$\frac{w^I - w^m}{\eta} = \frac{\chi}{h^I}\frac{1}{1+\psi}\left(\psi(h^I)^{1+\psi} + 40^{1+\psi}\right) \tag{40}$$

$$\begin{aligned} MRP &= \frac{P(L)L}{L} \\ &= \left(\frac{\epsilon-1}{\epsilon}\right) P^0 T^{1-1/epsilon} L^{-1/epsilon} \end{aligned} \tag{41}$$

$$P(L) = P^0 L^{-1/epsilon} \tag{42}$$

$$L = N + G(w^I)I\frac{h^I}{40}. \tag{43}$$

To obtain the comparative statics in Proposition A-1.3 implicitly differentiate these conditions to obtain the following lemma.

Lemma A-1.2. *The equilibrium response to an exogenous shock in I is characterized by the following system of equations:*

$$\frac{dMRP}{dI} = \frac{c''(\frac{N}{I})}{I}\left(\frac{dN}{dI} - \frac{N}{I}\right) \tag{44}$$

$$\frac{dMRP}{dI} = \frac{1+\eta}{\eta}\frac{dw}{dI} + \chi\frac{\psi}{1+\psi}(h^I)^{\psi-1}\frac{dh^I}{dI} + \chi\frac{1}{1+\psi}40^{1+\psi}(h^I)^{-2}\frac{dh^I}{dI} \tag{45}$$

$$\frac{dMRP}{dI} = \frac{dw^I}{dI} + \chi\psi(h^I)^{\psi-1}\frac{dh^I}{dI} \tag{46}$$

$$\frac{dMRP}{dI} = -\frac{1}{\epsilon}MRP\frac{1}{L}\frac{dL}{dI} \tag{47}$$

$$\frac{dL}{dI} = \frac{dN}{dI} + G(w^I)\frac{h^I}{40} + g(w^I)I\frac{h^I}{40}\frac{dw^I}{dI} + G(w^I)I\frac{1}{40}\frac{dh^I}{dI}. \tag{48}$$

$$\tag{49}$$

Further, the wage and hours response are related by

$$\frac{dw^I}{dI} = \frac{\eta\chi}{1+\psi}40^{1+\psi}\left(\psi^2\left(\frac{h^I}{40}\right)^{1+\psi} - 1\right)(h^I)^{-2}\frac{dh^I}{dI}. \tag{50}$$

As a corollary, we can unambiguously sign the wage and hours responses when any level of hours incurs convex costs.

Proposition A-1.4. *Suppose the costs from changing hours was not centered at 40, i.e., the* *firm pays* $(\chi/40)(1+\psi)^{-1}(h^I)^{1+\psi}$ *per retained incumbent rather than* $(\chi/40)(1+\psi)^{-1}((h^I)^{1+\psi}-40^{1+\psi})$*. Then* $\frac{dw^I}{dI}<0$ *and* $\frac{dh^I}{dI}<0$.

We first prove Lemma A-1.2 and Proposition A-1.4. We then conclude this subsection with the proof of Proposition A-1.3.

Proof of Lemma A-1.2. Implicitly differentiate the FOCs in (37) - (43) to obtain (44) - (48).

To derive (49), recognize that

$$\begin{aligned}
&\frac{dw^I}{dI}+\chi\psi(h^I)^{\psi-1}\frac{dh^I}{dI}\\
&=\frac{1+\eta}{\eta}\frac{dw}{dI}+\chi\frac{\psi}{1+\psi}(h^I)^{\psi-1}\frac{dh^I}{dI}+\chi\frac{1}{1+\psi}40^{1+\psi}(h^I)^{-2}\frac{dh^I}{dI}.
\end{aligned}$$

Rearrange.

$$\begin{aligned}
\frac{1}{\eta}\frac{dw^I}{dI}&=\left(\chi\psi\frac{\psi}{1+\psi}(h^I)^{1+\psi}-\chi\frac{1}{1+\psi}40^{1+\psi}\right)(h^I)^{-2}\frac{dh^I}{dI}\\
\frac{dw^I}{dI}&=\frac{\eta\chi}{1+\psi}40^{1+\psi}\left(\psi^2\left(\frac{h^I}{40}\right)^{1+\psi}-1\right)(h^I)^{-2}\frac{dh^I}{dI}.
\end{aligned}$$

□

Proof of Proposition A-1.4. In every equation of (44) - (48), replace $40^{1+\psi}$ by 0. Then (49) becomes

$$\frac{dw^I}{dI}=\frac{\eta\chi}{1+\psi}\psi^2(h^I)^{1+\psi}(h^I)^{-2}\frac{dh^I}{dI}, \tag{51}$$

and since $\psi>0$, the sign of the wage and hours responses must be the same.

We claim that $\frac{dMRP}{dI}<0$. Suppose not. First consider the case of a positive derivative. Then $\frac{dN}{dI}>0$ and $\frac{dL}{dI}<0$ by (48), (47), and strict convexity of $c(N/I)$. Since the sign of the wage and hours responses are the same, (46) implies $\frac{dw}{dI}>0$ and $\frac{dh^I}{dI}>0$. But in that case, (48) implies $\frac{dL}{dI}>0$ because every term is strictly positive, a contradiction. Now consider the case of a zero derivative. Then

$$\frac{dN}{dI}=\frac{N}{I},\quad \frac{dw^I}{dI}=\frac{dh}{dI}=0$$

from (44), (45), and (49). This implies $\frac{dL}{dI}>0$, which contradicts $\frac{dMRP}{dI}=0$.

Since $\frac{dMRP}{dI}<0$, $\frac{dL}{dI}>0$. As before, the signs of the wage and hours responses must be the same, hence $\frac{dw}{dI}<0$, and $\frac{dh}{dI}<0$. The sign of hiring is ambiguous unless we ignore the scale effect so that (44) becomes

$$\frac{dMRP}{dI} = \frac{c''(\frac{N}{I})}{I}\frac{dN}{dI}.$$

In this case, $\frac{dN}{dI} < 0$. Note that these signs are possible because there is one term in $\frac{dL}{dI}$ which is always positive. $\square$

Proof of Proposition A-1.3. We first prove the results on the wage and hours response. The earnings results follow quickly as a consequence.

Suppose hours have been calibrated to 40 in some steady state by varying χ.

Then

$$\frac{dw^I}{dI} = \frac{\eta\chi}{1+\psi}40^{\psi-1}(\psi^2-1)\frac{dh^I}{dI} = \eta\chi 40^{\psi-1}(\psi-1)\frac{dh^I}{dI}. \tag{52}$$

The sign of the wage response depends on ψ. If $\psi > 1$, then the wage and earnings response are identical. The argument offered in the proof of Proposition A-1.4 proves that $\frac{dMRP}{dI} < 0$, $\frac{dw^I}{dI} < 0$, and $\frac{dh^I}{dI} < 0$. From (52), the larger η, ψ, and χ are, the larger the wage derivative is relative to the hours derivative. If $\psi = 1$, then the wage response is zero, hence the response of earnings must entirely be along the hours dimension.

Now suppose $\psi < 1$. Then the wage response takes the opposite sign of the hours response. The derivative of the third FOC w.r.t. h^I implies

$$\begin{aligned}\frac{dMRP}{dI} &= \left(\eta\chi 40^{\psi-1}(\psi-1) + \chi\psi(40)^{\psi-1}\right)\frac{dh^I}{dI}\\ &= \chi 40^{\psi-1}\left(\eta(\psi-1)+\psi\right)\frac{dh^I}{dI}\\ &= \chi\psi 40^{\psi-2}(\psi-\eta(1-\psi))\frac{dh^I}{dI}.\end{aligned}$$

The sign in this case depends on η. Recall that $\psi < 1$. If η is close to zero or ψ is close to 1, then the hours response takes the same sign as $\frac{dMRP}{dI}$. If η is large or ψ is close to 1, then the hours response takes the opposite sign.

To determine the sign of $\frac{dMRP}{dI}$, consider $\frac{dL}{dI}$. We have

$$\begin{aligned}\frac{dL}{dI} &= \frac{dN}{dI} + G(w^I) + g(w^I)I\frac{dw^I}{dI} + G(w^I)I\frac{1}{40}\frac{dh^I}{dI}\\ &= \frac{dN}{dI} + G(w^I) + \left(g(w^I)\eta\chi 40^{\psi-1}(\psi-1) + G(w^I)\frac{1}{40}\right)I\frac{dh^I}{dI}.\end{aligned}$$

Recall that

$$g(w^I) = \eta G(w^I)\frac{1}{w^I - w^m}$$

and also that

$$w^I - w^m = \frac{\eta\chi}{h^I}\frac{1}{1+\psi}\left(\psi(h^I)^{1+\psi} + 40^{1+\psi}\right) = \eta\chi\frac{1}{1+\psi}(\psi+1)40^{\psi} = \eta\chi 40^{\psi}.$$

It follows that

$$\begin{aligned}\frac{dL}{dI} &= \frac{dN}{dI} + G(w^I) + \left(1 - \eta^2\chi 40^{\psi}(1-\psi)\frac{1}{w^I - w^m}\right)\frac{1}{40}G(w^I)I\frac{dh^I}{dI} \\ &= \frac{dN}{dI} + G(w^I) + (1 - \eta(1-\psi))\frac{1}{40}G(w^I)I\frac{dh^I}{dI}\end{aligned}$$

Consider case (iii), in which we suppose $\eta(1-\psi) < \psi$. We prove $\frac{dMRP}{dI} < 0$ by contradiction. First suppose $\frac{dMRP}{dI} > 0$. Our parameter assumptions imply $\frac{dh^I}{dI} > 0$. Further, (44) implies $\frac{dN}{dI} > 0$. Then

$$1 - \eta(1-\psi) > 1 - \psi > 0,$$

with the latter inequality following from the fact that $\psi < 1$. Then $\frac{dL}{dI} > 0$, but that contradicts $\frac{dMRP}{dI} > 0$. Now consider $\frac{dMRP}{dI} = 0$. Then $\frac{dN}{dI} = N/I$, and $\frac{dh^I}{dI} = 0$, hence $\frac{dL}{dI} > 0$, a contradiction. Thus, $\frac{dMRP}{dI} < 0$, $\frac{dh^I}{dI} < 0$, and $\frac{dw^I}{dI} > 0$.

Finally, consider case (iv), in which we suppose $\eta(1-\psi) > \psi$ and $\eta > (1-\psi)^{-1}$. Proof by contradiction using similar arguments as before shows that $\frac{dMRP}{dI} < 0$. Under these parameter restrictions, $\frac{dh^I}{dI} > 0$, hence $\frac{dw^I}{dI} < 0$.

Now return to the earnings response. Using (52), the earnings response can be written as

$$\begin{aligned}\frac{dw^I h^I}{dI} &= \frac{dw^I}{dI}h^I + w^I\frac{dh^I}{dI} \\ &= \eta\chi 40^{\psi-1}(\psi-1)\frac{dh^I}{dI}h^I + w^I\frac{dh^I}{dI} \\ &= \left(\eta\chi(\psi-1)40^{\psi-1}h^I + w^I\right)\frac{dh^I}{dI}.\end{aligned}$$

Since h^I is calibrated to 40,

$$\frac{dw^I h^I}{dI} = \left(\eta\chi(\psi-1)40^{\psi} + w^I\right)\frac{dh^I}{dI}.$$

Equilibrium condition (40) and $h^I = 40$ implies

$$w^I = w^m + \eta\chi 40^{\psi},$$

hence

$$
\begin{aligned}
\eta\chi(\psi-1)40^{\psi}+w^{I} &= \eta\chi(\psi-1)40^{\psi}+w^{m}+\eta\chi 40^{\psi} \\
&= \eta\chi\psi 40^{\psi}+w^{m} \\
&> 0
\end{aligned}
$$

since $\psi > 0$. Therefore, the earnings response takes the same sign as the hours responses. $\square$

A-2 Evidence on Hours Response (Accident Insurance Data: 2010 to 2015)

We draw on data based on unique information from the German Statutory Accident Insurance to assess the effect of worker deaths on coworkers' work hours. For the years 2010 to 2015, information on workers' hours as reported by the firms are included in the IEB database (see also Gudgeon and Trenkle, forthcoming; Dustmann et al., 2022). Here, we first assess the reliability of the hours data. We then apply our research design for wages using hours-per-week as outcome variable and find no average hours response for the period from 2010 to 2015. However, we find some evidence consistent with negative hours effects of manager and high-skilled worker deaths on workers in other occupations. Overall, we conclude that we find that the hours data from 2010 to 2015 do not point to positive effects. As an important caveat to our analysis, we note that short-run changes in hours, e.g., due to overtime, may be imperfectly captured by the data we analyze.

Reliability of hours data. Before analyzing potential effects on hours, we discuss the reliability of the hours data and implement several validation tests. Employers could report hours in four different ways (see Dustmann et al., 2022, Online Appendix B.1): i) actual work hours, ii) contractual work hours, iii) hours according to a collective bargaining agreement or the annual fixed full-time reference value calculated by the accident insurance, or iv) a guess. Unfortunately, the data do not include the reporting scheme chosen by the employer. Dustmann et al. (2022) implement several adjustment heuristics to arrive at a measure of contractual working hours which lines up well with data from the German Socio-Economic Panel and the Structure of Earnings Survey (see Table B.2 in their Online Appendix). Since our analysis takes out employer-specific averages, we do not adjust hours across employers (e.g., by adding fixed overtime hours). We tabulate hours per week by gender and benchmark it against data from the Structure of Earnings Survey (*Verdienststrukturerhebung*) 2014 (see Statistisches Bundesamt, 2016). As Appendix Table A-3.13 documents, the summary statistics for the work hours in the administrative data are broadly in line with the information from the Structure of Earnings Survey. The average hours per week in the administrative data are 33.6 while the survey average is at 30.9 (including overtime). Both the administrative and the survey data show a pattern of higher work hours per week for men (34.5 vs. 34.7) compared to women (30.4 vs. 27.0). We also plot the distribution of hours per week in Appendix Figure A-3.4. We next follow a validation test from Lachowska, Mas, and Woodbury (2022) who assess the reliability of administrative hours measures using data from Washington state. Building on their procedure, we test whether changes log hours from year to year predict changes in log earnings. We find that changes in log hours within individual over time are positively correlated with changes in log earnings ($p < 0.001$), providing support for the reliability of the earnings measures. In addition, we run several other tests of worker-level predictors of work hours and, e.g., find that part-time workers work 16.72 (SE 0.11) fewer hours per week. We also note that Gudgeon and Trenkle (forthcoming), based on the same administrative data sources, report evidence documenting hours responses to a tax notch. For Dustmann et al. (2022), the reform they study occurs after the hours sample ends, although follow-up work has found only limited hours responses to the minimum wage in Germany (see, e.g., Biewen, Fitzenberger, and Rümmele, 2022).

While the analyses probing the informativeness of the hours data for our purposes are encouraging (and we do not have evidence to the contrary), we lack a direct, individual benchmarking with validated measures of actual work hours. We thus provide the caveat that the data underlying the following analysis may only imperfectly capture short-run hours changes.

Hours responses. Figure A-3.5 and columns (1) and (2) in the upper panel of Table A-3.14 extend our main specification to the new sample, using hours per week as outcome variable. We also report summary statistics for the new sample in Tables A-3.15 and A-3.16. On average, we find no evidence for hours increases in response to coworkers deaths. The short- and long-run effects on incumbent work hours are -0.01 (SE 0.22) and -0.11 (SE 0.23), respectively. That is, point estimates are close to zero and not statistically significant. We also analyze effects on earnings (incumbent worker wages) in the second panel of Table A-3.14. We find a positive effect of EUR 95.4 (SE 122.0) in the short run and of -38.1 (SE 138.2) in the long run. The estimates have wide standard errors that include our main sample point estimates (as well as zero). We next assess the effect within and across coworkers in the same occupation (columns (3) and (4) in the upper panel of Table A-3.14). We find small, positive effects on hours of 0.44 (SE 0.27) in the short run and of 0.29 (SE 0.30) in the long run for workers in the same occupation. We find negative effects on workers in other occupations, with estimate of -0.82 (SE 0.32) in the short run and of -0.87 (SE 0.31) in the long run. In terms of wages, we detect large, positive effects in the own occupation and negative effects among workers in other occupations with point estimates of 294.6 (SE 161.9.3) and -288.3 (SE 218.1), respectively. In the long run, effects on earnings of workers in the same occupation group are small (€44.9, SE 181.6), and, for workers in other occupation groups negative (€-419.6, SE 213.2).

The analysis of hours thus reveals, on average, zero effects on hours in the short run despite a sizeable (but not statistically significant) effect on earnings. We again find that the earnings response is driven by workers in the same occupation as the deceased who experience a statistically significant increase in earnings. We find a positive though not statistically significant change in hours for that group. Due to the size of the confidence intervals, we cannot reject that hours could account for the effect on earnings for workers in the same occupation as the deceased (though we also cannot reject that hours effects are zero in that group).

One potential factor explaining the absence of a positive hours response could be the institutional setup in Germany where labor law, agreements and contracts put sharp upper limits on work hours.[25]

Online Appendix References

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

A-3 Additional Tables and Figures

A-3.1 Additional Tables

Table A-3.1: Robustness Test: Probability of Future Deaths by Treatment Status

	Outcome: Indicator for Worker Death
Treatment	0.000201
	(0.000222)
Constant	0.011937
	(0.000167)
No. of Observations	1,165,326
No. of Clusters	63,926

Note: The table reports the results of a regression of an indicator variable that is equal to 1 if a firm experienced a worker death in a given year on treatment status for the sample of years after the actual or placebo death. The magnitude of the point estimates implies that firms in the comparison group face a 1.2% probability of a worker death in a given year and that this probability is on average only 0.0201% higher in the treatment group. Standard errors are clustered at the firm level.

Table A-3.2: Dynamics of Treatment Effect on Incumbent Worker Wages

Outcome:	Incumbent Worker Wages	Sum of Incumbent Worker Wages
Treated $\times$ $k = -3$	-3.69	-279.39
	(31.76)	(370.89)
Treated $\times$ $k = -2$	28.71	-95.47
	(27.19)	(299.25)
Treated $\times$ $k = -1$	omitted	omitted
Treated $\times$ $k = 0$	67.37	542.80
	(25.37)	(308.37)
Treated $\times$ $k = 1$	186.53	1670.79
	(35.78)	(391.17)
Treated $\times$ $k = 2$	152.26	1329.23
	(41.17)	(448.47)
Treated $\times$ $k = 3$	120.05	891.30
	(45.90)	(499.78)
Treated $\times$ $k = 4$	76.11	587.51
	(49.49)	(542.10)
Treated $\times$ $k = 5$	-1.17	-237.55
	(53.15)	(587.49)
No. of Observations	7,328,907	7,328,907
No. of Firms	71,966	71,966

Note: The table reports results based on the dynamic difference-in-differences model in (2). k denotes the year relative to the death of the worker. The mean of incumbent worker wages in year $k = -1$ in the control group is €29270 (2010 CPI). Observations are weighted inversely by the number of incumbent workers at the firm. Standard errors clustered at the firm level.

Table A-3.3: Treatment Effects for Additional Samples: Part-Time Incumbents and Apprentices

Sample:	Part-Time Incumbents		Apprentices		Main Sample: Full-Time Incumbents	
	Short-Run Effect	Long-Run Effect	Short-Run Effect	Long-Run Effect	Short-Run Effect	Long-Run Effect
Outcome: Wages						
Treated	-52.61	-170.80	197.04	157.43	186.53	106.76
	(117.27)	(128.43)	(294.19)	(295.24)	(35.78)	(39.30)
Outcome: Employed at Same Establishment						
Treated	-0.0006	0.0000	0.0347	0.0272	0.0043	0.0044
	(0.0052)	(0.0051)	(0.0133)	(0.0105)	(0.0012)	(0.0013)
Outcome: Full-Time Employment						
Treated	0.0022	0.0008	-0.0057	-0.0038	0.0034	0.0010
	(0.0048)	(0.0047)	(0.0151)	(0.0116)	(0.0012)	(0.0012)
Outcome: Part-Time Employment						
Treated	-0.0097	-0.0079	0.0051	0.0045	0.0002	0.0005
	(0.0068)	(0.0065)	(0.0056)	(0.0052)	(0.0005)	(0.0005)
Outcome: Promotion						
Treated	0.0022	0.0015	-0.0038	-0.0019	0.0010	0.0014
	(0.0014)	(0.0013)	(0.0031)	(0.0028)	(0.0003)	(0.0003)
No. of Observations	377325	377325	58761	58761	7328907	7328907
No. of Firms	14916	14916	3976	3976	63926	63926
Outcome: Occupation Mean Wage						
Treated	-49.06	-77.51	-8.45	22.58	62.90	49.66
	(118.85)	(113.93)	(164.15)	(153.30)	(31.00)	(30.06)
No. of Observations	303688	303688	40384	40384	6405051	6405051
No. of Firms	14880	14880	3973	3973	63923	63923

Note: The table displays treatment effects on several employment outcomes based on difference-in-differences regressions. The sample of part-time incumbents is defined as the set of part-time coworkers of the deceased in the year before death. Apprentices are defined as apprentices at the incumbent's firm in the year before death. The full-time incumbent sample is the main sample used for the analysis in the paper and included here as a benchmark. The set of firms used across samples is the same, specifically the firms are required to have at least one full time, working age worker in all years to be in the sample. Treated refers to the Post × Treated coefficient. Short-run effects refer to the diff-in-diff effects using year $k = 1$ post-death as the post period; long-run effects refer to the specifications using years 1 through 5 post-death as the post period. Employed at the same establishment is an outcome variable that is equal to one when an incumbent worker is still employed at the same establishment as in year $k = -1$. Full- and part-time employment are outcome variables that indicate the respective employment status independent of the establishment at which the individual is employed. Promotion is an outcome variable that is equal to 1 when an individual is employed at the same establishment in an occupation with a higher average wage than the occupation he or she worked in year $k = -1$. To calculate average wages at the 5 digit occupation level, we draw a 10% sample of individuals from the IEB and regress individuals' log wage on occupation dummies and individual fixed effects. We use the estimated occupation effects to measure promotions. Observations are weighted inversely by the number of incumbent workers at the firm of the deceased.

Table A-3.4: Dynamics of Treatment Effect on New Hire Wages

Outcome:	New Hire Wages	Residual New Hire Wages
Treated × $k = -3$	60.00	-34.00
	(117.27)	(103.43)
Treated × $k = -2$	-95.74	-36.45
	(113.59)	(101.38)
Treated × $k = -1$	omitted	omitted
Treated × $k = 0$	-107.98	-206.09
	(119.64)	(105.67)
Treated × $k = 1$	426.99	311.79
	(121.85)	(106.40)
Treated × $k = 2$	308.35	137.87
	(127.42)	(111.66)
Treated × $k = 3$	-46.94	-25.98
	(134.66)	(117.78)
Treated × $k = 4$	-149.76	-115.02
	(140.84)	(122.57)
Treated × $k = 5$	18.91	-36.76
	(146.43)	(129.09)
No. of Observations	164,989	

Note: The table reports results based on the dynamic difference-in-differences model in (1), with new hire wages as the outcome variable. k denotes the year relative to the death of the worker. The mean of new hire worker wages in year $k = -1$ in the control group is €27,229 (2010 CPI). Column 2 reports the effect on wage residuals. In the control group, we regress new hire wages on industry experience, occupation experience, gender, age, years of education, with broad occupation, broad industry, year, and region fixed effects to estimate the expected new hire wage given observables, and then calculate the residual wage as the difference between the actual wage and the expected.

Table A-3.5: New Hire Characteristics

	Wages	Age	New Hire Education			New Hire Experience	
			Low	Medium	High	Industry	Occupation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Short-run	426.99	0.242	-0.0004	0.0035	-0.0031	0.013	0.021
	(121.85)	(0.150)	(0.0040)	(0.0059)	(0.0048)	(0.051)	(0.063)
Long-run	111.51	0.078	0.0017	-0.0028	0.0011	-0.017	-0.003
	(89.65)	(0.109)	(0.0029)	(0.0043)	(0.0034)	(0.036)	(0.045)
No. of Observations	164989	164989	164989	164989	164989	164985	164989
No. of Firms	28314	28314	28314	28314	28314	28314	28314

Note: The table reports firm-level results based on the dynamic difference-in-differences model in (1), with various new hire characteristics as the outcome variable. Short-run effects refer to the DiD 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. Regressions condition on both a treated firm and matched control having new hires in the period before the death and the period considered

Table A-3.6: Wage Effects and External Labor Market Characteristics, All Occupations

Outcome: Wages of Incumbent Workers Co-Worker Sample:	All Worker Deaths		Worker Deaths in High Specialization Occupations		Worker Deaths in Low Specialization Occupations	
	Short-Run Effect	Long-Run Effect	Short-Run Effect	Long-Run Effect	Short-Run Effect	Long-Run Effect
	(1)	(2)	(3)	(4)	(5)	(6)
(A) Thickness Measured at Occupation Level						
Treated × Low Thickness (Occupation)	176.15	84.23	146.65	101.06	6.83	-105.37
	(70.65)	(77.92)	(144.18)	(156.63)	(120.77)	(136.32)
Treated × High Thickness (Occupation)	97.72	68.11	-81.66	-158.24	217.04	218.46
	(67.36)	(74.44)	(134.41)	(151.10)	(110.06)	(121.87)
No. of Observations	3934719		781686		1559169	
No. of Firms	36125		8575		13668	
(B) Thickness Measured at Industry Level						
Treated × Low Thickness (Industry)	216.96	112.30	361.68	167.04	22.44	20.91
	(68.22)	(74.60)	(134.95)	(148.04)	(120.52)	(132.09)
Treated × High Thickness (Industry)	148.89	48.29	-53.99	-227.93	153.70	105.58
	(63.92)	(69.99)	(127.90)	(141.21)	(107.00)	(118.87)
No. of Observations	4313178		883719		1663839	
No. of Firms	39677		9765		14529	
(C) Density of Local Labor Market						
Treated × Low Density	220.22	165.21	306.66	154.94	98.45	210.05
	(65.85)	(72.01)	(129.34)	(143.41)	(118.90)	(128.34)
Treated × High Density	105.78	17.50	143.96	121.74	33.93	-43.50
	(72.70)	(80.25)	(154.20)	(166.73)	(119.51)	(134.16)
No. of Observations	3970134		768114		1556010	
No. of Firms	36133		8445		13440	
(D) Local Unemployment Rate						
Treated × Low Unemployment	227.92	168.38	514.87	290.57	130.31	86.49
	(73.33)	(80.78)	(143.33)	(158.69)	(124.04)	(135.86)
Treated × High Unemployment	237.54	67.78	43.08	-253.97	185.71	86.03
	(69.56)	(76.60)	(137.13)	(148.90)	(119.76)	(135.70)
No. of Observations	4962447		1219671		2202939	
No. of Firms	34447		8151		12781	

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 $1(\text{period}_k)$ with the covariate. To calculate a specialization measure for the occupation of the deceased worker, we follow Bleakley and Lin (2012) and calculate returns to experience for each 5-digit occupation. We then use the estimated occupation-specific returns to experience to classify occupations into high- and low-specialization occupations based on a median split. All external labor market characteristics are measured at the commuting zone level based on median splits of the relevant measure. Thickness measured at the occupation level is used to categorize 5-digit occupation × commuting zone cells as thick or thin based on the relative share of workers in the 5-digit occupation in the commuting zone relative to the overall share of workers in that occupation in the labor market. Thickness measured at the industry level is defined analogously for the share of workers in the 3-digit industry × commuting zone level. Density of the local labor market refers to the number of workers in a commuting zone divided by that commuting zone's area. The unemployment rate is calculated as the number of unemployed workers in the commuting zone divided by the number of workers. Observations are weighted inversely by the number of incumbent workers at the firm of the deceased. Standard errors are clustered at the firm level.

Table A-3.7: Heterogeneity of Hiring Responses and External Labor Market Characteristics

Outcome: Hiring of Workers						
Sample:	All Worker Deaths		Worker Deaths in High Specialization Occupations		Worker Deaths in Low Specialization Occupations	
	Short-Run Effect	Long-Run Effect	Short-Run Effect	Long-Run Effect	Short-Run Effect	Long-Run Effect
	(1)	(2)	(3)	(4)	(5)	(6)
(A) Thickness Measured at Occupation Level						
Treated × Low Thickness (Occupation)	0.40	0.19	0.49	0.34	0.32	0.00
	(0.06)	(0.05)	(0.07)	(0.09)	(0.16)	(0.10)
Treated × High Thickness (Occupation)	0.40	0.16	0.37	0.19	0.36	0.17
	(0.04)	(0.04)	(0.08)	(0.12)	(0.07)	(0.07)
No. of Observations	347976		78669		128385	
(B) Thickness Measured at Industry Level						
Treated × Low Thickness (Industry)	0.37	0.15	0.48	0.36	0.34	0.06
	(0.05)	(0.05)	(0.07)	(0.11)	(0.12)	(0.10)
Treated × High Thickness (Industry)	0.43	0.13	0.39	0.09	0.36	0.06
	(0.04)	(0.04)	(0.07)	(0.07)	(0.07)	(0.06)
No. of Observations	385038		89937		137475	
(C) Density of Local Labor Market						
Treated × Low Density	0.38	0.14	0.39	0.17	0.32	0.03
	(0.05)	(0.04)	(0.09)	(0.08)	(0.08)	(0.06)
Treated × High Density	0.37	0.19	0.48	0.21	0.29	0.14
	(0.06)	(0.04)	(0.08)	(0.08)	(0.12)	(0.06)
No. of Observations	347994		77463		126288	
(D) Local Unemployment Rate						
Treated × Low Unemployment	0.44	0.18	0.49	0.24	0.39	0.12
	(0.06)	(0.05)	(0.08)	(0.08)	(0.14)	(0.10)
Treated × High Unemployment	0.38	0.17	0.25	0.03	0.37	0.19
	(0.06)	(0.05)	(0.15)	(0.11)	(0.12)	(0.07)
No. of Observations	436437		119079		181521	

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 $1(\text{period}_k)$ with the covariate. To calculate a specialization measure for the occupation of the deceased worker, we follow Bleakley and Lin (2012) and calculate returns to experience for each 5-digit occupation. We then use the estimated occupation-specific returns to experience to classify occupations into high- and low-specialization occupations based on a median split. All external labor market characteristics are measured at the commuting zone level based on median splits of the relevant measure. Thickness measured at the occupation level is used to categorize 5-digit occupation × commuting zone cells as thick or thin based on the relative share of workers in the 5-digit occupation in the commuting zone relative to the overall share of workers in that occupation in the labor market. Thickness measured at the industry level is defined analogously for the share of workers in the 3-digit industry × commuting zone level. Density of the local labor market refers to the number of workers in a commuting zone divided by that commuting zone's area. The unemployment rate is calculated as the number of unemployed workers in the commuting zone divided by the number of workers. Observations are weighted inversely by the number of incumbent workers at the firm of the deceased. Standard errors are clustered at the firm level.

Table A-3.8: Heterogeneity of Hiring of Workers in Same Occupation as Deceased and External Labor Market Characteristics

Outcome: Hiring of Workers in the Same Occupation Group as the Deceased						
Sample:	All Worker Deaths		Worker Deaths in High Specialization Occupations		Worker Deaths in Low Specialization Occupations	
	Short-Run Effect	Long-Run Effect	Short-Run Effect	Long-Run Effect	Short-Run Effect	Long-Run Effect
	(1)	(2)	(3)	(4)	(5)	(6)
(A) Thickness Measured at Occupation Level						
Treated × Low Thickness (Occupation)	0.38	0.15	0.41	0.19	0.36	0.09
	(0.03)	(0.03)	(0.06)	(0.06)	(0.06)	(0.05)
Treated × High Thickness (Occupation)	0.34	0.16	0.25	0.17	0.39	0.20
	(0.03)	(0.04)	(0.06)	(0.11)	(0.05)	(0.06)
No. of Observations	347976		78669		128385	
(B) Thickness Measured at Industry Level						
Treated × Low Thickness (Industry)	0.32	0.14	0.39	0.34	0.30	0.09
	(0.03)	(0.03)	(0.06)	(0.09)	(0.05)	(0.04)
Treated × High Thickness (Industry)	0.36	0.10	0.36	0.09	0.30	0.05
	(0.03)	(0.03)	(0.06)	(0.05)	(0.05)	(0.05)
No. of Observations	385038		89937		137475	
(C) Density of Local Labor Market						
Treated × Low Density	0.32	0.12	0.33	0.13	0.30	0.09
	(0.03)	(0.03)	(0.08)	(0.06)	(0.05)	(0.05)
Treated × High Density	0.33	0.14	0.40	0.17	0.31	0.09
	(0.03)	(0.03)	(0.07)	(0.06)	(0.06)	(0.04)
No. of Observations	347994		77463		126288	
(D) Local Unemployment Rate						
Treated × Low Unemployment	0.37	0.13	0.35	0.12	0.42	0.14
	(0.03)	(0.03)	(0.06)	(0.06)	(0.06)	(0.05)
Treated × High Unemployment	0.35	0.16	0.23	0.07	0.39	0.18
	(0.04)	(0.04)	(0.14)	(0.10)	(0.06)	(0.05)
No. of Observations	436437		119079		181521	

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 $1(\text{period}_k)$ with the covariate. Hires are counted if they are in the same 1-digit occupation group as the deceased. To calculate a specialization measure for the occupation of the deceased worker, we follow Bleakley and Lin (2012) and calculate returns to experience for each 5-digit occupation. We then use the estimated occupation-specific returns to experience to classify occupations into high- and low-specialization occupations based on a median split. All external labor market characteristics are measured at the commuting zone level based on median splits of the relevant measure. Thickness measured at the occupation level is used to categorize 5-digit occupation × commuting zone cells as thick or thin based on the relative share of workers in the 5-digit occupation in the commuting zone relative to the overall share of workers in that occupation in the labor market. Thickness measured at the industry level is defined analogously for the share of workers in the 3-digit industry × commuting zone level. Density of the local labor market refers to the number of workers in a commuting zone divided by that commuting zone's area. The unemployment rate is calculated as the number of unemployed workers in the commuting zone divided by the number of workers. Observations are weighted inversely by the number of incumbent workers at the firm of the deceased. Standard errors are clustered at the firm level.

Table A-3.9: Effects on Incumbent Worker Wages in Year $k = 0$ By Quarter of Death

Outcome:	Wage in Year $k = 0$
Treated × Death in July, August, September of $k = 0$	148.10
	(39.64)
Treated × Death in October, November, December of $k = 0$	84.47
	(41.28)
Treated × Death in January, February, March of $k = 1$	28.15
	(41.47)
Treated × Death in April, May, June of $k = 1$	-0.36
	(40.70)
No. of Observations	814,323
No. of Firms	63,926

Note: The table displays results of a difference-in-differences regression of wages in year $k = 0$ on treatment status interacted with dummies for the quarter of death of the deceased worker in the treated group. The positive and statistically significant coefficients for wage effects in year 0 of deaths that occur in Q3 or Q4 of $k = 0$ document that the positive wage effects in year $k = 0$ (see, e.g., Figure 2) are driven by deaths that occur in the same calendar year, as wages for most employees correspond to average wages calculated over a calendar year horizon so that deaths in, e.g., August will have an effect on average wages in that year. The table also demonstrates that deaths in the first quarter of the following calendar year do not have a statistically detectable effect on incumbent worker wages in the previous calendar year. Standard errors are based on 67,572 clusters at the worker death level. Observations are weighted inversely by the number of incumbent workers at the firm of the deceased.

Table A-3.10: Wages Effects in Firms with High vs. Low Wage Flexibility

	Incumbent Worker Wages	
	Short-run	Long-run
	(1)	(2)
Treated × Low Flex	99.32	102.07
	(72.00)	(80.49)
Treated × High Flex	182.54	55.10
	(64.96)	(71.14)
Treated × Low Flex × Same Occ	241.08	215.88
	(100.83)	(111.09)
Treated × Low Flex × Other Occ	108.42	137.55
	(145.55)	(164.80)
Treated × High Flex × Same Occ	191.67	39.27
	(98.13)	(107.29)
Treated × High Flex × Other Occ	87.58	-23.75
	(118.93)	(133.65)
No. of Observations	3,832,947	
No. of Firms	34,344	

Note: The table displays treatment effects on incumbent worker wages baed on difference-in-differences (DiD) regressions. Treated refers to the Post × Treated coefficient. Short-run effects refer to the DiD 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. We calculate wage rigidity or flexibility measures following Jäger et al. (2020). "High" wage flexibility is defined as an above median standard deviation of pre-period wage changes, implying less rigid wage setting policies of the firm. 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. Standard errors are clustered at the firm level.

Table A-3.11: Effects of Weekend Deaths

	Incumbent Worker Wages	
	Short-Run	Long-Run
	(1)	(2)
Treated x Weekend	277.86	265.44
	(68.10)	(74.75)
No. of Observations	2,057,130	
No. of Firms	19,709	
Main Results:		
Treated	186.53	106.76
	(35.78)	(39.30)
No. of Observations	7,328,907	
No. of Firms	63,926	

Note: The table reports treatment effects on incumbent worker wages based on difference-in-differences (DiD) regressions. Treated refers to the Post × Treated coefficient. Short-run effects refer to the DiD 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. Standard errors are clustered at the firm level.

Table A-3.12: Effects of Worker Death on Hiring and Retention

Dimension of Heterogeneity:	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
Hiring (all)										
Treated × Low	0.33	0.05	0.28	0.05	0.42	0.16	0.36	0.04	0.43	0.12
	(0.07)	(0.06)	(0.09)	(0.08)	(0.03)	(0.03)	(0.11)	(0.11)	(0.10)	(0.08)
Treated × Medium	0.41	0.18	0.44	0.20			0.32	0.06	0.41	0.18
	(0.03)	(0.03)	(0.04)	(0.03)			(0.06)	(0.05)	(0.04)	(0.03)
Treated × High	0.38	0.17	0.26	0.13	0.38	0.18	0.38	0.19	0.47	0.16
	(0.10)	(0.10)	(0.12)	(0.08)	(0.10)	(0.09)	(0.04)	(0.04)	(0.11)	(0.12)
Hiring (same occupation)										
Treated × Low	0.31	0.06	0.30	0.03	0.38	0.14	0.30	0.03	0.28	0.06
	(0.07)	(0.05)	(0.07)	(0.06)	(0.02)	(0.02)	(0.09)	(0.09)	(0.07)	(0.05)
Treated × Medium	0.36	0.15	0.39	0.16			0.28	0.07	0.39	0.17
	(0.02)	(0.02)	(0.03)	(0.02)			(0.05)	(0.04)	(0.03)	(0.02)
Treated × High	0.30	0.10	0.24	0.11	0.25	0.13	0.33	0.14	0.36	0.15
	(0.07)	(0.07)	(0.05)	(0.05)	(0.07)	(0.06)	(0.03)	(0.02)	(0.08)	(0.08)
Employment										
Treated × Low	-0.39	-0.28	-0.32	-0.08	-0.31	-0.13	-0.22	-0.22	-0.29	-0.07
	(0.09)	(0.12)	(0.11)	(0.16)	(0.04)	(0.05)	(0.14)	(0.23)	(0.14)	(0.18)
Treated × Medium	-0.33	-0.10	-0.29	-0.09			-0.40	-0.27	-0.35	-0.15
	(0.04)	(0.05)	(0.05)	(0.07)			(0.08)	(0.11)	(0.05)	(0.07)
Treated × High	-0.35	0.02	-0.54	-0.20	-0.52	-0.17	-0.44	-0.13	-0.26	-0.18
	(0.13)	(0.22)	(0.15)	(0.18)	(0.14)	(0.19)	(0.05)	(0.07)	(0.14)	(0.23)
No. of Observations	647,694		381,033		550,503		319,617		351,441	
Retention										
Treated × Low	0.0047	-0.0018	0.0085	0.0029	0.0048	0.0013	0.0135	0.0007	0.0027	0.0043
	(0.0032)	(0.0018)	(0.0042)	(0.0023)	(0.0014)	(0.0007)	(0.0055)	(0.0030)	(0.0041)	(0.0025)
Treated × Medium	0.0044	0.0017	0.0064	0.0013			0.0045	0.0006	0.0059	0.0006
	(0.0014)	(0.0007)	(0.0019)	(0.0010)			(0.0029)	(0.0016)	(0.0019)	(0.0010)
Treated × High	0.0014	0.0040	0.0000	0.0047	-0.0058	0.0026	0.0024	0.0033	0.0024	0.0024
	(0.0053)	(0.0030)	(0.0043)	(0.0025)	(0.0053)	(0.0031)	(0.0023)	(0.0012)	(0.0069)	(0.0038)
No. of Observations	5,164,830		2,990,207		4,397,610		2,607,115		2,770,030	
No. of Firms	63,926		39,299		54,962		33,092		36,219	

Note: The table shows results based on the difference-in-differences framework in equation (2). The outcome variable are all new hires and new hires within the same 5-digit occupation as the deceased. 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. 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). To calculate a specialization measure for the occupation of the deceased worker, we follow Bleakley and Lin (2012) and calculate returns to experience for each 5-digit occupation. We then use the estimated occupation-specific returns to experience to classify occupations as follows: occupations with returns to experience below the 20th percentile are classified as low specialization occupations, occupations with returns to experience between the 20th and 80th percentile are classified as medium specialization, and occupations above the 80th percentile of returns to experience as high specialization occupations. In the manager column, low refers to workers we identify as non-managers and high refers to managers. We measure the managerial status of the deceased's occupation as proxied by occupations requiring "complex specialist activities" (requirement level 3) or "highly complex activities" (requirement level 4) based on the 2010 Classification of Occupations. These occupations are characterized by managerial, planning and control activities, such as operation and work scheduling, supply management, and quality control and assurance and typically require a qualification as master craftsperson, graduation from a professional academy, or university studies (see *Klassifikation der Berufe 2010, Band 1: Systematischer und alphabetischer Teil mit Erläuterungen, Bundesagentur für Arbeit*). Observations are weighted inversely by the number of incumbent workers at the firm of the deceased. Robust standard errors in parentheses.

Table A-3.13: Summary Statistics on Hours per Week in Administrative Data and Structure of Earnings Survey

	All Workers		
	Administrative Data	Survey (Excluding Overtime)	Survey (Overtime)
Mean	33.63	30.62	0.29
Standard Deviation	10.71	12.53	1.49
	Women		
	Administrative Data	Survey (Excluding Overtime)	Survey (Overtime)
Mean	30.42	26.81	0.14
Standard Deviation	10.06	12.77	0.99
	Men		
	Administrative Data	Survey (Excluding Overtime)	Survey (Overtime)
Mean	34.52	34.24	0.42
Standard Deviation	10.71	11.15	1.83

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

Table A-3.14: Effects on Hours per Week and Incumbent Worker Wages

Outcome: Incumbent Worker Hours				
	Short-Run Effect	Long-Run Effect	Short-Run Effect	Long-Run Effect
Treated	-0.01	-0.11		
	(0.22)	(0.23)		
Treated × Same Occupation			0.44	0.29
			(0.27)	(0.30)
Treated × Other Occupations			-0.82	-0.87
			(0.32)	(0.31)
Outcome: Incumbent Worker Wages				
Treated	95.41	-38.08		
	(122.00)	(138.17)		
Treated × Same Occupation			294.58	44.9317
			(161.92)	(181.62)
Treated × Other Occupations			-288.31	-419.56
			(218.12)	(213.20)
No. of Observations	193,387			
No. of Firms	4,459			
No. of Observations (Same Occupation)			99,654	
No. of Firms (Same Occupation)			3,519	
No. of Observations (Other Occupation)			70,892	
No. of Firms (Other Occupation)			2,848	

Note: The table shows heterogeneity of the treatment based on the difference-in-differences framework in equation (2). The outcome variable are incumbent worker wages and hours per week among incumbent workers. The German Statutory Accident Insurance required all firms to report information on workers' hours of work as part of their administrative reporting processes in the time period from 2010 to 2015. Short-run effects refer to the treatment effects in year $k = 1$ post-death; long-run effects refer to the average treatment effects in years $k = 1$ through $k = 3$. 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. Observations are weighted inversely by the number of incumbent workers at the firm of the deceased. Standard errors are clustered at the firm level.

Table A-3.15: Individual-Level Summary Statistics (Hours Sample)

	Actual and Placebo Deceased Workers		Incumbent Workers	
	Treatment Group	Comparison Group	Treatment Group	Comparison Group
Age	49.92	49.93	41.95	41.83
	(8.51)	(8.52)	(11.64)	(11.54)
Female	0.13	0.13	0.23	0.23
	(0.33)	(0.33)	(0.42)	(0.42)
Earnings (€, 2010 CPI)	30368	29891	28421	28125
	(14205)	(14427)	(14086)	(13964)
Years of Education	10.35	10.35	10.52	10.53
	(1.36)	(1.38)	(1.52)	(1.54)
Tenure (Years)	6.90	6.77	5.79	5.75
	(2.99)	(2.99)	(3.28)	(3.30)
Hours per Week	34.83	34.73	34.29	34.15
	(10.73)	(10.99)	(11.30)	(11.35)
N	2,243	2,241	21,250	21,055

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. Hours per week differ from the overall averages in these firms because our final analysis sample restricts to workers who were full-time in the year before the death or placebo death.

Table A-3.16: Firm-Level Summary Statistics (Hours Sample)

	Treatment Group	Comparison Group
Total Number of Employees	14.73	14.74
	(7.26)	(7.30)
Number Part-Time Workers	2.85	2.71
	(3.19)	(2.95)
Number Apprentices	0.64	0.68
	(1.18)	(1.23)
Firm Age	10.01	9.97
	(1.39)	(1.42)
Primary Sector	0.02	0.02
	(0.14)	(0.15)
Secondary Sector (Manufacturing)	0.48	0.47
	(0.50)	(0.50)
Tertiary Sector (Service)	0.50	0.51
	(0.50)	(0.50)
N	2,243	2,241

Note: Standard deviations are reported in parentheses. All variables are measured in $k = -1$, the year before the actual or placebo death. Firm age refers to the number of years the establishment ID has been observed in the data.

Table A-3.17: Restricting to Stayers vs. Leavers

Outcome: Incumbent Worker Wages	Full Sample	At the Same Establishment	Not At the Same Establishment
Treated $\times$ $k = -3$	-3.69	49.60	-113.77
	(31.76)	(16.65)	(96.93)
Treated $\times$ $k = -2$	28.71	24.42	52.98
	(27.19)	(11.91)	(111.01)
Treated $\times$ $k = -1$	omitted	omitted	omitted
Treated $\times$ $k = 0$	67.37	67.37	omitted
	(25.37)	(25.36)	
Treated $\times$ $k = 1$	186.53	202.72	-91.82
	(35.78)	(29.09)	(149.13)
Treated $\times$ $k = 2$	152.26	210.50	-88.54
	(41.17)	(32.41)	(119.10)
Treated $\times$ $k = 3$	120.05	168.10	-68.46
	(45.90)	(36.15)	(106.40)
Treated $\times$ $k = 4$	76.11	194.48	-256.35
	(49.49)	(40.24)	(98.66)
Treated $\times$ $k = 5$	-1.17	210.85	-279.81
	(53.15)	(44.49)	(94.64)
No. of Observations	6,807,673	4,903,337	1,904,336
No. of Firms	63,926	63,926	60,773

Note: This table shows the earnings results for workers who are still in the same establishment in year k as they were in at time 0 (the time of the worker death) in column 2 (and are in the same spell, so excluding those who have left and returned), as well as wage changes in year k for workers who have left that establishment or not yet arrived (column 3).

Table A-3.18: Treatment Effects on Wages By Establishment Size

Outcome:	Incumbent Wages	
	Short-Run Effect	Long-Run Effect
Treated $\times$ (Employment $\leq$ 10)	273.01	162.57
	(73.82)	(80.35)
Treated $\times$ (10 $<$ Employment $\leq$ 20)	141.29	64.33
	(59.36)	(65.60)
Treated $\times$ (20 $<$ Employment $\leq$ 30)	32.17	-29.71
	(94.14)	(106.20)
No. of Observations	4,349,142	4,349,142
No. of Firms	44,170	44,170

Note: The table displays results of diff-in-diff specifications by initial establishment size. Observations are weighted inversely by the number of incumbent workers at the deceased's establishment.

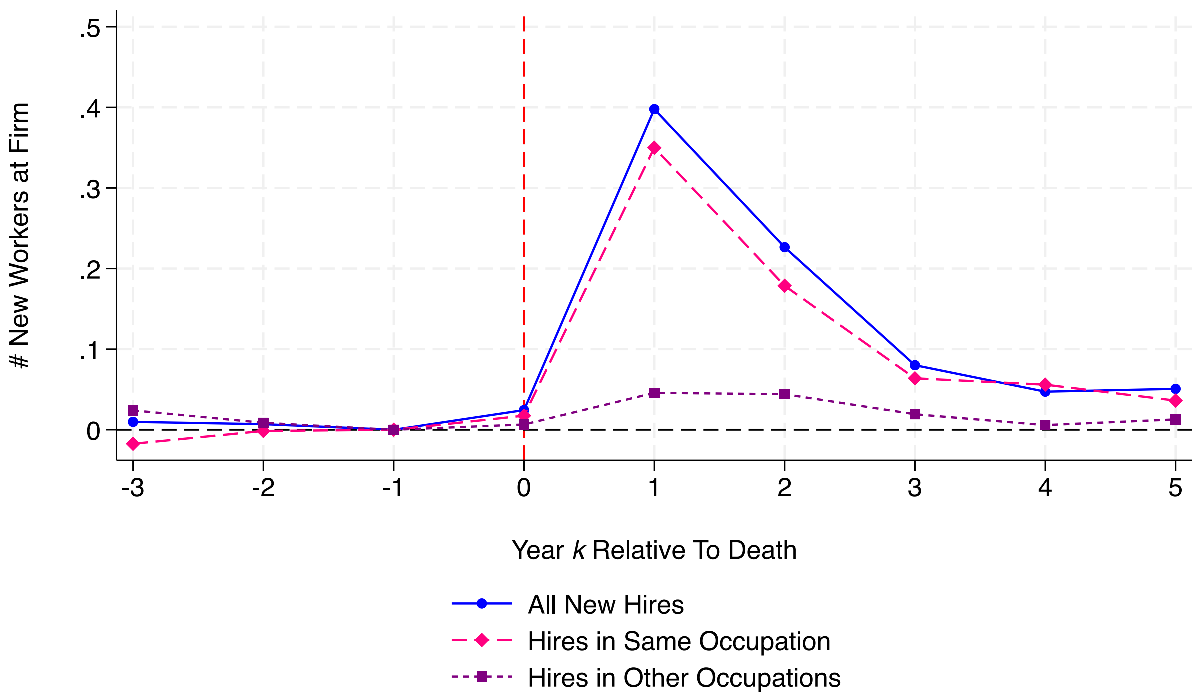
Table A-3.19: Estimation of Model Parameters and Implied Replacement Costs
Extensions to the Baseline Model

B. Extension to two worker types (by occupation):	
	Short-Run Estimation
$\gamma_{\text{same occ}}$	71672
$\lambda_{\text{same occ}}$	0.04
$\eta_{\text{same occ}}$	0.42
$w^m_{\text{same occ}}$	12364
$\overline{w}_{\text{same occ}}$	43249
$\gamma_{\text{other occ}}$	157601
$\lambda_{\text{other occ}}$	0.17
$\eta_{\text{other occ}}$	0.20
$w^m_{\text{other occ}}$	11948
$\overline{w}_{\text{other occ}}$	55530
α	0.5
$A_{\text{same occ}}$	1
$A_{\text{other occ}}$	1.61
ρ	0.95
ϵ	2
P^0	941560
Same Occupation Replacement Cost ($c'(\frac{N}{I})$)	66402
(Expressed as % of incumbent salary)	(207%)
Same Occupation Replacement Cost ($c'(\frac{N}{I})$)	115178
(Expressed as % of incumbent salary)	(367%)

Note: The occupation calibration draws on the two-type model and reports results additional results for the substitutability of workers across occupational boundaries. Marginal replacement cost uses the parameters from the same occupation cost function. The empirical estimates for new hire wages are 28127 in the same occupation and 27179 in other occupations; we scale down these wages by a constant proportional factor (a single free parameter), until $\epsilon = 2$. That scaling factor is 0.44; for a scaling factor of 0.85 we get $\epsilon = 1.001$, and for the empirical values (a scaling factor of 1) we would get an infeasible ϵ value less than 1. We discuss this issue more in Section 5.3. α, $A_{\text{same occ}}$, and ϵ are fixed, as is the ratio between $w^m_{\text{same occ}}$ and $w^m_{\text{other occ}}$ (the levels of $w^m_{\text{same occ}}$ and $w^m_{\text{other occ}}$ being pinned down by the scaling factor).

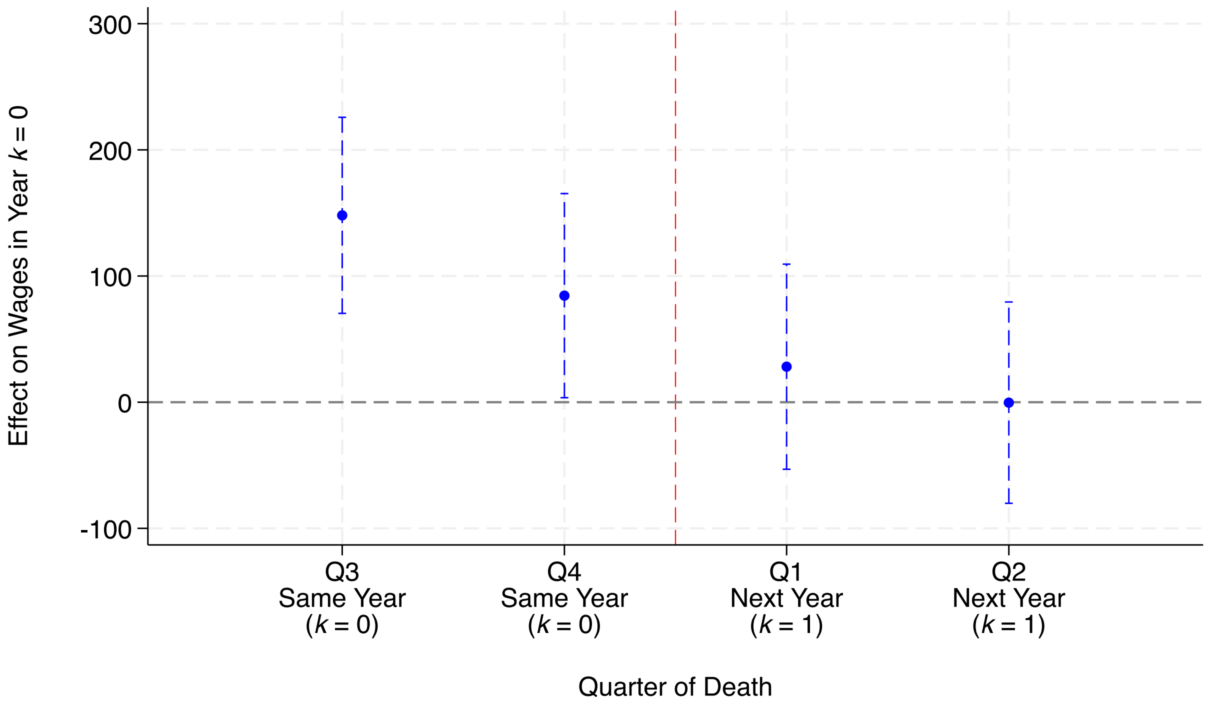
A-3.2 Additional Figures

Figure A-3.1: Decomposition of Effects of Worker Death on Hiring



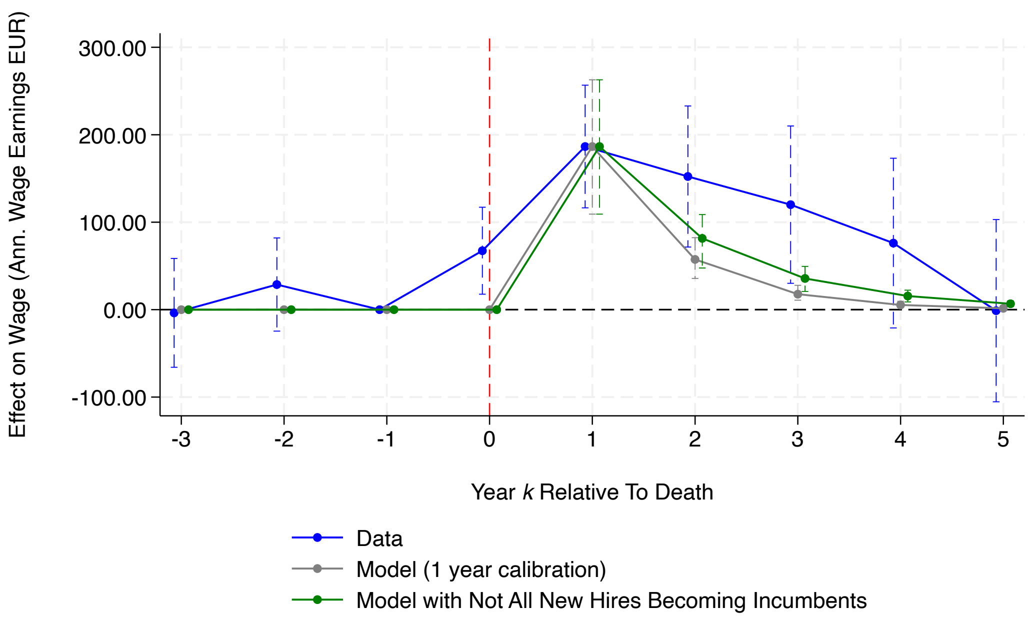
Note: The figure shows the treatment effect on hiring of new workers and decomposes the effect on total hiring (All New Hires) into hiring in the same 1-digit occupation as the deceased worker (Hires in Same Occupation) and hiring of workers into other occupations (Hires in Other Occupations). The treatment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

Figure A-3.2: Effects on Incumbent Worker Wages in Year $k = 0$ By Quarter of Death



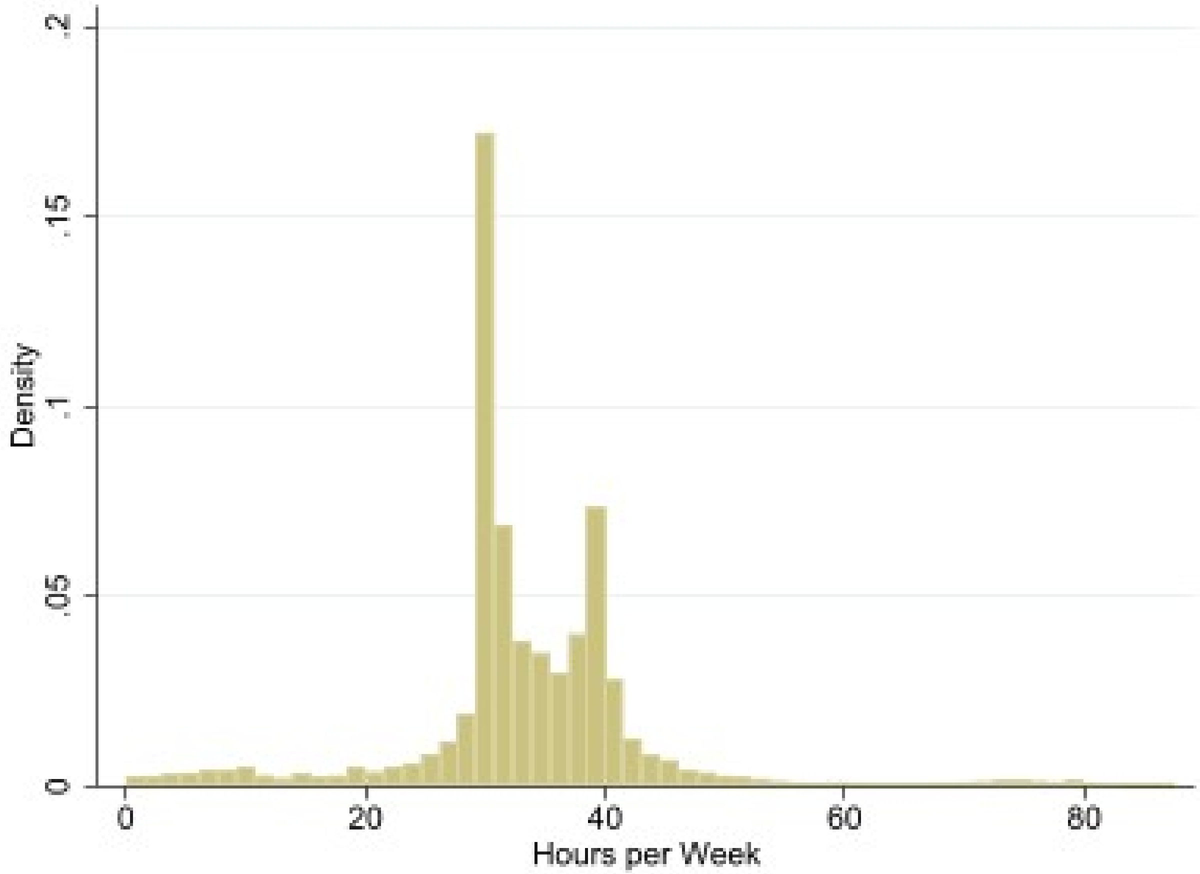
Note: The figure presents results of a difference-in-differences regression of wages in year k=0 on treatment status interacted with dummies for the quarter of death of the deceased worker in the treatment group. The positive and statistically significant coefficients for wage effects in year 0 of deaths that occur in Q3 (July, August, and September) document that the positive wage effects in year $k = 0$ (see, e.g., Figure 2) are driven by deaths that occur in the same calendar year, as wages for most workers correspond to average wages calculated over a calendar year horizon so that deaths in, e.g., August will have an effect on average wages in that year. The figure also demonstrates that deaths in the first quarter of the following calendar year do not have a statistically detectable effect on incumbent worker wages in the previous calendar year. Vertical lines denote 95% confidence intervals. See also Table A-3.9.

Figure A-3.3: Model Predicted Wages with New Hires Probabilistically Becoming Incumbents



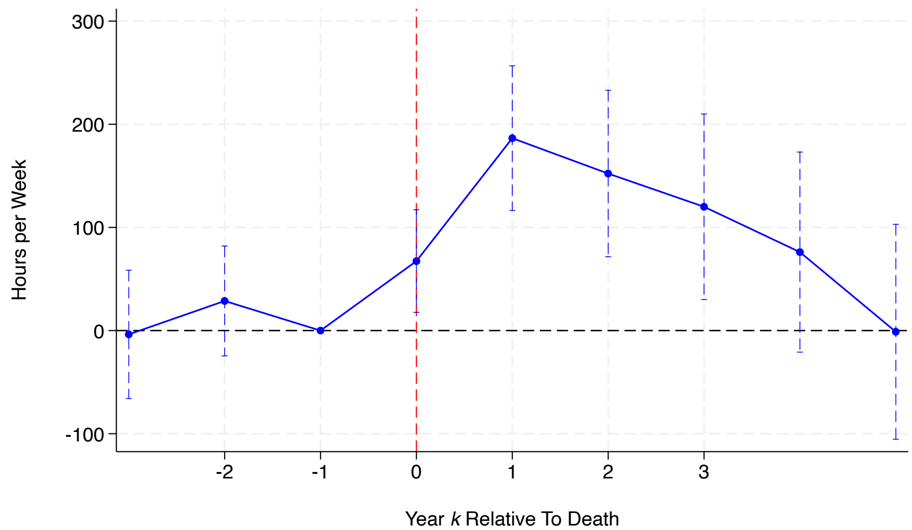
Note: The figure displays effects of worker deaths on several firm and incumbent worker outcomes. The blue lines report the measured effect in the data. The gray and green lines report predictions based on an estimation of the modified Kline et al. (2019) model using the method of moments. See Section 5 for more details. Confidence intervals on the values from the data are clustered at the matched-pair level. Confidence intervals on the model values are 95% bootstrap confidence intervals, computed by drawing targets with replacement from the matched firm pairs, recomputing the target wage, retention and hiring moments on the bootstrap sample, and then recalibrating the model to the new targets, for 400 bootstrap draws. Wages are the model-implied coworker event study differences between a treatment firm that experienced a worker death and a control firm that remained in steady state, accounting for the fact that some coworkers exit the treated firm due to better outside offers, so the magnitude of the change in offered wages or hours by the firm is larger than the realized change following all coworkers, including those who leave.

Figure A-3.4: Distribution of Hours Per Week



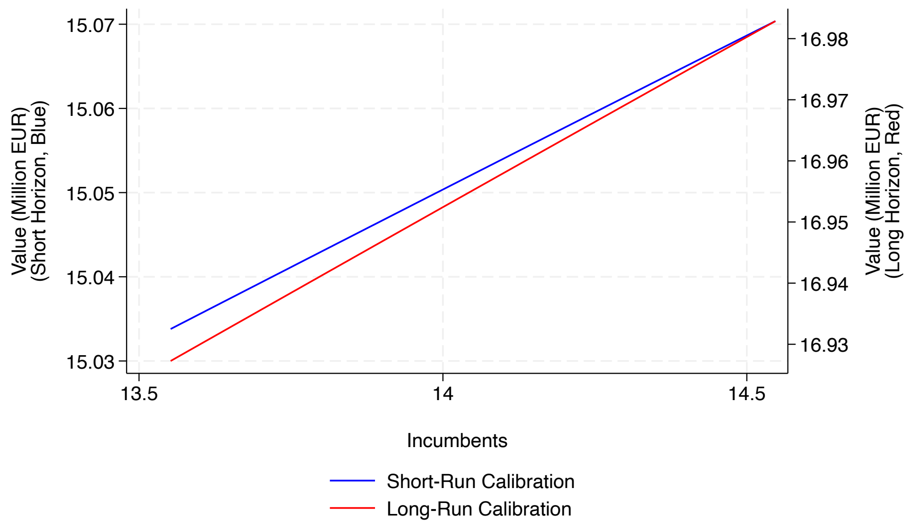
Note: The figure shows a histogram of hours per week based on administrative data from the German Statutory Accident Insurance, which required all firms to report information on workers' hours of work as part of their administrative reporting processes in the time period from 2010 to 2015. We drop outlier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

Figure A-3.5: Effect of Worker Deaths on Incumbent Worker Hours



Note: The figure displays regression coefficients and associated 95% confidence intervals for the difference between incumbent worker in the treatment and comparison group, i.e., the β_k from equation (2). The coefficients in $k = -1$ are normalized to zero. The outcome variable are the reported hours per week of incumbent workers. Incumbent workers are defined as full-time coworkers of the deceased or placebo deceased in the year before death. The data on hours per week stem from administrative data from the German Statutory Accident Insurance, which required all firms to report information on workers' hours of work as part of their administrative reporting processes in the time period from 2010 to 2015. We drop outlier observations below the 1st percentile (zeros) and above the 99th percentile of hours per week.

Figure A-3.6: Model Estimated Value Function



Note: The figure displays the estimated value function over the region for which it is valid, bounded above by I^*, the steady state level of incumbents, which we find to be 14.55, and below by $I^* - 1$, which is the level of incumbents a treated firm experiences after a death. The value function is subtly concave over this region: extrapolating from the derivative at $V'(I^* - 1)$ would lead to a 3% overestimate of $V(I^*) - V(I^* - 1)$